

The Dynamics of Applied Anthropology in the Twentieth Century:  
The Malinowski Award Papers

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## References Cited

- Baber, Willie  
1999 St. Clair Drake: Scholar and Activist. In *African-American Pioneers in Anthropology*. E. Harrison and Faye V. Harrison, eds. Pp. 184–204. Urbana: University of Illinois Press.
- Wax, Murray L.  
1976 Introduction to the Plenary Address, 35th Annual Meeting of the Society for Applied Anthropology, St. Louis, Missouri. *Human Organization* 35: 264–265.

## About the Editor

Thomas Weaver earned a B.A. (1955) and an M.A. (1960) in anthropology at the University of New Mexico. His Ph.D. in anthropology is from the University of California at Berkeley (1965). After teaching in medical schools at Kentucky (1965-67) and Pittsburgh (1967-69), he was director of the Bureau of Ethnic Research (now called the Bureau of Applied Research in Anthropology) at the University of Arizona from 1969 to 1977. In January 2002 he retired from the University of Arizona, where he had held the rank of Professor of Anthropology since 1975. His publications include the edited volumes, *To See Ourselves: Anthropology and Modern Social Issues* (Glenview, IL: Scott, Foresman and Company, 1973), *Indians of Arizona* (Tucson: University of Arizona Press, 1974), and *Handbook of Hispanic Cultures in the United States: Anthropology* (Houston: Arte Public Press, University of Houston, 1994), and a book in Spanish, *Viente Siglos de Adaptacion Cultural en el Suroeste Grande* (Twenty Centuries of Cultural Adaptation in the Greater Southwest), *Handbook of Indigenous Cultures in the New World*, Volume 20 (Madrid: Mapfre Publishing Company, 1992). He has also published numerous articles on urban anthropology, policy studies, border studies, and applied anthropology, and he has served as a consultant for the World Bank, the Mexican government, and other governmental agencies, courts, and Indian reservations. His current research interests include northern Mexican Indians and North American Indians; the political economy of forestry; policy, organizational culture, development, and change; and the articulation of modes of production in Mexico, Chile, Argentina, and Spain. He has served in many professional offices including as president of the Society for Applied Anthropology and the Southwestern Anthropological Association, and as first president of the Association of Latina and Latino Anthropologists.



# **Chapter 1**

## **The Malinowski Award and the History of Applied Anthropology**

**Thomas Weaver**

April 7, 2000, marked 116 years since the birth of Bronislaw Kasper Malinowski and more than half a century since his death in 1942 at the relatively young age of 58 years. The Society for Applied Anthropology (SfAA) paid homage to his work in applied anthropology by naming a prize in his honor in 1950. Since 1973, when the award was reestablished after a hiatus of twenty years, each awardee has presented a talk, most of which were published in the society's journal, *Human Organization*. In this collection we present all the talks that exist in written form, as well as a biographical essay on each awardee and on Malinowski himself, as a tribute to the awardees and to Malinowski and as a record of numerous high points in the history of applied anthropology.

Judging from the achievements of the recipients, the Malinowski Award is the most prestigious award in the world of applied social science and ranks among the top awards in any field. In this chapter we examine some of the characteristics of the awardees and the relationship of their careers and presentations to the history of applied anthropology. We also examine the relationship of the awardees to Malinowski, personally and in the context of applied anthropology. To what extent were the awardees instrumental in developing applied anthropology? And to what extent was their applied anthropology parallel to that of Malinowski?

### **The Award**

The announcement of a Malinowski Award in the fall 1950 issue of *Human Organization*, the major publication of the SfAA, described Malinowski as “an original member of the Society for Applied Anthropology and, before his death, one of its strongest supporters” (Anonymous 1950:1). An anonymous donor had provided \$100 for the Class A First Prize and \$50 for the Class A Second Prize (both for professionals) and also \$50 for a special student award. Entrants were required to submit a previously unpublished paper that represented the results of fieldwork in the form of “concrete cases” exhibiting technological, environmental, or other change, or explaining the purpose of the introduced change, the existing situation, and the consequences. The spring 1952 issue of the journal announced the receipt and publication of the Class A Malinowski Award (Anonymous 1952). Henry F. Dobyns received First Prize (Dobyns 1951) and Second Prize went to Leonard R. Sayles (Sayles 1952). The editorial noted, “the editors were unable to award the Special Student Prize to any of the papers submitted” (Anonymous 1952:3). The expectations concerning papers submitted were clarified, but, perhaps

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because of the turnover of officers and board members from 1952 to 1968, neither the professional nor the student award was presented for two decades.

In 1969 discussions about establishing an award honoring an outstanding applied social scientist arose at the SfAA Executive Committee meeting held during the New Orleans meetings of the American Anthropological Association. Some members remembered a distinguished award, but no one could remember its name. After some additional research, the SfAA Executive Committee, meeting in Boulder in 1970, revived the Malinowski Award for professionals, to be presented for significant accomplishments over time, rather than for a particular paper. It has been presented every year since 1973. (An SfAA student award was reestablished in 1990 with the Peter K. New Award.)

It was the understanding in 1970 (mistaken perhaps) that Dobyns and Sayles had received the student award; Dobyns, at least, was a student at the time he received the award. Gonzalo Aguirre Beltrán, winner of the Malinowski Award in 1973 after it had been reconstituted as an award for sustained accomplishment rather than a single paper, came to be considered the first Malinowski Award winner. The custom of inviting the awardee to present a plenary address commenced in 1973 with the presentation of the award to Aguirre Beltrán. A medallion has been presented to the awardee since 1991.<sup>1</sup>

The guidelines for selecting the Malinowski Award winner have changed over time. The current mandate stipulates that the award should be presented to an outstanding social scientist in recognition of efforts to understand and serve the needs of the world's societies, a person who has actively pursued the goal of solving human problems using the concepts and tools of social science. More specifically, the SfAA sets forth these criteria for selection:

1. The nominees should be of senior status, widely recognized for their efforts to understand and serve the needs of the world through the use of social science.
2. The nominees should be strongly identified with the social sciences. They may be within the academy or outside of it, but their contributions should have implications beyond the immediate, the narrowly administrative, or the political.
3. The awardee shall be willing and able to deliver an address at the annual meeting of the Society for Applied Anthropology.
4. The nominees should include individuals who reside or work outside the United States.

Current practice is for the SfAA Board of Directors to appoint a committee of four or more members and to rotate membership every three years. Nominations are made by the general society membership. Each nominee is kept on the roster for five years, after which he or she must be renominated to continue to be considered.

### **The Awardees**

Although the nominees for the award can be from any social science, as of 2000 the award has gone mostly to anthropologists, with one each to an economist (Gunnar Myrdal), a physician-anthropologist (Gonzalo Aguirre Beltrán), and a psychiatrist-anthropologist (Alexander Leighton), and two to sociologists (Everett Hughes and Michael Cernea). (See Table 1.) All the anthropologists have been sociocultural anthropologists, except for Juan Comas who

is a physical anthropologist. Most of the earlier recipients were full-time academics and came from a generation when applied anthropology was in a developing stage. Six awards have gone to women (Laura Thompson, Elizabeth Colson, Margaret Lantis, Margaret Clark, Bea Medicine, and Beverly Hackenberg), and one each has gone to an American Black (J. G. St. Clair Drake) and an American Indian (Medicine). By country, the United States has been home for most of the awardees, with two from Mexico (Aguirre Beltrán and Comas), two from Great Britain (Raymond Firth and Ronald Frankenberg), and one each from Sweden (Myrdal), China (Fei Xiaotong), and Spain (Claudio Esteva Fabregat).

**Table 1. Characteristics of Malinowski Award Winners**

Award Year	Awardee	Discipline	Citizenship	Birth Year
1973	Gonzalo Aguirre Beltrán	Anthropology & Medicine	Mexico	1908
1974	Everett C. Hughes	Sociology	United States	1897
1975	Gunnar Myrdal	Economics	Sweden	1898
1976	Edward H. Spicer	Anthropology	United States	1906
1977	Sol Tax	Anthropology	United States	1907
1978	Juan Comas	Anthropology	Mexico	1900
1979	Laura Thompson	Anthropology	United States	1995
1980	Fei Xiaotong	Anthropology	China	1910
1981	Raymond Firth	Anthropology	Great Britain	1901
1982	George M. Foster	Anthropology	United States	1913
1983	Omer C. Stewart	Anthropology	United States	1908
1984	Alexander H. Leighton	Anthropology & Psychiatry	United States	1908
1985	Elizabeth Colson	Anthropology	United States	1917
1986	Philleo Nash	Anthropology	United States	1909
1987	Margaret Lantis	Anthropology	United States	1906
1988	Frederick L. K. Richardson	Anthropology	United States	1909
1989	Lauriston Sharp	Anthropology	United States	1907
1990	J. G. St. Clair Drake	Anthropology	United States	1911
1991	Conrad Arensberg	Anthropology	United States	1910
1992	M. Margaret Clark	Anthropology	United States	1925
1993	Ronald Frankenberg	Anthropology	Great Britain	1929
1994	Claudio Esteva Fabregat	Anthropology	Spain	1918
1995	Michael M. Cernea	Sociology	United States	1933
1996	Bea Medicine	Anthropology	United States	1924
1997	Ward Goodenough	Anthropology	United States	1919
1998	Robert A Hackenberg	Anthropology	United States	1928
	Beverly H Hackenberg	Anthropology	United States	1928
1999	Thayer Scudder	Anthropology	United States	1930

Many Malinowski Award recipients are recognized today primarily as academic rather than as applied anthropologists.<sup>2</sup> These include Firth, Omer Stewart, Colson, St. Clair Drake, and Medicine. However, each contributed to applied social science and this is demonstrated in their acceptance addresses. Firth believed that his theoretical work led directly to the solution of social problems in the Pacific. Stewart was forceful in defending the rights of followers of the Native American Church who used peyote as a sacrament, and he researched and testified for American Indians in their land-claims legal cases. Colson's work on resettlement problems in Africa contributed to the World Bank's resettlement policy. St. Clair Drake and Medicine promoted Black and American Indian rights and education.

Today an increasing number of anthropologists can be found in full-time employment in nonacademic settings, such as in government or international agencies. Among the awardees,

Aguirre Beltrán, Philleo Nash, Richardson, and Thompson share with Cernea long periods of mainly agency-related applied work with only occasional teaching in university settings. Lantis served for many years in government agencies until her full-time employment at the University of Kentucky. George Foster was a public servant for almost ten years in international agencies funded by the U.S. government. Comas, Edward Spicer, Leighton, Colson, Hughes, Stewart, Ward Goodenough, and Lauriston Sharp were involved in varying periods of government service.

A number of the awardees played prominent roles in anthropological organizations. Four were among the founders of the Society for Applied Anthropology: Conrad Arensberg, Richardson, Thompson, and Sharp. Eight of the fifty past presidents of the society have received the Malinowski Award. They are Hughes, Sol Tax, Stewart, Lantis, Richardson, Nash, Arensberg, and Goodenough. Hughes was also president of the American Sociological Association. Spicer served the society as vice president. Awardees who have been president of the American Anthropological Association (AAA) are Tax, Foster, Spicer, Arensberg, and Clark. Editors of the *American Anthropologist* among the awardees include Spicer, Tax, and Goodenough, but only Arensberg has served as editor of *Human Organization*. Tax was founder and first editor of *Current Anthropology*, and he inspired the founding of the SfAA's career-oriented publication, *Practicing Anthropology* (Wolfe 1979).

The awardees' average age at the time of receipt of the Malinowski Award was 75 years (N = 28). The most recent awardees have tended to be younger than the average at the time of receipt of the award, with awardees from 1993 through 1999 averaging 69 years of age. Still, it has clearly been a "senior" award.

The recipients of the Malinowski Award have received many other honors, some coming before and some after the award, thus validating the society's choices. Myrdal received the Nobel Prize for economics. Four awardees were or are members of the National Academy of Science (Spicer, Foster, Colson, and Goodenough), and four were or are members of the American Academy of Arts and Sciences (Myrdal, Foster, Colson, and Goodenough). Spicer, Leighton, and Goodenough are or were members of the American Philosophical Society. The American Anthropological Association has conferred the Distinguished Lecturer citation on Colson and Firth, and the AAA Distinguished Service Award has gone to Spicer, Foster, Nash, Medicine, and Goodenough. Cernea received the Solon T. Kimball Award for Public and Applied Anthropology from the AAA. The Society for the Anthropology of Work, a unit of the AAA, created the Conrad Arensberg Award, with Arensberg named its first recipient in 1991. Malinowski Award recipients who have had prizes or awards named after them are Tax and Spicer (by the Society for Applied Anthropology), Comas (by the American Association for Physical Anthropology), Stewart (by the High Plains Society for Applied Anthropology), Sharp (by his department), and Clark (by the Gerontological Society).

## **The Awardees' Careers**

The recipients of the Malinowski Award were mostly trained in the earliest theoretical paradigms of the first half of the twentieth century—historicism, acculturation, functionalism, and culture and personality. The earliest of our American recipients were trained in historical diffusionism by first generation Boasians. This was certainly true of the early University of California at Berkeley (UCB) graduates (Thompson, Lantis, Stewart, and Foster), although they espoused more recent theories, such as culture and personality, acculturation, and functionalism,

in their later works. The most recent UCB graduate (Clark) reflects in her work even later theories that had begun to filter into medical anthropology, such as cultural ecology. Those who were educated at the University of Chicago followed a more Radcliffe-Brownian functionalism (Tax, Spicer, Nash, St. Clair Drake, and perhaps Hughes), although they too also adopted later frameworks. The four Harvard graduates (Sharp, Richardson, Arensberg, and Colson who graduated from Radcliffe) were influenced by W. Lloyd Warner's type of functionalism overlaying historicism. This is not as clear with Colson who left for Africa soon after her doctorate. St. Clair Drake was also influenced by Warner at Chicago and was directed by Warner in a community study in Wales (Bond 1988:773). Two of the five Chicago awardees (Hughes and St. Clair Drake) followed in part the urban sociology paradigm pioneered there. Fei, who received a doctorate from the London School of Economics, reflected in his work the remains of nineteenth century evolutionism.

Several awardees were involved in the Bureau of Indian Affairs (BIA) during the years John Collier led the agency from 1934 to 1945. Collier had become interested in the potential use of anthropology in his earlier work with Pueblo Indians in New Mexico. Employment for anthropologists was provided in BIA-sponsored or influenced projects such as the personality and culture studies directed by Thompson and in the War Relocation Authority (WRA) project in which Spicer, Leighton, Colson, Lantis, Thompson, and Arensberg were involved. Collier encouraged the use of anthropologists and social scientists in WRA and other non-BIA projects, and he was instrumental in pan-American indigenous policy matters, including the founding of the Inter-American Indigenous Institute. In the last activity he worked with Mexican anthropologists and Malinowski Award winners Comas and Aguirre Beltrán.

During the same era pioneering studies in industrialism were undertaken, first at Harvard, then at Chicago, led by W. Lloyd Warner.<sup>3</sup> Arensberg and Richardson were pioneers in the development of industrial studies in the U.S. in the 1940s, and Esteva Fabregat began industrial studies in Mexico and pursued these interests later in Spain. The studies by Hughes of real estate and industry are some of the earliest, but they come out of urban sociology rather than the work of Warner. Contributions by Lantis, Arensberg, and Richardson on leadership and organization are relevant to industrial studies. Frankenberg also conducted industrial studies and studies of mining unions.

The involvement of anthropologists in projects related to World War II is truly a landmark in the history of applied anthropology, and it is no accident that the Society for Applied Anthropology was founded during the war. The vast majority of anthropologists alive at the time participated in some way in the war effort. This concentration of anthropological talent remains unique in the history of anthropology, perhaps only challenged by the John Collier years of the Bureau of Indian Affairs and by the poverty program projects of the 1960s. Of those awardees that were of age to participate in World War II, only Tax seems not to have been involved in any related projects. Leighton, Spicer, Thompson, Colson, Lantis, and Arensberg changed policy in the treatment of interned Japanese Americans. Nash, Lantis, and Goodenough did much to improve the lives of those affected by the war in the U.S., in the U.S. Army, and in the Pacific. Leighton and Arensberg were commissioned officers in the navy and army, respectively, and Leighton worked in the Foreign Morale Analysis Division of the Office of War Information. Awardees involved in intelligence work included Nash, Stewart, and Goodenough (in the armed forces), Arensberg (army), Richardson (State Department), and Thompson and Goodenough (naval research activities). Sharp served in the Southeast Asian Division of the State Department.

Other landmark events of this era in applied anthropology with awardee participation include the Malinowski seminar at the London School of Economics (LSE), later directed by Firth, where many applied anthropologists and administrators trained and where others like Colson presented reports of their work. Colson's research was part of the Rhodes-Livingstone Institute then doing pioneer work in application in Africa. Also during the war, in 1943, the Institute of Social Anthropology was created, where Foster worked for ten years influencing health and other policies in Mexico and Latin America. After the war awardees participated in matters relating to the Indian Land Claims Commission (Stewart), the Cornell program in applied anthropology originated by Sharp (Leighton, Spicer, Goodenough, and Robert Hackenberg), the Fox Project and action anthropology (Tax and Sharp), and the development of resettlement policy (Colson, Thayer Scudder, and Cernea). Other postwar programs in which the awardees were involved include the Peace Corps (St. Clair Drake) and the Poverty Program of the Office of Economic Opportunity (Spicer).

Clark, Cernea, Frankenberg, the Hackenbergs, and Scudder, the awardees born and trained latest in the twentieth century, reflect in their careers the changes and emergent issues that developed after the 1970s. These include the tilt of applied anthropology away from the academy toward practice; new and expanded interests in preparing people for applied roles; the extension of social scientists' interests first to policy advising, then to policy activism; and the increasing institutionalization of applied social science roles in domestic and international agencies (Art Gallaher, Jr., e-mail to author, March 25, 1998). Other late twentieth century trends reflected in the work of Malinowski Award winners are concerns for ethics and ethnic welfare, the creation of separate programs and societies for medical and educational anthropology (Foster, Clark, Frankenberg, and the Hackenbergs), and the development of special foci within applied anthropology through journals, bulletins, and societies for practicing anthropology (Weaver n.d.).

Indigenous policy is a theme that marks the contributions of several of our Malinowski Award winners. The first recipient of the Malinowski Award, Aguirre Beltrán, became undersecretary of education and was director of the National Indian Institute of Mexico for almost three decades. The second recipient from Mexico, Comas, was secretary general of the Inter-American Indigenous Institute. With Manuel Gamio, then the director of the institute, he developed an international indigenous policy. Nash also helped shape and direct indigenous policy and affairs as U.S. Commissioner of Indian Affairs. Other awardees who contributed to specific areas in indigenous policy were Spicer in Indian community development, Lantis in Inuit health, Stewart in peyotism and religion, Tax in indigenous rights, Thompson in education, and Medicine in Indian rights and education. These contributions by American anthropologists, while significant, occurred in a context in which anthropologists were in the minority, in contrast with the situation in Mexico where Indian policy was directed and staffed almost exclusively by applied anthropologists until recent years.

Health policy is another area to which awardees contributed. Lantis' work in federal health agencies constitutes an important contribution to policy formation. As a physician and director of Indian Affairs in Mexico, Aguirre Beltrán gave priority to problems of health and nutrition. Comas' anthropometric studies of indigenous groups in Mexico led to his concern with health and other social problems accompanying regional economic development and with the role of powerful elites in contributing to the perpetuation of these problems. Foster contributed to changes in international health policy in Latin America and elsewhere and, with Barbara Anderson, coauthored an important text in medical anthropology (Foster and Anderson 1978).

Richardson conducted many studies of medical institutions, but, like Lantis, did not publish much of this material. In both cases, this unpublished work was reflected in other publications, lectures, and public policy. Leighton contributed to health policy through his ethnopsychiatric and culture and personality interests and his health survey in Nova Scotia.

Other awardees provided important information and advise to health and other policymakers. The work of the Hackenbergs changed health policies involving chronic diseases in the Southwest U.S., Central America, and the Philippines where they broke ground with the initiation of a community health worker program. Clark contributed to policy for the aged. Frankenberg focused on AIDS and how anthropologists can best help in developing solutions to this problem. Cernea used information from Colson and Scudder's projects in Africa and the work of other anthropologists to formulate resettlement and indigenous policies in the World Bank. Esteva Fabregat was instrumental in changing education, language, and administrative policies in Spain. Contributions to industrial studies, such as those by Arensberg, Richardson, Esteva Fabregat, and Frankenberg, had broad implications for corporate and labor policy.

Most of the recipients of the award have been directly or indirectly concerned with the problem of inequality. In this effort they join the likes of Franz Boas, Ruth Benedict, Margaret Mead, and Ashley Montagu, to mention only the most prominent social scientists who have addressed inequality. Comas participated at the international level in drafting proposals and documents with wide application. One of the themes of Myrdal's address was the fight against inequality, following upon his major contribution to this battle, his well-known book *An American Dilemma* (1944). In a life-long battle against racism and inequality, Tax acted on behalf of American Indians as spokesperson, advocate, and organizer of conferences and organizations.

As advisor to two presidents, Nash influenced policy changes aimed at eliminating racism in the armed forces and was behind the creation of the Commission on Civil Rights. St. Clair Drake, Medicine, Stewart, and Sharp fought long and hard to improve education and living conditions for American minorities and persons from the Third World. Fei challenged old ideas in organizing anthropology and theory to support minorities by providing research and suggesting policy changes in communist China, sometimes at the expense of his political standing. Hughes was involved in the study of race relations and the betterment of ethnic groups. Aguirre Beltrán long ago called attention to the hidden or little-acclaimed populations of Blacks in Mexico. This generation of Malinowski Award winners raised the consciousness of succeeding generations to an interminably unsolved problem.

### **The Malinowski Award Presentations**

The Malinowski Award speeches provide an interesting reflection on issues and events during the formative years of applied anthropology in the U.S., Great Britain, and Africa. Most of the awardees focused their talks on their own field of study. Whether the awardees shared new perspectives on past work or presented new ideas, their talks allow us to glimpse snatches of history in the making as they recount their participation in landmark projects and events. Themes that thread through the acceptance addresses include such topics and arenas for applied work as the association between theory and practice; policy anthropology; the importance of interdisciplinary perspectives; shared paradigms in development anthropology; industrial studies; indigenous policy; medical anthropology; and interests in racism and inequality.

The early recipients were adamant in their addresses about the necessity for a guiding theory in applied anthropology and for a place for policy in this context. By taking this stance they followed an orientation similar to that of Malinowski in his work in Africa. Myrdal, one of the first Malinowski Award recipients, made excellent observations about the place of policy involvement, interdisciplinary cooperation, and development studies (1975). Spicer's laudatory comments on Goodenough's use of applied data for theory building are particularly relevant in this context (Spicer 1976:134, cited in Cernea 1995:348). Of special note is Spicer's insistence on the need for policy involvement by anthropologists, a view previously raised by Myrdal. Spicer's experiences in the WRA were instrumental in forming his perspective on policy. Leighton (1984), who also served in the WRA, also addressed the issue of applied theory with the same emphasis on the importance of policy. Firth (1981), Colson (1985), Fei (1980), and the Hackenbergs (1999) provide statements or observations along this line. Cernea's presentation (1995) provides one of the best recent examples of attempts by social scientists to address the problem of theory and application. Any attempt to produce a theory of practice must consider the works of these people as a base.

Missing in the awardees' discussion of theory and application are several theories that are commonly mentioned in this context today. The most obvious omission is modernization theory. This theory underlies all applied anthropology, even if applied anthropologists have different orientations (if not objectives) than do government and development agents, among whom the model is pervasive. Although there are a few references to multinational and global frames of reference, none of the award winners refers to world-system, dependency, neo-Marxist, or critical paradigms in their addresses. There are no references to Gramsci, Levi-Straus, Althusser, Bourdieu, or Foucault, for example, and the few references to Marx are critical or dismissive. Frankenberg and St. Clair Drake are the only awardees recognized as Marxists.

Since most of our recipients were involved in interdisciplinary research and cooperation, it is perhaps not surprising that the interdisciplinary nature of application is highlighted in many of the Malinowski Award lectures, especially in the context of economic development. Today, development is a major focus of anthropology, both in publications and in the employment of non-academics. Interestingly, this is a topic that was addressed in the earliest award lectures. Myrdal was critical of economists and anthropologists alike in the management of development work and called for greater interdisciplinary cooperation and involvement in policy and decision making (1975). Cernea's (1995:344–345) position on the necessity for anthropologists to become more involved with economists and others in development work reflects the earlier call by Myrdal for social scientists to change the development model. Others who were strong advocates for interdisciplinary cooperation were Spicer (1976), Leighton (1984), and Goodenough (1999).

Development studies usually have an international focus, as illustrated by the work of Sharp (Weaver 2002b), St. Clair Drake (Weaver 2002a), and the Hackenbergs (1999) in Southeast Asia and Africa, all discussed in the presentations. Some of our lecturers, however, worked in domestic community settings. Spicer, for example, worked in community development with the Tohono O'odham with a focus on the economic programs that accompanied the civil rights awakening in the U.S. The Hackenbergs (1999) continued this involvement in the 1950s and 1970s with the Tohono O'odham. The development of action anthropology by Sol Tax, although not mentioned in his Malinowski Award address (1977), falls into the category of community development. Fei (1980) takes the concept of development to a higher plain by suggesting the introduction of results and policies into national policymaking bodies to improve the welfare of local communities and minorities.



A concern for policy and influencing policymakers is revealed in the presentations of several of our awardees. Chief among them is the clear exposition by Myrdal; much of his work could serve as a good background for recent forays into this field. Myrdal was one of the first social scientists to link social theory, policymaking, change, and development. He saw policy as a variable constricted by and reacting to exogenous conditions and changes. He foreshadowed the ideas of Lasswell and others who developed policy science as a discipline. He believed that social science was essentially a political science and that practical conclusions are important for theoretical formation as well as for problem solving (Myrdal 1975:328; 1944:1045). Spicer clearly mirrors Myrdal's earlier conclusions and provides groundbreaking commentary on policy issues in the WRA. His experience in the WRA taught him that students must be trained in application while they are in graduate school. An important part of the training he proposes is in dealing with administrators and policymakers and in the study of the policy process (Spicer 1976:341). He believed that training should include case studies focused on regionally centered problems and involve comparative policy-program analysis and design.

Firth provided an interesting perspective on policy issues that is in line with the statements by Myrdal and Spicer. Firth was concerned with orienting anthropological research to public education (Firth 1981:193). He cautioned that anthropologists are often "unwilling to learn the language of the technicians among whom they work, too rigid to see the problem from the practical person's angle as something demanding a decision and action, perhaps too concerned with disturbance to the few to envisage the benefits to the many" (Firth 1981:198). One of the risks faced as a result of calling attention to human factors in unexpected ways is that anthropology may become "the uncomfortable science" in the eyes of the public (Firth 1981:198).

### **The Awardees and Malinowski**

A review of Malinowski's contributions to practicing anthropology (discussed in more detail in Chapter 2 of this volume) demonstrates his concern for the welfare of indentured peoples in Africa. His assessment of the conditions under which aboriginal folk lived, with the imposition of foreign laws, values, and economies, showed that the natives did not fare well under colonial rule. His concept of culture change called for a holistic study of the interactions of the conquered and the conqueror—interactions that left societies in different stages of adaptation and non-adaptation. He raised themes that were to pervade the field of applied anthropology, such as policy and policymaking; research in support of the native; concern with racism, poverty, education, and physical conditions; and the relationship between theory and practice. Malinowski was influential during his lifetime in directing applied behavioral science, especially after his many visits to the United States and his ultimate residence at Yale.

Goodenough and St. Clair Drake studied with Malinowski, but Firth and Fei are the only two of the awardees who could be called his students (although in a sense we are all his students). Malinowski wrote the introduction to Fei's first book (1939) and also to Thompson's (1940), although Thompson only met him once. Foster met Malinowski on three occasions. Leighton knew Malinowski well and was influenced by him in his subsequent work in culture and personality. Colson met him during her work in Africa and Great Britain and lectured in his seminar. Spicer met Malinowski in Tucson and supported his appointment at the University of Arizona before Malinowski decided to accept the position at Yale.

Many of the award recipients mention, quote, or cite Malinowski briefly. Of the citations, most are to his *The Dynamics of Culture Change: An Inquiry into Race Relations in Africa* (1945), *Argonauts of the Western Pacific* (1922), *The Sexual Life of Savages in North-Western Melanesia* (1929), and *The Scientific Basis of Applied Anthropology* (1940), in this order of frequency. Leighton states, “I owe a tremendous debt to Malinowski for introducing me to the general concept of function in cultural anthropology” (letter to author, 1998). Colson suggests that his influence was indirect:

For one thing, his insistence on looking at what was happening around one meant that people . . . saw it as a legitimate anthropological task to do current ethnography on Indian reservations, American cities and towns, etc. And it was this willingness to do current ethnography that made applied anthropology possible. Secondly, he and his students did publish on what was happening in Africa . . . Whatever else anthropologists and potential anthropologists read in the 1920s and 1930s, they would have read . . . what Malinowski and his students were writing (e-mail to author, 1998).

Although the themes Malinowski raised were to permeate our work, he appears to have had very little direct influence on the course of applied anthropology in the U.S. after his death in 1942. The extent of Malinowski’s influence on applied anthropology in Great Britain is also unclear. Part of what obscures Malinowski’s influence in both cases is the general lack of prestige of applied anthropology as a whole until recent decades. The goal of anthropology then, as now in a few departments, was to produce theory from ethnographic study. Applied anthropology was considered important only if it fulfilled this goal. In addition, Malinowski’s work in practical anthropology in Africa occurred at the same time as Radcliffe-Brown’s functionalism, which had greater theoretical influence. Malinowski’s death at an early age may also have limited his influence. Had he lived longer, stationed as he was at the heart of the birth of the Society for Applied Anthropology, his influence would probably have been much stronger. It is not too difficult to imagine his publications appearing in the early issues of the society’s journal, then called *Applied Anthropology*. Much of his work in defining and outlining the dimensions of culturally indentured peoples can be seen as the basis for current approaches to applying social science concepts to the solution of social problems, even if it is not always so recognized.

## Conclusions

From the academically based to those affiliated with governmental agencies as employees or as contract agents, the Malinowski Award recipients have created the applied social science of today. They have come from all of the social sciences and their contributions reflect both the rich history of the applied field and the current state of the field, from fighting poverty and racism internationally to health, development, policy, resettlement, and other subjects. From this handful of applied workers whose contributions were especially notable have come the thousands who work to solve the social problems of the world today. The contributions by the recipients of the award validate the naming of this prize after such a venerable person Bronislaw Malinowski.

## Notes

<sup>1</sup> Emory Sekaquaptewa of New Oraibi, who is a Hopi artist and craftsman and a lawyer and lecturer in anthropology at the University of Arizona, created the Malinowski Memorial Medallion in 1991. Previously, he crafted the Elsie Clews Parsons medallion for the American Ethnological Society. A photograph in Malinowski's book (1929) influenced the design of the Malinowski medallion. The medal depicts a boat moving from left to right, or west to east. You can imagine, if you wish, that it shows Malinowski, with notebook on knee, being transported from one island to another, or of goods being traded in the Kula Ring, or maybe it was a water house (a word found in many Uto-Aztec languages) carrying ancestors of Uto-Aztec speakers across some body of water, perhaps the Bering Straits.

<sup>2</sup> A brief sketch of the career and contributions of each awardee introduces each of the Malinowski Award presentations in Chapters 3 through 29 of this volume. Five of the Malinowski papers were not published in *Human Organization* for various reasons. Richardson, Sharp, St. Clair Drake, and Arensberg, award recipients in 1988, 1989, 1990, and 1991 respectively, were prevented from submitting publishable versions due to terminal illness. A manuscript by Richardson evidently intended for publication was found and is included in this collection (Richardson 2002). Efforts to locate copies of the other presentations did not meet success. In lieu of the missing addresses, a brief summation is made of the career and what is known about the recipient's presentation. Ronald Frankenberg's essay was published elsewhere (1995) and is republished in this volume.

<sup>3</sup> As a student in California, W. Lloyd Warner became a disciple of Radcliffe-Brown. He followed him to Australia where he completed a study of Australian aborigines. Upon his return to the U.S. he became involved in industrial studies with Elton Mayo at Harvard. He applied functionalism and holism gained from his work with indigenous people to the production of the multi-volume Yankee City series. After this he went to Chicago where he commenced industrial and community studies. Prominent among the scholars influenced by Warner are Malinowski Award recipients Hughes, Sharp, Richardson, St. Clair Drake, and Arensberg. The work in industry by Arensberg, Richardson, and Esteva Fabregat also exhibits stimulation from Warner.

## References Cited

Anonymous

1950 The Bronislaw Malinowski Awards. *Human Organization* 9(3):1.

1952 The 1952 Bronislaw Malinowski Awards. *Human Organization* 11(4):3–4.

Bond, George C.

1988 A Social Portrait of John Gibbs St. Clair Drake: An American Anthropologist. *American Ethnologist* 15(4):762–781.

Cernea, Michael

1995 Social Organization and Development Anthropology. *Human Organization* 54(3):340–352. (Republished at <<http://www.sfaa.net/>>.)

Colson, Elizabeth

1985 Using Anthropology in a World on the Move. *Human Organization* 44(3):191–196. (Republished at <<http://www.sfaa.net/>>.)

Dobyns, Henry R.

1951 Blunders with Bolsas. *Human Organization* 10(3):25–32.

Fei, Xiaotong

1939 Peasant Life in China. London: G. Routledge & Sons. (Reprinted in 1979.)

1980 Toward a People's Anthropology. *Human Organization* 39(2):115–120. (Republished at <<http://www.sfaa.net/>>.)

- Firth, Raymond  
1979 Engagement and Detachment: Reflections on Applying Social Anthropology to Social Affairs. *Human Organization* 40(3):193–201. (Republished at <<http://www.sfaa.net/>>.)
- Foster, George M., and Barbara G. Anderson  
1978 *Medical Anthropology*. New York: John Wiley
- Frankenberg, Ronald  
1995 Learning from AIDS: The Future of Anthropology. *In* *The Future of Anthropology: Its Relevance to the Contemporary World*. A. S. Ahmed and C. N. Shore, eds. Pp. 110–133. London: Athlone. (Republished at <<http://www.sfaa.net/>>.)
- Goodenough, Ward  
1999 Communicating 10,000 Years into the Future. *Human Organization* 58(3):221–225. (Republished at <<http://www.sfaa.net/>>.)
- Hackenberg, Robert A., and Beverly H. Hackenberg  
1999 You CAN Do Something! Forming Policy from Applied Projects, Then and Now. *Human Organization* 58(1):1–15. (Republished at <<http://www.sfaa.net/>>.)
- Leighton, Alexander H.  
1984 Then and Now: Some Notes on the Interaction of Person and Social Environment. *Human Organization* 43(3):189–197. (Republished at <<http://www.sfaa.net/>>.)
- Malinowski, Bronislaw  
1922 *Argonauts of the Western Pacific*. London: George Routledge & Sons, Ltd. (Reprinted in 1932.)  
*The Sexual Life of Savages in North-Western Melanesia*. New York: Readers League of America. Distributed by Eugenics Publishing Company.  
1940 *The Scientific Basis of Applied Anthropology*. Atti dell' VIII Convegno—'Volta'—4–11 October 1938. Pp. 5–24. Rome: Reale Accademia d'Italia.  
*The Dynamics of Culture Change: An Inquiry into Race Relations in Africa*. P. M. Kaberry, ed. New Haven: Yale University Press.
- Myrdal, Gunnar  
1975 The Unity of the Social Sciences. *Human Organization* 34(4):327–331. (Republished at <<http://www.sfaa.net/>>.)
- Myrdal, Gunnar (with the assistance of Richard Sterner and Arnold Rose)  
1944 *An American Dilemma: The Negro Problem and Modern Democracy*. Ninth Edition. New York: Harper and Brothers, Publishers.
- Richardson, Frederick L. W.  
2002 Career Development, Strategies, Discoveries, and Turning Points. *In* *The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers*. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.
- Sayles, Leonard R.  
1952 A Case Study of Union Participation and Technological Change. *Human Organization* 11(1):5–15.
- Spicer, Edward H.  
1976 Beyond Analysis and Explanation? Notes on the Life and Times of the Society for Applied Anthropology. *Human Organization* 35(4):335–343. (Republished at <<http://www.sfaa.net/>>.)
- Tax, Sol  
1977 Anthropology for the World of the Future: Thirteen Professions and Three Proposals. *Human Organization* 36(3):225–234. (Republished at <<http://www.sfaa.net/>>.)

## Thompson, Laura

- 1940 Fijian Frontier. Hawaii: Institute of Pacific Relations. (New edition, Octagon Press, 1973.)
- 1979 The Challenge of Applied Anthropology. *Human Organization* 38(2):114–199. (Republished at <<http://www.sfaa.net/>>.)

## Wax, Murray L.

- 1976 Introduction to the Plenary Address, 35th Annual Meeting of the Society for Applied Anthropology, St. Louis, Missouri. *Human Organization* 35(4):331–333.

## Weaver, Thomas

- 2002a J. G. St. Clair Drake: Activist-Advocate before His Time. *In* The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.
- 2002b Lauriston Sharp: Innovator of Applied Anthropology Programs. *In* The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.
- n.d. An Appraisal of Applied Anthropology in the U.S.: 1985–1998. To appear in *Revista de Antropologia Aplicada*, Barcelona.

## Wolfe, Alvin W.

- 1978 How It All Began. *Practicing Anthropology* 1(4):3,19. (Republished in *Classics of Practicing Anthropology: 1978–1998*. P. J. Higgins and J. A. Paredes, eds. Pp. 15–19. Oklahoma City: Society for Applied Anthropology.)

## **Chapter 2**

# **Malinowski as Applied Anthropologist**

**Thomas Weaver**

Bronislaw Malinowski, like all of us, was a creature of his time. His professional career was demarcated by the two World Wars, roughly between 1912 and 1942. The importance of Malinowski as an anthropologist, his skill as a writer, and the time elapsed since his most important contributions more than half a century ago warrant a reassessment of his work for the benefit of new generations of students of applied anthropology.

It was not until the latter part of the 1930s that Malinowski focused effectively on practical anthropology, the term he used for applied anthropology, although he had begun this work in the 1920s. In the United States, anthropology was not to become involved extensively in solving social problems until that time, but there had been isolated efforts to apply anthropology to practical affairs even in the prior century—efforts by Lewis Henry Morgan, Alice Fletcher, and others. More extensive work was done in the Bureau of Indian Affairs during the 1930s and during World War II, when the Society for Applied Anthropology was founded. In Great Britain, anthropologists had been working with colonial administrators and providing training for overseas bureaucrats since early in the century (Herskovits 1936). The founding of the International Institute of African Languages and Cultures in 1926 (International Institute of African Languages and Cultures 1932) and, later, the Rhodes-Livingstone Institute provided new opportunities in Great Britain for developing applied anthropological involvement with administrators and politicians. These were opportunities presented to Malinowski and his students.

### **His Life**

Bronislaw Malinowski was born in 1884 in Cracow, Poland, which was part of Austria at the time. His father was a professor of Slavic philology at Jagiellon University. Malinowski's first training was in physical chemistry and the natural sciences, and he received a doctorate in 1908 in physics and mathematics. He subsequently studied with Wilhelm Wundt, the father of modern psychology. Arriving in England in 1910, he studied for four years with Edward Westermarck, Leonard T. Hobhouse, James G. Frazer, William H. R. Rivers (better known as W. H. R. Rivers), and Havelock Ellis. In his training as an anthropologist Malinowski followed the works of Franz Boas and Émile Durkheim, each of whom subscribed to a notion of holism. He was awarded a doctorate in 1916 for a library

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study of Australian kinship and gave a lecture on the topic at the London School of Economics (Firth 1957:3; Malinowski 1913a). In the same year he accompanied other anthropologists to Australia to attend the meetings of the British Association for the Advancement of Science and to conduct fieldwork. He was supported by scholarships negotiated by C. G. Seligman while waiting for additional funds. With support from the government of Australia, Malinowski went to the islands off New Guinea, ultimately ending in the Trobriands, where he carried out his best-known field research from 1914 to 1918.

In 1918 Malinowski married Elsie Masson, the daughter of an Australian professor of chemistry. Shortly after his return to Great Britain he was appointed to the first chair of social anthropology at the London School of Economics in 1924, a position he retained until 1938. After visiting the U.S. several times, he taught at the University of California at Berkeley, Cornell University, and the University of Arizona. He spent the winter of 1938–39 in Tucson for health reasons and lectured frequently. The University of Arizona offered him a faculty position, but he declined the offer to accept the Bernice P. Bishop Fellowship at Yale that enabled him to do fieldwork on market systems in Oaxaca (Malinowski and de la Fuente 1982). The following year (1939–40) he was given a faculty appointment at Yale where he died in 1942 (Troy 1998; Raymond H. Thompson, personal communication, October 2 and November 20, 1998).

### **His Accomplishments**

In the more than 50 years since his death Malinowski's contributions to functionalism and fieldwork have drawn more attention than his other work (Kaberry 1957). This is because the core of anthropology, both in the U.S. and in Great Britain, was focused on theory and attaining first-hand knowledge of aboriginal peoples. A frequent public speaker, Malinowski was famous during his life and widely published. He was read outside of anthropology, particularly in psychology and psychiatry, and he was a great popularizer of the field, with a penchant for titles of books and talks that titillated the public. Malinowski was best known for contributions to fieldwork methodology, as a teacher, and for theoretical contributions in functionalism, culture and personality, culture change, and magic and religion.

Early in his career Malinowski focused on studies of family and kinship (1913a), totemism (1926d), religion and mythology (1926d), economy (1922), warfare (1926c), sex (1929b), and psychoanalysis (1927a). Among his best-known works are the ethnography of the Trobriand Islanders, *Argonauts of the Western Pacific* (1922) and such more popular works as *The Sexual Life of Savages* (1929b), *Crime and Custom in Savage Society* (1926c), *Myth in Primitive Psychology* (1926d), and *The Father in Primitive Psychology* (1927a).

Around 1925 a new phase began for Malinowski as he became more concerned with social issues, the changing native, and practical anthropology. His interest and work in these areas of applied anthropology, the focus of this essay, are less well known. From the perspective of applied social science his most influential contributions are studies of culture contact and the changing native, efforts to define anthropology as a science, and the development of a practical anthropology that focused on the study of the social problems of modern aboriginal peoples.

## Fieldwork and Methodology

Malinowski's most famous work, at least among anthropologists, is his ethnography of the Trobriand Islands. He also conducted brief fieldwork among the Yaqui, Hopi, and Navajo in Arizona, the Chagga of East Africa, and the Zapotec of Oaxaca, Mexico. He is often given credit for having established the notion of long-term first-hand field research, living with the subjects, learning the native language, and presenting the information gathered within a holistic framework. His prescription for the appropriate behavior of the fieldworker and for research methodology, found in the introduction to his first Trobriand monograph, bears reading by every student going to the field today (1922:1–25). Here he emphasized the need for good training in theory, having a scientific aim, spelling out the methods of collecting data, awareness of emotional conditions during fieldwork, collecting concrete data such as genealogies, selecting informants carefully, using synoptic tables to show holistic links among different types of information, and joining the natives in activities (later known as participant observation).

Many of his followers claimed precedence for him in making direct observation a key part of fieldwork. Adam Kuper, for example, claimed that Malinowski as founder of social anthropology in Great Britain “established its distinctive apprenticeship--intensive fieldwork in an exotic community . . . and virtually everyone who wished to do fieldwork in the modern fashion went to work with him” (Kuper 1983:1). In fact, there were others who pioneered this technique. In the United States extensive ethnographic work had been underway since before the turn of the twentieth century among American historicists under the tutelage of Franz Boas. Boas himself had conducted fieldwork among the Inuit for two years commencing the year of Malinowski's birth in 1884. Diamond Jenness spent two years among the Inuit of Bernard Harbor in 1914–1915, out of which came his book *The People of the Twilight* (1928; George Foster, letter to author, July 7, 1998). Many of the characteristics of the Malinowskian fieldwork methodology were also present in the work of earlier explorers, such as Joseph Lafitau in 1724 and Merriwether Lewis in 1804, naturalist-anthropologists such as Lewis Henry Morgan in 1851, and Zuni ethnographer Frank. H. Cushing in 1887 and many others. Even in Great Britain, the 1898 Cambridge Torres Straits expedition organized by Alfred C. Haddon, with team members Rivers, Seligman, and J. L. Myers, involved extensive fieldwork. Nevertheless, Malinowski provided a widely read example of the methodology and gave it an emphasis and visibility not awarded before.

Malinowski inculcated in his students the need for intense, accurate, contemporary reporting of observations in the field. He shared with Boas an empirical approach and a distrust of unfounded generalization. Information derived from the past, or from incidental observations of missionaries and travelers, could not yield scientifically valid conclusions in his view. Malinowski was 12 years old when Boas launched his attack on the evolutionists and set out the premises of historicism and the importance of first-hand fieldwork (Boas 1896). As a graduate student and young professional, he must have read the American historicists who provided the only antidote at that time to nineteenth century evolutionism. Later he took every opportunity to ridicule and criticize the nineteenth century evolutionists, including his teachers, and also the diffusionists—the British heliocentric and Austrian *kulturkreiselehre* versions, as well as the American historicists. Malinowski referred to his field as social anthropology, a term originated by Frazer in 1906, and as a branch of sociology as distinguished from ethnology, ethnography, and cultural anthropology (Kuper 1983:2).



## Teaching

Malinowski's first seminars were held at the London School of Economics in October 1924.<sup>1</sup> The Trobriand materials were the basis of his teaching, and he used the Socratic method, posing problems and asking questions after reading portions of his manuscripts to assembled students. He used a series of charts and tables for synoptic presentations of cultures and as a guide for fieldworkers. Gaps in fieldwork were identified and analyzed, and questions formulated through this method. Charts formed the basis of discussion and testing of theories. His seminars were attended by other professors or invited persons, who were questioned and encouraged to participate in discussions (Kuper 1983:22–24).

Students characterized Malinowski's relationship with them as stimulating. He was encouraging and complimentary to his students, although rude, intolerant, and abrupt at times (Francis L. K. Hsu, personal communication, 1975). In an edited volume written to evaluate his work the authors paid homage to his teaching and his pioneering ethnographic work (Firth, ed. 1957). There, Firth assessed Malinowski's considerable influence and the methods he used to buttress this influence.

To his pupils, Malinowski's stimulus lay in a combination of many qualities: his subtle power of analysis, his sincerity in facing problems, his sense of reality, his scholarly command of the literature, his capacity for integrating detail into general ideas, and his brilliance and wit in handling discussion (Firth 1957:9).

## Functionalism and Theory

Malinowski was one of two originators, along with A. R. Radcliffe-Brown, of functionalism. Radcliffe-Brownian functionalism focused on society, social system, functional integration, and synchronic study as opposed to the diachronism of the evolutionists and the American historicists. His theory held that acts, rituals, values, and other elements in culture had a purpose in the perpetuation of the social system. Malinowski, too, focused on functional integration and synchronous study, but he put more emphasis on the individual and on biological needs. The two versions of functionalism were derived from the French sociologist Émile Durkheim. Malinowski was exposed to the work of Durkheim early in his career; one of his first publications was a review (1913b) of Durkheim's *Les Formes Élémentaires de la Vie Religieuse* (1912).

Malinowski agreed with Radcliffe-Brown about the functional integrity of cultural units, which he called institutions. The purpose of institutions is to meet the seven basic human needs: metabolism, reproduction, bodily comfort, safety, movement, growth, and health. Primary institutions provide for these needs and have personnel, ideology, a legal charter, continuity, and purpose (Malinowski 1944). Malinowski maintained that culture everywhere has the same aspect—material, economic, legal, religious, political, aesthetic, and linguistic. At the cultural level institutions function to provide food, form kinship groups, satisfy bodily comforts, promote survival, release tensions, provide training, maintain health, and gratify sexual drive (Mair 1965:234). By emphasizing the physiological and psychological basis of culture Malinowski hoped to provide a universal character to his theory. This differed from the functionalism of Radcliffe-Brown that emphasized social rather than biological needs; Radcliffe-Brown held that

analysis should focus on the functional needs of *society* (a concept rarely used by Malinowski) rather than on the individual.

Some of the same criticisms directed at Radcliffe-Brown and his followers came to characterize comments on the Malinowskian variety of functionalism as well. Both varieties of functionalism ignored any lack of functional consistency in particular cultural settings and the phenomenon of change of function. Additionally, Malinowski was accused of being too empirical, and while his focus on the individual was ahead of its time, the biological needs approach was diminishing in psychology. Malinowski's anthropological focus, encompassing ethnography, methodology, culture and personality, culture change, and social issues, as well as policy, was also considered too broad during a time when the rest of the field had a less encompassing vision. Unlike his protagonist Radcliffe-Brown, Malinowski was not accused of having a conservative ideology that supported the status quo, colonialism, and capitalism. Judging from Malinowski's desire to support the native, study social issues, and advise administrators and policymakers, one could make the case for his liberalism.

Elizabeth Colson, who participated in Malinowski's seminars at the London School of Economics, maintains that Malinowski's functionalism was especially relevant to applied anthropology and that it helped to give anthropologists legitimacy in working to solve social problems.

We were trained [by Malinowski's functionalism] to look for interconnections across fields of action in a systematic fashion and to ask "If this changed, what else would happen?," no bad directive whether one is an applied or an academic anthropologist. In fact, while functionalism never was very much a theory it provided a good working method. Today, of course, one would use other terms since each generation needs its own vocabulary--and so we might claim to be developing working models, or writing thick description, or adopting a holistic approach, rather than undertaking a functional analysis. The old term, however, meant something more in the context of applied anthropology, for if something can function it also can fail to function, and this calls for investigation of what has gone wrong; and if something is working, one needs to ask what will happen if one tries to tinker with a working system by introducing change. In that time and place that honored technician and engineer, the term "functionalism" invoked associations that made a place for the anthropologist as technician (Colson 1985:192).

### **Malinowski as an Applied Anthropologist**

After the middle of the 1920s Malinowski's career transitioned to reflect a greater concern with social issues, the problems of natives overrun by colonial powers, and the practical use of anthropology. This change in focus commenced, perhaps, with his writing on the forces of law in the primitive community in 1925. The following year he wrote a letter to an editor on anthropology and administration (1926a) and an article on social hygiene (1926b). In 1927 came an article on "Useful and Useless Anthropology" (1927b) and finally in 1929 the better-known article on "Practical Anthropology" (1929a). Malinowski's writing on practical issues continued in this vein on administration (1930a), race and labor (1930b), native education (1936), changing

cultures (1938a), the scientific basis of applied anthropology (1940a), European rule (1940b), war (1941), and the pan-African problem (published posthumously in 1943). A posthumous book, *The Dynamics of Culture Change*, included essays on application and social problems (1945). The book continued themes previously noted such as the necessity for the anthropologist to act as interpreter and advocate for the native, the need to study the “changing native,” the potential advantage in viewing different administrative policies as controlled experiments, and the use of holism and functionalism in the study of modern problems in Africa.

The development of practical anthropology by Malinowski paralleled similar developments in the United States, Mexico, and Holland. In Great Britain, as in the U.S., anthropologists had been proclaiming the usefulness of anthropology since its inception in the middle of the nineteenth century (Partridge and Eddy 1987; van Willigen 1986; Weaver 1985, n.d.). However, in both countries, this proclamation often served as a means to acquire funding for basic research, rather than to achieve any tangible social or policy ends (Forde 1953; Kennard and Macgregor 1953; Hinsley 1979). The Dutch had used anthropology in their colonial efforts in the second half of the nineteenth century. The training of colonial administrators in anthropology had taken place in England since the beginning of the twentieth century (Forde 1953; Hogbin 1957:248). In Mexico, Manuel Gamio, influenced by Boas, had used anthropology in the solution of social problems in the Teotihuacan Valley in the second decade of the twentieth century (Nahmad Sitton and Weaver 1991). In the U.S., anthropologists worked with government programs, particularly with the Bureau of Indian Affairs after John Collier became commissioner in 1934, and during World War II most anthropologists worked in some capacity for the government.

Malinowski's 1929 essay on practical anthropology was addressed to “the Institute” (presumably the International Institute of African Languages and Cultures established in 1926), and its purpose, perhaps, was to gain funding for his students. Malinowski and his students subsequently worked closely with this institute and administrators for many years. In the 1929 article, Malinowski asserted that a mutually stimulating collaboration could be forged between scientifically trained anthropologists and “practical men.” The terrain could be safely divided with the former studying practical problems and the latter (the statesman and the journalist) sharing responsibility for political decisions. The article ended with a list of recommendations for the institute (1929a:37–38). Practical problems to be investigated as part of the anthropology of the changing native included direct versus indirect rule, land tenure, political organization, primitive law, economics, indigenous financial systems and taxation, principles of indigenous education, population problems, hygiene, and changing outlook (by which he probably meant changing worldview).

One of the issues that preoccupied Malinowski in 1929 was direct versus indirect rule in the British colonies. Direct rule had a number of harmful consequences for the native: “forced labour, ruthless taxation, a fixed routine in political matters, the application of a code of laws to an entirely incompatible background. [In] education [it meant] . . . making of the African into a caricature of the European” (Malinowski 1929a:24). Malinowski supported a policy of indirect rule because it was less destructive. However, the problem required study and the solution called for slow and gradual change.

Studies of land tenure were important because they were linked to the occupation of native lands by colonialists and the question of how much land should be left for subsistence needs. Land tenure, too, was joined to other aspects of culture. The study must be done not by inquiry, but by mapping ownership and by determining the minimum use requirements of the

native (1929a:31–32). Other important problems of land tenure were related to rights of conquest, historical prerogatives, native rights, demand for maximum land for European use, and a need to safeguard native interests (1929a:30).

Previously anthropologists and administrators had studied political organization, but their reports were inadequate or poorly focused, according to Malinowski. African kingships were advanced and possessed extensive traditions and genealogies, ceremony and ritual, and well-developed systems of finance, military organization, and judiciary formations. In introducing change there was no need to touch the established native order; these institutions could continue to function as they had in the past. The problem presented by administrative or government-sponsored studies was that politically sensitive materials were never published. The assignment of two or three anthropologists could solve this problem because they could work more quickly and at less expense (1929a:31). The nature of anthropological work, too, must be changed, however. Past work by anthropologists was slanted to the study of “classical antiquity,” ritual mythology, quaint superstition, and magic. It had not considered how primitive politics actually work or identified the forces that underlie obedience to the king or his ministers (1929a:25).

Malinowski criticized previous studies of primitive law on similar grounds. The “continental school” consisting of J. J. Bachofen, A. H. Post, W. Kohler, and Durkheim, and others depicted natives as displaying blind and passive obedience. Contrary to the findings of the nineteenth century evolutionists, natives had a clearly defined criminal and civil law, principles of government and communal rights to land, and rights over manufactured objects and articles of consumption, as well as systems of inheritance and succession to office (1929a:26). Native law and politics could not be studied in isolation; they were tied holistically to other elements in the culture, such as family and community organization, kinship and descent, clan and local group organization, and language. Previous studies were dominated by the sensationalism of strange customs, such as the couvade, mother-in-law avoidance, the dispensing of the afterbirth, and quaint relations between cousins. Malinowski believed that we knew more about “anomalous forms of marriage or classificatory exaggerations of kinship” than about family organization (1929a:27). Further, only anthropologists were competent to deal with this question because they have no vested interests. They would be alert to the problem in prior studies of forcing terminology into terms borrowed from European law.

Understanding native economic organization could provide insight into several practical problems, such as hygienic conditions, labor, education, taxation, how wealth was capitalized, and the psychology of gift and exchange (1929a:32). Anthropologists must study primitive production and consumption, types and phases of economic activity, and relations to religion, magic and practical arts—not “origins and stages” or “diffusion and histories” (1929a:33). Questions of labor were important including the abolition of slavery and conscription or forced labor versus labor contracts. Subjects that needed investigation included the procurement of food and materials for housing, clothing, and weapons and their preparation and use. Other pertinent subjects included labor associated with the storing and preservation of food, the development of traps, and the production of luxuries, art and monuments, personal ornaments, painting, sculpture, and ritual objects (1929a:34). The study of economy and labor could lead to more appropriate adjustments, for example, of native work patterns to plantation work in Melanesia (1929a:34). (Presumably, Malinowski meant this as an example of a positive adjustment in native labor, but the statement demonstrated an ignorance of the deleterious effects of the plantation system.)

## The Changing Native and Culture Contact Studies

It was an easy step from viewing the native “as he is,” that is, unreconstructed, to a concern for a solution to the social problems of the changing native. Despite the judgments of some that Malinowski failed to develop a theory (Firth, ed. 1957), one must admire the integrity of his observations. The development of “culture contact” studies in Great Britain was equivalent to acculturation studies in the United States (Beals 1953), but unlike their American counterpart, British studies explicitly focused on practical application. Malinowski avoided the use of the term acculturation because he considered it linked to traits and trait complexes, and he preferred to identify the basic unit of culture as the institution. He sometimes used the term “transculturation” after the suggestion of Fernando Ortiz who saw the situation as a two-way process (Malinowski 1940a).

Change, for Malinowski, was a process wherein societies were transformed, either by internal growth or rapidly through the contact of two different cultures. The first process led to cultural evolution and the second to diffusion. His reference to diffusion was not related to American or European uses of the transmission of traits or trait complexes, but to the transmission of and change in institutions (Kaberry 1945:vii–viii; Malinowski 1929a:36). Malinowski’s contributions, had they been compared and linked with acculturation studies in the U.S., would have provided an antidote to some of the more mechanistic efforts to sort out traits from “two or more cultures in face to face contact” (Redfield, Linton, and Herskovits 1936). A synthesis of the two approaches might have resolved many of the criticisms of acculturation studies that followed in the decades after inception of the theory in the U.S. (Beals 1953).

Malinowski called for the study of changing cultures in the context of Western contact (1938a). The study of the changing native must be a separate field, he maintained, from evolutionist and diffusionist studies of the U.S. and Europe. This new field called for building new methods and principles of research and a new branch of anthropology (1938a:xii). According to Malinowski, studying the contact situation required three processes: identifying the nature of culture contact and change, finding the best methods of fieldwork appropriate to the problem, and translating the theoretical results into practical rules of conduct for the administrator, missionary, entrepreneur, or teacher.

Malinowski suggested that the anthropologist must give up studies of “anthropometry, detailed technology, [and] the burdensome task of collecting and labelling specimens” (1938a:xvii). Instead, knowledge must be gained of world politics and economics, finance, colonial policy, overseas education, and missionary goals and activities. Fieldwork must be conducted on Europeans in Africa as a special focus, but also on the changing native as well as on the unchanged native in remote locales. Contact results in three culture types: a shrinking traditional culture, an intrusive European culture, and newly emerging syncretic cultures such as the mining camp and the urban slum. In a brief “birds-eye view” Malinowski portrayed the interactions of representatives of these cultures. He spoke of the “color bar,” the activities of missionaries, acceptance of European cultural patterns by some Africans, and an isolated colonial society where few were even aware of indigenous ways of life (1938a:vii–x). Even if the “subject matter threatens to disappear,” Malinowski believed that the study of the detribalized native and other segments of the society must be included as part of this new science.

Lucy Mair and Monica Hunter, students of Malinowski and contributors to a 1938 collection based on seminars conducted by Malinowski at the London School of Economics (Mair, ed. 1938), spoke of constructing a cultural “zero point” from which to measure change

(Mair 1938; Hunter 1938), and Schapera (1938) studied culture contact under the subheading “Reconstruction of Tribal Culture.” Malinowski disagreed with them and postulated that the reconstruction of a past indigenous culture would depict an idealized culture and not a living reality (Malinowski 1938a:xxv). He cautioned against reconstructing “a savage who does not exist anymore” (Malinowski 1938a). In Melanesia the native had ceased to exist a generation ago, in Africa two generations ago, and in North America about 100 years ago. Malinowski insisted that the focus must be on the study of the changing native “as he is” and on how institutions function, not on how they originated or diffused (1929a:28). The anthropologist must avoid the “highly emotional vision of the past as it lives in present-day mythology of the pre-European Golden Age” or as a “Paradise lost” and “train his vision forward rather than backward” (1938a:xxvi).

Meyer Fortes, Gunter Wagner, and A. T. and G. M. Culwick, also contributors to the 1938 monograph, suggested that focusing on a reconstruction of the past led to bad policy. The memories of old informants were described by Fortes as “a mesh of lies” (1938), and by the Culwicks as producing two distinct, but erroneous pictures, “one of a black Utopia and the other of a bloody reign of terror” (1938). In fact, a more or less substantial residue of the past still survived in smatterings in secret or remote places, including war, slavery, witchcraft, ancestor worship, criminal retributions, exotic sex customs, and sex taboos (Malinowski 1938a:xxviii–xxix). The reconstructed past taught nothing. Living tradition with remnants of past society need not be reconstructed; it can be observed first-hand during fieldwork.

Malinowski disagreed with Schapera and Fortes in their suggestion in the same monograph that African cultures in contact must be viewed as integrated parts of a single society. He did not see the administrator, missionary, or teacher as part of the indigenous system. They represented the European component and remained aloof, possessed little knowledge of the system they were impacting, and might even hold prejudicial views of it. They should be studied separately and then linkages among the three systems demonstrated (Malinowski 1938a:xiii–xvii, xxxvi). He affirmed the value of the functionalist approach, not in the context of a single group, but in the study of surviving institutions and their adaptations to new strains derived from European influences. Functional study was to be conducted not only of surviving institutions but also of new European ones in the context of African culture including mines, factories, courts of justice, and schools (Malinowski 1938a:xxxvii).

Further, Malinowski chastised Schapera for suggesting that the study of the impact of Western civilization on African culture was a special problem. “Impact, after all, is a relation. In the study of this, we cannot cut out one side and study what remains as a mere fragment. Impact and change are action and reaction. They are a mutual interpenetration” (Malinowski 1938a:xxxv). Malinowski held that the three cultures living in Africa were not a “mixture of partially fused elements,” as held by Hunter (1938). Malinowski agreed with Fortes, who maintained that culture contact was “not a mechanical pitch-forking of elements of culture like bundles of hay from one culture to another . . . [nor was it] a transference of elements of one culture to another, but . . . a continuous process of interaction between groups of different culture” (Malinowski 1938a:xix, quoting Fortes).

Malinowski then supported his agreement with Fortes by analyzing African labor in mines, factories, and plantations. The introduction of European institutions and capital had required a new organization of African labor, and the new system needed to be treated as a whole and not separated into African or European components. Forces such as prejudice and segregation, political and economic dominance, special protection of European standards, and

isolation impacted the use of labor in Africa. Malinowski identified the special conditions that created a unique African labor force.

There is no European prototype for colour-bar legislation or practice; for recruiting on reserves; for the method of unemployment insurance by throwing back superfluous labour on the tribal areas in times of slump. [Also not present elsewhere are the type of labor] contract with unilateral criminal sanctions, . . . [and] the inducements to sign on . . . African labour, again, knows no collective bargaining: it is a commodity which is not allowed to conform to the laws of supply and demand; it differs from European labour legally, economically, and socially . . . [and from] tribal economics. . . . No sorting of elements is possible; no invoicing back to a parent culture. We here have to deal with a vast phenomenon which in its essence is defined by a set of economic, legal, and social arrangements which have arisen in response to . . . the large scale exploitation of African resources by Europeans, for Western ends, and by means of African labour (Malinowski 1938a:xx–xxi).

Malinowski described other changes in African culture as entirely new products of the conditions of contact between European and old African groups. “The school in the Bush has no antecedents in Europe, nor yet in African tribalism. The question of educating men and women to professions from the practice of which they are then legally debarred occurs neither in Europe nor yet Bantu Africa” (1938a:xxii). The idea that European culture is a source of new elements for Africa is balanced by the selectivity of its donation (cf. Foster 1960). Some of the items not shared with the African include:

instruments of physical power, such as fire-arms, bombing planes, poison gas, and all that makes effective defense or aggression possible . . . [as well as] instruments of political mastery. . . . Sovereignty remains [in the hands of the conquerors]. [Africans have] no votes [and are not equal citizens, even] when given Indirect Rule . . . [They are not provided] the substance of economic wealth and advantage . . . except the inadequate wage. . . We do not admit them as equals to Church Assembly, school, or drawing-room (Malinowski 1938a:xxii–xxiii).

These passages have much the sound and feel of modern critical anthropology influenced by neo-Marxist precepts and of modern applied anthropology. They could equally be read as part of either of these two bodies of literature, even if written two-thirds of a century ago.

### **Science in Basic and Applied Anthropology**

The relationship of basic to applied anthropology has long preoccupied anthropologists, and it was particularly important to Malinowski. The issue first arose in the middle of the nineteenth century when a nascent anthropology attempted to pattern itself after the natural sciences. However, the discipline failed to develop a clear-cut methodology suitable to its changing subject matter and failed to identify and agree upon the major components of the research endeavor. Anthropology’s efforts at identifying elements for investigation have not been clearly

or sufficiently spelled out, or have varied so much that subsequent investigators find it difficult to replicate findings and results.

Malinowski advocated focusing on social problems in developing theory. “The pose of academic detachment and persistent blindness to the fact that theoretical anthropology can learn quite as much from practical issues as it can teach in return, have considerably handicapped modern developments in the Science of Man” (1938a:xxxiii). Elsewhere, Malinowski adopted an even more radical stance:

Anthropology must become an applied science. Every student of scientific history knows that science is born with its application. . . . [The anthropologist must] formulate criteria of practical guidance, . . . define indices of maladjustment, and . . . show the way in which sound knowledge can be translated into useful practice (1938a:x–xi).

Malinowski held the view that anthropology was no less a science than any of the natural sciences. The same principles of observation, testing, and empirical verification were as much present in anthropology as in the physics of Copernicus, Galileo, Newton, or Faraday.

A theory which fails must be amended by discovering why it has failed. [It begins] . . . when general principles have [are] put to the test of fact, and when practical problems and theoretical relations of relevant factors are used to manipulate reality in human action . . . [and] practical application (1938b:11).

He did not believe that anthropology should copy blindly the methodology of the natural sciences, especially their “belief that counting and measuring define the line of distinction between science and loose talk” (1938b:14). Qualitative information was just as valid as quantitative information. Science in anthropology meant that it must identify a legitimate subject, isolate process, and establish concepts and principles in terms of general laws. Each theoretical principle must translate into a method of observation by incorporating practical problems as beneficial to the scope of broad theories (1938b:14).

Malinowski’s position on the definition of science and its social context was similar to the approach taken later in the sociology of knowledge and by Thomas Kuhn (1962). Although Malinowski, like Kuhn, used the concept “paradigm,” for Malinowski it was more a synoptic chart than a dynamic scientific framework. He was addressing, however, the necessity for revamping current ideas about the place of theory in application, and hence, in Kuhnian terms, the creation of a new paradigm. Further, Malinowski’s statements on the subject sound much like those of Gramsci and other neo-Marxists in holding that any observation is grounded in some theory or ideology.

There is no such thing as description completely devoid of theory. Whether you reconstruct historic scenes, carry out a field investigation in a savage tribe or a civilized community, analyze statistics, or make inferences from an archaeological monument or a prehistoric find—every statement and every argument has to be made in words, that is, in concepts. Each concept, in turn, is the result of a theory which declares that some facts are relevant and others adventitious, that some factors determine the course of events and others are



merely accidental by-play; that things happen as they do because personalities, masses, and material agencies of the environment produced them (1944:7–8).

Malinowski expanded this view of science with the belief that scientific activity is a human endeavor that applies equally to scientists and to indigenous people (1944:7–14). They use the same methodology that begins with observation and leads to prediction. Primitive “science” includes efforts in creating, constructing, and developing culture, in fire making, constructing implements, or building shelters (1944:8). These activities require observation, selection of appropriate materials and forms, principles of performance, and transmission of acquired knowledge. Primitive science was not limited to material culture, but also applied to other aspects of life.

The scientific attitude, embodied in all primitive technology and also in the organization of primitive economic enterprises and social organization, that reliance on past experience with the view to future performance, is an integral factor which must be assumed as having been at work from the beginning of mankind . . . (1944:10).

The main point that Malinowski made here was not so much that “primitive man” had science, but that the scientific attitude “is as old as culture” and that “science is derived from any pragmatic performance” (Malinowski 1944:10). These observations on the principles of science not only permeated his approach to the egalitarian study of “savage” society, but also suggested the related concepts of practice as theory, cultural relativity, holism, and advocacy. The integrity of his observations and his analysis of related concepts would validate Malinowski, not only as academician and theoretician, but also as major contributor to a modern theory of application.

Ian Hogbin, a contributor to the 1957 Firth volume evaluating Malinowski as an anthropologist, agreed that in the operation of the natural sciences basic science was directly linked to the solution of applied problems, although the practitioners of the scientific and practical subfields might not be the same persons. In the natural sciences, the knowledge that comes from experiment and analysis is utilized for exploring theory and for technical goals.

While the academic physicist is probing more deeply into the structure of matter, the civil engineer, designing bridges and viaducts, is utilizing the existing data on the behaviour of stone and metal under conditions of stress; and while the academic biologist strives to find out more concerning the principles of heredity and the mutation of genes, the plant breeder can proceed on established facts to develop strains of seed suitable for areas of low rainfall or great extremes of temperature (Hogbin 1957:245).

Hogbin believed that the anthropologist was less like an engineer and more like a meteorologist forecasting tomorrow’s weather. The weather forecaster could say what to expect provided that pressure systems continued along known paths and rates. Applied anthropologists could not rely on prior theories worked out by other investigators. New problems encountered require new theories and methods. He held that most anthropological theory was based erroneously on the descriptive, structural, and organizational goals of static systems, rather than on the dynamic social problem issues found in changing human environments. Hogbin identified

as a major problem that policymakers and the public at large would accept the word of scientist-experts on plants and bridges, but considered themselves as capable as the anthropologist at making observations on society and social problems (Hogbin 1957:246).

### **The Anthropologist as Advocate**

Malinowski felt it was the duty of anthropology to chronicle contemporary events that occurred in conjunction with Westernization. At the same time, he cautioned that as a humanist the anthropologist must be aware that the process of Westernization was under the control of agents of Western civilization. Malinowski appealed to the moral obligation of every scientist and especially the anthropologist to be a “fair and true interpreter of the Native”; anthropologists had an obligation to explain the native to all Europeans (1945:3).

In advancing the idea that anthropologists should be advocates for native rights, Malinowski did not mince words or shy from strong language.

In reality, the historian of the future will have to register that Europeans in the past sometimes exterminated whole island peoples; that they expropriated most of the patrimony of savage races; that they introduced slavery in a specially cruel and pernicious form; and that even if they abolished it later, they treated the expatriated Negroes as outcasts and pariahs (1945:4).

Malinowski encouraged anthropologists to continue as advocates in the face of the long-standing vested interests of administrators, missionaries, and industrialists (1945:153). The following statement resonates with Firth’s later depiction of anthropology as “the uncomfortable science” (Firth 1981:198):

The voice of the anthropologist here is even more inconvenient than the harangue of the pro-Native, because he speaks in terms of fact and figures, and of the irrefutable logic of measuring ends and protestations against the means adopted for their achievements. But this cannot silence the voice of the scientific research worker, especially when he knows that what he has to say is the truth (Malinowski 1945:153).

In spite of field techniques that brought anthropologists into close contact with natives, guided by the scientific approach and their humanism, they had, in Malinowski’s view, consistently avoided involvement with the native’s social problems. This aloofness, he believed, was wrong. “The science which claims to understand culture and to have the clue to racial problems must not remain silent on the drama of culture conflict and of racial clash” (1938a:x). The many letters to editors, publications, and speeches he gave during his lifetime are indicative of his direct advocacy and not just advocacy of advocacy.

### **Policy Science**

Malinowski’s observations about the use of policy appeared almost a decade before the first statements of policy scientists (Lasswell 1951) and decades ahead of the first statements by other

social scientists (Weaver 1985). Malinowski pleaded eloquently with anthropologists to register the tragic errors of the past and to have the courage to apply necessary remedies. In repetitive calls for advocacy for the native he emphasized the necessity of influencing policymakers and becoming involved in political matters. One who failed to do so remained “an antiquarian covered with academic dust and in a fool’s paradise” (Malinowski 1945:4). He warned, however, of the practice of politicians to appoint a commission of inquiry after harm has been done to cover up the problem and to “deadend the pangs of conscience” (1945:4).

Shall we, therefore, mix politics with science? In one way, decidedly “yes,” because if knowledge gives foresight and foresight means power, it is a universal stultification of scientific results to insist that they can never be useful or used by those who have influence (1945:4).

Malinowski’s approach to culture change and the study of social problems in African colonial society was intricately related to policy. He viewed policy not only as an appropriate arena for the application of scientific knowledge, but also as a controlled experiment through which social theory could be tested by observation in fieldwork (1945:7). Here Malinowski was referring to the necessary study of planning, execution, and aftermath of such policies as indirect rule and European-directed education.

## Conclusions

There have been many who praised Malinowski and perhaps an equal number who have criticized his work. A recent biographer of Margaret Mead, another anthropological icon in her time, commented on the ambivalence displayed toward Mead by her peers. The biographer spoke of the “paradox that while Mead was acclaimed by the public, her reputation as an anthropologist was often dismissed by her academic peers—perhaps because of the ‘female’ topics she often focused upon (such as parent-child relationships and socialization), perhaps because of her theoretical focus on culture and personality, or perhaps simply because of professional jealousy” (Lutkehaus 1998:13). This statement could apply as well to Malinowski with his popularity with the public, his focus on applied topics as outlined in this essay, and his pioneering work in functionalism and practicing anthropology as alternatives to just pure jealousy.

Malinowski was a pioneer in many fields and certainly his work in applied anthropology was at the leading edge of the discipline in the 1930s. Herskovits (1936) hailed the new direction Malinowski had charted for applied anthropology. By the 1940s a strong basis for developing applied anthropology had been laid in many countries, especially in Great Britain, the Netherlands, Mexico, and the United States. The retreat of anthropologists to academia after World War II allowed applied anthropology to fall into a state of neglect, however. Had anthropologists not retired to university teaching, the inroads into development and practicing anthropology that have been achieved in the last two decades would have occurred much earlier.

Kuper paid compliment and homage to Malinowski at the end of a chapter that is acute and balanced.

Malinowski’s greatness lay in his ability to penetrate the web of theories to the real man, boasting, hypocritical, earthy, [and] reasonable; and he passed on to his students an invaluable awareness of the tension which is always there between

what people say and what they do, between individual interests and the social order. It was Malinowski too who was the first to show the way in which the principle of reciprocity might serve to bind the individual, in his own interests, to the community (1983:35).

Kuper maintains that Malinowski's book *Argonauts of the Western Pacific* (1922) and its discussion of ceremonial exchange stimulated Mauss' *The Gift* (1954) and ultimately was the central inspiration for Levi-Straus and the French structuralist perspective (Kuper 1983:35). In the conclusion to his book Kuper continues this accolade:

If one can talk of a Malinowskian revolution, it is because Malinowski changed the relationship between theory and ethnography in social anthropology. To be crude and schematic, but not misleading, before the First World War the relationship between theory and ethnography (and theorist and ethnographer) was as master to servant. . . . There was, in short, a division of labour. The Brahmin-anthropologist pondered theories in his study and sent out his questions; the Sudra-ethnographer did the dirty work and more or less passively responded to the demands of the specialist (Kuper 1983:193).

If one substitutes "applied anthropologist" for ethnographer, the paragraph above reflects the relationship between academic and applied anthropology in the U.S. until recent times. The academic as "colonialist" viewed the applied anthropologist as an agent who went out among the natives and gathered data for the academic formation of theory in the safe confines of the ivory tower. Malinowski led the way to recognizing this relationship between ethnography and applied anthropology and theory formation.

The posthumous publication of Malinowski's field diary written during his Trobriand Island fieldwork (Malinowski 1967) created an uproar among social scientists. Malinowski expressed feelings in his diary that were quite at variance with his image as a fieldworker, participant observer, and advocate of the native. To be fair, the diary reflects some of the problems commonly experienced by anthropologists living in isolation from their own society. Compulsively reading novels, worrying about health without the presence of modern medicine, missing music one is accustomed to hearing, anger and disdain for informants who break promises, and yes, sexual longings are the fate of anthropologists in the field. Malinowski explained to his students that keeping a diary had been a venting mechanism from the boredom and emotions of the field that included sexual deprivation and loneliness (Kuper 1983:13; Wax 1972). He would have enjoyed the controversy at the anthropology meetings brought on by Francis L. K. Hsu's presidential address in 1975 in which he seemed to take joy in reporting publicly some of the material revealed in the diary. Malinowski would have been pleased to know that his writings have continued to create disquiet among his colleagues.

Perhaps, Michelle Rosaldo provided a more realistic perspective shared by many ethnologists regarding Malinowski's diary when she wrote:

Although my field diaries are replete with evidence of Malinowski's now scandalous ambivalence, and our first trip, in particular, was by far the most emotionally and psychically demanding experience of my life, I find it difficult to recall old feelings of pain and confusion. Instead, my memories of Ilongot living

are touched with the romantic cast that was, of course, part of what led me to seek such “exotic” surroundings. Writing at this moment, I find myself overwhelmed with gratitude and nostalgia for a world that is typified by the warmth, consideration, and playfulness of people who tolerated and cared for us, and finally, became our dear friends (Rosaldo 1980:xiv).

It is regrettable that Malinowski’s diary appeared almost twenty years after his death. One wonders had he been alive whether, reflecting on his experience, he would have written a passage like Rosaldo’s that she would have quoted in 1980.

### Notes

<sup>1</sup>His students over the years included Ashley Montagu, E. E. Evans-Pritchard, Raymond Firth, Camilla Wedgwood, Audrey Richards, Monica Hunter (later Wilson), Gregory Bateson, Isaac Schapera, Meyer Fortes, S. F. Nadel, Fei Xiaotong (Hsiao-tung), and F. L. K. Hsu (Firth 1975; Kuper 1983; Leach 1984). Before becoming anthropologists, Fortes was an experimental psychologist, Firth was an economist, and Nadel had doctorates in philosophy and psychology as well as being an accomplished musician. Others who attended his seminar included Talcott Parsons, Hortense Powdermaker, Elspeth Huxley, Jomo Kenyatta, Prince Peter of Greece, and L. S. B. Leakey (Wax 1976:332–333).

### References Cited

- Beals, Ralph  
1953 Acculturation. *In* *Anthropology Today*. A. L. Kroeber, ed. Pp. 621–641. Chicago: University of Chicago Press.
- Boas, Franz  
1896 The Limitations of the Comparative Method of Anthropology. *Science* 4(103).
- Colson, Elizabeth  
1985 Using Anthropology in a World on the Move. *Human Organization* 44(3):191–196. (Republished at <<http://www.sfaa.net/>>.)
- Culwick, A. T., and G. M. Culwick  
1938 Culture Contact on the Fringe of Civilization. *In* *Methods and Study of Culture Contact in Africa*. L. P. Mair, ed. Pp. 38–45. International Institute of African Languages and Cultures. Memorandum XV. Oxford: Oxford University Press.
- Durkheim, Émile  
1912 *Formes Élémentaires de la Vie Religieuse: Le Système Totémique en Australie*. Paris: Félix Alcan. Translated by Joseph Ward Swain, 1915. London: George Allen & Unwin, Ltd. First Free Press Paperback Edition, 1965. New York: The Free Press.
- Firth, Raymond  
1957 Introduction: Malinowski as Scientist and as Man. *In* *Man and Culture: An Evaluation of the Work of Bronislaw Malinowski*. R. Firth, ed. Pp. 1–14. London: Routledge and Kegan Paul.  
1981 Engagement and Detachment: Reflections on Applying Social Anthropology to Social Affairs. *Human Organization* 44(3):193–201. (Republished at <<http://www.sfaa.net/>>.)
- Firth, Raymond, ed.  
1939 *Man and Culture: An Evaluation of the Work of Bronislaw Malinowski*. London: Routledge and Kegan Paul.

- Forde, Daryll  
1953 Applied Anthropology in Government: British Africa. *In Anthropology Today: An Encyclopedic Inventory*. A. L. Kroeber, ed. Pp. 841–865. Chicago: University of Chicago Press.
- Fortes, Meyer  
1938 Culture Contact as a Dynamic Process. *In Methods and Study of Culture Contact in Africa*. L. P. Mair, ed. Pp. 60–91. International Institute of African Languages and Cultures. Memorandum XV. Oxford: Oxford University Press.
- Foster, George  
1960 Culture and Conquest: America's Spanish Heritage. Viking Fund Publications in Anthropology, Number Twenty-Seven. New York: Wenner-Gren Foundation for Anthropological Research.
- Herskovits, Melville J.  
1936 Applied Anthropology and the American Anthropologist. *Science* 83(2149):215–222.
- Hinsley, Curtis M.  
1979 Anthropology as Science and Politics: The Dilemmas of the Bureau of American Ethnology, 1879 to 1904. *In The Uses of Anthropology*. W. Goldschmidt, ed. Pp. 15–32. Special Publication Number 11. Washington, D.C.: American Anthropological Association.
- Hogbin, H. Ian  
1957 Anthropology as Public Service and Malinowski's Contribution to it. *In Man and Culture: An Evaluation of the Work of Bronislaw Malinowski*. R. Firth, ed. Pp. 245–264. London: Routledge and Kegan Paul.
- Hunter, Monica  
1938 Contact Between European and Native South Africa: 1. In Pondoland. *In Methods and Study of Culture Contact in Africa*. L. P. Mair, ed. Pp. 9–24. International Institute of African Languages and Cultures. Memorandum XV. Oxford: Oxford University Press.
- International Institute of African Languages and Cultures  
1932 The Five-Year Plan of Research. *Africa* 5(1).
- Jenness, Diamond  
1928 People of the Twilight. New York: The Macmillan Company. (Reprinted 1959, University of Chicago Press.)
- Kaberry, Phyllis  
1945 Introduction. *In The Dynamics of Culture Change: An Inquiry into Race Relations in Africa*. P. M. Kaberry, ed. Pp. v–xiv. New Haven: Yale University Press.  
1957 Malinowski's Contribution to Fieldwork Methods and the Writing of Ethnography. *In Man and Culture: An Evaluation of the Work of Bronislaw Malinowski*. R. Firth, ed. Pp. 71–91. London: Routledge and Kegan Paul.
- Kennard, Edward A., and Gordon Macgregor  
1953 Applied Anthropology in Government: United States. *In Anthropology Today: An Encyclopedic Inventory*. A. L. Kroeber, ed. Pp. 832–840. Chicago: University of Chicago Press.
- Kuhn, Thomas  
1961 The Structure of Scientific Revolutions. Chicago: University of Chicago Press. (Phoenix paperback edition, 1964; Second edition, enlarged, 1970.)

- Kuper, Adam  
1983 Anthropology and Anthropologists: The Modern British School. Revised edition. London: Routledge and Kegan Paul.
- Lasswell, Harold D.  
1951 The Policy Orientation. *In* The Policy Sciences, Recent Developments in Scope and Method. D. Lerner and H. D. Lasswell, eds. Pp. 3–15. Stanford: Stanford University Press.
- Leach, Edmund  
1984 Glimpses of the Unmentionable in the History of British Social Anthropology. *Annual Review of Anthropology* 13:1–23.
- Lutkehaus, Nancy  
1998 Margaret Mead and the Media: The Making of an American Icon. Discussion cited in 1998 Annual Report. Pp. 13. Santa Fe: School of American Research.
- Mair, Lucy  
1938 The Place of History in the Study of Culture Contact. *In* Methods and Study of Culture Contact in Africa. L. P. Mair, ed. Pp. 1–8. International Institute of African Languages and Cultures. Memorandum XV. Oxford: Oxford University Press.  
1965 An Introduction to Social Anthropology. Oxford: Clarendon Press.
- Mair, Lucy P., ed.  
1938 Methods and Study of Culture Contact in Africa. International Institute of African Languages and Cultures. Memorandum XV. Oxford: Oxford University Press.
- Malinowski, Bronislaw  
1913a The Family Among the Australian Aborigines. With an Introduction by J. A. Barnes. London: University of London Press. (Reprinted 1963 by Schocken Books.)  
1913b Review of Les Formes Élémentaires de la Vie Religieuse by Émile Durkheim. *Folk-lore* 24:525–31. London.  
1922 Argonauts of the Western Pacific. London: George Routledge and Sons. (Reprinted in 1932.)  
1925 The Forces of Law and Order in a Primitive Community. London: Royal Institution of Great Britain, Weekly Evening Meeting, February 13. Pp. 1–19.  
1926a Anthropology and Administration (letter to the editor). *Nature* 128:768.  
1926b Address on Anthropology and Social Hygiene. London: Foundations of Social Hygiene. Issued by the British Social Hygiene Council, Pp. 54–84.  
1926c Crime and Custom in Savage Society. London: Routledge & Kegan Paul, Ltd.  
1926d Myth in Primitive Psychology. *Psyche Minatures*. General Series, No. 6. London: K. Paul, Trench, Trubner & Co., Ltd..  
1927a The Father in Primitive Psychology. New York: W. W. Norton.  
1927b Useful and Useless Anthropology (review of Race and History by E. Pittard). *The New Republic* 1(6410):109–111.  
1929a Practical Anthropology. *Africa* 2(1):22–38.  
1929b The Sexual Live of Savages in North-Western Melanesia. New York: Readers League of America. Distributed by Eugenics Publishing Company.  
1930a The Rationalization of Anthropology and Administration. *Africa* 3:405–29.  
1930b Race and Labour. *The Listener* 4 (supplement 8).  
1936 Native Education and Culture Contact. *International Review of Missions* 25:480–515.  
1938a The Anthropology of Changing African Cultures. *In* Methods and Study of Culture Contact in Africa. L. P. Mair, ed. International Institute of African Languages and Cultures. Memorandum XV. Oxford: Oxford University Press.  
1938b The Scientific Basis of Applied Anthropology. *Transactions of the Volta Congress*. Rome: Royal Italian Academy.  
1940a The Scientific Basis of Applied Anthropology. *Atti dell' VIII Convegno—'Volta'*—4–11 October 1938, Pp. 5–24. Rome: Reale Accademia d'Italia.

- 1940b Modern Anthropology and European Rule in Africa. Atti dell' VIII Convegno —'Volta'—4-11 October 1938, 18:6-26. Rome Reale Accademia d'Italia.
- 1941 An Anthropological Analysis of War. *American Journal of Sociology* 46:521-50.
- 1943 The Pan-African Problem of Culture-Contact. *American Journal of Sociology*. Volume 48.
- 1944 A Scientific Theory of Culture and Other Essays. Chapel Hill: The University of North Carolina Press.
- 1945 The Dynamics of Culture Change: An Inquiry into Race Relations in Africa. New Haven: Yale University Press.
- 1967 A Diary in the Strict Sense of the Term. New York: Harcourt, Brace and World.
- Malinowski, Bronislaw, and Julio de la Fuente
- 1982 Malinowski in Mexico: The Economics of a Mexican Market System. S. Drucker-Brown, editor. London: Routledge & Kegan Paul. (Paperback edition 1985.)
- Mauss, Marcel
- 1954 The Gift, Forms and Functions of Exchange in Archaic Societies. Translated by Ian Cunnison. Glencoe: The Free Press.
- Nahmad Sitton, Solomon, and Thomas Weaver
- 1991 Manuel Gamio, El Primer Antropologo Aplicado y sus Conexiones con Antropologos Norte Americanos. *América Indígena* 4:292-321.
- Partridge, William L., and Elizabeth M. Eddy
- 1987 Development of Applied Anthropology in America. *In* Applied Anthropology in America. Second Edition. E. M. Eddy and W. L. Partridge, eds. Pp. 3-58. New York: Columbia University Press.
- Redfield, Robert, Ralph Linton, and Melville Herskovits
- 1936 Memorandum for the Study of Acculturation. *American Anthropologist* 38:149-152.
- Rosaldo, Michelle Z.
- 1980 Knowledge and Passion: Ilongot Notions of Self and Social Life. Cambridge: Cambridge University Press.
- Schapera, I.
- 1938 Contact Between European and Native in South Africa: 2. In Bechuanaland. *In* Methods and Study of Culture Contact in Africa. L. P. Mair, ed. Pp. 25-37. International Institute of African Languages and Cultures. Memorandum XV. Oxford: Oxford University Press.
- Troy, Timothy
- 1998 Professor Bronislaw Malinowski's Visit to Tucson. *Journal of the Southwest* 40(2):129-186.
- van Willigen, John
- 1986 Applied Anthropology: An Introduction. South Hadley, Massachusetts: Bergin and Garvey Publishers, Inc.
- Wagner, Gunter
- 1938 The Study of Culture Contact in its Practical Applications. *In* Methods and Study of Culture Contact in Africa. L. P. Mair, ed. Pp. 92-105. International Institute of African Languages and Cultures. Memorandum XV. Oxford: Oxford University Press.



Wax, Murray L.

1972 Tenting with Malinowski. *American Sociological Review* 37:1–13.

Weaver, Thomas

1985 Anthropology as a Policy Science: Part I, A Critique. *Human Organization* 44(2):97–105.

n.d. An Appraisal of Applied Anthropology in the U.S.: 1985–1998. To appear in *Revista de Antropologia Aplicada*.

# **Chapter 3**

## **Gonzalo Aguirre Beltrán: Applied Anthropology and Indigenous Policy**

**Thomas Weaver**

The demand for equality represented by the civil rights movement that began in the U.S. in the 1960s, soon spread to other parts of the world. In Mexico, with agitation from a Marxist movement present from early in the twentieth century, the call came in 1968. It found Gonzalo Aguirre Beltrán at the center of the storm and symbol of the government's assimilationist policy and its failure to resolve the social problems that beset the Mexican Indian. The problems of poverty and domination identified by anthropologists and others at the beginning of the century still persisted. Critical anthropologists raised the public ire by emphasizing the embarrassing conditions of the Mexican Indian. The result after the 1968 riots in Mexico was that Marxist anthropologists were given responsibility for operation of the Indian Institute and other important ministries, such as Agrarian Reform and Education. This was in direct opposition to the mainly conservative and assimilationist policies of Aguirre Beltrán.

Gonzalo Aguirre Beltrán (1908–1996) was the first person to receive the Society for Applied Anthropology's Malinowski Award (as revived in 1970).<sup>1</sup> Aguirre Beltrán received a medical degree from the National University of Mexico in 1931. In the nine years after this, he carried out archival and community studies in land tenure conflict among indigenous groups (1940a) and pioneering research on Blacks in Mexico (1940b). After the publication of these two studies he undertook postgraduate work in anthropology with Melville Herskovits and Irving Hallowell at Northwestern University in 1945–46. His earlier ethnohistory and fieldwork with Blacks in Mexico was revised as a result of his contact with Herskovits.

On his return to Mexico Aguirre Beltrán was appointed director of the Dirección General de Asuntos Indígenas (Indian Affairs) in the Secretaría de Educación Pública. Asuntos Indígenas became the Instituto Nacional Indigenista (INI, National Indian Institute) in 1948. In 1951 he was appointed director of the first Indian coordinating center in the country in Chiapas, and in 1952 he was appointed assistant director of INI, as it is most commonly known. He served as president of the University of Vera Cruz for seven years beginning in 1956 and as elected representative to the Mexican National Congress from 1963 to 1966. After this he became director of the Interamerican Indian Institute. In 1970, Aguirre Beltrán became the third director of INI and Assistant Secretary for Popular Culture and Extracurricular Education. As a result of these appointments he became responsible in 1970 for charting and executing indigenous policy in Mexico, a responsibility he fulfilled for almost three decades.

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It is difficult to discuss the early career of Aguirre Beltrán without also considering the influence of fellow anthropologist Julio de la Fuente. De la Fuente worked with Aguirre Beltrán, first in Chiapas, and later as assistant director of the Indian Institute. He introduced regionalism and functionalism to the collaboration as a result of his work with Malinowski in a study of the regional market systems in Oaxaca in 1940 and his reading of Robert Redfield's studies of Tepoztlan and of acculturation in the Yucatan peninsula (Hewitt de Alcántara 1984:44–49). Aguirre Beltrán's contribution to the collaboration included the acculturation approach of Herskovits and Redfield, which he converted into an assimilationist policy (Weaver 1982). Aguirre Beltrán's publications included works on Black ethnohistory, acculturation, land reform, medical anthropology (1995), and the education and social condition of Indians (1979, 1992).

Aguirre Beltrán's Malinowski Award address, entitled "Applied Anthropology in Mexico" (1974), sketched the development of anthropology in Mexico. The social and political value of anthropology was recognized earlier in Mexico than in many other countries. All major anthropologists came to hold high government offices in education, land tenure, indigenous affairs, and other topics. This began with Manuel Gamio and Juan Comas in the second decade of this century (Nahmad Sitton and Weaver 1991). But the wedding of anthropology and public service was by Moisés Sáenz, a Columbia University Ph.D. in education, who suggested that anthropology would be of value only if it considered the realities of current living conditions. Moisés Sáenz began his tenure as Secretary of Education as an incorporationist but ended as an indigenist believing in a nation built on pluralism.

Early Spanish colonial indigenous policy had relegated Indians to minority status because they were considered to possess inferior reasoning capabilities. After gaining independence from Spain, Mexico launched an immigration policy with the goal of "whitening" the race by encouraging the immigration of Europeans. Later, the encouragement of foreign investment and economic development by President Porfirio Díaz after the 1890s exacerbated the condition of an indigenous population that had been disorganized and disenfranchised by Spanish policies and actions.

In 1917 Manuel Gamio, a child of the hacendado class who studied at Columbia University and was influenced by Franz Boas, commenced what was to be the major direction of Mexican indigenous policy for the remainder of the century. This included, on the one hand, recognition of the plight of the Indian, and on the other, the development of programs designed for the national integration of the Indian (Aguirre Beltrán 1974:3). The development of regional coordinating centers by anthropologist Alfonso Caso was an important advance in furthering the policy of assimilation. Other influences in Mexico's indigenous policy came from the work of Robert Redfield in Yucatan and of the Institute of Social Anthropology in the Tarascan Project directed by George Foster. It can be seen that community study, functionalism, acculturation, and a regional approach were the basis for the development of Aguirre Beltrán's assimilationist indigenous policy.

The key to Aguirre Beltrán's indigenous policy was the utilization of anthropologists and other social scientists to research, plan, and put into action an integral strategy to assimilate Indians into the Mexican Nation. Using the coordinating centers with regional headquarters and offices located in the places where Indians lived, Aguirre Beltrán created interdisciplinary teams to service Indians in all aspects of their lives. Integral action also meant that attempts were made to coordinate the responsibilities and services provided by other federal agencies. These included bilingual education, land tenure, economy, loans, legal advice, boarding and local schools, and utilization of the native language. The principle that underlay this program was "ideological

integration, . . . [that is] complete and equal participation of social classes and categories of population in the task of overall development. This means Indians and non-Indians must share equally . . . as much in obtaining goods and services leading to economic and moral growth, as in active contribution to the progress of the intercultural region and to the nation as a whole” (Aguirre Beltrán 1974:5). A clear and more extensive description of Aguirre Beltrán’s indigenous policy was expressed in his book, *Regions of Refuge*, first published in 1967, and in other publications. (The fourth edition of *Regions of Refuge* was translated and published by the Society for Applied Anthropology in 1979.)

Aguirre Beltrán was one of the most important anthropologists in Mexico and was honored and eulogized by his countrymen (Anonymous 1995). He was a founding member of the Mexican Academies of Education and of Scientific Investigation and elected member of the Mexican Academy of Medicine and of the Mexican National Academy of Science. Aguirre Beltrán served as president of the Mexican Society of the History and Philosophy of Medicine. He received a special citation from the Society for Medical Anthropology, many honorary doctorates, and numerous named awards and prizes.

### Notes

<sup>1</sup>This annual award is given to a senior colleague in recognition of efforts to understand and serve the needs of the world through social science. See Thomas Weaver’s “The Malinowski Award and the History of Applied Anthropology” (2002b) for a brief history of this award and an overview of the recipients and their work, and Weaver’s “Malinowski as Applied Anthropologist” (2002a) for an introduction to Bronislaw Malinowski and his work.

### References Cited

- Aguirre Beltrán, Gonzalo
- 1940a El Señorío de Cuauhtochco: Lucas Agrarias en México Durante el Virreinato. México: Ediciones Fuente Cultural.
  - 1940b La Población Negra de México, Estudio Etnohistórico. México: Ediciones Fuente Cultural.
  - 1974 Applied Anthropology in Mexico. *Human Organization* 33(1):1–6. (Republished at <<http://www.sfaa.net/>>.)
  - 1979 *Regions of Refuge*. Boulder, CO: Society for Applied Anthropology. Translation of the Fourth Edition. Originally published in 1967 as *Regiones de Refugio, El Desarrollo de la Comunidad y El Proceso Dominical en Mestizoamérica*. México: Instituto Indigenista Interamericano.
  - 1992 *El Proceso de Aculturación*. Cuarta Edición. México: Fondo de Cultura Económica. Primera edición publicado en 1957 por la Universidad Autónoma de México.
  - 1995 *Antropología Médica, sus Desarrollos Teóricos en México*. Segunda Edición. México: Fondo de Cultura Económica. Primera edición publicado por CIESAS en 1986.
- Anonymous
- 1995 Gonzalo Aguirre Beltrán, Homenaje Nacional. Xalapa, Veracruz: Universidad Veracruzana. Gaceta Numero 32, Septiembre.
- Hewitt de Alcántara, Cynthia
- 1984 *Anthropological Perspectives on Rural Mexico*. London: Routledge & Kegan Paul.
- Nahmad Sitton, Salomon, and Thomas Weaver
- 1991 Manuel Gamio, El Primer Antropólogo Aplicado y sus Conexiones con Antropólogos Norte Americanos. *América Indígena* 4:292–321.

Weaver, Thomas

- 1982 From Primitive to Urban Anthropology. *In* *Crisis in Anthropology: View from Spring Hill*, 1980. E. A. Hoebel, R. Currier, and S. Kaiser, eds. Pp. 203–221. New York: Garland Publishing, Inc.
- 2002a Malinowski as Applied Anthropologist. *In* *The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers*. T. Weaver, ed. < <http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.
- 2002b The Malinowski Award and the History of Applied Anthropology. *In* *The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers*. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.

## Applied Anthropology in Mexico

Gonzalo Aguirre Beltrán

First, I should like to thank the Society for Applied Anthropology for the opportunity extended me to speak before such a distinguished audience.<sup>1</sup> Mexico's longstanding interest in utilizing the social sciences in the quest for better and more just relationships among human groups is well known, and, therefore, I feel it appropriate tonight to address my remarks to the field of Mexican social anthropology, including current trends in this discipline.

The initial task confronting students of anthropology in Mexico was that of defining its content. At the beginning of the present century, anthropology began to explore new paths. In Europe, the British took as their model the positivist sociology of Durkheim, oriented toward the study of social relations; whereas in the United States the school headed by Boas supported a broader range of interests, including within the scope of cultural anthropology human behavior in both its historical and contemporary dimensions.

In Mexico the conjunction of the two trends produced a generally broad approach; but, because of changes in social structure occasioned by the Revolution, the study of social relations and related problems was stressed. The goal was knowledge which could be immediately applied.

At a time when the social movement of 1910 was still in its infancy, Manuel Gamio, in a work which today is considered classic, stated the necessity of studying human populations in order to assure just application of laws, and assigned this task to anthropology. However, it was Professor Moisés Sáenz who shaped anthropology into the form which the discipline has today in Mexico.

Given the problems faced by the revolutionary administration, particularly the national integration of a large indigenous population, Sáenz felt that anthropological studies could be of immediate value only if they took account of the realities of the contemporary situation. That is, archeology, linguistics, and ethnography were placed at the service of rural sociology, the discipline to which fell the ultimate responsibility of assisting the peasant population to satisfy its needs and overcome its handicaps.

To this combination of anthropology and sociology Sáenz gave the name "social" anthropology. Also, just as with Gamio, Sáenz believed that the acquired knowledge (of anthropology) should be immediately applicable. As a consequence, the distinction between social anthropology and applied anthropology (in Mexico) was essentially obliterated. While it may be true that other fields of anthropology also have potential for application, social anthropology (in Mexico) is specifically oriented to the study of changes in the social relations and cultural systems of marginal populations, with the ultimate goal of facilitating their integration into the national society. Thus, in reality, it is not easy to distinguish between applied social anthropology and Indian policy. In fact, for many years in Mexico, social anthropology had the sole function of studying Indian problems and proposing solutions. Only recently have

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<sup>1</sup>This address was previously published in *Human Organization* 33,1(1974):1-6.

Mexican social anthropologists begun to turn their attention to such other matters as agrarian problems, land tenure, migration, and urbanization.

Considering the identification of social anthropology with Indian policy, we perceive that the antecedents of our discipline lie in the epoch of colonial domination. As a result of the discovery of the New World and capitalist expansion, it became necessary for the conquerors to design a policy to deal with the Indian population. The exploitation of some men by others required rationalization and it was found in the aristocratic ideal of Aristotle, the hierarchical universe of Thomas Aquinas, and the unscrupulous nationalism of Machiavelli.

Ginés de Sepúlveda contributed the political theory of the “imperial metropolis” based on the notion that the Indians possessed an inferior reasoning capacity which placed on more “civilized” societies the obligation of intervening in their lives, even of usurping their sovereignty. The goal of such intervention was to guide and help the Indians, not only through conversion to Christianity, but through imposing, by force if necessary, benevolent and paternal institutions that would lead them to the true faith and to civilization.

The Indian policy adopted by the colonial administration relegated Indians to the status of minors in age, and placed them in the social structure as a caste of serfs. The first missionaries (Las Casas, Sahagún, Vasco de Quiroga, and others) violently opposed the metropolitan policy, or accepted it in part in order to propose alternatives which they intended to put into practice. This was the case with the *hospitales-pueblo*, the College of Santa Cruz in Tlatelolco, the conquest of Verapaz, and other projects that never became more than utopias temporarily contradicting policies of colonial expansion.

The strength of the educated elite by the end of the colonial period, as well as the influence it received from the French and North American Revolutions, led to Mexican Independence, and with it, the immediate need for the creation of a bourgeoisie that could build a nation. The patricians of that time encountered problems in the form of ethnic heterogeneity, lack of geographic articulation, multiplicity of languages, differing degrees of cultural evolution, and such inequities in the distribution of economic goods that only a limited few could make use of the property and the wealth of the country.

The Indian policy of the Independence period was tied to the liberal philosophy of capitalism which by that time had gained sway in the Western world, and which advocated laissez-faire as a means of promoting the “survival of the fittest.” In Mexico, those who regarded themselves as “the fittest” were so few in number that, among other proposals, they advocated an immigration policy which would give them proportionately greater strength and solve the problem of heterogeneity by racially “whitening” the country. At the same time, the new expression of individualism led to destruction of the colonial system and secularization of institutions through separating civil and religious affairs.

Overthrow of the colonial system had two results: (1) disorganization within indigenous communities; and (2) distribution of the land among members of these communities as private property. Immigration failed to integrate the nation, and, on the contrary, resulted in loss of one large part of Mexico. Secularization and individualization of institutions destroyed a considerable number of Indian communities which fell under the dominion of the hacienda.

The growth and modernization of communications at the end of the last century combined with the investment of foreign capital to further despoil Indian communities. As a result, the nation was left alienated by a scientific oligarchy that Justo Sierra, speaking more from fancy than from fact, called “the Mexican bourgeoisie.”

In spite of the above, the beginning of industrialization in Mexico produced remarkable changes in scientific thought. In 1904 José López Portillo in a small work entitled *La Raza Indígena*, began a frontal attack on the prevalent theory of Social Darwinism which left Indian groups marginal to the national society. He proposed broad concessions of economic, political, and civil liberties to the Indian communities to raise the intellectual level of the natives.

He postulated that with free traffic and trade, the mixture of races would eventually produce both physical and spiritual integration; and concluded with an observation now commonly accepted in social anthropology: "The true division that exists among men does not depend on race, but on culture."

In 1910 Ricardo García Granados wrote his famous essay, "El Concepto Científico de la Historia," to refute Spenserian ideas. In this rebuttal he expressed the same conviction as López Portillo, widening its social aspects as follows:

Through the centuries there has never existed the invariable superiority of any race. The diversities among men are not basically anthropological, but the product of culture. There is no reason to reject the idea that any race that now exists or is in the process of formation, would be able to raise itself to the highest level of civilization. Races raise themselves or fall according to the efficiency or deficiency of their social conditions as well as in relation to historical circumstances.

The ideas generated by the industrial movement made obsolete the agrarian structure of the country. Andrés Molina Enríquez developed an evolutionary scheme based on concepts of land tenure, and placed the ethnic groups of the country in particular categories. At the bottom of the scale were those which lacked any notion of territorial rights. Above them were groups which had developed an elementary concept of territory; and still farther along were those which recognized territorial possession. Most advanced were groups which embraced the idea of private property. This last concept was only understood by the enlightened elite.

In focusing on the agrarian problem, Mexican Social Anthropology is in debt to the anarchical philosopher Ricardo Flores Magón. The anarchism of Flores Magón and his slogan *Tierra y Libertad* (taken from the Russian *narodniki* of the middle of the last century) crystalized in the land movement of Zapata, in the constitution of 1917, and in agrarian reform legislation. The redistribution of land, carried to its highest level during the regime of President Cárdenas, by itself obtained the incorporation of large masses of people into the national life, thereby enriching the cultural and human resources of the country.

The ideas of all the aforementioned persons are important to social anthropology and to indigenist policy in Mexico.

Within the mental climate which placed social and cultural factors above biological ones and gave emphasis to the problems of land tenure, Manuel Gamio produced his opus *Forjando Patria* which signaled the real beginning of contemporary social anthropology in Mexico. Gamio is known in the academic world for his insistence on applying anthropology to the governing of men and for his rejection of studies whose results are buried in university archives to be consulted by only a chosen few.

Research, according to him, was intended to discover through scientific means the characteristics and the conditions of human societies in order to improve them. With reference to Mexico, the need for research and knowledge of this type seemed evident and vital to the conduct of good government.



The ultimate goal of social anthropology for Gamio was to build the notion of nationality. The idea was not original with him, but was derived from the philosophies of earlier social scientists. It can be said that all Mexican anthropological thought since the Jesuit Clavijero supported this idea, the most basic concept in countries emerging from European colonialism with independent status, but lacking a national spirit.

Gamio, in contemplating the situation of the country when the Revolution began, realized that Mexico did not constitute a true national society. Although integrated into one political unit which included a dominant minority and an indeterminate number of small *patrias* consisting of many Indian groups, Mexico was so heterogeneous it was impossible to speak of her as a single nation.

The movement to establish rural schools, cultural missions, and other informal educational agencies which could reinterpret Indian cultures in modern terms had as its aim the incorporation of marginal groups into the mainstream, although the rationale for achieving this aim was broader than simply that of overcoming real or presumed deficiencies within the indigenous population. The community studies of Robert Redfield, made with the moral support of Gamio, furnished knowledge about the Indian population and mechanisms of the acculturation process.

These community studies had a double value. On the one hand, they contributed to the growth of the science of man, documenting preconceived sociological schemes and giving profundity to historical perspectives. Additionally, they expanded the amount of fieldwork and extended the area of investigation to non-Indian communities.

On the other hand, they served as instruments of professional growth, uniting teachers and students in research and publication. The Institute of Social Anthropology of the Smithsonian Institution had an enormous and lasting influence on the consolidation of social anthropology in Mexico. The *Tarascan Project*, implemented as an application of community studies, showed how useful social science could be for the development of an Indian group, particularly when it made use of the language and the popular culture of the group as a starting point.

North American influence was not the only one. A European Anthropologist, Bronislaw Malinowski, left his mark on Mexican social anthropology through his association with Julio de la Fuente. Both studied the market at Oaxaca and made clear the regional character of integration at the Zapotec, Mixe, and Castilian villages dispersed throughout the valley. In addition, they studied the function the market played in this integration. With this work, they crossed the limits of single community studies and provided the basis for later development of a regional approach.

Malinowski came to Mexico attracted by the chain of transformations that the Revolution produced in the social structure of the country. His theoretical and practical interest—in the decade of the thirties—was centered on social change and on the application of the postulates of the science of man to the problems of administration. His stay in Mexico provided him a perspective quite distinct from that previously held by colonial administrators.

In addition to the above-mentioned currents, the Mexican Revolution liberated others, such as extending to greater numbers of people the benefits of health care, better income, and political participation. Alfonso Caso institutionalized and gave expression to these ideas when he created coordinating centers as agencies of integral action and regional development. In this task he was helped by a group of anthropologists who, through the interplay of theory and practice, have over the past 20 years produced a model which has become the standard for Mexico with respect to matters of national integration.

The basic elements of this model can be summarized in terms of integral action, planning, and research. The idea contains three other postulates: functional coordination, operative regionalization, and ideological mobilization. To emphasize the importance given to each of these postulates, we must examine them separately.

Research, planning, and integral action provide a foundation for the application of anthropology to the governing of heterogeneous societies. The thesis of *integral action* is recognition of the problems under consideration as dynamic biological, psychological, social, and cultural totalities which must be considered in their complete historical and geographical contexts. Problem solving requires multidisciplinary teams of researchers, since groups of experts with specialized techniques offer greater possibilities for adequate and comprehensive studies. Implementation necessarily is left in the hands of the professionals within each distinct branch of knowledge.

This kind of integral action, which can also be called “sectorial” (divided into sectors or spheres of knowledge), is the one used by Mexican anthropologists in studying the population of the Valley of Teotihuacán, as well as in the movement that developed the Mexican rural school. In both cases, but on different levels of intellectual excellence, the region and the community were contemplated as totalities. Their integration required work embracing the strategic components of human activity in all its complexity.

Integral action joins fields of activity that under normal conditions are carried out separately. Education, health, economy, agriculture, stock raising, and other programs that government departments conceive as separate functions, are united and juxtaposed, aggregating and interrelating in ways that mutually reinforce and support each other.

But, integral action is even more comprehensive than this. A focus of a different type is concerned with ecology, regarding man and his environment as a relatively homogeneous, sociogeographic unit which is called a region. The theory of *Regions of Refuge*, for which I am in a great part responsible, is based on an approach of this kind and facilitates the study of areas in which Indians and non-Indians live together in mutual dependency.

Integral action includes examination of the physical composition of the region and the communities contained within it: the metropolis and its hinterland, the main city and its satellites, the village and its *barrios*, the natural resources and their potentialities. Thus projected, it reveals an ecological integration that emphasizes the permanent interdependence of man and the environment in which he lives. Such an approach not only overcomes the deficiency of narrow focus in simple community development, but also transcends the superficiality of the ordinary planning of a more general nature.

Within intercultural “regions of refuge” the population is divided into ethnic categories with historically assigned statuses of servitude and privilege. *Ladinos* and *indios*, *gente de razón* and *naturales*, *vecinos* and *paisanos* are names that identify sectors of the population with different cultures and languages; but, more importantly, they are ethnic groups which are unequally situated in a caste organization. Integral action puts this separation into perspective, taking into account human groups and conflicts, qualifying cultural differences, and identifying the degree of social evolution necessary to assure, through unification, equivalent development of the ethnic categories of which the region is composed.

With respect to these categories, integral action is concerned with the critical education of all the interacting groups, since it strives for a non-alienated integration. Education for freedom cannot include privileges for one group and discrimination against others. Democracy, as a model for Indian education, must stress intercultural understanding as an instrument to build the nation.

A fourth form of integral action has social structure as a frame of reference. An imperialist philosophy concedes the benefits of development to a minority elite which ascribes to itself the status of a dominant class while exploiting or condemning to marginality groups which exceed it in size. Indians are among the marginal groups (in Mexico), but their marginality is not complete segregation. Individually and temporarily they form part of the proletariat or of the lumpenproletariat and they contribute in their limited way to raise the standard of living of the culturally and economically more advanced.

Integral action has as a goal a structural reintegration leading to profound changes in the social relations of production, distribution, and consumption. Because of its transcendency, this effort requires the most attention. Agrarian reform, fiscal reform, and credit reform are important because their achievement alters the structure of the system and some of its strategic components. The results can be known in a short period of time and they constitute the basis from which other reforms can be initiated to consolidate social change. Education—through the redistribution of techniques, knowledge, and values—also modifies the social structure. It is a slow process, but such changes are lasting and of great strength.

Through approaching problems from diverse angles, integral action is productive for research, planning, and implementation. As it becomes more profound, it reveals the numerous facets that must be considered in a unified approach.

In the case of the intercultural “regions of refuge,” a new and distinct point of view is indispensable. In spite of the varied implication of integral action, ideological implications are very often evaded in practice, discussion, and analysis; yet ideological integration constitutes the very reason for the existence of the indigenous movement.

Through ideological integration, we seek complete and equal participation of social classes and categories of population in the task of overall development. This means that Indians and non-Indians must share equally in national integration, as much in obtaining goods and services leading to economic and moral growth, as in active contribution to the progress of the intercultural region and to the nation as a whole. Thus, marginal people can free themselves from the undesirable state in which they have been maintained and be incorporated into the evolving society as full citizens with complete rights, obligations, and loyalties. This ideological integration, based on the equal participation of all the inhabitants, is the thesis that permanently sustains the Mexican Revolution and toward whose achievement the struggle continues.

A second basic postulate of Indian action programs is a complement of integral action known as *functional coordination*. Sectionally oriented, this postulate involves concentration of specific effort at only one point, within one region and under one authority and orientation, with the aim of adapting the total program to the peculiar character of Indian communities which function as a unit.

Functional coordination is the complement of multilateral action. It is the sum of a set of efforts of different kinds, but not simply the sum. Rather, it is the mutual reinforcement of a series of unilaterally implemented projects to stimulate regional development. Coordination, in a certain way, suggests the partial cession of freedom to act; but this cession is only partial, because at the same time independence is retained. Coordination is not the subordination of some to others, but the union and cooperation of all.

The third postulate, that of *operative regionalization*, derives from the need for an ecological integration which can replace and go beyond the limited focus of community development and the generalized view characteristic of planning at the national level. Operative regionalization has as its basis a theory of rational development which contemplates the linking

of Indian communities into a region and the joining of this region with others of the country and with the totality of global society.

The fourth and final postulate of Indian action programs, and by no means the least important, is that of moral and *ideological mobilization* as a necessary condition for the establishment of a just society: the abolition of overt and covert exploitation; the creation of equal social opportunities for Indians and non-Indians; the development and enrichment of the resources of downtrodden cultures; the provision of aid of all kinds to those who are socially, economically, and culturally dependent in order to permit them the experience of their own development; the right to united and active participation in making political decisions; and the formation of public conscience that recognizes integration of the Indian as necessary for national happiness. These are the elements involved in ideological mobilization.

The social crisis which Mexico underwent in 1968 placed in doubt the capacity of the revolutionary movement of 1910 to resolve problems of development and integration within the context of the postulates presented above. Some critical anthropologists questioned not only the *raison d'être* of Indian policy (*indigenismo*), but anthropology itself. The current administration in my country has given decisive support to applied anthropology by increasing both the number of programs and the budgets for each, in an attempt to modify substantially the undesirable situation created by the "regions of refuge" and, thus, promote national integration.

But, at the same time, the government has placed the responsibility for directing the oldest anthropological institution of Mexico in the hands of the critical anthropologists in order to assure an open door for their ideas. It has done this because of its conviction that the new generation will continue confronting the tasks of integration and development in moral terms, and not purely and simply as scientific problems.

#### Notes

<sup>1</sup>Dr. James Officer, Professor of Anthropology at the University of Arizona, provided valuable assistance in translating and editing this presentation for publication.

## **Chapter 4**

### **Everett C. Hughes: Urban Sociology, Social Problems, and Ethics**

**Thomas Weaver**

At the turn of the twentieth century, while British sociologists were studying the urban poor, as represented by the publications of Charles Booth (1902–03) and Sidney and Beatrice Webb (1913), American urban sociologists began to focus on adjustment of ethnic groups and immigrant populations to the city. According to Everett C. Hughes, recipient of the Society for Applied Anthropology's Malinowski Award in 1974, fieldwork and survey techniques began to dominate the research approach of American urban sociologists.<sup>1</sup> The British sociologists were still concerned with speculation and surmise based on nineteenth century evolutionism. The American urban ecology model was to have much influence on social science, and particularly on the work of Robert Redfield and Oscar Lewis in the construction of urban anthropology (Weaver 1982).

Everett Hughes (1897–1983), Professor of Sociology at Boston College at the time of the Malinowski Award, was trained in the heyday of American urban sociology, a field much concerned with the study of social problems. His teachers were Albion Small, Robert Ezra Park, Ernest W. Burgess, Ellsworth Faris, and other practitioners of the dominant paradigm in sociology of the time. The last four doctorates granted from the University of Chicago in 1928 before the Department of Sociology and Anthropology split were presented to Herbert Blumer, Charlotte Gower, Robert Redfield, and Everett Hughes. Hughes' doctoral work was on the real estate market.

As a student at Chicago Hughes was exposed to a wide range of scholars who were anthropologists at nearby universities and who presented lectures and seminars at Chicago. They included Franz Boas, Bronislaw Malinowski, A. R. Radcliffe-Brown, Ralph Linton, and W. Lloyd Warner. Malinowski's *Argonauts of the Western Pacific* (1922), his economic studies, and his concept of culture were important to Hughes. Hughes regretted not being one of the founders of the Society for Applied Anthropology, but he had become a member by the appearance of the third volume of the journal, *Applied Anthropology*, as *Human Organization* was first named (Hughes 1974:333).

Hughes published on interdisciplinary approaches to the study of society, the history of sociology, social change, personality and division of labor (1928), marginal man, the sociology of knowledge, professions and occupations (1937, 1938), and race relations in industry (1946). According to George Foster (letter to author, 1998), Hughes' work on professions "is still, in my view, required reading for any budding medical anthropologist." He was one of the major figures

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to break away from the empiricism of the early American sociologists. Along with Howard Becker, Charles Loomis, Robert Merton, Talcott Parsons, and Robert Redfield, he helped to develop, in opposition to neo-positivism, a framework that included the importance of theory in research, the concept of system, the structural-functional approach, and the necessity of giving priority to qualitative research (Becker and Boskoff 1957:193–196).

In his Malinowski Award acceptance speech, entitled “Who Studies Whom?” (1974), Hughes recounted some of the early history of the Society for Applied Anthropology and provided a sketch of some of the contributions of its early members. He compared the development and practice of sociology and anthropology in Britain and the United States before and at the turn of the twentieth century. He claimed that anthropology began in the British Empire through the study of native peoples in the colonies. He mentioned the important contributions of Malinowski and Radcliffe-Brown and the beginnings of applied anthropology in Britain. About the students of the two great teachers, Hughes lamented:

The British anthropologists who were trained by Malinowski and Radcliffe-Brown were but a passing generation. They did their work between the great World Wars. They did their fieldwork in their youth, then came home, got academic positions, trained another generation, and are now being retired. None of the Malinowski students turned from the study of “natives” to study Britain itself (Hughes 1974:329).

The major thrust of his acceptance address related to the question of who is studying whom and the problem of access to information.

The person doing the studying used to simply arrogate to himself and his kind the license to study the other. The studied were helpless to prevent being studied except by the means common to all humans of shutting their mouths, by not communicating, by misinforming in one way or another (Hughes 1974:330).

The ramifications of this situation involve matters of trust, of who is paying for the information gained and its ultimate use and whether the researcher has control of this use, of social distance between investigator and people studied, and of reciprocity. “The mere presence of an observer causes danger to people being observed. It can also cause danger to ask certain questions” (Hughes 1974:332). Hughes was also concerned with the use of the privileged information gathered, a point directly relevant to the existence of applied anthropology. “It is as if we were employed by the keepers of information to gather more and more information which they might use for their purposes” (Hughes 1974:331).

Hughes captured the goal of applied and “pure” social science in prediction and service when he wrote:

We want to understand the minds of people of all kinds, all ages, all places, and even all times. We seem to be pushing the boundaries of our mental territories further back into prehistory, down into the subconscious, and out into the cultures and symbol systems far beyond any previous limits. We even try to predict the future in these terms, and to control it (1974:331).

He concluded by observing that we are approaching equality with the subjects of our study. He also made the prediction that soon “someone out in Melanesia will make a study of Malinowski and how he acted when he was among them. They might include a discreet note on who the sponsor was to whom the information was to be delivered” (Hughes 1974:332–333).

A widely published scholar, Everett Hughes also served as president of the American Sociological Association and of the Society for Applied Anthropology.

### Notes

<sup>1</sup>This annual award is given to a senior colleague in recognition of efforts to understand and serve the needs of the world through social science. See Thomas Weaver’s “The Malinowski Award and the History of Applied Anthropology” (2002b) for a brief history of this award and an overview of the recipients and their work, and Weaver’s “Malinowski as Applied Anthropologist” (2002a) for an introduction to Bronislaw Malinowski and his work.

### References Cited

- Becker, Howard, and Alvin Boskoff, eds.  
1957    Modern Sociological Theory in Continuity and Change. New York: Holt, Rinehart and Winston.
- Booth, Charles  
1902–03 Life and Labour of the People in London. Third Edition. 17 Volumes. London: MacMillan & Company.
- Hughes, Everett C.  
1928    Personality Types and the Division of Labor. *American Journal of Sociology* 33:754–768.  
1937    Institutional Office and the Person. *American Journal of Sociology* 43:404–413.  
1938    Position and Status in a Quebec Industrial Town. *American Sociological Review* 3:709–717.  
1946    The Knitting of Racial Groups in Industry. *American Sociological Review* 11:512–519.  
1974    Who Studies Whom? *Human Organization* 33(4):327–334. (Republished at  
          <<http://www.sfaa.net/>>.)
- Malinowski, Bronislaw  
1922    Argonauts of the Western Pacific. London: D. Rutledge and Sons. Ltd.
- Weaver, Thomas  
1982    From Primitive to Urban Anthropology. *In* *Crisis in Anthropology: View from Spring Hill, 1980*. E. A. Hoebel, R. Currier, and S. Kaiser, eds. Pp. 203–220. New York: Garland Publishing, Inc.  
2002a    Malinowski as Applied Anthropologist. *In* *The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers*. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.  
2002b    The Malinowski Award and the History of Applied Anthropology. *In* *The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers*, T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.
- Webb, Sidney, and Beatrice Webb  
1913    English Local Government from the Revolution to the Municipal Corporation Act: The Story of the King’s Highway. New York.

## Who Studies Whom?

Everett C. Hughes

Living among one savage tribe after another, the anthropological fieldworker becomes convinced that culture is something which is constantly at work, which is there for the satisfaction of elementary human needs, which in turn creates new wants and provides means for their fulfillment (Malinowski 1924:36).

The Society for Applied Anthropology was young when I joined it. The members were more interested in problems than in disciplinary boundaries. We were not many. I swore, about that time, never again to attend a learned meeting of more than a hundred people. Some members were engaged in attempts to analyze and measure interaction in small groups: Eliot Chapple (anthropologist), F. L. W. Richardson (geographer), W. F. Whyte (sociologist). Others focused on study of community: Margaret Mead and the late Robert K. Lamb (historian). Some were working in industry. I had been studying race relations in various Chicago industries, and was finding out that workmen don't want things disturbed by the introduction of strange creatures—foreigners, Negroes (they hadn't yet become Blacks), or women—into their circle of fellow workers.<sup>1</sup>

The tenth annual meeting in 1952 was to be a departure. Instead of letting people bring in their own problems and papers, we “intended to conduct a systematic stock-taking of our knowledge in the field of human organization.”<sup>2</sup> A few of us held planning meetings during the year. At each a paper was presented on a phase of that subject. Eliot Chapple presented a document on “The Individual and the Small Group.” I did one on “The Institution.” Margaret Mead fittingly took “The Community.” We held long and lively discussions on each of the topics, under the leadership of Robert K. Lamb, the President. An economic historian by training, experienced in study of the workings of various corporations under one of the Roosevelt agencies, he was teaching social science at MIT [Massachusetts Institute of Technology]. He had the breadth that comes from critical and sensitive study of a variety of social organizations. He produced for his MIT students a document on “Suggestions for a Study of Your Hometown” (Lamb 1952). Robert Lamb died of lung cancer before that 1952 meeting was held.<sup>3</sup> The planned discussions were held at the annual meeting, but we did not carry through the publishing of the book about them. We did learn something about human organization. The small lively group grew and changed as it grew. Now, 22 years after that tenth anniversary meeting there is again talk as to whether anthropology can be applied; and indeed, whether it can be done without being applied.

What about Malinowski? I get a clue from Raymond Firth's (1972) Radcliffe-Brown lecture. He said it was *for* Radcliffe-Brown, not about him. But surely it is fitting to say something about my own Malinowski experience. His *Argonauts of the Western Pacific* was published when I was a student in the then department of sociology and anthropology (sociology

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This address was previously published in *Human Organization* 33,4(1974):327–334.



named first) at the University of Chicago.<sup>4</sup> Franz Boas' *The Mind of Primitive Man* was our gospel in the campaign against race and instinct. I do not remember whether the *Argonauts* made a special impression among the students at that time. Ellsworth Faris had a seminar on social origins in which at least two students went after economic matters in preliterate societies. Malinowski's section on building of a canoe captured my interest. He talked of the organization of work rather than of the division of labor. Functions were divided but it was the organization of the working community that he emphasized. I have used those passages ever since in teaching.

A little later the *Encyclopaedia of the Social Sciences* was published. In it was Malinowski's article on "Culture," which I found very useful. He says:

The primary concern of functional anthropology is with the functions of institutions, customs, implements and ideas . . . . Institutions are not correlated simply and directly to their functions; one need does not receive one satisfaction in one institution. But institutions show a pronounced amalgamation of functions and have a synthetic character (Malinowski 1930:621–645).

About the same time another person who is known as a functionalist, Radcliffe-Brown, had written "By the function of an institution I mean the part it plays in the total system of social integration of which it is a part" (Radcliffe-Brown 1931:13). Both these men were interested in functions, but took them as something to be discovered, on the long inductive road that must be traveled to close the system before the quick deductive kill. Firth says of Radcliffe-Brown that his ideas were better than his ethnography of the Andaman Islanders (1933). The same is said of Malinowski's practice as compared with his "Proper Conditions of Ethnographic Work." Both men taught and coached many brilliant and skillful social scientists. The pupils were, fortunately, not mere disciples.

In the 1930's Malinowski attended a meeting of the American Anthropological Association at Chicago. In a plenary session, Ralph Linton and Malinowski argued sharply on method or perhaps about Freud. As the debate went on, H. J. Spinden rose and desperately asked, "Why all this talk about method? Why can't we just go out and do ethnology like we used to?" At least, my memory recalls it so, whether these are his exact words or not. That was the one occasion on which I had near-personal contact with Bronislaw Malinowski.<sup>5</sup>

Spinden, a man of tremendous ethnological sophistication, did not enlarge on how ethnology had been done in those premethod(ology) days. Malinowski did tell us how we ought to do it and something of how he actually did it. He also left diaries which have been published, and much talked about. In them, we are told, it is revealed that he had not fully practiced what he had preached (Wax 1972). The *Argonauts*, moreover, was dedicated to his "friend and teacher, Professor C. G. Seligman" and was prefaced by James G. Frazer. Frazer praises Malinowski for associating so closely and so long with the people he describes and for getting at their motives, for seeing them in the round. Yet Frazer himself is quoted as having exclaimed "but Heaven forbid" when asked if he had met any of the natives he had written about. This accords with Malinowski's own statement that "the native is not the natural companion for the white man"; however, that apparently belittling remark occurs in a rather long and detailed account of his many relations with his natives, including seeking them out as a relief from loneliness.

Seligman, to whom Malinowski dedicated the *Argonauts*, has written the article on "Applied Anthropology" for the *Encyclopaedia Britannica*. The practical application he advocated was that of colonial administrators and of missionaries. The administrator should

learn, for instance, the system of inheritance and bride price of the native under his charge; he should learn that diminution of population may result from stopping headhunting. The missionary and the administrator have no qualms about interfering in the intimate affairs of “native” life. Seligman also notes that while moral ideas have changed about enslaving or exterminating peoples, unable or unready to adopt or adapt to a higher civilization, “there had been nothing before comparable with the persistence and energy with which the higher race is imposing itself on the backward peoples.”

R. R. Marett, who wrote on “Anthropology” in the same *Britannica* in which Seligman wrote of the “Applied Anthropology,” says there is much that the administrator and the missionary can learn from anthropology that will be of practical help in their work. There is no suggestion that the work will not continue, or that the sun will ever set on the Union Jack.<sup>6</sup>

These sketchy references to the fathers of anthropology in the British world show that anthropology was a phenomenon of empire’s reach out into its colonies. Natives, aborigines, the undeveloped and backward races, savages, and pagans are named as objects of study. The people who study them are of the higher race, the superior race; they speak of themselves as White men. There were also missionaries and administrators who were expected to be in the field at work on the natives, but who would do their work better if they studied and understood the culture of their charges and the souls they were to save (for the good of their own).

None of these masters thought of abandoning empire; that is, of leaving the “natives” to themselves. They were, however, convinced that these peoples were interesting, and that, as Marret says, “Have we the courage to seek to know ourselves as truly we have been and now are—whatever else we may aspire to become? If so, then anthropology may go forward.”

These anthropologists see the main grounds for studying savages in a scientific way simply in the fact that we know so little about them; it is assumed that knowledge must invade unknown fields, it must go on and on. These are the men who gave us our knowledge of other human societies, our ideas concerning their likenesses and differences, and our methods of study.

Empires with colonies and areas occupied by natives who may be studied by people who come out from the metropole with permission of the authorities are fading away. The natives were never forced to help the anthropologist in his or her investigations; the pax Britannica could generally restrain natives from doing violence to the visitors; and even from doing violence to one another, which never in any case was so devastating as the violence Europeans did to them by sword, the gospel, and destruction of their ecology. We Americans and the Canadians could study our natives on reservations or on the remaining fringe of wilderness whenever we wished. The government didn’t mind, and the “natives” could—if they objected seriously—defeat the intruder by passive resistance.

The British anthropologists who were trained by Malinowski and Radcliffe-Brown were but a passing generation.<sup>7</sup> They did their work between the great World Wars. They did their fieldwork in their youth, then came home, got academic positions, trained another generation, and are now being retired. None of the Malinowski students turned from the study of “natives” to study Britain itself. Pax Britannica became shaky. Native nationalistic movements did not always welcome Europeans who came among them to study their cultures. They became sensitive to their image in the eyes of the larger world. Now that some former colonies have become independent of empire, whoever holds the reins may be greatly concerned about how the local cultures are described. When the natives come to power, they sometimes send curious outsiders home. If European colonists take over, as in Rhodesia and South Africa, strict police control may be established over movement of people and information. At any rate, the era of easy access to

people and institutions in the former colonial areas, for purposes of description or “the enlargement of human knowledge,” is over, and may be over for a long time to come. In due time, these regions under their own authority and with their own bureaucrats and intelligentsia may study and analyze their societies on their own terms. China already sets the terms of gathering information, but of course China was never a simple preliterate society. But she was colonial in the sense that Europeans enjoyed extraterritoriality for many purposes.

The physical, political, economic, and social distance between the centers of empire and the people “out yonder” has greatly decreased—as may be seen in Margaret Mead’s (1956) two studies of her Manus. As shown in the Preface and Acknowledgments to *New Lives for Old*, the number of people involved in her later study was considerable.

In the last decade of the nineteenth century and the prewar decade of the twentieth, a new movement arose in the seat of empire, the great English cities. It was the social survey movement. Industrialists, and others interested in social welfare, set out to study the poor in their own country. The man who organized and financed the first great survey, *The Life and Labour of the People of London*, was a shipping magnate, Charles Booth.<sup>8</sup> His team, including Beatrice Potter (Mrs. Sydney Webb), went from door-to-door in London gathering very much the same information as Malinowski and others in the South Seas.

“Who are the people of England? How do they really live? What do they really want? Do they want what is really good? And if so, how is it to be given them?” were Booth’s guiding questions. Those last two questions were not in the anthropologists’ concern about New Guinea, except in a minor degree. But the other questions were much the same: their houses, their food, their amusements, their family life, their work, their religion, and their health. Seebohm Rowntree (1901)—another industrialist—studied York, in much the same detail, seeking to find “the poverty line,” the level below which life could not be decent. Rowntree described in detail how people lived, and the mistakes made by middle-class reformers who build working-class houses with a sitting room—for people who always sat in the warm kitchen except at weddings and funerals. In London, the same middle-class people were busy trying to stop the publicans from selling a bucket of beer to children to take home to their father when he came from work—in the belief that it would lead the children to early drunkenness—which showed ignorance as abysmal as that of missionaries who tried to break up polygynous marriages.

The difference between those social surveyors and ethnologists was that the surveyors had a stronger sense of purpose—the purpose of solving the “social problem”—i.e., poverty and its accompaniments. The anthropologist and those social surveyors had one favorable circumstance in common. No one tried to keep them from moving about among their natives and asking what questions they pleased. Such was the freedom of the late nineteenth-century English city, although it is reported in the Booth survey that the police always went in pairs in the Limehouse district.

The early American social surveyors apparently had somewhat the same freedom. But what they found was a complex of ethnic communities, each inhabited by immigrants of some European region and their institutions. In the case of Pittsburgh (Kellogg 1909–14), the industrial leaders and the police practically closed the city to social investigation for many years after that survey. Social studies in America became essentially the study of people of other ethnic origins by middle-class people of older American stock. Eventually, old-stock Americans were studied if they were very poor or of Negro descent. In this country, but not so much in England, the people who did social studies of this kind came to call themselves sociologists. The financing of the studies came, not from the people studied or their agents, but from philanthropic, religious, and

welfare sources. In most parts of the country, the person who wished to make studies could gain access to the people he wished to study at their homes. To see them at their places of work was more difficult. More and more people entered the labor force and thus spent a considerable part of their time in closed places of work. Children went to school, another closed institution.

American sociology became closely associated with urban studies of the poor, and of the immigrant populations of the cities.<sup>9</sup> The British anthropologists continued to do their fieldwork out among savages, but spoke of their aim being that of sociology—of a systematic analysis of the societies they worked in. In this country, as some of us remember and as Lucy Mair tells, some people got the idea that methods of anthropological field observation could be applied in industry and other institutions in the observers' own society. The people who did that were the founders of this society. As Firth (1930) says in "Social Anthropology," the relationships studied are primarily those that are standardized or institutionalized. In our society the role of field observer itself is institutionalized, hence the pertinence of the question—who can and who does study whom. To begin with one has to get access to people in various relationships in order to study them. Who gives the access and on what terms? How does one succeed in getting the pertinent information and live to observe another day in the same or another setting?

I have belabored somewhat the development of social study called anthropology, which is based on observation of people of one kind by people of another. The person doing the studying used to simply arrogate to himself and his kind the license to study the other. The studied were helpless to prevent being studied except by the means common to all humans of shutting their mouths, by not communicating, by misinforming in one way or another.

In principle, any person is the peer of any other. Thus, in his quality as human being, any of us has the right to study any other and also to protect himself from the peering eyes of others. In our special roles or offices, as students, adults, parents, sociologists, anthropologists, we gather and disseminate information apparently suitable to our position and that of the people we study in their position. Much of our information is privileged; to be used for particular purposes by particular people and disseminated to particular publics or closed groups. We are certainly a society in which a great deal of information is gathered about all of us for various purposes. We are not always sure that it will be used only for the purpose for which it was intended. Usually, or at least often, information is given in some sort of exchange; there is a bargain about it, explicit or implied. Some of the information is got in very impersonal form, stored, and retrieved. It is used for administrative purposes, for recruiting armies, workers, for entry to universities, for distributing services and goods, for controlling and predicting elections, choosing mates, and so on. As a result, we have become very sensitive about inquiries, and about being watched, but also curious concerning what is found out about our neighbors and other people. The new social surveys, which are really opinion surveys, are highly impersonal and done with guarantees of anonymity. On the other hand, the number of items which are on many of the instruments used in these surveys is so great that a person could, by the statistical methods of the surveys, trace down the informants if he wished. Our trust in these instruments, and in the people who make these surveys, is amazingly high. I believe that people in some countries consider us, indeed, rather gullible to believe that information will not be used for purposes for which it was not intended. However, we are very trustful; we, who are engaged in social anthropological and sociological fieldwork, must keep the trust with the information which we have and, consequently, help keep the trust of the public in those of us whom they have trusted.

We who do the gathering of information about our culture and society are, to use the phrase of Simone de Beauvoir, in full *force de l'age*. We are in the years of full energy,

moderately solvent, consider ourselves highly educated, and beyond question as to our motives. We set out to study the not-yet-born, the young, those who are so old as to be supposed to be of waning powers, and ignorant, those whom we call retarded, those who are deviant in some way that is troublesome to society, those who need financial help, and those who are more or less under the control of the fully vigorous. It is the ethos of our time and of our particular kind to expand our knowledge in all directions. I think this desire to know about all kinds of people and circumstances, social as well as physical, is a mark of our age. Sometimes we act as the agents of the very rich in gathering information, or rather the agents of those who are in a position of power and command over people and who govern some parts of the lives of people. This gives them much privileged information. It is as if we were employed by the keepers of information to gather more and more information which they might use for their purposes.

But that is not the whole story in spite of what people say of us. There are very few people engaged in social anthropology, applied or pure, who do not have a real interest in knowledge for its own sake, or vaguely for the good of all concerned. We are part of a generation of people who are colonists of the mind. That is to say, we want to understand the minds of people of all kinds, all ages, all places, and even all times. We seem to be pushing the boundaries of our mental territories further back into prehistory, down into the subconscious, and out into the cultures and symbol systems far beyond any previous limits. We even try to predict the future in these terms, and to control it.

One important prerequisite of entering any social system is to know something about it in advance. This we can achieve by our own social experience, by our use of language, by the social gestures we have seen, and by our knowledge of the meaning of whole, vast systems of symbols.

It seems evident to me that people vary tremendously in their sensitivity to social gestures and systems as learned in their experience growing up in society. Whether some positions in society are inherently better observation posts than others, I don't know. But I think that is likely. I have had students whose fathers were in some kind of retail business, and who had learned to be exceedingly sensitive to the social gestures of the potential buyer of shoes, shirts, or whatever. I used to hear my father preach what was apparently the same sermon to two or three different congregations of decidedly different education and social background. The sermon was altered to suit the case, and I did not fail to note the differences and to try to account for them. One of the things I see coming out of the recent studies of natural communities of lions, chimpanzees, and so on, is not how their brains work, but how they play and how they learn to know what is a serious gesture probably leading to injury and what is play (Schaller 1972; Goodall 1971; Washburn and McCowan 1972). I believe our human education would be much better done and so would our sociological and anthropological research if we stimulated the sensitivity that comes from noting the social gestures of others and learning to compare them regardless of the particular substance. As teachers of social science, and as social observers, we have an obligation to catch the social points of sensitivity in our students and the people whom we study. Having found it we should expand it, and make it more abstract so that it can be used as a scientific instrument. As a matter of fact, all anthropology is applied in this sense. We learn more by applying what we already know. This means, of course, that we must enlarge the area within which we can feel identification with others, and in which we can compare their social gestures.

Here comes the question of reciprocity. When we are studying those other people, are they also observing us, and interpreting the meaning of what we are doing? Of course they are. And that is what makes them so interesting. But in our observations of others, we have to look

further as professional social scientists than they will. We have to sense whether in them we have colleagues who are playing the game in the same way as we are, people who can be trusted with the kind of findings we will get from our observations. Educating young people into our trade of social observation is in considerable part a question of determining whether there lie in them the qualities which will make of them good social science colleagues who will know just what social distance to keep from the people they study, what codes to observe in disseminating information about them.

I think it is clear by now that most of us believe that on principle anyone can study anyone else and that anyone has a right to protect himself against the effects which come from giving up information. We then must understand what set of threats we create when we go into any organization; what risks will come to the people whom we study from the fact of our having studied them. This is brought out beautifully in the study which Barrie Thorne (1971) made of the draft resisters. The draft resisters were in a position of risk from the very fact that they were resisting compliance with the law. Barrie Thorne, in studying them, seemed to be participating in this movement which was risky to the participants, but not to her, the observer. Since she was a woman, she could not undertake their risks. This is a kind of thing that is commoner than one may believe, and which we should become very sensitive to. I was once asked in Atlanta, Georgia, in a talk that was being broadcast, to tell the southern Black leaders that they should be braver in resisting White domination, White police rules, and so on. I did not give that encouragement as it might have caused harm to my hosts. There are many situations where the mere presence of an observer causes danger to people being observed. It can also cause danger to ask certain questions. If one asks a question, he implies that there may be more than one answer to it. And if it is a question to which, in that circle, there is only one accepted answer the questioner puts his listeners into jeopardy, or may do so. Understanding the risks of various social gestures, questions, and actions in a social setting is one of the most important applications of anthropology. In *Die Letzten Tagen der Menschheit* (*The Last Days of Mankind*) of Karl Kraus, there is a woman reporter in Austria who, in order to observe what is going on at the battlefield, stands up conspicuously to see what the enemy is up to. In so doing, she reveals where the Austrian soldiers were. Very often a question or an item of information can be used in such a way that revealing it will reveal the position of the people being studied, and thus cause them distinct harm. I have had a bit of this problem, incidentally, in my dealings with the Québécois, and the other Canadians.

This brings up the whole matter of social distance between investigators and the people being studied. It seems that Malinowski didn't go to dinner very often with the people out there in the Trobriands. But how often should he, as a fieldworker, go to dinner and with whom? When we were doing the medical school at the University of Kansas, we broke the team into three positions. Becker and Geer played with the students. Strauss did the interns at night, poor devil, and I did the staff. We were not seen often together in the hospital or the medical school. We kept our distance from one another. In a way we replicated the social structure of the hospital in our own associations.

There are a number of reasons for maintaining social distance. One is that the person doing the fieldwork might have need of some of his own home-style cooking, now and again. He wants to keep his own identity, and he has his own tastes. On the other hand, he wants to get close to the people he is studying, but not be drowned by them. Every social role is a complex of distances. He must leave some time, and he must be prepared to leave. He must keep a certain distance in order to be objective. Finally, he must not go native. There is a certain temptation for

students of medical institutions to become little doctors, instead of remaining sociologists. They may take on the mannerisms and some of the attitudes of the people they are studying. That is all right provided they keep them carefully separated from their attitudes appropriate to other social roles. These things all have to be discovered, and they are all applications of what one has learned already. If one makes some mistakes in these applications, that, too, should be turned into an occasion for learning more about the organization he is studying. I think it is true in a very special sense that in anthropological fieldwork one is constantly applying what he has learned already. What he learns may be applied by others, by a sponsor, by an employer, or by someone who simply reads what he has written, for some other purpose than that which the investigator has in mind himself. As a matter of fact, if what one learns is fundamental in its importance, and it is true, he cannot be sure of what uses will be made of it. He may hope that what he learned will not be put to evil uses, but he cannot guarantee it.

I seem to be getting this somewhat confused, but the whole thing is confusing, and that is as it must be. We can stop now and again, draw up an elaborate questionnaire and put it through all the proper machines and find the paths from here to there. But that will always be nothing more than a stopping place. One must take off again, get new experiences, and make new discoveries enlarging and alternating what one knows already.

As I look at the program of this meeting, I discover that most of the topics have to do with people who are not members of the Society for Applied Anthropology. We elect to have a dinner here to which we are not inviting all the people whom we have studied. Many of them live in the slums and eat food which we would not like or at least not like every day, although we are more adventurous about food than nineteenth-century Englishmen. But we seem to be approaching the time when social study is carried on among people who might become our peers if we were a bit more successful in our work; although they might not want to be.

Malinowski went out to the Trobriands and studied people whom he probably would not have liked to have to dinner. Radcliffe-Brown studied the Andaman Islanders who ate pig which they cooked on sticks over a fire. It is called anthropology, the study of anthropos, man. When finally we got around to studying the people here at home in our own cities we called that study "sociology," the study of our social companions. But in neither case did we as a rule study the people who are our closest companions. We are still studying people who are relatively deprived. We still keep, in practice, to the idea that there are those with a mandate to study, and those with the fate of being studied whether to be preserved, as were the aborigines, or to be enlightened and rehabilitated. But we are approaching equality, whether we call ourselves anthropologists or sociologists, with the objects or subjects of our study. We may one day be studying no one but our peers, people who might just as well be studying us. It may not be long until someone out in Melanesia will make a study of Malinowski and how he acted when he was among them. They might include a discreet note on who the sponsor was to whom the information was to be delivered.

I am not content with this talk at all. It is a thing which gets very confusing because there are so many circumstances which one cannot foresee in undertaking social studies. Before closing, however, let me read to you a paragraph of a letter from a man whom many of you know and who has written an article on cockfights out in Bali (Geertz 1972). He went to an illegal cockfight and while there the police were heard approaching and the crowd had to run in order not to be arrested; for cockfighting, like gambling here, is not legal but is widely practiced. I asked that man whether he had a right to observe the cockfight and whether he had a right to burst into the garden afterwards. He answers thus:<sup>10</sup>

I am not sure who has a right to observe whom, or indeed whether that formulation makes any sense anyway. As for the cockfight, I was invited to it by my landlord, who was a full citizen of the village, and so I guess I had as much right to be there as any other guest of a village inhabitant. As for bursting into the garden afterwards to save my hide, my own view is that when one's hide is at stake, one has the right to burst into anyone's garden. No Balinese would disagree.

### Notes

<sup>1</sup>The small circle who founded the society was composed of a number of persons who had been at Harvard in the 1930's in anthropology or the school of business or both. Some had worked with W. L. Warner in Yankee City; others in the Western Electric study in Hawthorne, Illinois. Margaret Mead had worked already as an anthropologist in Melanesia, and had taken as active interest in many programs of study and action in the United States. I was not of that founding circle, but joined the society in time to receive Volume 3 (1944) of *Applied Anthropology*, as our journal was then called.

<sup>2</sup>The Society had set up in Chicago a "Clearing House for Research in Human Organization" under the direction of Charles Harding. For an excellent account and discussion see Mair (1968). She includes the story of the origin of this society.

<sup>3</sup>The members were sent a letter with the plan of that 1952 meeting. We were to discuss, as to method and findings, three studies of French Canada. The paper which I prepared for my session was called "The Natural History of a Research Project." It was later published in *The Sociological Eye* (1971).

<sup>4</sup>Robert Redfield, Charlotte Gower, Herbert Blumer, and I were in the last batch of doctors before the department was split. We were the 1928 doctors.

<sup>5</sup>Malinowski and Radcliffe-Brown did much to break down the idea, common when I entered graduate school, that primitive man obeyed his customs without deviation from the norms.

<sup>6</sup>The current *Bulletin of the Royal Anthropological Institute of Great Britain and Ireland* documents further that the early anthropologists saw themselves as protectors of Aborigines out in the empire.

The members of the Institute are lineal successors to the founding members of the Ethnological Society of London, who in February 1843 formed a breakaway group of the Aborigines' Protection Society which had been founded in 1837 in the aftermath of the early nineteenth century Quaker campaign against the African slave trade. The new society was to be a "centre and depository for the collection and systematization of all observations made on human races," and its members were distinguished from those of the parent body by their bias towards scholarly rather than humanitarian ideals. Almost from the start, the membership found itself divided over racist issues, and between 1863 and 1870 there were two organizations, the Ethnological Society and the Anthropological Society. The Anthropological Institute of Great Britain and Ireland (1871) was the result of an amicable merger between these two rival bodies. Permission to add the word "Royal" to the title was granted in 1907.

<sup>7</sup>Outstanding among these students were Evans-Pritchard, Meyer Fortes, Raymond Firth, and Audrey I. Richards. Especially pertinent to this, our subject, are the following two books: Raymond Firth's *Primitive Economics of the New Zealand Maori*, in which Chapter 1, "The Study of Primitive Economics," is another important statement on method and theory; and Audrey Richards's, *Hunger and Work in a Savage Tribe: A Functional Study of Nutrition Among the Southern Bantu*. Malinowski wrote an important preface to this volume. Richards's chapter is an important document on the functional method in anthropology.

<sup>8</sup>Here are some important books about the English survey movement: *Charles Booth—Social Scientist* by Simey and Simey; *Charles Booth on the City: Physical Pattern and Social Structure* by Harold Pfautz; and *Social Surveys* by Caradog Jones.

<sup>9</sup>There was another branch, rural sociology, which was very much an applied subject, whose practitioners kept close to the people they studied. LePlay had a good deal of influence on British studies of the standard of living; less so in America.

<sup>10</sup>Personal communication, letter from Clifford Geertz.



### References Cited

- Booth, C.  
1892     The Life and Labour of the People of London. 17 Volumes. London: Macmillan.
- Firth, R.  
1929     Primitive Economics of the New Zealand Maori. New York: Dutton.  
1968     Social Anthropology. *In* The International Encyclopedia of the Social Sciences. Vol. I:320–324. London: Macmillan.  
1972     The Skeptical Anthropologist? Social Anthropology and Marxist Views on Society. The Proceedings of the British Academy 58:1–39.
- Geertz, C.  
1972     Deep Play: Notes on the Balinese Cockfight. *Daedalus* 101:1–37.
- Goodall, J.  
1971     In the Shape of Man. New York: Dell.
- Hughes, E.  
1971     The Natural History of a Research Project. *In* The Sociological Eye. Chicago: Aldine-Atherton.
- Jones, C.  
1949     Social Surveys. London: Hutchinsons.
- Kellogg, P. U., ed.  
1909–14 The Pittsburgh Survey. 6 Volumes. New York: Russell Sage.
- Kraus, K.  
1957     Die Letzten Tage der Menschheit. Munchen: Kosel-Verlag.
- Lamb, R.  
1952     Suggestions for a Study of Your Hometown. *Human Organization* 11:19–32.
- Mair, L.  
1968     Applied Anthropology. *In* The International Encyclopedia of the Social Sciences. Vol. I:325–329. London: Macmillan.
- Malinowski, B.  
1922     The Argonauts of the Western Pacific: An Account of Native Enterprise and Adventure in the Archipelagoes of Melanesian New Guinea. London: D. Rutledge and Sons, Ltd.  
1924     The Life of Culture. *In* Culture, the Diffusion Controversy. New York: W. W. Norton.  
1930     Culture. *In* Encyclopedia of the Social Sciences. Vol. 4:621–645. London: Macmillan.
- Mead, M.  
1956     New Lives for Old: Cultural Transformation—Manus, 1928–1953. New York: Wm. Morrow and Company.
- Pfautz, H. W., ed.  
1967     Charles Booth on the City: Physical Pattern and Social Structure. Chicago: University of Chicago Press.
- Radcliffe-Brown, A. R.  
1931     The Present Position of Anthropological Studies. *In* British Association for the Advancement of Science. Centenary Meeting. Section H, P. 13, London.  
1933     The Andaman Islanders. London: Cambridge University Press.

- Rowntree, S.  
1901     Poverty: A Study of Town Life. London: Macmillan.
- Richards, A. I.  
1932     Hunger and Work in a Savage Tribe: A Functional Study of Nutrition Among the Southern Bantu. London: Rutledge.
- Schaller, G. B.  
1972     The Serengeti Lion: A Study of Predator-Prey Relations. Chicago: University of Chicago Press.
- Simey, T. S., and M. B. Simey  
1960     Charles Booth—Social Scientist. London: Oxford University Press.
- Thorne, B.  
1971     Resisting the Draft: An Ethnography of the Draft Resistance Movement. Ph.D. Dissertation, Brandeis University, Waltham, Mass.
- Washburn, S. L., and T. McCowan  
1972     Evolution of Human Behavior. *Social Biology* 19:163–170.
- Wax, M. L.  
1972     Tenting with Malinowski. *American Sociological Review* 37:1–13.

## **Chapter 5**

### **Gunnar Myrdal: Interdisciplinary Research, Policy Science, and Racism**

**Thomas Weaver**

In a tribute to Swedish economist Gunnar Myrdal, recipient of the Society for Applied Anthropology's Malinowski Award in 1975, Immanuel Wallerstein elaborated on themes that characterized his life work—underdevelopment and racism, separation of the social sciences, and the promotion of interdisciplinary research (1991:80–103).<sup>1</sup> Myrdal was one of the first social scientists to link social theory, policy making, change, and development. The separate social science disciplines, Myrdal argued, “had come into existence only to serve the convenience of specializations in research and teaching” (Myrdal 1975:327). Foreshadowing the ideas of Harold Lasswell and others who developed policy science as a discipline, Myrdal saw policy as a variable that is constricted by and reacts to exogenous conditions and changes. He believed that “social science is essentially a ‘political science’” (Myrdal 1975:328) and that practical conclusions are important for theoretical formation as well as for problem solving (Myrdal 1944:1045).

Gunnar Myrdal (1898–1987), who was Morton Geobus Visiting Distinguished Professor at the City College and the Graduate Center of the City University of New York when he received the Malinowski Award, is most famous for a ground-breaking study of racism in the United States, *An American Dilemma* (1944). In 1938 the Carnegie Corporation had asked Myrdal, then at the University of Stockholm, to look at the problem of the Negro in America. Myrdal's belief in interdisciplinary research was reflected in his choice of collaborators and consultants—over fifty of the latter, among them anthropologists M. F. Ashley-Montagu, Allison Davis, Melville Herskovits, and fellow Malinowski Award winner John Gibbs St. Clair Drake (Weaver 2002a) and sociologists Arnold Rose, Edward Shils, and Louis Wirth. The book he produced concluded that the “Negro problem” was really a white problem. Early in the treatise he identified the inconsistency between Americans' behavior toward Blacks and the American Creed of “equality for all.” His use of class and caste as organizing principles ignored Marxist ideas of class division and struggle. He recommended improvements in education to eradicate racism—in separate, but equal facilities—a curious recommendation in light of the notion he also expressed that more contact between the groups would improve relations. While Myrdal was criticized for not providing a better cross-culturally applicable theoretical framework in *An American Dilemma*, and for not explaining the origins of prejudice, the book became a major cornerstone for later studies (Madge 1962:255–286).

In keeping with his interdisciplinary interests, Myrdal's Malinowski Award acceptance lecture was on “The Unity of the Social Sciences” (1975). Whether dealing with racism or

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underdevelopment, his framework consisted of examining the problem in its broadest context by considering not just economic variables, but also what his profession called noneconomic elements. Noneconomic elements include “educational and health facilities, . . . the distribution of power, . . . economic, social and political stratification, . . . [other] institutions and . . . intentionally induced policy measures applied in order to change one or several endogenous factors” (Myrdal 1975:328). Myrdal recognized that social systems are subject to what he called circular causation by which he meant “if one condition changes other conditions will change in response, and those secondary changes in their turn cause new changes” (Myrdal 1975:328).

Myrdal was critical of most economic research on underdevelopment. Economists, he believed, depended too much on such factors as GNP, assumed that consumption existed outside of growth models, and misused concepts such as unemployment. Unemployment, as typically defined by economists, did not measure “involuntary worklessness,” a condition that characterized underdeveloped regions. In his study of development in South Asia (1968), Myrdal criticized the belief that the introduction of capital leads to self-sustained growth. His approach differed from standard development economics by subscribing to a holistic perspective that embraces all social science in a unified or integrated development model and examines the interrelated human, social, and institutional elements in development studies.

Despite his criticism of economics, Myrdal still proudly considered economists the “cavalry of the social sciences,” characterizing them as never timid in

constructing macromodels and producing economic plans for a nation and for the whole world. . . . [If] you place an economist in the capital of an underdeveloped country and give him a few assistants, he will on demand produce a plan for development. . . . No anthropologist, sociologist, [or] psychologist . . . would ever think of behaving in this way . . . But megalomaniac or not, it is a socially useful inclination. What nations and their political leaders do need, are plans for how they should move (Myrdal 1975:330).

While economists still today monopolize development planning, Myrdal felt that social scientists should assume a stronger role in policy recommendation, a process he referred to as social engineering. Other social scientists were not providing the information required in economic planning and not conforming to the planning process (Myrdal 1975:331). Research must be oriented to the future and be concerned with the scientific planning of change. Myrdal criticized anthropologists and at the same time urged them to become involved in the planning process by communicating with planners, policymakers, and economists. He chastised anthropologists for not taking holism to its logical end by including the institutional, national, and international contexts in their studies of underdeveloped societies.

Anthropologists must challenge the economic planning of policy makers. This was a theme that permeated many of Myrdal’s other contributions (1958, 1960, 1970). Anthropologists have an advantage in that they subscribe to a holistic model, although they deal with smaller segments of a society, and the familiarity gained with social problems in underdeveloped regions can be useful in the integrated work proposed by Myrdal. Myrdal’s ideas have equal application today in applied, policy, and development anthropology (Weaver n.d.).

Gunnar Myrdal’s distinguished career included political as well as academic appointments and elections, nationally and internationally. His awards include 32 honorary doctorates from

universities from every corner of the world. In Sweden, Myrdal was an elected senator, minister of commerce, and chair of numerous commissions, and he was General Secretary of the United Nations Economic Commission of Europe. In 1974 he was awarded the Nobel Prize in Economics for “pioneering work in the theory of money and economic fluctuations and penetrating analysis of the interdependence of economic, social and international phenomena.” He was co-recipient with his famous wife, Alva, also a diplomat and politician-scholar, of West Germany’s Peace Prize for literature and public service. He was a member of the Royal Swedish Academy of Sciences, the Econometric Society, the American Economic Association, Americans for Democratic Action, the Hungarian Academy of Arts and Sciences, and the American and the British Academies of Arts and Sciences. He is by far the most internationally acclaimed recipient of the Malinowski Award.

### Notes

<sup>1</sup>This annual award is given to a senior colleague in recognition of efforts to understand and serve the needs of the world through social science. See Thomas Weaver’s “The Malinowski Award and the History of Applied Anthropology” (2002c) for a brief history of this award and an overview of the recipients and their work, and Weaver’s “Malinowski as Applied Anthropologist” (2002b) for an introduction to Bronislaw Malinowski and his work.

### References Cited

- Madge, John  
1962     The Origins of Scientific Sociology. New York: The Free Press.
- Myrdal, Gunnar  
1944     An American Dilemma: The Negro Problem and Modern Democracy. New York: Harper and Brothers Publishers.  
1958     Rich Lands and Poor, the Road to World Prosperity. World Perspectives 16. New York: Harper.  
1960     Beyond the Welfare State; Economic Planning and its International Implications. Kyale Law School. Storrs Lectures on Jurisprudence, 1958. New Haven: Yale University Press.  
1968     Asian Drama: An Inquiry into the Poverty of Nations. New York: Pantheon Books.  
1970     The Challenge of World Poverty: A World Anti-Poverty Program in Outline. Christine A. Herder Lecture Series, 1969. New York: Pantheon Books.  
1975     The Unity of the Social Sciences. Human Organization 34(4):327–331. (Republished at <<http://www.sfaa.net/>>.)
- Wallerstein, Immanuel  
1991     Unthinking Social Science: The Limits of Nineteenth-Century Paradigms. Cambridge: Polity Press.
- Weaver, Thomas  
2002a     J. G. St. Clair Drake: Activist-Advocate before His Time. *In* The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.  
2002b     Malinowski as Applied Anthropologist. *In* The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.  
2002c     The Malinowski Award and the History of Applied Anthropology. *In* The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.  
n.d.     An Appraisal of Applied Anthropology in the U.S.: 1985–1998. To appear in Revista de Antropologia Aplicada, Barcelona.

## The Unity of the Social Sciences

Gunnar Myrdal

As I wrote your President, Professor Gonzalez, when she informed me that I had been chosen to be the recipient of this year's Malinowski Award, I do not feel myself worthy of this honor. I have not made any contribution within your discipline. The question can thus properly be raised, how I could have the audacity to accept it. I suppose the honest answer must be that I am too human and defenseless against flattery. I also looked forward with much pleasure to being together with anthropologists for a couple of days. I, like all others who read books, have, of course, enjoyed occasionally following an anthropologist's adventures among primitive people even more than reading a novel.

And in my own studies I have often had occasion to take into consideration works done by anthropologists. This was true when more than 30 years ago I wrote *An American Dilemma*. At that time it was a rather new thing that anthropologists turned to research in Western countries, but a few had then done so in the field of my study on American race relations, particularly in the rural South Asia, I could not altogether overlook the numerous village studies produced by anthropologists, sometimes calling themselves sociologists. At both times my necessarily rather cursory studies of their articles and books taught me much of my own work. But it also gave rise to certain humble, but systematic criticisms of their approaches, to which I shall return toward the end of my address.

If I am allowed to continue along this personalized line of recollection, my conception of the social sciences as forming a unit was already present in my scholarly life when in the early thirties I turned toward problems of equality in my own country, Sweden. I, who had started out as an ardent "theoretical" economic theorist, found that my theory left me with tools that were inadequate for the study of social reforms. Such a study could not rationally be confined to economic factors like production and distribution, but had to be conceived in terms of practically all human relations.

I was brought to state already then, that in reality there are no economic, sociological, or psychological, etc., problems but just problems, and they are all mixed and composite. Our separate disciplines in the social sciences had come into existence only to serve the convenience of specializations in research and teaching. They had no logical justification, only a practical one.

I was also then brought to lose my inhibitions about transgressing the boundaries of separate social sciences. In dealing with a problem, it could never be a legitimate excuse that certain facts or causal relations between facts lay outside one's own field of knowledge. Then as now, however, I was not giving in to dilettantism but insisted upon expertness. That, of course, represents a real difficulty that has to be overcome. But I never felt that difficulty overwhelming. Both the rule and the established practices for truthseeking are very similar, if not identical, whatever one is studying.

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I gather that, in principle, this has always been the anthropologist's philosophy, that he is dealing with a total cultural situation. And this then represents a bond between us.

In this way preconditioned, I came then toward the end of the thirties to the study of race relations in America, which resulted in *An American Dilemma*. At that time, as now, it was a widespread preconception that there was a "basic factor" and that, at bottom, the Negro problem was an economic problem and rooted in the economic system. This approach could be shared by the more radically inclined researchers, who argued for a change of that system, and the conservatives, who did not want, or anyhow did not believe in the likelihood of such a change. Both could on this ground deprecate campaigns for winning piecemeal legal redress through the courts, and also the efforts of education and all campaigns for reform. Then as now, this approach was felt to bear the mark of hardboiled scientific objectivity in contradiction to the credulity of the do-gooders.

Without today having the opportunity to go deeper into the methodological problem I here touch upon, I only want to say that, then as now, I looked upon this approach as superficial and wrong-headed. There is no "basic factor." Everything is cause to everything else. It is not even feasible to define clearly this "economic factor," supposed to be basic.

In one section and in a methodological appendix I developed the approach of the interdependence between all factual conditions that were of importance, directly or indirectly, to the status of the Negro and to changes in it. And I made this approach the main hypothesis for my analysis of the various aspects of the problem I had to deal with.

After this general reference, and without further illustrating it, let me now in the most general form give the essence of this approach, which I have thereafter always tried to use, whatever problem I had to deal with, more specifically in *Asian Drama*. My further comments will refer to the problems of underdeveloped countries, but they have a wider bearing.

Crucial to the economist is the concept of development, about which I will say more later. By development, upwards or downwards, I mean the movement of the *entire social system*, and I believe this is the only logically tenable definition of development.

As I see it, this social system encloses, besides the so-called economic factors, all noneconomic factors of relevance for the movement of the system, including, for instance, educational and health facilities, but also more fundamentally the distribution of power in society, more generally economic, social and political stratification, and broadly institutions and attitudes—to which we have to add, as an exogenous set of factors, intentionally induced policy measures applied in order to change one or several of these endogenous factors.

The dynamics of this social system are determined by the fact that among the endogenous conditions there is *circular causation*, implying that if one condition changes other conditions will change in response, and those secondary changes in their turn cause new changes all around, and so forth. The conditions and their changes are thus interdependent.

This is why all of them must be taken into account when studying the movement of the system and also when analyzing what happens to one set of the conditions, for example, economic factors; or one economic indicator, such as production or GNP. Only a holistic, what I call "institutional," approach is logically tenable.

The changes which in this model are defined as exogenous—that is, policy measures—are under a wider perspective also dependent on the endogenous conditions and their changes, to which they are reactions, and which also, in many ways, constrict their scope and direction. They are kept separate in this model of circular causation, in order to preserve a

freedom for *development planning*—that is, policy deliberations and decisions conceived of as not entirely restricted and determined by the other, endogenous conditions and their changes.

There are three aspects of circular causation. One important characteristic is that usually, though not always, a change of one condition results in secondary changes of other conditions proceeding *in the same direction*. To give an abstract example: improved nutrition will tend to raise productivity of labor, and, in turn, higher productivity normally will increase the opportunity to improve nutrition. This is why circular causation normally will tend to make changes having *cumulative effects*. By feedbacks causing more primary changes to go on changing in the same direction, the ultimate results of circular causation, to the good or to the bad, might be quite out of proportion to the initial impulse of change.

Second, whether a change of one condition is going upwards or downwards must be determined from the point of view of whether it contributes to *development* upwards or downwards. Usually the answer to that question can be given without much hesitation from our general knowledge of the process of circular causation. But it is quite possible, and advisable, to state explicitly your set of value premises. In my own studies of underdeveloped countries I have sought my value premises in the modernization ideals. For the most part they are not in conflict with traditional valuations, though they add to them a dynamic dimension.

Third, the coefficients of interrelations among all the conditions in circular causation as the system is moving—and the inertia, the time lags, or in extreme cases the total nonresponsiveness of one or several conditions to changes in other conditions or some of them—usually are not known with certainty or our knowledge of them is *utterly imprecise*. This is largely true even in developed countries with their much more complete analysis of all social conditions and their more perfected statistical services. But it is particularly true in underdeveloped countries. Consequently our analysis of their development problems must mostly end by broad generalizations and often mere hypotheses, built upon limited observations, discernment, and conjectural judgments.

Our endeavor, of course, should be to direct empirical study to ascertain these coefficients of interrelations and thus to fill with knowledge, as much and as rapidly as possible, these tremendous areas of less than complete, reliable, and precise knowledge. It serves no rational purpose to overlook the imperfection of our knowledge and pretend precise knowledge which does not stand scrutiny.

Let me against the background of this abstract reasoning, continually in very general terms, briefly recapitulate my somewhat heretical views on the performance of those in my own profession, the economists, in dealing with the development problems of underdeveloped countries.

My first observation is that before the Second World War and the hurricane of decolonization that swept over the globe, we had generally given little attention to the problems of poverty, backwardness, and lack of development in what then were called the “backward regions,” a static term. To the new world political situation we rapidly responded by a massive redirection of our research interests toward the development problems of what then became called the “underdeveloped countries,” a dynamic term implying also that they should try to develop by planning induced changes in their social systems.

In the beginning and to a large extent continually, economists made things too easy for themselves by keeping their analysis to the so-called “economic factors,” very narrowly conceived, and, indeed, narrower than had been customary in the great traditions from Adam Smith to Marshall and his more eclectic followers. Indeed, some 15 or 20 years ago, a few



economists could feel themselves breaking new ground by pronouncing that education could also play a role in development, which illustrates the narrowness of the common approach. But they conceived of it as “human investment” to be placed beside physical investment in the capital/output theory, a way of accounting for the development effect of education that Marshall had warned against.

By not observing the interdependence in a system, even when dealing with only economic factors, economists isolated production in terms of GNP from distribution, while particularly in underdeveloped countries they are crucially interdependent. And they commonly held consumption outside their growth models, though this is much less warranted in the underdeveloped countries with the great deprivation in levels of living among the masses.

They also freely transferred concepts from our analysis of conditions in developed countries that are not adequate to conditions in underdeveloped countries. One example is the concept of unemployment. In developed countries—with some qualification, for instance in regard to the slum population in the United States—unemployment can be defined as entirely involuntary worklessness, and it can be assumed that the unemployed have the skills for particular jobs and are fully aware of the market. In underdeveloped countries these assumptions are unrealistic for by far the larger part of the labor force. Huge amounts of statistics on unemployment—and underemployment—are produced and generally quoted, though they are largely meaningless. It is easy to imagine how economic historians would react to the use of the modern unemployment concept to the facts of worklessness in preindustrial Europe.

I cannot in this brief address develop further my criticisms of the inadequacies of approaches, models, and concepts in studies by economists of the development problems of underdeveloped countries, when in the new world political situation after the war, they were hurriedly faced with focusing research on such problems. But increasingly, and particularly in the most recent years, there has been a beginning of a movement toward a more institutional approach, taking into account not only what more conventional economists perceive of as the “economic” factors, but also institutions and attitudes and the productivity effects of very low living levels.

These other factors are generally much more important in underdeveloped countries with a long history of stagnation, which are often stubbornly resisting development efforts, particularly when such efforts are not in a planned way intentionally directed toward changing these factors. Keeping them out of the analysis is the basic reason why so much of the labor of economists on the development problems of underdeveloped countries has gone wrong, when we can now view what has actually happened.

The very recent clamor for a “unified” or “integrated” approach to the development problems of underdeveloped countries has even been reflected in resolutions in the various organizations within the United Nations’ system and in studies and reports by experts called together by them. These strivings are directed against the tendency of conventional economists to think only in terms of “economic” factors when planning for development.

What is demanded is, in fact, what I have called an “institutional” approach to the study of underdeveloped countries. Development must be understood as the movement upward of the entire social system, where there is circular causation and interdependence between conditions and changes of conditions with cumulative effects.

There is still much confusion, and much housecleaning to be undertaken. Many inadequate concepts, for instance unemployment, are still carelessly used, and many misrepresentative statistics presented which have been collected and analyzed in terms of such

concepts. As this movement toward a more realistic approach is gathering momentum, interest will by necessity come to be focused on what the other social sciences and, in particular, anthropology, have to contribute to the problem of development of underdeveloped countries.

But before I come to some remarks on that problem, let me stress that in one respect the economists have a characteristic which has given them superiority and made them the cavalry of the social sciences in this regard. In the tradition of more than 200 years they have, in their different sects, all been political economists, even those whose policy conclusions were non-interference in the market. They have never been scared of constructing macromodels and producing economic plans for a nation and for the whole world. To illustrate this peculiarity of my profession, I used to point out that if you place an economist in the capital of an underdeveloped country and give him a few assistants, he will on demand produce a plan for development of that country. No anthropologist, sociologist, psychologist, or what have you, would ever think of behaving in this way.

I have often reflected that this mental trait of us economists may appear slightly megalomaniac to our colleagues in the other social sciences. But megalomaniac or not, it is a socially useful inclination. What nations and their political leaders do need, are plans for how they should move. We are the depositories of the idea and the practice of planning for development.

Preserving this intention to produce macromodels and plans, the new movement toward a unified or integrated approach to planning for development should regularly imply stressing the importance of research in the other social sciences which are directly working on institutions and attitudes, particularly social anthropology, sociology, and psychology. From an economist's point of view, their work should be expected to provide him much of the pertinent knowledge he would need about human behavior in order to make his policy "integrated" and "unified."

As a matter of fact, such studies in a sense have a head start, as, different from the economists, these other social scientists had not before the war permitted themselves to let their work lie fallow in regard to the "backward regions" as the economists had done in their field of study. Particularly anthropologists for a long time have had their work almost exclusively directed toward the peoples in these regions.

Their studies at that time were largely static in conception, and intended to analyze social relations without much interest in how things could be improved, which was only natural in that era. The interest in kinship is a case in point. In fact, changes were often dealt with as "disturbances." I know I am exaggerating, and that there were already then important exceptions, but on the whole it is true that the anthropologists of that time were not often laboring to find out how the entire social system of the people they studied could have developed upwards.

This is now gradually changing. But I hope you don't find it impertinent if I say that anthropologists like other researchers in the behavioral social sciences find it difficult to think in terms of planning for national development. They are still laboring with finding out how people live and survive, and they are regularly, different from us economists, dealing with only segments of the national society, and also mostly focusing their work on certain problems that have traditionally been at the center of their attention, like, for instance, caste in India. They have seldom attempted systematically to lay bare the circular causation between all conditions in a society they are studying.

If on this point I will be permitted a critical remark, it has struck me how often many of the anthropological and sociological village surveys in South Asia, even now, avoid taking into due account the changes of population pressure, and still more often the nutrition and health

deficiencies, the direction and content of school education, and how the occurring changes in all these conditions affect social and economic development. They remain in this regard still too static.

In *An American Dilemma*, written more than 30 years ago, I had an appendix on Research on Caste and Class in a Negro Community where, among other things in the same line, it was stated:

The ideal community study should start out from a careful *statistical analysis* of vital, social, and economic data concerning the individuals and families making up the community being studied. The less measurable data on attitudes, cultural traits, behavior patterns in which social stratification is expressed, and the “feeling” of social status or toward social status on the part of members of the various groups, should then be observed and the results *integrated into the framework of statistical knowledge*. Only when so treated do they reveal their full meaning. The entire analysis should be dominated by the recognition that the Negro class structure is rapidly changing. The dynamics of the problem do not consist merely in the tensions, frictions and movements *within* the class structure. Even more important is the resultant *movement of the whole class structure* and, incidentally, the actual import of a position in this structure for Negroes in various social classes (1942:1130).

But leaving all this aside, the real and inherent difficulty meeting us now when we are on the verge of calling for a unified approach, and seeing the necessity to conceive of the interactions between conditions in an entire social system, is the following. Any careful study must deal with a complex reality that is immensely diversified and shifting in every underdeveloped country. Hope of reaching a really satisfactory solution of the planning problem would therefore assume very much more detailed and localized studies than we now have of all important conditions, their changes, and the interactions of these changes. These studies should be informed by the concept of an entire social system, and also by the purpose of initiating by planning induced changes which could start the system moving upwards.

But these studies cannot then be allowed to be aimlessly undertaken here and there, only directed and confined to the problems which have been defined in a standardized fashion in our traditional disciplines. We need studies by trained anthropologists prepared to enter into new fields which are important for national planning. More careful studies of the health situation in selected villages in a country like India could be immensely valuable for planning, particularly if they were guided by anthropologists, who by their training are prevented from believing that this is simply a medical problem.

Likewise, a study of the educational situation in a few districts could be very valuable for planning. Starting out from the grossly inadequate census statistics on literacy and the equally inadequate administrative statistics on school enrollment, they could be directed toward finding out the actual degree of literacy of the inhabitants, how many children actually go to school and with what regularity, and also what their schools and perhaps some efforts in adult education actually meant to the students, not only in regard to abilities of various kinds, but more broadly to their attitudes and world outlook. Such studies could mean a breakthrough in planning education—particularly if they were guided by what I conceive of as the anthropologist’s broad conception of the social structure and the interdependence of changes in a local situation and

within the broader national setting.

But this is mainly futuristic speculation. For the time being the situation is that while, on the one hand, we economists, who monopolize national planning for development, are on the verge of coming to see the necessity of a much broader approach to this planning, our colleagues in the other social sciences and, in the first place, the anthropologists, are not providing us with the information we need. Even in fields where relevant studies have been made, the findings are seldom organized to be suitable for our purposes, as they have been prepared by scientists who have not worked under the planning perspective.

They can raise a warning finger urging the economist planner not to forget this or that. But not being themselves accustomed to think in terms of planning for development, they are not equipped effectively to challenge our planning. When economists now, very belatedly, are gradually coming to see the need for a “unified” approach, this has on the whole not been caused by systematic criticism from our colleagues in the other social sciences, responsible for what we call the “noneconomic” factors. It has mainly been some of the economists themselves, and some politicians and administrators, who have been finding out the need for a broader knowledge base for planning.

Attempts in interdisciplinary research are already half a century old or more. Meanwhile specialization, even within the several social sciences, is still the dominant trend, often making it difficult for students within one discipline to understand each other. I have all sympathy for the demand for more interdisciplinary work. But we are all aware that interdisciplinary conferences and the composite volumes resulting from these conferences have seldom been pathbreaking accomplishments.

For the time being, I have more trust in the efforts of independent students to involve themselves in transdisciplinary research, whether it be an economist trying to broaden his views, or an anthropologist invading the territory of national planning. More generally, I believe that the borderlines between our traditional disciplines should be transgressed systematically.

#### References Cited

Myrdal, G.

1941     An American Dilemma. New York: Harper & Row.

1968     Asian Drama: An Inquiry Into the Poverty of Nations. New York: Pantheon Books.

## **Chapter 6**

### **Edward H. Spicer: The Yaqui, Culture Change, and Ethnicity**

**Thomas Weaver**

Edward H. (Ned) Spicer was the 1976 recipient of the Society for Applied Anthropology's Malinowski Award.<sup>1</sup> His early training and academic career were at the University of Arizona, where he received a bachelor's degree in economics in 1932 and a master's in archaeology in 1933. Then he studied at the University of Chicago with A. R. Radcliffe-Brown and Robert Redfield, receiving a doctorate for work with the Yaqui in 1936–37. His dissertation was a structural-functional analysis of Yaqui migrants to Tucson who had maintained their ethnic identity. He continued to study the Yaqui throughout his life and produced several ethnographies and other books on them (1980). He was professor of anthropology at the University of Arizona at the time of the award.

Spicer (1906–1983) was a superb synthesizer, a characteristic most evident in *Cycles of Conquest* (1962), his classic study of four hundred years of culture change brought about in the Greater Southwest by the governments of Spain, Mexico, and the U.S. In his later works Spicer was guided by his concept of “persistent cultural identities,” a concept that has achieved classic standing (1971). His major interests, as reflected in his publications, were American Indians, culture change and acculturation (1961), ethnicity, health, development anthropology, indigenous policy, history of theory and application (1977), and training programs in applied anthropology (1976). Spicer's professional association with John Provinsé, the first president of the Society for Applied Anthropology (SfAA) and on the faculty of the University of Arizona when Spicer was a student there; Margaret Mead; fellow Malinowski Award winners George Foster (1982), Alexander Leighton (1984), Sol Tax (1977), Lauriston Sharp (in 1989; see Weaver 2002a); and other pioneers in applied anthropology were important in the formation of his ideas.

Spicer's applied experience included a stint with the War Relocation Authority (WRA), the U.S. Japanese internment program during the Second World War. Under the leadership of Alexander Leighton and working with other behavioral scientists, Spicer was able to mitigate some of the more penurious elements of the program. After this he accepted an academic position at the University of Arizona in 1946, where he spent the rest of his professional career. His experiences in applied anthropology also included community development programs during the 1960s with the Tohono O'Odham (Papago), an Office of Economic Opportunity project with the Pascua Yaqui, teaching a course in applied anthropology, and co-teaching a course with sociologists on community development. He consulted with the Bureau of Indian Affairs, the

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Stanford Research Institute, and other government agencies. His other important contribution to the field of application was a book of case studies he edited as part of the Cornell program in applied anthropology (Spicer 1952). Spicer influenced and trained three generations of applied anthropologists. He also maintained a life-long interest and participation in helping American Indians, often as consultant to government agencies and valued advisor to tribal councils (Officer 1990; Gallaher 1984 and e-mail to author, March 25, 1998).

Spicer began his Malinowski Award acceptance address, “Beyond Analysis and Explanation? Notes on the Life and Times of the Society for Applied Anthropology,” with an accolade: “One of the most important events in the development of anthropology during the twentieth century was the organization in 1941 of the Society for Applied Anthropology” (1976:335). His speech traced the development of themes in the then 35-year history of the Society for Applied Anthropology (SfAA). He pondered some of the problems faced by the founders of the society and their belief that it was possible to apply anthropological premises in the service of humankind. Spicer emphasized the need to bridge the gap between professional societies, academics, applied anthropologists, and administrators. He lauded the SfAA for being the first professional group to establish and continue to revise a code of ethics.

Spicer noted that anthropology’s retrenchment after the Second World War into academia, away from the applied developments of the 1930s and the war years, had led to a greater focus within the SfAA on theory development and, by the 1960s, the academic takeover of the society. The society’s journal, *Human Organization*, came to be dominated by academic contributions, yet a focus still remained on interdisciplinary work and on case studies illustrative of applied problems. Academically based applied anthropologists kept applied principles alive through their publications and their work as part-time consultants. Furthermore, the SfAA continued to play a major role in socializing anthropologists to their responsibilities to the societies in which they lived and worked. The expansion of departments of anthropology in the 1950s and 1960s eventually led to the production of a surplus of Ph.D.s. In 1976, at the time of Spicer’s writing, practicing anthropology was not yet a common phrase, but a revolution was brewing.

Spicer identified several important but unfinished agendas for the SfAA and applied anthropology (many still relevant today). Most important was the need to provide all anthropologists with grounding in application while they are in graduate school. He expressed a widely held view that the many anthropologists drawn by choice or necessity in the 1960s and 1970s into nonacademic fields were poorly trained. An important part of the training he proposed was the anthropological study of the policy process, policymakers, program development, and program implementation. He clearly mirrored Gunnar Myrdal’s conclusions in his earlier Malinowski Award address (Myrdal 1975) concerning the need for anthropological involvement in planning and policy development. Analysis of planned intervention, or what Spicer and others earlier called directed culture change, is a vital aspect of this training. This must be done within the context of the “emic-holistic-historical-comparative approach,” as he labeled the distinctive character of anthropology and the bases of what anthropology has to offer in dealing with problems of application (Spicer 1976:341). Training should include case studies that are focused on regionally centered problems and involve comparative policy-program analysis and design.

Evidently Malinowski did not have much influence on Spicer’s development as an applied anthropologist. He mentions Malinowski only once in his acceptance speech, as one of several early anthropologists who believed in the efficacy of anthropology for solving social problems. During the time Spicer was at the University of Chicago, Radcliffe-Brown was a visiting

professor. Perhaps the well-known competition between his teacher and Malinowski prohibited any influence in this respect. Nevertheless, Spicer had supported the offer of a position (in 1939) to Malinowski in the Department of Anthropology at the University of Arizona.

During his productive career Spicer received two Guggenheim awards and a National Science Foundation senior fellowship to conduct comparative studies of Indian communities in Mexico, Peru, and Ecuador. Another fellowship permitted fieldwork in Spain, Ireland, and Wales. He served as vice-president of the Society for Applied Anthropology, president of the American Anthropological Association (AAA), and editor of the *American Anthropologist*. He was elected to membership in the National Academy of Sciences and the American Philosophical Society. He also received the Distinguished Service Award of the AAA and the Southwestern Anthropological Association's Outstanding Scholars Award.

### Notes

<sup>1</sup>This annual award is given to a senior colleague in recognition of efforts to understand and serve the needs of the world through social science. See Thomas Weaver's "The Malinowski Award and the History of Applied Anthropology" (2002c) for a brief history of this award and an overview of the recipients and their work, and Weaver's "Malinowski as Applied Anthropologist" (2002b) for an introduction to Bronislaw Malinowski and his work.

### References Cited

- Foster, George  
1982     Applied Anthropology and International Health: Retrospect and Prospect. *Human Organization* 41(3):189–197. (Republished at <<http://www.sfaa.net/>>.)
- Gallaher, Art, Jr.  
1984     Obituary: Edward H. Spicer. *American Anthropologist* 86(2):382.
- Leighton, Alexander H.  
1985     Then and Now: Some Notes on the Interaction of Person and Social Environment. *Human Organization* 43(3):189–197. (Republished at <<http://www.sfaa.net/>>.)
- Myrdal, Gunnar  
1975     The Unity of the Social Sciences. *Human Organization* 34(4):327–331. (Republished at <<http://www.sfaa.net/>>.)
- Officer, James E.  
1990     Edward H. Spicer and the Application of Anthropology. *Journal of the Southwest* 32(1):27–35.
- Spicer, Edward H.  
1961     Types of Contact and Processes of Change. In *Perspectives in American Indian Culture Change*. E. H. Spicer, ed. Pp. 517–544. Chicago: University of Chicago Press.  
1962     Cycles of Conquest, The Impact of Spain, Mexico, and the United States on the Indians of the Southwest, 1533–1960. Tucson: University of Arizona Press.  
1971     Persistent Cultural Systems. *Science* 174:795–800.  
1976     Beyond Analysis and Explanation? Notes on the Life and Times of the Society for Applied Anthropology. *Human Organization* 35(4):335–343. (Republished at <<http://www.sfaa.net/>>.)

- 1977 Early Applications of Anthropology in North America. *In* A. F. C. Wallace, J. L. Angel, R. Fox, S. McLendon, R. Sady, and R. Sharer, eds. *Perspectives on Anthropology*, 1976. Special Publication Number 10. Pp. 116–141. Washington, D.C.: American Anthropological Association.
- 1980 *The Yaquis: A Cultural History*. Tucson: University of Arizona Press.
- Spicer, Edward H., ed.  
 1952 *Human Problems in Technological Change: A Casebook*. New York: Russell Sage Foundation.
- Tax, Sol  
 1980 Anthropology for the World of the Future: Thirteen Professions and Three Proposals. *Human Organization* 36(3):225–234. (Republished at <<http://www.sfaa.net/>>.)
- Weaver, Thomas  
 2002a Lauriston Sharp: Innovator of Applied Anthropology Programs. *In* *The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers*. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.  
 2002b Malinowski as Applied Anthropologist. *In* *The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers*. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.  
 2002c The Malinowski Award and the History of Applied Anthropology. *In* *The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers*. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.



## **Beyond Analysis and Explanation? Notes on the Life and Times of the Society for Applied Anthropology**

**Edward H. Spicer**

One of the most important events in the development of anthropology during the twentieth century was the organization in 1941 of the Society for Applied Anthropology [SAA, usually abbreviated as SfAA]. The significance of this event is the theme of my talk. I am not at all sure that more than a handful of anthropologists, or any other kind of people, understand the importance of the SAA, but I hope that more will, and that is why I am addressing you.

Before taking up the main theme I want to dwell briefly on the interesting situation in which we find ourselves now 35 years after the founding of the Society for Applied Anthropology. The SAA was not enthusiastically welcomed into the world of anthropology. The newborn was regarded by most anthropologists as something of a monstrosity and as a consequence it began its first growth in the limbo of illegitimacy. Its spokesmen, the editors of its journal, felt the need to explain the nature of the infant over and over again, so that successive definitions of applied anthropology became recurrent editorial matter. The cold welcome was strongly felt, and there was a sharp reaction by the editors. This was couched in such terms as opposition to conceiving research as the study of “a kind of ectoplasmic substance” called culture (Editorial, *Human Organization* 1955). About the same time the new organization decided to give up the term “anthropology” in the title of the journal. The ground for this was that the connotations of anthropology were “too varied” (Editorial, *Human Organization* 1949:3). Actions of this kind did not promote feelings of kinship between the SAA and other established anthropological organizations. Alienation developed on both sides. The odor of illegitimacy continued in some degree on through the 1960s. Then quite suddenly the paternity was acknowledged. The American Anthropological Association took the SAA to its bosom and included it as one of the sister associations participating formally in the functional organs of the Association. The sharp contrast in the situation of the SAA in 1941 as compared with 1971 is worth pondering. My talk proceeds in part from consideration of the issues which this remarkable change, over the span of a generation, throws into relief.

In this bicentennial year in the United States we hear much of founding fathers. It is appropriate therefore, even though it is not my intention to present a systematic historical narrative, to call attention at the beginning of this talk to the particular individuals who brought the SAA into existence. The persons who may be regarded as the founders of the Society were the following: Conrad Arensberg, Eliot Chapple, Charles Loomis, Gordon MacGregor, Margaret Mead, John H. Provinse, Frederick L. W. Richardson, Laura Thompson, and William Foote Whyte. There were others present at two initial meetings in 1941 in Cambridge and Washington<sup>1</sup> but these ten may perhaps be regarded as carrying the burden of the action. Their interest grew directly out of experiences in the 1930s in which applications of anthropology and sociology had been hopefully undertaken in industry, in agriculture, and in Indian administration. Most of those

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<sup>1</sup>This address was previously published in *Human Organization* 35,4(1976):335-343.

immediately involved had had direct experience in such applications and had developed the conviction that the social sciences, and especially anthropology, had already accumulated knowledge the utility of which its practitioners had made insufficient effort to understand. John Provinse, the first president of the SAA, and Eliot Chapple, the first editor, were vigorous advocates of the point of view that anthropologists were lagging in an effort of vital importance both to the discipline and to society. It should be added that in a certain sense this viewpoint was a traditional one in anthropology, despite the early rejection of the SAA by a majority of American anthropologists. Leading anthropologists during the previous 60 years had adopted this position at regular intervals, as exemplified in statements of Tylor (1960:275), Brinton (1895:14–17), Boas (1962:245), and Malinowski (1945:4–8), to mention only some of the more prominent.

The development of the SAA divides readily into four phases and these tend to coincide roughly with the decades of the 1940s, the 1950s, the 1960s, and the 1970s. The first phase was one of vigorous editorializing and articulation of the ideas which stimulated the founders. There were three basic assumptions, as indicated in the pages of the journal, in terms of which the spokesmen saw the future and directed their activities. First, there was the certainty that a body of anthropological knowledge already existed which could be promptly employed as soon as it could be communicated. John Provinse (1940) was eloquent on this score, although it cannot be said that he and his associates were consistently explicit about just what this knowledge was. Rather they tended to state their position in terms of another assumption to the effect that the methods employed by anthropologists—and some other social scientists—would steadily yield useful knowledge.

The second assumption on which the editors built their program was that out of the reporting of direct observation of the activities of people in industry, government administration, and other areas of “human relations” would grow the application of social science. There was constant emphasis on the need for “concrete” information, rather than the “abstract” (Editorial, SAE 1947:1–2). The editors hammered on the theme that the journal existed to make case histories available. The cases would make possible learning from the “actual” rather than spending time talking about the “logical consequences of the theoretically possible” (Editorial, SAE 1947:2). In this connection they rejected the use of the concept of culture and insisted that the field of observation had to be specified in terms of social relations. Said one editor in this phase: “It is not sufficiently realized perhaps that the Society for Applied Anthropology is the only Society in existence whose primary concern is the study of organization problems” (Editorial, *Human Organization* 1953:3).

The third clear theme of this first phase consisted in the assumption that there were two entities which needed to be brought together. These were “administrators” and anthropologists, or as the latter were more often called “social scientists,” especially during the years when the word “anthropology” was dropped from the title of the journal and the phrase “Human Organization” adopted. The importance of bridging the gap between these entities was emphasized (Editorial, *Human Organization* 1956:1–2). The view of the role of the SAA in the bridging function involved what often seems a conception of “the administrators” as actually anthropologists turned administrators like John Provinse in the Bureau of Indian Affairs and the War Relocation Authority, or Burleigh Gardner in industry. There seems to have been an expectation that there would be more such individuals and also that there would be the growth of a new variety of professional, namely, the non-academically based consultant to industry and government whose training would have been in the social sciences. Through the analysis of

cases, knowledge applicable in the various practical fields would be supplied, and the consultants and the anthropologists turned administrators would constitute an important element in the newly established chain of communication. Prescriptions for the behavior of consultants became an important concern of the editors (Editorial, *Human Organization* 1952:3–4). They discussed unfair competition offered by academically based consultants through the use of cheap graduate student labor; sticking to tested facts and eschewing mere opinion in consulting, urging that social science consultants behave precisely as engineers do; and the need for being constantly aware that professional consulting is a wholly different matter from advocating reform (Editorial, *Human Organization* 1954:3–4).

The second phase in the development of the SAA saw a different set of emphases, as anthropology took a new direction apparently not envisioned by the founders. It was in this phase that the SAA made its most important contribution to anthropology as a whole. Under the leadership of Gordon Brown, Eliot Chapple, and Margaret Mead members of the Society began to wrestle with the problems of professional ethics (Editorial, *Human Organization* 1949:20–21). Some twenty years ahead of any other field of anthropology, a tentative code was formulated. This was wholly unsatisfactory, but the effort continued down to the present amid vigorous criticism and discussion. This concern grew out of real problems in industrial and other kinds of consulting, as well as out of problems of invasion of privacy in reporting community studies. The whole array of the now familiar problems discussed as “research on human subjects” came up. It was years before anthropologists outside of the SAA were pushed by world events to formulate their conception of professional responsibilities. Meanwhile the SAA had made three or four formulations each one closer to the needs of the working realities than the one preceeding.

At the same time the pages of the journal began to be filled with discussions of values as related to practical programs. The idea that there is such a thing as “value-free” research and that the principle of cultural relativity required anthropologists to stand clear of action programs wielded tremendous influence among anthropologists generally in this phase of the development of the discipline. However, in the pages of *Human Organization* suddenly appeared descriptions of new kinds of behavior which anthropologists had felt themselves justified in carrying out; this new behavior went under the labels of “participant intervention” and “action anthropology,” as described by Alan Holmberg and Sol Tax (1958:2–33). Impatient of “administrators” some anthropologists were seeking themselves to be programmers; they were unwilling to wait for the “building of the bridge” and themselves undertook by the 1950s to bypass the usual agencies and bring about a direct relationship between the “administered” and the social scientists. In doing so they kept one foot in the academic world, never wholly forsaking it as had some of the early founders. The editor of *Human Organization* declared that the SAA had pioneered in this work in its early consideration of “experimental anthropology.” Nevertheless this kind of activity struck anthropologists as something new and called forth vigorous discussion, strongly encouraged by the SAA in the journal. The discussions served the purpose of moving that segment of anthropologists who maintained connection with the SAA closer to a realistic view of what is involved in administration and the organizations in which it takes place. The separation of value positions and practical involvement was no longer possible for those on whom these discussions made any impact. The beginnings of a serious approach to the world of policy and hence to the context of administration had now been made.

By the end of the editorship of William F. Whyte marking the close of the second phase of the SAA, it had become apparent that the right, tight little world in which the SAA had begun

was no longer so clearly bounded as it had been thought to be. During this phase the journal had responded actively to the expanding universe of intellectual, ethical, and cross-cultural responsibilities. At the same time anthropology in general had begun a strange transformation. This had been noted in earlier editorial remarks. First Eliot Chapple (1951:3–4) had reported what he hoped was just a rumor, namely, that graduate students in anthropology were no longer finding time to do fieldwork, because of the pressure “to meet the requirements” for degrees; the editor was deeply concerned. Then it was noted in the early 1960s by Clyde Kluckhohn that he could not persuade his best students to concern themselves with applied anthropology. He said he was afraid that they regarded it as unworthy of their effort. These signs were indicative of a new, peculiar phase in the development of American anthropology, during which the orientation of the SAA also shifted markedly.

During the fifties and sixties many new departments of anthropology formed, and existing departments expanded faculty rapidly as they responded to greatly increasing enrollment of both undergraduates and graduates. It became a world of academic positions far in excess of persons trained to fill them. The established departments settled down to training the scores of Ph.D. holders needed to meet the demand. The activities of anthropologists necessarily turned inward, focused on the academic world and its needs and only peripherally on the role of anthropology in the larger society. Concurrently funds for academically oriented research increased tremendously. By the mid-1960s narrowly based specialists in kinship, in cognitive anthropology, in “structuralism,” in specialties both old and new had begun to replace the older generalists who had once emerged with their Ph.D.s ready to teach in any subfield from linguistics to physical anthropology. Training became intensive and methodologically rigorous, focused on theory construction. In contrast with the anthropology of the 1930s and 1940s, the discipline was no longer straining toward a role in the world of public policy and program facilitation; academic anthropology was increasingly a world unto itself. It was now in an era of intensive concern with theory such as it had never quite been before and therefore with a high concern for what the editor of the SAA journal in the early 1950s had inveighed against as “the abstract.”

The effect on the SAA and *Human Organization* of the conditions of this third phase was interesting; the SAA did not die. On the contrary, its membership continued to increase. There were more submissions to the journal. By all external signs it was a healthy organization, showing no signs of slowing down in growth. It was clear, however, that a new trend had been set in the journal. Steadily the pages were being filled by productions of the academically oriented anthropologists and other social scientists. Their takeover was nearly complete by 1970, when it was reported that more than 90% of the articles published were by anthropologists and sociologists holding academic positions (Thomson 1970). An editor complained during the 1960s that articles were now written by academics rather than by persons doing practical work. The journal and to a large extent the annual meetings were forums for academic people to talk with one another. What they talked about was as often as not some kind of “change program,” that is to say for example, an economic development effort somewhere which constituted a case of “resistance to change” or perhaps an individual rehabilitation program in a hospital or a mental health clinic, or a description of a neighborhood council’s “war on poverty” battle. However, the pages more frequently than before gave space to authors concerned with discussing what social scientists thought ought to be done about making use of social science knowledge. Rarely did anyone not academically based contribute an article. The days of exuberant editorializing which had so stimulated some anthropologists and so antagonized others gave way

to an era of overworked editors with apparently little time or inclination to proselytize or to exhort. The journal had become an important outlet for professors of sociology and anthropology who enjoyed thinking about some problems of the application of social science.

Nevertheless something was still going on related to the goals of the founding fathers and mothers. In the first place, the SAA had become more interdisciplinary, as indicated in the nearly fifty-fifty distribution of sociologists and anthropologists as contributors to *Human Organization*. The distrust of a compartmentalized anthropology on the part of the founders had borne some fruit with regard to intellectual cross-fertilization, and this seemed to be true at the level of much reporting of practical action in the journal. In addition, it was also true that at least a little of the tradition of case reporting inaugurated by the SAA, of recording concrete instances of directed change with a focus on the details of social relations through which such programs were carried out, continued to constitute some portion of the articles. A smaller proportion of such reports was being published than at the beginning in the late 1940s, but it was still more than half. Although reported from the vantage point almost entirely of academic positions, they were nevertheless present and thus fulfilling to some extent the original goals. This was something of an accomplishment and we shall have more to say about it below.

*Human Organization* was still as in the previous decade a place of active self-criticism for anthropologists. They vigorously discussed issues raised by ethical considerations, and pioneering analyses and polemics appeared in the journal regarding the unconscious ethnocentrism and power involvement of anthropologists and other social scientists. It was in the journal and at the meetings of the SAA during the 1960s that Bonfil Batalla (1966) and others launched critiques of anthropologists as uncritical advocates of programs which enabled politically dominant segments of modern societies to maintain their dominance at the expense of nondominant peoples. This discussion was part of the ideological turmoil of the 1960s which ultimately affected the American Anthropological Association and brought about the formulation of new bases of working relationships by anthropologists generally, whether in applied fields or elsewhere. The SAA thus continued its role as pioneer in the analysis of the foundations of relationships between the social sciences and the political and economic world in which “programs” have their being.

The current phase of the SAA in the 1970s continues trends of the 1960s. It is now an organization primarily of academic people who discuss general theory regarding cultural change and often present cases from their experience in which they have been observers or participants of some kind. The follow-up monitoring of cases of intervention earlier proposed by F. L. W. Richardson in *Human Organization* has not been an important feature of the journal. There is a good deal of discussion about what anthropology should and sometimes does contribute in action programs somewhere; more often than not this sort of discussion is general rather than specific, abstract rather than concrete (as an earlier editor would say). The self-criticism that began in the second phase continues, but has been democratized, taking place in the newly added “commentary” section rather than in editorials. The phrase “de-colonializing applied social sciences” gained currency following its use in *Human Organization* (Stavenhagen 1971). Thus the trends of the present have been pretty thoroughly established in the direction set during the third phase.

This direction is probably best indicated by the events mentioned at the beginning of this talk. When the SAA’s legitimacy was recognized by the American Anthropological Association, the status of the Society was confirmed by an invitation to nominate an editor to serve, with other anthropologists selected by their respective associations, in the newly organized Board of Editors

for the *American Anthropologist*. After some months it became apparent that there was much overlap of articles submitted to the “social-cultural” editor and the “applied” editor. The general editor, anxious to maintain equitable division of labor among editors, asked the officers of the SAA to make clear the essential character of articles to be considered in the field of applied anthropology. They prepared a statement from which I have taken the title of this talk. They said in essence that articles to be classified as in the field of applied anthropology should go “beyond analysis and explanation . . . to deal with issues about the uses that may be made of . . . knowledge (Wolfe 1975). It might have surprised the founders of the SAA that such a statement had to be made 30 years later, and they probably would have felt that it was too academically oriented. The fact that it had to be made is indicative of the extent to which the SAA had moved in the direction of concerning itself almost entirely with the academic end of that bridge about which the founders talked, while largely losing interest in the bridge itself.

This situation is one result of the profound change which has come over anthropology as a whole and which the founders did not foresee. The great proliferation in the number of academic anthropologists and in funds for academic research was part of the triumphant ascendancy of universities in American life during the 1950s and 1960s. It was a plush period of academic power. One feature of this phase of university life in the United States has been a very considerable development in the employment of academic consultants by government agencies and to some extent by private business, consultants whose base of operation has remained in the universities. Anthropology shared in this growth in a large way. In departments of anthropology with which I have had acquaintance during the past 20 years, perhaps 70% of professors at all levels have had extensive consultant experience. Some have been involved for from one to three years with various federally sponsored programs; some have worked off and on for various corporations under private management. The anthropologists concerned have carried on the work with no intention of forsaking their academic bases, and moreover they have not had to do so; it has been perfectly possible to adjust their consulting work so that it has been part-time, during summer months, or for a quarter or a semester at a time. The consultation and associated research has usually been undertaken at the request of some kind of administrator, which is to say that it has been part of policy and program situations in which the anthropologist served as staff adviser, not as policy maker.

This new world—new since 1941—of academic consulting and research for clients outside of academia, for administrators of various kinds, has been well described by George Foster in his textbook called *Applied Anthropology* (1969). The SAA and its journal have made a special and successful adaptation to the filling of the needs of the academic consultant and researcher. It is on this basis that it has won its way to formal legitimacy in the world of the parent discipline. This has been worthwhile certainly, but I believe that it is not enough. Before I develop that perhaps, in this audience, not very popular position, I want to return to what I stated at the beginning would be my major theme tonight, that is, the justification for asserting that the founding of the SAA was one of the most important events in the development of American anthropology during the twentieth century.

I made that assertion with four specific points in mind. One, the SAA and its journal have nurtured the yearning felt by many anthropologists through the years to see their kind of knowledge employed usefully in the general society. This yearning, although not coupled with very clear ideas about the processes of application, has been expressed repeatedly by our most prominent figures ever since Edward Tylor first voiced his conviction that anthropology is a reformer’s science (1958:539). Some moved to make the hope a reality by establishing working

relationships between anthropologists and practical people involved in the solution of problems of their times, notably, for example, Bronislaw Malinowski (1940) and W. Lloyd Warner (Warner and Low 1947), the latter of whom contributed to foundations on which practitioners from other disciplines have since built extensively (Roethlisberger and Dickson 1939). It is not too much to say, I think, that the SAA has institutionalized in anthropology the persistent sentiment of our major figures that there is indeed practical value in anthropological knowledge. The SAA has done this, first, by establishing (true to their convictions) a *human organization* comprised of a little band of prophets, and second, keeping that organization at work until its very structure has been incorporated into the larger organization of the discipline, without any loss of its own identity. This is certainly a great achievement considered especially with reference to the odds against which the little band of prophets had to work. I am hard pressed to say whether this is the major accomplishment, for the other three seem to me also to be of considerable significance. The SAA's deep conviction that organization is important has resulted in hundreds of anthropologists for more than a generation wrestling in various ways with the issues inherent in putting knowledge to use. This has taken place even during a phase in the development of anthropology when academic interests have assumed an undreamed of ascendancy and when applications might have been expected to have been eclipsed in the shadow of the triumphant departments of academic anthropology.

Two, what seemed of greatest importance to many of us who were associated with various of the founders was their programming of the journal and the annual meetings to provide a proving ground for ideas formed in the systematic study of concrete cases. It was a major expectation in the early days that the journal would be loaded with closely observed examples of attempts to guide the course of human events, that is, cases of what some of us called "directed culture change," although that phrase was not in very wide use at that time. There was excited anticipation that the journal would encourage intensive study of a sharply delimited field of observation. What we wanted to do looked fairly simple, namely, to compare such cases and understand them as instances of success or of failure in introducing change and then to proceed to isolate the factors making for the differing results. The editors for a period spoke in these terms and seemed to be successfully identifying a special kind of human behavior, that of human planners and administrators of change. This gave promise that we were getting into a manageable field of observation and analysis from which could be extracted "principles" for practical guidance. Gradually this kind of emphasis weakened and the approach seems to have been submerged in a vague and limitless category of cultural or social change, or just "change." Something of the original vision still shines through the pages of *Human Organization* and perhaps may be revived. Meanwhile the promise of that early phase remains an important landmark in the development of the field which the SAA set out to cultivate.

A third reason for maintaining that the SAA has been and remains important is that in some degree it has fostered, as was originally hoped, some interdisciplinary communication and even cooperation. The figures at the end of the 1960s regarding the proportions of contributors to the journal give some measure of this. Sociologists and anthropologists are indicated as nearly equally involved in the enterprise of the SAA, with sociologists having something of an edge with respect to number of contributed articles. During the early years persons elected to office in the SAA, including the presidency, were frequently sociologists or from some field other than anthropology. The trend, however, by the late 1950s settled down to a predominance of anthropologists in the policymaking roles, a trend that coincided with the great upsurge in the numbers of anthropologists generally in the universities. Nevertheless the policies that have

characterized the SAA seem to have been such that others than anthropologists have been increasingly attracted to some kind of participation in the publication and other programs.

The fourth major contribution of the SAA has been its persistent pioneering in recognizing the responsibilities of men of knowledge within the society which produces them. The dominant trend, until World War II, in anthropology, as in most other fields of scientific activity in the United States, was to ignore such matters. Anthropology was in general probably one of the most conservative of the social sciences in this respect. It was an indication of the seriousness with which the SAA took its newly conceived professional role that as early as 1949 it faced the problem of ethical responsibility. It has continued to the present to stimulate its members, and through them anthropologists more widely, to work out acceptable positions regarding professional ethics. Similarly the SAA has faced one issue after another among those involving linkage with the general society. The forum that its annual meetings and its journal have provided has aided in the adaptation of those concepts— cultural relativism and value-free research—which have been especially inhibiting for anthropologists. We might sum up the significance of the SAA in this respect as having consisted of a major contribution to the socialization of anthropologists in the society in which they live. The contribution of the SAA in this important process has been incalculable. What is quite clear is that it has moved in advance of other organized segments of the discipline and has stimulated and supported an active leadership.

It is for these reasons that I suggest that the SAA is one of the most important influences on anthropology generally. It would be worthwhile to compile a more detailed and careful study of this influence by examining the record of what has happened at annual meetings and, most important, the teaching, consulting, and other related activities of individuals whose roles in the total society have been developed in conjunction with participation in the activities of the Society for Applied Anthropology. I have relied here merely on a cursory consideration of the successive volumes of the journal.

In the light of whatever perspective this sketch of the role of the SAA provides for us, I wish now to turn to the needs of the present. I do this in awareness of my inability in recent years to have kept abreast of the fast-moving events in the application of social sciences. I realize also that my remarks proceed from the standpoint of one of those anthropologists who turned during the 1950s from insistent invitations to engage directly in administration to take on the challenge of the training needs of a burgeoning academic department and ultimately to root myself in that academic environment.

My first recommendation is a thoroughly academic one. It grows out of the experience of seeing many anthropologically trained persons move out from training at various levels in anthropology to some kind of career in administration and to lose sight, as they do so, of a vision which they had at the start of that career. From my point of view such people have been inadequately trained and I believe that they could be much better trained. Moreover I believe that departments of anthropology are obligated to train all kinds of anthropologists, not just those who will go into the usual teaching posts from which they make occasional forays into consulting and occasional policy-oriented research. This indeed is my first general point, namely, that basic training in anthropology be developed to provide better for the whole range of anthropologists who are now going to work in our society.

I should like to see many departments of anthropology institute a type of course or series of courses which would be treated as basic anthropological training, although not necessarily courses which all graduate students would be expected to take. What I have in mind would be



basic in the usual sense that courses in social organization, in symbol and ritual systems, in processes of change, in economic and political anthropology are basic. It would consist in courses in which the essentials of anthropological method would be employed, that is, what I like to call the emic-holistic-historical-comparative approach.

I will come back to this horrendous phrase, because it is fundamental in my conception of the distinctive character of anthropology and therefore the foundation of what we have to offer in the application of social science. The courses of which I am speaking would simply organize, analyze, and interpret their subject matter in terms of the essential anthropological viewpoint. In that respect it would be precisely like other basic courses in anthropology. It must be insisted, however, that the subject matter would be different, as different as kinship systems are from human ecosystems. It is the nature of the subject matter which makes these courses most relevant to applications.

The subject matter consists of what the founders of the SAA apparently had in mind, that which goes by many names today—directed change being one of the more common. Other labels are also employed—planned change, developmental change, planned intervention, and a half dozen others. I think that “planned intervention” comes closest to what I have in mind, for I do not think that “change” is universal in this field of observation. Planned intervention may be engaged in to inhibit or to prevent changes. The essential character of this subject matter consists in events in the world of social relations in which persons intervene to influence those events in accordance with their plans. This, of course, is the world in which administrators work. When I speak of administrators I have in mind people who formulate policies (which is to say, set goals), who select programs to achieve the goals, and who manage these programs. The subject matter, if you will, may be called the behavior of administrators and those whom they administer. This is a fascinating world of planning and of plans that go wrong or right in relation to the administrators’ and the administereds’ hopes or expectations. It is in broadest definition just about the whole of human behavior, but there is an area within this whole on which we can and must concentrate. This is the operation of the processes of policymaking, or program-formulation, of program selection, of program evaluation, the area of human behavior in which these processes are at work in formal bureaucratic organization.

I suggest that the study of these processes yields, or can yield, knowledge of the kind of phenomena which all concerned with applying social science should know most about. I do not like to see us misled by holding that application requires the knowledge of all the social sciences before we can begin work. I do hold that it requires the essential knowledge, for anyone, of one or more of the social sciences. It is through the approach that I speak of that an anthropologist can acquire the essential knowledge of his discipline. This means that he or she spend time analyzing and comparing specific examples of policymaking, program selection, and program administration. In the study there should be use of the emic approach, that is, the gathering of data on attitudes and value orientations and social relations directly from the people engaged in the making of a given policy and those on whom the policy impinges. It should be holistic, that is, include placement of the policy decision in the context of the competing or cooperating interests, with their value orientations, out of which the policy formulation emerged; this requires relating it to the economic, political, and other contexts identifiable as relevant in the sociocultural system. It should include historical study, that is, some diachronic acquaintance with the policy and the processes giving rise to it. Finally it should include consideration of conceivable alternatives and of how other varieties of this class of policy have been applied with what results, in short, comparative understanding. This is a sketch of some features of an ideal

prescription, which will probably never be fully realized in the actual courses I am suggesting. Nevertheless it is urged that not only policy processes, but also program selection and administration be studied to the same prescription.

I shall mention three other essentials besides this basic anthropological approach. The courses, or seminars, or whatever they turn out to be at a given institution, should deal with actual regional problems where the university is located, so that the emic approach may be constantly employed where it becomes apparent data are scarce. Secondly, it should be made clear very early that there are other disciplines besides anthropology which have developed knowledge for understanding a given policy-program; this can be brought about by keeping the approach problem-focused rather than discipline-centered. The third necessary feature is that the courses not stop at analysis, but go on to design. Thus, for example, a semester of work on comparative policy-program analysis should be followed by one in policy-program design. By this means some preparation for the responsibilities of administration may be achieved, as students undertake to design, in the light of possible alternatives, a particular policy and then to implement it by designing programs for achieving its goals.

There are many ways of organizing the courses, but perhaps enough has been said to suggest what the objectives should be. The purpose would be to familiarize students, while they are still students, with the nature of that fascinating world in which they will be working as administrators later on. The technique employed in this part of training is not taking a course in public administration or bureaucratic organization, but rather handling the materials of policymaking and program administration in the several perspectives which constitute the approach of anthropology. I must repeat that the technique is the same as that which we employ in other good anthropology courses. Here it is not the ethnography of a region that we teach, but the ethnography of a type system of sociocultural processes, as we teach, for example, courses in kinship systems or political systems or ritual systems. Ordinarily a student who goes from anthropology into some applied field is quite ignorant of the vast area of policy-program ethnography in modern life.

What I am sketching seems to me very similar, if not identical, to what Roger Bastide has recently written a book about, now translated into English (1973). He urges that we develop a separate discipline, as he sees it, from anthropology which he calls "applied anthropology." This discipline as it turns out in his own presentation is the comparative study of administrative programs. If you are interested in this idea I urge that you read Bastide. However, I think that he is very much on the wrong track in holding that this kind of study should be compartmentalized as a separate, or even as a sub-, discipline. I see it as an aspect of basic anthropology and it should be so taught, giving the student a window on the world of administration from the standpoint of anthropology.

I hope it is clear that I do not pretend that what I propose is anything but academic training, albeit training in handling the phenomena of administration and policy in a laboratory manner. I am suggesting the broadening of the training base in academic anthropology so that all kinds of anthropologists get the necessary basic training, those who go into applied fields as well as those who don't. The internships, the specialized "track" courses preparing for specific kinds of application—all of these are of course necessary and they are beginning to be offered in various forms. What I propose will forestall the ever-present tendency to compartmentalize and will emphasize the relevance of basic anthropology to fields of application, in short, will provide a more solid foundation in anthropology.

Just as in my first recommendation I looked back to the founders of the SAA and decided

that one of their most emphasized ideas was a good one, so in my second recommendation I do the same. The first idea was a simple one, namely, that planned intervention should be recognized as a field in itself worthy of special study and that anthropologists and others ought to be stimulated to carry on the study. The second idea is also a simple one, namely, that a bridge is needed connecting anthropologists (and other social scientists) and administrators for communication, for cooperation, and for mutual criticism. The bridge was not successfully built in accord with plans projected. It turned out that one of the bridgeheads, the one rooted in the academic world, changed character and in part became in itself a channel of communication, as professors became part-time consultants on a fairly large scale. Now many of these professors publish in the journal and talk to one another at annual meetings and to some extent discuss their experiences in the nonacademic world. They are not isolated and inexperienced to the degree that they were in 1941. Hence there has been some transformation of anthropologists in the direction hoped for by the founders. This certainly is all to the good, but it is not enough, and the question must be asked: Is the old bridge as conceived in 1941 unnecessary in the new reality which has developed?

The new reality includes not only experienced professors, but also a kind of administrator just beginning to come into existence in the early 1940s. These are persons who have some degree of anthropological training, with academic credentials ranging from bachelors' to doctors' degrees, who have deliberately sought careers outside of academia. The number of these has been steadily increasing. In Washington and in other cities of the United States, once an effort is made to locate them, it turns out that they exist, not in large, but in considerable, numbers. In a small place like my home town of Tucson, a mailing list of nearly a hundred was developed in a few weeks. They are working in many fields ranging from health planning to rural development, some 20-odd fields in the southern Arizona region alone. Who are these people?

I think we can tentatively say that their core consists of individuals for whom the academic life simply lacks attractions. They left it after more or less training and have stayed happily out of it. This is not only a recent process; it has been going on since World War II at least. So that when the current sharp reduction of demand in academic anthropology began a few years ago, there was a considerable scattering of such persons in many different parts of the country. Now their numbers have been increasing as academic jobs have decreased. The point is that there are hundreds of persons trained in anthropology who are now committed to nonacademic careers. From my experience among them, it appears, they are people with two commitments, both to anthropology as a result of its influence on them as students at some level and to the practical world where they now make their living and focus their lives. The commitment to anthropology is intellectual, or perhaps better said, anthropology for them is a continuing source not only of ideas but also of life goals in the work in which they have become involved.

There has, in short, developed a bridgehead in the world of the administrators, as the founders dubbed it. There are today more anthropologists turned administrators in the manner of the first president of the SAA. They are working in various ways at various levels. In general, they have found the SAA rather far from their interests, although they seek contacts and communication with the world of anthropology. It seems to me that the SAA might make some special effort to rethink its organizational form and its organizational goals. With the two bridgeheads that now exist, an overall bridging function should be reconsidered. What new form of organization is called for, I do not know or whether the SAA can invent it. But for specialists in human organization the situation would appear to be a significant challenge.

Meanwhile interesting new organizations have developed among some of the anthropologists turned administrators. This is exemplified in the active Society for Professional Anthropologists (SOPA) in Tucson. Its aims and structure have been outlined in a recent number of the *Anthropology Newsletter*. It has arisen, I think, out of the feeling that there is fairly complete domination of the traditional anthropological organizations, including the SAA, by academic people. SOPA took the view that an organization would serve their interests which unfolded a broader umbrella, in other words, an organization which included and emphasized the interests of the many nonacademic anthropologists who exist. Therefore they went ahead and organized. It is so far a local, or regional, organization which brings about communication and stimulation among not only the nonacademic anthropologist-administrators who dominate its policymaking and other activities, but also between them and the academic anthropologists who have no local organization, but who participate in the meetings and indeed the steering committee of SOPA. It seems to me that this organization points some of the directions of the immediate future, as academic anthropology moves out of its strange cocoon characteristic of the 1950s and 1960s. The present epoch is one in which a wider variety of anthropologists are asserting themselves and new forms of organization and training are called for to meet their needs.

#### Notes

<sup>1</sup>Personal communication, Philleo Nash, March 19, 1976, at St. Louis, Missouri.

#### References Cited

- Bastide, R.  
1873      Applied Anthropology. New York: Harper and Row.
- Boas, F.  
1962      Anthropology and Modern Life. New York: W. W. Norton.
- Bonfil Batalla, G.  
1966      Conservative Thought in Applied Anthropology: A Critique. *Human Organization* 25:89–92.
- Brinton, D. G.  
1895      The Aims of Anthropology. *Proceedings of the American Association for the Advancement of Science* 44:1–17.
- Chapple, E.  
1951      Field Work—Anthropology's Indispensable Approach. *Human Organization* 10(3):3–4.
- Editorials  
1949      Our New Look. *Human Organization* 8(1):3.  
1952      Research—Business or Scholarship? *Human Organization* 11(3):3–4.  
1953      The Challenge of the Science of Organization. *Human Organization* 12(2):1–2.  
1954      The Role of the Consultant. 13(1):3–4.  
1955      Applied Anthropology. *Human Organization* 14(2):2.  
1956      How Far Have We Come? *Human Organization* 15(2):1–2.

- Foster, G. M.  
1969     Applied Anthropology. Boston: Little, Brown.
- Malinowski, B.  
1939     The Scientific Basis of Applied Anthropology. *Reale Accademia d'Italia* 18:6–26.  
1970     Dynamics of Culture Change. New Haven: Yale University Press.
- Provinse, J. H.  
1971     Anthropology in Program Planning. Address, Annual Meetings of the American Anthropological Association, Philadelphia, Penn.
- Roethlisberger, F., and W. J. Dickson  
1972     Management and the Worker. Cambridge, Mass. Harvard University Press.
- Stavenhagen, R.  
1973     Decolonializing Applied Social Sciences. *Human Organization* 30:333–57.
- Society for Applied Anthropology  
1949     Report of the Committee on Ethics. *Human Organization* 8(2):20–21.  
1958     Values in Action: A Symposium. *Human Organization* 17(1): 2–33.
- Thomson, W. D.  
1970     Human Organization: A Three Year Review (1967-69). *Human Organization* 29:215–22.
- Tylor, E.  
1958     Religion in Primitive Culture. New York: Harper and Bros.  
1960     Anthropology. Ann Arbor: University of Michigan Press.
- Warner, W. L., and J. Low  
1947     The Social System of the Modern Factory. New Haven: Yale University Press.
- Wolfe, A. W.  
1974     Statement Defining Applied Anthropology. Memorandum to Editorial Board of the *American Anthropologist*. Unpublished manuscript.

## **Chapter 7**

### **Sol Tax:**

### **Advocate for Indian Rights and Promoter of Anthropology**

**Thomas Weaver**

George Stocking begins his ode to Sol Tax, recipient of the Society for Applied Anthropology's Malinowski Award<sup>1</sup> in 1977, with words that summarize well his aspirations and actions: "innovative kinship analyst and economic anthropologist, facilitator of 'action anthropology' and the modern Native American movement and organizer of international anthropology . . ." (Stocking 1995:43). The first phrase in Stocking's statement refers to Tax's early training as a kinship specialist under A. R. Radcliffe-Brown at the University of Chicago (Tax 1937, 1955). The second refers to his work in the highlands of Guatemala, with the backing of Robert Redfield, based on which he produced *Penny Capitalism* (1953). The last two of Stocking's phrases, which correspond to the last two phases of Tax's career, concern us, as applied anthropologists, the most. Tax attributed his renewed interests in social action and in using anthropology to solve social problems to the Second World War, the nuclear bomb, and Hitler (Tax 1988:8). His development of "action anthropology," in particular, came as a result of questions raised by his students while working with the Fox Indians in 1948. Action anthropology involved Indians in setting their own goals and actions for change. The rest of Tax's professional career was spent helping Indians help themselves and organizing and facilitating international meetings and publications.

Sol Tax (1907–1995) studied at the University of Wisconsin with Ralph Linton and at the University of Chicago. From there he went with a Beloit Logan Museum archaeological expedition to North Africa in 1930 and the following year attended an ethnological summer field school among the Mescalero Apache with Ruth Benedict. His dissertation, entitled "The Social Organization of the Fox Indians," qualified him for the doctorate in 1935 and was published in 1937. He taught at the Escuela Nacional de Antropología e Historia in Mexico and then, after 1944, at the University of Chicago.

Tax's entrepreneurial skills in international meetings and publications are legendary. In 1961 he coordinated the American Indian Chicago Conference. The 800 Indian leaders attending the conference created a document entitled *Declaration of Indian Purpose*, and the American Indian Youth Council was also founded at this time (Stocking 1995). In 1973 he organized the meeting in Chicago of the Ninth International Congress of the Union of Anthropological and Ethnological Sciences. His organizational feats included the distribution of papers for advance reading, simultaneous translations of the deliberations into five languages, and the publication of

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the panels and symposia in a 91-volume series on world anthropology. Most unusual for an academic meeting was the commissioning of an opera to commemorate the opening of the congress.

Tax's Malinowski Award address, entitled "Anthropology for the World of the Future: Thirteen Professions and Three Proposals" (1977), reflects several of the major interests of his life work. Foremost was a concern for the welfare of indigenous groups and his attempts to provide guidelines so that they could independently create their own futures. The third proposal in his address laid out a plan for placing anthropology at the service of indigenous peoples; in particular, he proposed that U.S. anthropologists "assist North American Indians in their efforts to resist further erosion of their rights and to increase their resources" (1977:231). Only a few of the many projects Tax himself initiated to help Indians are mentioned in his address. For a more extensive discussion one needs to refer to his autobiographical assessment published in 1988. Most prominent among these projects, perhaps, was the Congress held in Chicago in 1950 that brought together people from many tribes to consider their future. The ideas reflected in Tax's concept of "action anthropology" permeated all his work with Indians and were influential in later concepts developed by applied anthropologists known by such names as advocacy anthropology and participant development.

Other of Tax's interests included the organization of vehicles for expressing and disseminating anthropological knowledge and futuristic visions about the intersections between world and local society and government. Most of his acceptance address dealt with the place and practice of anthropology in a problem-filled world. The first proposal in his acceptance address was for anthropologists to join together internationally to produce educational and training materials for the human sciences. His second proposal was to create a journal through which the thousands of persons with anthropological training in the workforce outside of academic could communicate; this suggestion led to the initiation of the Society for Applied Anthropology's career-oriented publication, *Practicing Anthropology*, approved by the SfAA at the San Diego meetings where Tax received the Malinowski Award (Wolfe 1979). As a result of such education and communication efforts, Tax believed anthropologists would be better able to spread the pluralistic values basic to his model for the future: a two-level world society that consists of small, interactive, self-governing units that are based on the acceptance of other's values.

Throughout his later career Tax spoke out against the termination of Indian reservations (1957) and against the utility of the concepts of acculturation and assimilation. He assisted many tribes in many ways—by giving them a voice through publications, by speaking out in court and public places, but mostly by providing platforms for them to speak out for themselves. As he became more involved in action anthropology with Indian populations, he moved away from ethnology and history. "I find that I cannot treat them, as once I could, as subjects of study. Indians and I are friends, respecting each other as equals who are different" (Tax 1988:15).

Tax was among several Malinowski Award winners who served as editor of the *American Anthropologist* and as president of the American Anthropological Association. He was also founder and first editor of *Current Anthropology*. In recognition of his many contributions, the Society for Applied Anthropology established a Distinguished Service Award in his name in 1909. Tax's greatest honor, however, is that Indians called him "friend." This brief assessment and the work reflected in his acceptance speech do not do sufficient credit to a career dedicated to fighting inequality and racism, especially on the side of American Indians.

### Notes

<sup>1</sup>This annual award is given to a senior colleague in recognition of efforts to understand and serve the needs of the world through social science. See Thomas Weaver's "The Malinowski Award and the History of Applied Anthropology" (2002b) for a brief history of this award and an overview of the recipients and their work, and Weaver's "Malinowski as Applied Anthropologist" (2002a) for an introduction to Bronislaw Malinowski and his work.

### References Cited

Stocking, George W., Jr.

1995 Sol Tax. *Anthropology Newsletter* 36(3):43.

Tax, Sol

- 1937 The Social Organization of the Fox Indians. *In* *Social Anthropology of North American Indian Tribes*. First Edition. F. Eggan, ed. Pp. 243–285. Chicago: University of Chicago Press.
- 1953 *Penny Capitalism: A Guatemalan Indian Economy*. Washington, D.C.: Smithsonian Institution Press.
- 1955 From Lafitau to Radcliffe-Brown: A Short History of the Study of Social Organization. *In* *Social Anthropology of North American Indian Tribes*. Second Edition. F. Eggan, ed. Pp. 445–481. Chicago: University of Chicago Press.
- 1957 Termination vs. the Needs of a Positive Program for American Indians. *Congressional Record* 103(116), July 3.
- 1977 Anthropology for the World of the Future: Thirteen Professions and Three Proposals. *Human Organization* 36(3):225–234. (Republished at <<http://www.sfaa.net/>>.)
- 1988 Pride and Puzzlement: A Retro-introspective Record of 60 Years of Anthropology. *Annual Review of Anthropology* 17:1–21.

Weaver, Thomas

- 2002a Malinowski as Applied Anthropologist. *In* *The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers*. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.
- 2002b The Malinowski Award and the History of Applied Anthropology. *In* *The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers*. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.

Wolfe, Alvin W.

- 1979 How It All Began. *Practicing Anthropology* 1(4):3,19. (Republished in *Classics of Practicing Anthropology: 1978–1998*. P. J. Higgins and J. A. Paredes, eds. Pp. 15–19. Oklahoma City: Society for Applied Anthropology.)



## **Anthropology for the World of the Future: Thirteen Professions and Three Proposals**

### **Sol Tax**

On an occasion like this, when I am of an age to receive a coveted honor, before an audience of valued colleagues, and able to speak without competition, perhaps I have some right to make a grand review of the views I have developed over a lifetime. But it crosses my mind that although these views must be uncommonly important to justify being wined and dined like this, they also ought to be so well known as not to bear repetition. So I should sit right down.

But this is the very best platform from which to present proposals of good things for anthropologists to do. I shall talk about three which complement one another, and I shall bolster them with a dozen of what I shall call Professions, which word I remember from a 1915 article by A. L. Kroeber which he called Eighteen Professions. Kroeber's professions concerned his belief in the separation of biology and psychology on one side from history and culture on the other. I like the term because it implies that I surely believe that these dozen propositions are true, without requiring that you have to believe them too. Therefore, I need only tell you why I suppose they are valid at least for myself. We can then join together in the actions for whatever different reasons we may individually have.

My experience in anthropology goes back to the summer of 1928, almost 49 years ago, when I read a little book called *Anthropology* by R. R. Marrett. When I returned to the University of Wisconsin that autumn, Ralph Linton was a new acquisition, and I enrolled in his first course. It might have been after his first lecture—or a week or two later—that I shifted my major from economics to anthropology. When I approached Professor Linton, he told me that there were only 50 professional anthropologists in the United States and the opportunities were excellent. Indeed they were, and—as you see—it has all worked out for me well enough.

Today a pall has settled over the field. It is said that there are more B.A.s than there are fellowships, more Ph.D.s than there are jobs. We have begun to fight among ourselves for what appears to be a limited good. It is suggested that we close ranks and practice birth control. Too many anthropologists.

But wait. The pall that has settled over our field is part of a large cloud that has settled over the world. The road of technological progress which the species has travelled seems now to be a dead end. We who are in the vanguard and see the truth continue to push forward to the inevitable because there is no turning off place; and besides we are pressed from behind by others who have not yet enjoyed our primrose path. So we ask whether the limited good for which we compete is even a good. Why anthropology? Why science or education? Why life itself?

But wait again. Perhaps what seems now inevitable is not. Is there really no path ahead, no way to backtrack? Mightn't there even be a technological solution, perhaps including now

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This address was previously published in *Human Organization* 36,3(1977):225-234.

social engineering? And if there is any chance of that at all, isn't anthropology part of the solution?

Clearly, speaking before the Society for Applied Anthropology on a formal occasion in honor of an ancestor—and especially in the face of a most doleful alternative—you expect me to make such a case. And this is indeed the context in which I am offering proposals which assume that anthropology is critical to the future of the world.

The first proposal asks us to join with our colleagues abroad in mobilizing resources for research, training, and education in the human sciences:

As part of the American contribution to the Xth International Congress in New Delhi (December, 1978) we should propose a transformation into modern terms of the *1953 Anthropology Today* international review of our sciences. We have 18 months to prepare specific proposals for the international production of a common body of materials for education, training, and research, and for the proposed International Human Sciences Year.

Educational materials might include (1) paperback modules each reviewing a specific topic, translated into appropriate concepts and languages for universal use in colleges and by leaders and policy-makers; and (2) a series of videotapes for schools and for general audiences.

Training materials might include computer programs and visual as well as printed materials which can be used in common. Research tools might include a common bank for storage and interchange of data, hypotheses, discussion, and conclusions.

Preparation for the International Human Sciences Year (or Decade) might include (1) a document which projects future changes in the world of which anthropology must particularly take into account, and (2) concrete plans and priorities for research to be undertaken in all of the anthropological sciences.

You see that there is little time; but please realize that we have already come far since this was first discussed at the meeting last December of the American Anthropological Association. A group of volunteers at the meeting, coordinated by Dorothy Willner of the University of Kansas, have already organized dozens of sessions at meetings this Spring, including those here in San Diego. Now Demitri Shimkin has undertaken to help Dr. Willner to develop the further program to prepare for the Xth International Congress in December of next year when we should have concrete plans to be approved, or changed and implemented. To make this possible, a midwestern consortium will be formed in two weeks at the University of Illinois, to establish means of communication and voluntary participation on a national scale. Then in Houston, immediately before the AAA Meeting in November, there will be six half-day substantive conferences followed by three Plenary session reports of results and a series of volunteer symposia running throughout the meetings. We fortunately have the fullest cooperation of the Program Editor, Edward Norbeck. By the end of the Houston meetings, tasks should have been set for implementation in February by Section H of the AAAS, in Washington, and at the spring meetings of the specialized and regional societies. We must also find means to report our progress, as we go along, to our colleagues around the world. Hopefully, they will be waiting to respond when we all reach New Delhi.

Now I turn to discuss briefly the first set of four professions which for me justify and

demand this large effort:

(1) Through understanding of the human species in its fullest biological, social, behavioral, cultural and ecological context, Anthropology provides an essential and unique ingredient for understanding also problems which beset the whole or whatever parts of the species.

(2) It is the cumulative knowledge which we inherit and develop—knowledge of phenomena, method, and theory concerning the species—which gives to anthropologists whatever usefulness and influence we have.

(3) Anthropology consists of a variety of disciplinary, geographical, and temporal specialists who work with peers of other disciplines and also interact with one another.

(4) We are necessarily international and interclass both in the exchange of information and in the application and exchange of situationally and culturally differing points of view.

The first profession says that anthropology has something unique and important to contribute to understanding phenomena, and hence problems that beset the world. The problems to which I refer are those of all times and places and levels: problems of individuals in their views of themselves and their worlds, in their interrelations and in whatever else accounts for differences in their well-being; problems of intermediate institutions such as families, voluntary organizations, rural and urban neighborhoods, businesses, governmental and private bureaucratic organizations of all kinds, religious and educational institutions, labor and professional organizations, and others that I do not mean to neglect; and problems of the largest institutions, nation states, multinational corporations and political organizations and indeed the whole world system, past and contemporary. In short, my intention is to include all domains in which humans are involved and to which therefore anthropology has a contribution to make.

Please note that I speak of understanding rather than of solving problems, to avoid any implication that anthropologists might ever be the major instruments in resolving complex social problems in which knowledge of others is essential and for which the purposeful action of people is critical.

I have added the second profession to emphasize that it is not an anthropologist in isolation who has what it is important to offer. What is useful is rather the cumulative knowledge that we have inherited and to which we individually contribute only in minor ways. This proposition implies also my belief that we deal not with a distinction between pure and applied anthropology but rather an amalgam or continuum of the two, a differing mixture of modes in all of us who are anthropologists.

One confirmation of a 30-year feeling that these first two professions are true came in an interdisciplinary conference on anthropology and mental health in 1972. Anthropologists prepared papers—published later in a book edited by Thomas Maretzki and Laura Nader—on particular aspects of work in mental institutions and concerning social problems. When I read the papers in advance it seemed to me that we were making assertions that psychiatrists, sociologists, and others who had been working in such institutions for generations would have made long since, or would dispute us. In the conference I had a moment to raise this question, and learned from the other behavioral scientists around the table that the observations and conclusions we

were reading were both new and valid. I am sorry that the theoretical discussion of why this should be the case is not included in the book; but I recall that the anthropological tradition of fieldwork experience in very different cultural situations induce us uniquely to look afresh, and positively, at differences wherever we see them. Whether or not this is part of the explanation, that group around the table recognized a clear difference.

The argument is not that we are better, but that we are different. Economists, sociologists, psychologists, political scientists, and others may rightly say the same about the uniqueness of their respective contributions. I think, however, that we may be more different than is each of them from the others. I once thought of the analogy of a pot of soup which gets its character and quality from the meat and vegetables which are principal ingredients. The salt is a miniscule addition that makes a lot of difference; and it makes somewhat the same difference no matter what kind of soup it is. I thought of anthropology as that small ingredient that produces a qualitative change in understanding many different situations. I am content if other disciplines say that the analogy applies to them equally well; what is needed as we move on in my professions is acceptance of the thesis that anthropology has something unique and important to offer.

My preference for combining learning and using, on the grounds that each feeds on the other does not mean that I think the two are inseparable. They are separated by the division of labor among anthropologists and in the timing by each. Rare is the anthropologist who has never pursued a theoretical problem for its own sake, if only as a learning experience; and just as rare the anthropologist who pursues theoretical problems and avoids coming to understand particular human situations, especially when fieldwork is involved. The differences among us can be of great degree, however. And at different times one of us can be doing much that is theoretical and general; at other times much more that is particular and applied. And at still other times engage in activities that are inextricably intertwined. In any case, to generalize and to publish general conclusions which can be tested and used by others is the heart of our professional craft. It is a secondary consideration whether these are developed in the course of theoretical and abstract play with data and ideas or in the course of work with people in their own environments. It is the business of anthropologists to add to the structure of our common knowledge. It is that process and that ideal which give us the wherewithal collectively to be useful in the historic sense, even though most of us at most times, and individually, may appear to be involved only in our own problems. A goose which takes no food will soon lay no eggs, golden or otherwise.

My third profession assures that the knowledge we get comes through the agency of individual scholars each of whom faces in two directions. They are at once specialists who face scholars in other disciplines related to their specialties, and they are anthropologists facing one another in their common pursuit of the holistic study of man. Thus some work with—indeed are—anatomists, physiologists, haematologists, geneticists, primatologists, ethologists, geologists, palynologists, agronomists, paleontologists, or chemists—it seems an endless list for those who pursue archaeological, prehistoric, paleoanthropological and human biological specialties. Others of us, pursuing understanding of human communication, identify with some of these and also with psychiatrists, psychologists, linguists, literary and performing and other arts scholars, and many others. Ethnologists, folklorists, and specialists on different areas of the world again face and identify as some of the disciplinarians mentioned and/or also technologists, sociologists, economists, educators, legal and political scholars, and the range of professionals and practitioners who study and deal with our complex society. If it is our knowledge that gives us power, all of the specialists with whom we work, and the techniques they develop which we

are able to apply to anthropological problems, add to our strength.

But this strength comes at least equally from our mutual interest in the human species as a whole. In my lifetime I have seen very little conflict of identity or of interest, and a great deal of cooperative interchange, as we wander among the disciplines and regroup freely. Back home, our department colleagues provide us with in-groups usually happier than in other disciplines, for we do share important social values as well as interests; and where not, the outside interdisciplinary connections provide important relief from the tensions in a corporate group of colleagues who make fateful decisions about one another.

The fourth profession—that anthropologists are peculiarly international and interclass in the exchange both of information and of points of view arising from different cultural and situational backgrounds—has been thoroughly confirmed by my experience traveling for, and editing *Current Anthropology*. My successor Editor, Cyril S. Belshaw, has just published a Guest Editorial in the *American Anthropologist* which moves in the same direction with some newer ideas. But I shall say no more on this except that there should be no doubt that we are nothing if not international and cross-cultural.

I move now to the fifth profession which will lead directly into the second proposal:

(5) Anthropologists are self-selected for pluralistic propensities and values in which we become confirmed through association and practice, and under the influence of the variety of cultures which we engage.

Whatever propensities and values may unite and distinguish anthropologists, first among them is a view of life that is relativistic and pluralistic. We do not partake equally of this predilection; all is relative. Nor are we the only relativistic and pluralistic people. But we are among those who are most relativistic and pluralistic; and we are the only profession, or even community, for which this view of life is definitive. Without it, being an anthropologist might be unpleasant. Our central concept of culture, our other-people and long-term perspective, and our fieldwork ethos must influence us in this direction.

It must be kept in mind that anthropology is a free association. Nobody has to stick with it, or with us. Hence, the self-selection for propensities and values becomes confirmed by association, through the books we read, and by the research we do. Students who come into our courses, and decide to major in anthropology have taken a first step. They may or may not seek contacts with fellow students in anthropology, join the club if there is one, and come to parties. These are further steps. One with strong racist tendencies would presumably not go this far; and one with pluralist tendencies would have them strengthened. If they move into graduate work, even if they never get degrees or become professional anthropologists—but only behaved as if they wanted to—they might be irrevocably committed to the values. One assumes that on the face of it, they might also associate this commitment with the term anthropology. Many more persons might have these same values which they do not associate with anthropology; with them the values are neither reinforced by contact with our profession, nor verbalized in vocabulary that anthropologists might use. If 20% of our population are—in terms of values—potential anthropologists, I suppose that only one in a hundred of these would have had enough contact with us to associate their values with anthropology. But even that small proportion would still add up to 200,000 adults. It is this universe that my second proposal—to bring anthropology into the everyday social and occupational structure—concerns:

There are thousands of anthropologists in the workforce who have had training in anthropology. On the initiative of anthropologists at the University of South Florida, the program will identify as many of these as possible, and help them to inaugurate a journal through which they can recognize one another and intercommunicate. It is hoped that this communication network will soon reach international academic anthropologists, undergraduate and advanced secondary school students, and interested others: and thus will quickly broaden the base and influence of anthropology.

Suppose there are only 50,000 of them in the U.S.A., and corresponding numbers abroad, who have a commitment to, and some relevant knowledge of, anthropology. My proposal is to find ways to include them in our professional quest. I hope that they might be sitting by the telephone, waiting: they might instead long since have turned away from us. Most likely, there is some proportion who would like to rejoin our ranks. In any case, I am glad to report that the Department of Anthropology of the University of South Florida is about to find out. The thought is to offer to a sample of the population of persons who have received degrees in anthropology over the years a means to communicate through a new journal, perhaps called *Practicing Anthropology*. I hope that they can be offered participation or membership in the enterprise, rather than subscriptions with the implication that they will be readers or spectators.

I foresee this possible future: (1) that some thousands of them who are in the nonacademic everyday work force will tell one another publicly that they are behaving differently from their colleagues, and in some ways much more effectively, and that they would like to have other anthropologists join them at work; (2) personnel offices, Civil Service Commissions, and others would recognize the positions for which training in anthropology is beneficial; and (3) more and more identified anthropologists would enter the occupational structure, and the process would snowball. Since the inclusion of anthropological understanding and values in bureaucratic organizations of all kinds would indeed be beneficial, this alone would improve our society. But perhaps much more would happen.

In 1955 some of my students and I were instrumental in developing what we called a Summer Workshop in Indian Affairs. It began innocently enough when Galen Weaver, a nationally prominent clergyman interested in race relations and in American Indians told me that the churches were providing college scholarships to young American Indians who were then unhappy in the colleges where they found themselves, usually alone, because everybody there assumed that since they were themselves Indians they would know all about American Indian Affairs. Fred Gearing and I suggested a residential workshop, which began the first summer with 30 students from about as many reservations. It did not occur to me that simply bringing them together to compare notes would be the beginning of an American Indian youth movement and perhaps more. But that is what happened; and now I realize that this could happen also if anthropologists working separately were brought together.

And such a movement might make a large difference in shaping the next century. I suppose that the main danger before us is the contradiction between a rapidly approaching single, interdependent world economy while we still maintain competing nation states with lethal power. Let me describe briefly the kind of model for the future that might help to resolve this contradiction; then say a word more on the kinds of values that might be needed by people who implement the model; and relate these to anthropology.

Suppose that our rapidly developing multinational corporations find it necessary—and

have the power—to veto wars among nation states and encourage or suffer a fast evolution away from nation states toward the kind of 2-level model that I have invented for the present purpose: at one level the world is redivided into about 10,000 relatively self-sufficient local territories ranging in population from 100,000 to 800,000, with an average of half a million, which would together accommodate a world population stabilized at five billion. Each of these units, called “Localities,” produces most of what it uses in both services and products and extras of some things for exchange. People live and work here, organizing and spacing themselves as they find meet, and having full local control. Interlocality trade moves in a kind of free market only as far as necessary to satisfy needs.

The second level consists of a system of public electronic communication which keeps track of all that happens in the interrelations of the localities. Although the system has operators, the salient sacred constitutional rule is that every person or family or institution can initiate input into the system. The system—that is what it is called—constitutes the only government that is necessary. It is programmed to assure that every changing interest is represented, with the weight of representation open to discussion and change. The constitution developed through, and programmed into the system is egalitarian and libertarian and also specifically designed to limit the growth of power of individual localities, and to prevent arrangements which give localities power through alliances.

The localities are the highest governing units in the world, and within broad limits decided through the system, may develop and experiment with any constitutional form of decision making. Unlike the world, the localities have in their governments people as well as electronic communications, which are public within the community (for public matters) and private outside except when constitutional issues are raised by individuals or groups, which action brings into play world procedures developed and changed through the system.

There is constitutionally free movement of persons within localities and between localities anywhere in the world, temporary and permanent.

Privacy in the locality and world systems, subject to constitutional limitations, is provided for kith-kindred and extended family groups; voluntary organizations of all kinds; ethnic and language groups; groups pursuing educational or recreational programs; and everybody on occasions that now require telephones or first-class postage stamps.

I think that is enough for present purposes, although I would like to imagine the kinds of cultures that under these circumstances would develop in different parts of the world, including our own.

I shall instead turn to a model of the relativistic pluralistic *person* who might suit this society and bring it into being. Again I think there is a single value to be invoked, which corresponds to the no-growth of power value which dominates my model of the new world. Whether we call it negatively “judge not” or positively “live and let live,” I suppose it is the heart of the pluralistic value system to which anthropology is professionally committed. It is also the only value system that could operate the society I have described. To get such a society requires anthropology and anthropologists.

I shall spell out a version of this value system which I have used only in classes, calling it a “value triangle.” One leg of the triangle is a personal preference: I like or dislike something. I like apples, I like peace; I dislike spinach, I dislike cannibalism. A second leg is a judgment: apples are good, peace is good; spinach is bad, cannibalism is bad. The third leg is an edict: people should be encouraged to eat apples and to keep the peace, forbidden to eat spinach or to be cannibals. Now put them in another form. I like apples; everybody ought to like apples;

therefore a campaign for eating apples is legitimate. The second phrase is silly but human and harmless; we know that tastes differ, but we enjoy promoting our own; and it may be legitimate to use persuasion to broaden peoples' repertoires. Now try this: I dislike spinach; everybody ought to dislike spinach; let us taboo spinach. The second term sounds worse than silly and it surely is not harmless, since it leads to a restriction rather than an expansion. Now try this one: I abhor killing; everybody ought to abhor killing; therefore, forbid murder and war. The form is the same, but many will object if I call it silly to say that everybody ought to abhor killing. Is it because a moral judgment is different from a matter of taste? No. On the contrary we call it a moral judgment because our culture has independently given us that edict, "Thou Shalt Not Kill." After all, the same culture that gave us our ten commandments also forbids its adherents to eat pork.

My conclusion is that a pluralistic, relativistic view accepts the same rules for morals that it does for tastes. The fact that I abhor killing does not make killing wrong; so we have to skip the general judgment. The phrase "Everybody ought to abhor killing" has to be left out. If we leave out all of the general judgments we can call ourselves relativistic. But when we discard the judgment that killing is wrong that does not mean that we permit murder. There are many reasons to forbid murder, and I always vote to do so. It only means that I do not rationalize my preference—my vote—by calling up a universal moral judgment. The point is perhaps made more evident to anthropologists if I substitute the word polygamy for murder. I prefer one wife for myself: that does not make monogamy right and polygamy wrong for everybody in the world. If I ever need to vote on an issue like polygamy or homosexuality or abortion, the only moral judgment that I might consider using is the one most anthropologists hold and which I have described as necessary to move us to a safe and livable society: "Live and let live."

Let me take a harder case:

A few years ago I was invited to a liberal arts college celebrating an American Indian day. I shared a platform with an Indian educator before a large audience of students and faculty. I had spoken about policy and about White-Indian relations, rather than about anthropology. But during a lull in the question period he turned and asked me to justify the Bering Straits origin story that we tell and that runs counter to tribal traditions. My response included the fact that man's closest primate relatives are native only to the Old World. This involved our theory of evolution, which he soon made clear was not accepted by Indian people, who have their own creation myths. My only recourse was to plead pluralism in reverse, saying in effect that evolution is one of the things anthropologists believe and that I was stuck with it: an anthropologist saying before a college audience that biological evolution is an alternative myth!

Since I spend time with Indian friends all or most of whom are very serious in their feelings in the matter, the problem stays with me. Although of course I believe that biological evolution is a more probable explanation of the distribution of species than that each American Indian tribe was specially created according to its own mythology, I find it difficult to persist in this view, since the Indian's explanation is for him 100% true! If I had to write a textbook now, I would discuss the Asian origin of American Indians in the context of the conflict as I am describing it. For isn't what I have said the pluralistic anthropological truth?

Some of you are perhaps thinking, "Very well, in dealing with another culture, we can live with pluralism. But what about the people here in California and elsewhere who demand that schools teach the Book of Genesis together with natural selection?" Since unlike our American Indian friends those are the same people who insist on legislating their private preferences in any matter, do we have to give in to them? Perhaps we do, in the same way that the American Civil



Liberties Union proudly defends the civil rights of Nazis and child-abusing pornographers: or perhaps we don't have to give in on other practical grounds. But we have to rethink the problem in terms of pluralistic values.

I bring this problem to us to make sure that we all understand what I mean by pluralistic values. I am arguing first that anthropologists are more likely to accept this philosophy than any other professional group; second, that this philosophy is one of the cornerstones of what is needed "to save the world"; and third that hence anthropologists have some obligation to see that we occupy as many positions as possible in our society so that our philosophy becomes more and more practiced in places that count and the numbers who practice it are increased. *Teaching* our philosophy is our honored craft. Teaching it to colleagues on the job will be a function less of preaching than of practising. Hence the second proposal whose rationale may be summarized by reading my sixth and seventh professions.

(6) Human nature, formed in hunting-gathering communities over hundreds of thousands of years in which cooperation and harmony among persons were positively valued, has been increasingly violated by the emergence in the last few millennia of hierarchical societies of competing individuals and nation states whose successes in technology threaten continuation of the species, and whose failures otherwise induce malaise.

(7) With little time to develop alternative models for living for the peoples of the world, and to overcome the obstacles to movement from where we are to any new model, persons with anthropological knowledge and with pluralistic values are urgently needed in all parts of the social structure as examples, teachers, movers.

But as important as we might be in saving the world, the people from whom we learned so much, and who confirm and live the values that are needed, are even more important. So, my eighth profession says:

(8) Since surviving societies with the cooperative nonhierarchical ethos in several parts of the world provide urgently needed patterns for restoration of communities, it is necessary as well as humane to protect and learn from them, and anthropologists are their obvious brokers and advocates.

Needless to say we want protection against destruction for all peoples, but the developing economies of the world have been most destructive of the special peoples who have survived into our world maintaining the ethos which characterized humankind during the long paleolithic. To say hunting-gathering is to mention a good correlate of the cooperative nonhierarchical ethos. In characterizing the peoples who are the subject of my third program, it seems best, however, to look first to the neolithic and urban changes which resulted in larger societies, social classes, war and conquest, empires and kings and lords with subjects, slaves, serfs, vassals. We are taught to admire the rise of civilizations and their glorious works and to worry only about how they fell. The people who remained after the fall were the exploited, economic base of the whole, and the pawns in the struggles for power. They are the peasant peoples whom most of us have studied. I have greatly enjoyed their company in Mesoamerica. They tend to be economically oriented, competitive, individualized, hierarchical, and still exploitable by larger politico-economic

structures. These are the people—most of the world—that I am *not* talking about. In every part of the world there are also enclaves of independent, indigenous peoples who have never been coopted or have escaped.

Often they are peoples who were there earlier than the major populations of peasant peoples: names like the Ainu of Japan, the Hill tribes of Assam, the Montagnais of Viet Nam, the Lapps of Norway, the Andamanese, the Bushmen of Southern Africa, are all familiar to our students. A survey of these on a world scale, and historical and comparative study, would doubtless show that there should not be a single word from them, except something like “independent.” These are peoples that have often been considered remnants, about to disappear. “Urgent anthropology” has meant studying their cultures and their languages before they disappeared. It clearly ought to mean even more urgently keeping away from them those others who have brought them to this pass and especially those who threaten still functioning societies—mainly for their own sakes of course, but also now because they are so important to the future of the species.

Among such peoples, the North American Indians in the year 1500 constituted what must have been the largest number that had escaped becoming peasants and pawns. The Australian aborigines and the Islanders of the Pacific were in somewhat comparable numbers and positions, and possibly also the peoples of the forests and tundras of Asia. The North American Indians are a special obligation for American anthropologists, however, and so justify our own special attention. The third proposal has therefore this general part as well as one for North America and especially the U.S.A. It concerns anthropology in support of indigenous peoples:

In cooperation with the I.U.A.E.S. Commission on Ethnocide and Genocide, it is proposed to offer our services, at least as individual anthropologists, to assist North American Indians in their efforts to resist further erosion of their rights and to increase their resources. For the U.S.A. it is suggested specifically that if invited we help them (1) to ask Congress to establish procedures wherein without prejudice to their present legal rights individual nations and communities may with Federal government support and funds enter into negotiations with local governments and interests to improve Indian resources and opportunities; and then (2) if invited we help any Indian group in its negotiations.<sup>1</sup>

I hope we all remember that the sciences of anthropology and ethnology originated in the antislavery movement of the early 19th Century, and specifically in the Society for the Protection of Aborigines. Although our forebears were often blinded by the culture and class norms from which they came, and infected with ideas of progress, civilization, the White man’s burden, and the melting pot, they were among the *least* blinded, the *least* infected, the least willing to act on such assumptions. And they were among the first to rebel. Today we are among the first to see and to want to mitigate the consequences of the “success” of our technological civilization, especially in its effects upon peoples who get only the evil. But let me move at once to the specific part of the task, involving Indians in the U.S.A.

I understand that long before they first saw Europeans, our Indians had opportunity to observe and perhaps to experience the civilization that had developed in Mexico, and they rejected it. If so, it is easier to understand the speed and intensity of their rejection of those who brought from across the Atlantic samples of a similar large and populous class-divided,

economically competitive, hierarchical and evangelical society.

To civilize the savages was the first and still remains the goal of the Europeans. One wonders if the Indian nations, recently decimated by epidemic germs which preceded the human immigrants, might not have given to the colonists large reservations in which to practice their economy and build their towns, on a live-and-let-live basis, if the newcomers had not so desperately wanted also their souls. At any rate, the Europeans grew in number and in power, and had but one answer for the Indians—become like us. So the Indians became a problem of the frontier to be dealt with by military and diplomatic means: and the new nation went about to achieve its manifest destiny. By the time Chief Justice Marshall wrote his decision recognizing Indian nations to be sovereignties, albeit dependent, for whom the United States had what came to be called trust responsibility, the issue of ultimate power appeared settled. It was probably assumed by everybody except the Indian people that their eventual disappearance was inevitable.

The framers of the Constitution had no choice but to deal with the Indian nations which if not recognized would join the enemies of the young nation. But one wonders whether Marshall and his concurring justices would have given to Indian nations perpetual status unless they took for granted that the problem would solve itself. What went wrong was that the Indians wouldn't go away.

In 1881 Helen Hunt Jackson published her angry book called *A Century of Dishonor*, to arouse the conscience of the American people with respect to our infamous treatment of Indians, as *Uncle Tom's Cabin* had aroused sentiment for the abolition of slavery. She sent advance copies of the book to every member of Congress and to officials including Interior Secretary Carl Schurz, the famous liberal. The result was a second century of dishonor. Congress decided that the reason why American Indians had not become assimilated to White ways was that they held their lands in common. Since people will only progress when they develop individual responsibility for their own property, the Dawes Act gave title of land to individual Indians. The Act was passed partly because of this theory and partly because land-hungry constituents were waiting impatiently to get the land. Thereafter more Indians became more impoverished, but again they did not disappear. Fifty years later the Meriam report described the desperate situation in which Indians found themselves; then Franklin D. Roosevelt's Interior Secretary, Harold Ickes and John Collier, his Commissioner of Indian Affairs, induced Congress to give to Indians a New Deal. The Indian Reorganization Act worked on the theory that Indians would adjust better and faster if their own cultures and tribal authority were respected, and their communities provided with better educational and economic opportunities. Tribal control of land, and even some increases in holdings were foreseen.

Before passage some of the features of the act which might have made it much more effective were eliminated. Even so, it was excellent and brought a lot of enthusiastic idealists into the system, including anthropologists. Its success was limited by three factors, in my opinion. First, although the original intention was to give more time for developing political institutions, the law asked tribes to adopt constitutions by a simple majority, and elect councils in the same way. Indians prefer time to negotiate substantial general agreement, so that 51 people do not have to live with 49 people who are forced to do something against their wills. Second, all important tribal actions were subject to veto by the Interior department, which gave to the system the character of indirect rule associated with colonialism and leading to a division between collaborators and resisters. How much the factionalism in American Indian communities now derives from these two faults is difficult to say; but they did not help. Third, there was no time really to test the New Deal for Indians. World War II began as early as 1938 to occupy the

government, which soon found it had to neglect such matters. The BIA actually was moved to Chicago for the duration of the war.

By the time the war ended, Congress and the Bureau were embarked on a policy that reversed the philosophy on which the Indian Reorganization Act was based. First they called it “Withdrawal” and then “Termination,” and it called for gradually and finally ending the special relationship of the U.S. government with Indian tribes which the Constitution and the courts and the treaties and the Congressional agreements had sanctioned over the years.

Termination too was soon discredited, since communities of Indians deemed “ready” to be cut off and treated like other citizens turned out (like the Menomines) not to be ready at all, and it was the tragedy of the Allotment Act all over again. A new word will have to be found to replace “termination.” The next time it will be needed is already upon us. And the danger is greater than ever because in the past ten years Indians have gone to the courts more than ever before and more successfully; and have succeeded in establishing their rights even to property that Whites have long used as their own. For example, about 2/3 of the land of the State of Maine; for example, the town of Mashpee, Massachusetts; for example, half of the annual catch of fish in the State of Washington.

The Constitution of the United States, parcelling out powers between the Federal government and the States, provides (Art. 1, Sec. 8) that “The Congress shall have power . . . to regulate commerce with Foreign nations and among the several States, and with Indian tribes.” It is the Federal government, therefore, and the Congress particularly, that has stood between the Indian tribes and the people who have wanted their land and their water and minerals. When the Indians were not vigilant, however, their influential White neighbors took away what they could. Now the Indians are getting some back. But the influential Whites can still win in Congress; and a bill has already been introduced to provide for review of all treaties to see their effects on the various interests involved. Congress may well have the power to revoke unilaterally even formal treaties, not to mention its own previous acts under the commerce clause. And a move to terminate the treaties and the special rights of Indians will sound reasonable to many Americans who think that a special legal position for Indians is an anachronism. The wars are over, they will say. Indians are now citizens and should get everything that they and all the rest of us are entitled to. Aren’t we for equal rights and integration? If we owe them money for past wrongs, let us just pay up and forever end this nonsense. Against such views, the only defense may be for Indians to take a strong initiative, as suggested in my last Professions:

(9) North American Indian people survive self-consciously. Those who have left their communities and traditional ways—whether by accident, temptation, persuasion, or force—and have disappeared into the general society are no longer Indians. The growing number who remain purposefully retain their tribal identities with, in most cases, distinctive, rich cultural heritages which have been partially hidden from anthropologists because of our overemphasis on outward manifestations of precolumbian culture-traits (which over the years were disappearing before our eyes) and also because we shared the general myth of the melting pot.

(10) The forebears of Blacks, Puerto Ricans, and Mexican Americans, like those of North American Indians, became residents of the U.S.A. involuntarily; but only the last—who were the first—have a special legal relationship to the governments of North America. Confirmed by treaties and agreements which

were in part negotiated sales of lands, hence protected by common and by constitutional law, Indian rights are among the important sacra of Indian nations and people. In the U.S.A. the Constitution gave to the Federal government the obligation to protect these rights against private interests working through state and local governments.

(11) Although the courts have recently been supportive of Indians in interpreting treaties and relevant acts of Congress, they also reassert the constitutional power of Congress to regulate Indian affairs. Whether Congress might again try to exercise this power to terminate the special legal position of Indians depends on its willingness to perpetrate a violation of human rights. Under pressures of local interests competing with Indians for land, minerals, scarce water, disappearing fish and game—and without countervailing support from liberals who do not understand the need to preserve for Indians their special legal position—that form of genocide might nevertheless come to be accepted.

(12) Even if we anthropologists in substantial numbers now place our knowledge, skills, and influence at the disposal of our Indian people, their cause might yet be lost. We have no choice but to discuss with them how we might help to counter the forces pressing Congress and the President to substitute cash settlements for the legal rights of Indian tribes and people.

(13) Anthropology, in which I also believe.

We have by now learned that Indian people are the best judges of what they should do. But we can help them, and I imagine they will recognize that we might be useful in helping to counteract the forces on the other side.

I am reminded of the 1960 annual meeting of the American Anthropological Association in Minneapolis, when we mobilized anthropologists to help Indians prepare the great national Pow Wow in Chicago in June of 1961. This was the beginning of a new era for them. It was reported fully by Nancy Lurie in the December 1961 issue of *Current Anthropology*. I see people in this room who participated in that effort. This next effort might take years instead of months. But for American Indians these would be years insuring their survival; for us and for the American people they would be years of great education; and they might just help us all to remake the world.

### Notes

<sup>1</sup> On April 17, 1977—nine days after my own proposals were voiced—the Field Foundation of New York released a 34 page report by Vine Deloria, Jr., the Standing Rock Sioux Indian who has taught anthropologists much, entitled *A New Day for Indians*. In it, he recommended seven reforms to be enacted into law by the Congress of the United States:

- (1) A uniform recognition of Indian communities, to provide rights to those now discriminated against.
- (2) A clarification of tribal membership, to recognize the rights of Indian individuals now excluded from tribal rolls.
- (3) A standard definition of the status of an Indian tribe, to apply more benefits to communities which have fewer than they might.
- (4) The creation of a “Court of Indian Affairs” to resolve disputes between Indians (on one hand) and other political entities and to supervise and monitor its decisions on the other.

- (5) Arbitration of long-standing claims, to restore lands illegally taken from the Indians.
- (6) Rejuvenation of the Indian land base, by purchasing for Indian communities lands which they need.
- (7) Universal eligibility for government aid, based on need, with block grants from which Indian tribes can provide individual members with educational, health, welfare, and unemployment benefits.

These reforms may be substituted for the U.S.A. program which my third proposal calls on anthropologists to support. The basic idea is for us to help Indian communities to obtain what they believe will help to restore to them the autonomy and resources needed for their well-being and their survival. Thus, we should support compatible reforms on which Indians themselves unite.

Copies of Vine Deloria's Report are available from the Field Foundation, 100 East 95th St., New York, N.Y. 10028.

## **Chapter 8**

### **Juan Comas:**

### **Indigenous Policy and the Fight Against Racism**

**Thomas Weaver**

The most important contributions of Juan Comas, the Society for Applied Anthropology's 1978 Malinowski Award recipient, were his promotion of an intercontinental indigenous policy and his fight against racism.<sup>1</sup> Gonzalo Aguirre Beltrán, a fellow Malinowski Award recipient (Aguirre Beltrán 1974), paid tribute to Comas in the following words:

No other scientist approaches, with such abundant and complex evidence, testimony, and rational argument, the task of proving the intellectual abilities of indigenous people and the fitness of their culture; no one else has demonstrated with such lucidity the cunning and false argument that underscores the stigma of inferiority assigned by ideological racists to these groups (Aguirre Beltrán 1980:26; my translation).

Juan Comas (1900–1979), a refugee from the civil war in Spain, adopted Mexico as his home in 1940. He had received a master's in education in 1917 in Mallorca after finishing high school the prior year. He earned a doctorate in physical anthropology in 1942 and was certified in biology in 1950, both in Switzerland. Before moving to Mexico he had published works on psychoanalysis, intelligence, and pedagogy and had served in educational administration in Spain (Rex 1980). Comas became a founding professor of the Escuela Nacional de Antropología e Historia shortly after his arrival in Mexico (Medina Hernández 1980). With osteological collections of the National Museum of Anthropology, he began life-long research and publication in physical anthropology (Romano Pacheco 1980, Faulhaber 1980). He advanced the scientific study of physical anthropology in a manual that was translated into English (1957) and contributed many articles on humoral medicine and a book on theory and applied anthropology (1964).

Bioanthropological study among the Trique Indians made Comas aware of the social problems caused by the dominant society (1942). His training as a biologist and physical anthropologist helped him underscore the physical, nutritional, and health as well as political and educational issues relating to the social conditions found among indigenous groups. The solution to these problems, he maintained, was to address with appropriate policies the socioeconomic, political, and cultural environment.

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Comas' association with anthropologists who worked with indigenous affairs changed the direction of his career. As executive secretary and editor of *América Indígena*, Comas developed the InterAmerican Indian Institute, together with Director Manuel Gamio, as an instrument for supporting indigenous welfare and goals throughout Latin America (Aguirre Beltrán 1980). *América Indígena* became the primary vehicle for airing the conditions and problems of indigenous peoples and for recommending policies for their amelioration (Comas 1953). In connection with the institute, Comas and Gamio traveled widely in Latin America to verify and report on behalf of indigenous peoples to governmental agencies and leaders. An important contribution Comas made was his meticulous, punctual, and tireless editing and founding of journals, chief among which were *Anales de Antropología*, *América Indígena*, *El Boletín Indigenista*, *Acta Americana*, and *Boletín Bibliográfico de Antropología*.

Comas' interest in racism was aroused by the treatment of ethnic groups in Falangist Spain. It was at the University of Geneva, however, that Comas began understanding and fighting racism. His Swiss anthropology professor, Eugène Pittard, had written a book on the subject that Comas translated as *Las Razas y la Historia* in 1959. His own publications on the history of anthropology and racism had begun even earlier, with the most important being *La UNESCO y los Prejuicios Raciales* in 1948 and *Los Mitos Raciales* in 1953 (1953a), both translated into six languages. In these and other writings he helped to spell out the unity of the human species and to distinguish genetic from cultural variation, as well as to negate such popularly held views as the heredity of inferior and superior cultural elements. Further, these documents affirm the scientific conviction that all people have the same biological capacity to attain any cultural level (Aguirre Beltrán 1980).

Comas' Malinowski Award address, entitled "The International Fight Against Racism: Words and Realities" (1978), was published the year before his death. Here he traced the history of international efforts to define and combat racism, which, he concluded, had produced more words than deeds. He proposed that scientific information about racism be widely disseminated among the world's indigenous populations, who are among those who suffer most from the phenomenon. In his speech, he also expressed disdain for the attempts by bureaucrats to defeat scientific intentions and deny the participation of scientists in important discussions at UNESCO meetings.

I do not know who subverted the suggestions of the experts, but I believe it can be safely assumed that one or several of the government delegates to UNESCO thought that it would be inconvenient if propaganda against racial discrimination should reach their countries. They must have seen that such propaganda could have had repercussions inimical to the interests of those anxious to maintain political and economic power over natives and Blacks. . . . (Comas 1978:338).

Comas ended his Malinowski Award presentation by outlining short- and long-term recommendations for ending racism. He stressed education on racism, both through the schools and by making documents available to the oppressed in their own languages.

Juan Comas is widely considered a great scientist, teacher, advocate for Indian rights, and editor. He received several honorary doctorates in Spain, and the Wenner-Gren Foundation and the American Association of Physical Anthropologists initiated The Juan Comas Award in his honor in 1969 (Faulhaber 1980).



### Notes

<sup>1</sup>This annual award is given to a senior colleague in recognition of efforts to understand and serve the needs of the world through social science. See Thomas Weaver's "The Malinowski Award and the History of Applied Anthropology" (2002b) for a brief history of this award and an overview of the recipients and their work, and Weaver's "Malinowski as Applied Anthropologist" (2002a) for an introduction to Bronislaw Malinowski and his work.

### References Cited

Aguirre Beltrán, Gonzalo

- 1974 Applied Anthropology in Mexico. *Human Organization* 33(1):1–6. (Republished at <<http://www.sfaa.net/>>.)
- 1980 El Indigenista. *In* Instituto de Investigaciones Antropológicas. *In Memoriam*; Juan Comas Camps (1900–1979). Pp. 23–27. México: Universidad Nacional Autónoma de México.

Comas, Juan

- 1942 El Problema Social de la Región Trique, Oaxaca. *América Indígena* 2(1):51–57.
- 1949 La UNESCO y los Prejuicios Raciales. *Revista Bimestre Cubana* 64:5–11. La Habana.
- 1953a Los Mitos Raciales. Paris: UNESCO.
- 1953b Ensayos Sobre Indigenismo. México: Instituto Indigenista Interamericano.
- 1957 Manual de Antropología Física. México: Fondo de Cultura Económica. (English translation published in 1960 by Thomas in Springfield, Ill.)
- 1964 La Antropología Social Aplicada en México: Trayectoria y Antología. Serie Antropología Social. México: Instituto Indigenista Interamericano.
- 1978 The International Fight Against Racism: Words and Realities. *Human Organization* 37(4):334–344. (Republished at <<http://www.sfaa.net/>>.)

Faulhaber, Johanna

- 1980 El Antropólogo Físico. *In* Instituto de Investigaciones Antropológicas. *In Memoriam*; Juan Comas Camps (1900–1979). Pp. 19–22. México: Universidad Nacional Autónoma de México.

Medina Hernández, Andrés

- 1980 Juan Comas, Como Historiador de la Ciencia. *In* Instituto de Investigaciones Antropológicas. *In Memoriam*; Juan Comas Camps (1900–1979). Pp. 29–35. México: Universidad Nacional Autónoma de México.

Pittard, Eugène

- 1959 Las Razas y la Historia: Introducción Etnológica a la Historia. Juan Comas, trans. México: Urban Tipografica Editorial Hispano Americana.

Rex, Domingo

- 1980 Juan Comas en España. *In* Instituto de Investigaciones Antropológicas. *In Memoriam*; Juan Comas Camps (1900–1979). Pp. 11–13. México: Universidad Nacional Autónoma de México.

Romano Pacheco, Arturo

- 1980 El Maestro. *In* Instituto de Investigaciones Antropológicas. *In Memoriam*; Juan Comas Camps (1900–1979). Pp. 15–18. México: Universidad Nacional Autónoma de México.

Weaver, Thomas

- 2002a Malinowski as Applied Anthropologist. *In* The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.

- 2002b The Malinowski Award and the History of Applied Anthropology. *In* The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology

## The International Fight Against Racism: Words and Realities

Juan Comas

Knowing the distinguished researchers who have preceded me in receiving the Malinowski Award (Aguirre Beltrán, Everett Hughes, Gunnar Myrdal, Edward Spicer, and Sol Tax), I feel that my designation is not justified, that my academic merits do not correspond to what should be expected from whoever receives such an honor. However, my initial ambivalence turned into intrepid acceptance when the subject of this address was suggested to me: the worldwide antiracist struggle. I consider this struggle to be vital to the future safety of mankind. Anthropologists can play a major role in the struggle to effectively eliminate racial discrimination.

Professional university training in physical anthropology is usually considered part of the sciences, whereas anthropologists who study culture are usually assigned to the field of humanities. In spite of this, those of us who are united here in Merida today recognize the impossibility of thinking, talking, or acting independently in either of these anthropological fields. Anthropology integrates into a whole. The biological man and the cultural man complement each other; they constitute a unity. It is in this context, axiomatic for us, that I will treat the question of racism and the struggle to eliminate it.

All men are born free and equal in dignity and rights. This democratic principle, universally proclaimed, is being threatened wherever the relations between groups are influenced by political, economic, social, or cultural inequalities. One of the most important obstacles to the recognition of the dignity of all human beings is racism. Racism continues to threaten the world. As a social phenomenon of great importance it requires the attention of all those who are part of the sciences of man. Racism is an obstacle to the development of its victims; it perverts those who practice it; it divides nations internally; it worsens international tensions; and it threatens world peace (UNESCO 1975).

This peace, this ecumenical atmosphere, may seem like utopia to us; nevertheless, we are obliged to do our utmost to attain this ideal. We must encourage understanding, mutual respect, and togetherness as a means to diminish the danger of war. The prejudices on which racist ideology is based, and the consequent discrimination and segregation, are major instigators of violence.

The notion of “might makes right” was, for most of history, a widely accepted justification for territorial conquest, economic exploitation, and the enslaving of some groups by other more privileged ones. But there came a time when it was necessary to look for other reasons that, with a cloak of morality and even justice, would permit the continuance of socioeconomic domination of peoples who were considered somatically and psychologically “inferior.” It was reasoned that “inferior” peoples are subject (by natural law) to the guardianship of those who designate themselves as “superior” peoples. Anthropological, biological, and psychological proofs were sought to support this thesis.

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This address was previously published in *Human Organization* 37,4(1978):334–344.

The dangerous doctrine of the “natural superiority” of Western people originated with

Gobineau in the second half of the 19th century, and really gained popularity between the end of World War I and the end of World War II (1920–1945). During this period stereotypes, prejudices, and racial discrimination were widely supported: Arianism, the “color” prejudice against Blacks, the prejudice against racial miscegenation, and anti-Semitism were all subjects of abundant, widely read literature. Against this aggressive, racist orientation in the supposedly “scientific” field, there were many anthropologists, biologists, psychologists, and geneticists. They made their objective arguments public in order to try and neutralize the pernicious socioeconomic effects on certain groups which were sanctioned by a pseudo scientific anthropological approach.

The existence of racial discrimination in favor of so-called “White people” has been repeatedly recognized and denounced by international organizations which have adopted agreements and resolutions against racism of all kinds. Consider the Latin American case.

Nowhere in Latin America are interracial relations as inhumane and discriminatory against colored groups as they are in some other regions. But it would be false to affirm, as happens frequently, that racial discrimination does not exist in Latin America. In fact, the socioeconomic inferiority, and the lack of integration of indigenous groups in Latin America are due, at least partially, to a kind of discrimination which is qualitatively and quantitatively different from that observed in other parts in the world. Discrimination is a more or less generalized practice, but it is by no means institutionalized.

In order to remedy this situation there have been several attempts to take continent-wide measures that would enable indigenous populations in particular (and all “color” groups in general) to benefit from human rights without “racial” limitations or restrictions:

(a) The *First International Teachers’ Convention* (Buenos Aires, 1918); the *International Economic Conference* (Buenos Aires, 1924); the *VIIth Panamerican Conference* (Montevideo, 1933); the *VIIth American Scientific Congress* (Mexico, 1935); the *IInd General Assembly of the Panamerican Institute for Geography and History* (Washington, 1935); the *Ist Panamerican Conference on Education* (Mexico, 1937); and the *VIIIth Panamerican Conference* (Lima, 1938) were evidence of an interest in improving the situation of indigenous populations. These congresses can be considered as the legal background for the *1st Interamerican Indigenist Congress* (Patzcuaro, Mexico, 1940), for the creation of the *Interamerican Indigenist Institute* (I.I.I.) in 1941 (based in Mexico), and for the successive *Interamerican Indigenist Congresses* that took place in Peru (1949), Bolivia (1954), Guatemala (1959), Ecuador (1964), Mexico (1968), and Brazil (1972). The *VIIIth Interamerican Indigenist Congress* is planned for next year (1979) and will take place in Panama.

The activities of these congresses (and of the I.I.I.) to integrate indigenous groups into their respective nations, are hindered by economic exploitation. This exploitation victimizes native peoples to a higher degree than it does the working classes in general; and this is due precisely to the existence of *racial* discrimination and prejudice.

(b) Resolution No. 12 of the *Ist Interamerican Demographic Congress* (Mexico, October, 1943), while absolutely rejecting all racially discriminatory policies and actions, states:

We consider all tendencies whose purpose is to encourage feelings of racial superiority to be antiscientific. Besides being against the conclusions of science, they deny the exalted principles of social justice maintained by all the American nations.

(c) The *Interamerican Conference on Problems of War and Peace* (Mexico, February, 1945), resolved

that the governments of the American republics should omit from official textbooks used in the schools everything that, directly or indirectly, supports racial theories or that could be liable to endanger the existing friendly relations among the nations of the continent (Comas 1958:130).

(d) The *American Declaration of Human Rights* was signed in Bogota, Colombia, in February, 1948, by official delegates from most of the American nations.<sup>1</sup>

(e) In the *Regional Seminar on Education in Latin America* (Caracas, August, 1948) it was resolved

that there should never be discrimination in the schools based on race, creed, class or politics; on the contrary, an authentically democratic regime should be established in them (Comas 1958:130).

(f) The *Cultural Action Committee* of the Organization of American States (1951) specified, among its objectives, the following points:

To distinguish, with honesty and exactitude, the real and legitimate differences between peoples and individuals from the traditionally repeated myths and cliches; to avoid the stereotypes and resentments that can be inculcated through the discipline of history and which constitute permanent seeds for misunderstandings and conflicts; to attack racial and social prejudices in general because they are contrary to the true understanding of historical facts and to international solidarity (Comas 1958:131).

(g) The *Institut International des Civilisations Differentes*, based in Brussels, starting from its sessions in London in 1955 and Lisbon in 1957, declared itself in favor of struggling against and eliminating all forms of discrimination, especially racial discrimination by “Whites” of “non-Whites.” They noted that this type of discrimination appears to be the most serious obstacle for peaceful coexistence between ethnic groups.

## **Legal Tools and Informative Documents Against Racism Prepared by the UN**

The first General Assembly of the United Nations took place in San Francisco in April, 1945. It seemed at that moment that the defeat of Nazism and facism meant an end to all sociopolitical movements instigating and supporting racial discrimination. The immediate consequence of the Allied victory was, in this respect, the generalized rejection of all discrimination—including, naturally, racial discrimination—and the establishment of international legal norms, the fulfillment of which would put an end to such a situation and would enforce the universality of human rights. After 1945, then, the United Nations and its specialized agencies became the nerve center for antiracist activities.

1. The International Labour Office<sup>2</sup> approached the work problems of “the American indigenous population” as a special case of economic exploitation requiring solutions beyond

those utilized for the working masses of the world in general (see ILO 1953). The special status accorded to these people resulted from blatant racial discrimination. This brought about the decision, taken at the Third Regional Work Conference (Mexico, 1946), to propose the creation of a “Committee of Experts on Social Problems of the World’s Indigenous Population.” The meetings of the *Comité de expertos en trabajo indigena* took place in La Paz (1951) and Geneva (1954). These meetings resulted in a series of recommendations which recognize the “existence of prejudices and discrimination based on origin and color” and the need to attack and eradicate such prejudices.

2. Article No. 68 of the UN *Charter* laid the foundation for the whole antiracist campaign by establishing the creation of a Human Rights Commission.

3. On December 10, 1948 the General Assembly of the UN formally subscribed to the so-called *Universal Declaration of Human Rights*. The document was ratified by all the signatory nations and has, consequently, legal character and is bound to be enforced.

Article No. 2 establishes that

Everyone is entitled to all the rights and freedoms set forth in this Declaration, without distinction of any kind, such as race, colour, sex, language, religion, political or other opinion, national or social origin, property, birth or other status.

Furthermore, no distinction shall be made on the basis of the political, jurisdictional or international status of the country or territory to which a person belongs, whether it be independent, trust, non-self-governing or under any other limitation of sovereignty (United Nations 1967:1).

4. The Economic and Social Council of the UN established (July, 1949) a “Commission for the Study of Slavery and Similar Work Forms”; this organization published the results of its investigation in a report entitled *Institution, Practices and Customs Similar to Slavery in America* (1950). This document confirms the economic exploitation that victimizes indigenous groups and which is due, at least in part, to racial discrimination.

5. The *Declaration on the Elimination of All Forms of Racial Discrimination* (Resolution No. 1904 of the General Assembly, November 20, 1963) assumed

that any doctrine of racial differentiation or superiority is scientifically false, morally condemnable, socially unjust and dangerous, and that there is no justification for racial discrimination either in theory or in practice . . . (United Nations 1967:21).

After taking this and several other premises, Article No. 1 states that

Discrimination between human beings on the grounds of race, colour or ethnic origin is an offense to human dignity, and shall be condemned as a denial of the principles of the Charter of the United Nations, as a violation of the human rights and fundamental freedoms proclaimed in the Universal Declaration of Human Rights, as an obstacle to friendly and peaceful relations among nations, and as a fact capable of disturbing peace and security among peoples (United Nations 1967:21).

6. The *International Convention on the Elimination of all Forms of Racial Discrimination* was unanimously approved by the General Assembly on December 21, 1965. This legal tool put into practice the principles established in the *Declaration* of 1963.

7. By its Resolution No. 2200 of December 16, 1966, the UN General Assembly approved the *International Pact on Civil and Political Rights* and the *International Pact on Economic, Social and Cultural Rights*. Both documents complement (and try to make effective) the *Universal Declaration of Human Rights* of 1948. Note that approval of these documents was delayed 18 years!!

8. In 1967, the UN General Assembly established March 21st as an “international day for the elimination of racial discrimination.” (The massacre of Sharpsville, South Africa, took place on March 21, 1967.)

9. The *International Seminar on Human Rights* was held in Mexico in 1968, to celebrate and contribute to the International Year of Human Rights, on the 20th anniversary of the Declaration of San Francisco.

10. The *International Conference on Human Rights*, held in Teheran in 1968, approved 1971 as the International Year for Action Against Racism and Racial Discrimination; the working program was approved by the UN General Assembly in its session of December 11, 1969, Resolution No. 2544.

11. The UN General Assembly, in its session of December 19, 1968, adopted Resolution No. 2446, entitled “Measures to achieve the prompt and total elimination of all forms of racial discrimination in general and of apartheid policies in particular.”

12. A world seminar was held in Yaounde, Cameroun, June 16–21, 1971, under the auspices of the UN Division of Human Rights on the subject “Measures that should be adopted at a national level to implement the UN resolutions designed to combat and eliminate racial discrimination and to promote harmonious racial relations.”

We turn now to the activities of UNESCO as an organization specializing in the area of racism.

The plan laid down by the Organization proceeds from a resolution [116 (VI) B (iii)] adopted by the United Nations Economic and Social Council at its sixth session, asking UNESCO “to consider the desirability of initiating and recommending the general adoption of a program of disseminating scientific facts designed to remove what is generally known as racial prejudice” (UNESCO 1961: 493–494). At its fourth session, UNESCO responded to this request by adopting three resolutions for the 1950 program:

The Director General is instructed: To study and collect scientific materials concerning questions of race; To give wide diffusion to the scientific information collected; To prepare an educational campaign based on this information (UNESCO 1961:494).

In order to accomplish the first point, a meeting of eight experts (anthropologists, sociologists, and psychologists) was held in Paris, December 12–24, 1949; they prepared a report, *Declaration on Race*, published on July 18, 1950; it synthesized in its 15 points the biological and cultural concepts considered essential to differentiate human races and to combat prejudice (UNESCO 1961:496–501).

Because of certain criticisms and because it was advisable to have a larger number of

specialists preparing such an important scientific document, UNESCO convened a larger meeting which was held in Paris, June 4–9, 1951; 14 biologists, anthropologists, physicists, and geneticists attended the meeting and wrote a new *Declaration on Race and Racial Differences*, published in 1952 (see Ashley-Montagu 1951).

This document was accepted by the great majority of scientists (biologists, geneticists, physical anthropologists) interested in and knowledgeable about the problem, without any refutations requiring fundamental modification of any of its nine points (UNESCO 1953).

Twelve years later, UNESCO called together another group of specialists (particularly biologists and geneticists) to revise the text of the 1952 Declaration. The meeting took place in Moscow, August 12–18, 1964, and 22 experts participated in it; the text of the new Declaration is composed of 13 points which—with no fundamental changes—improve and describe more objectively some of the concepts relating directly to the definition of human race.

In order to complement the strictly biological tone of the 1964 report, UNESCO convened another meeting in Paris, September 18–26, 1967, in which 18 social scientists participated; they prepared the text for a *Declaration on Race and Racial Prejudice*. UNESCO has widely diffused both of these informative documents (1964 and 1967) in several languages.

As part of its activities to combat racial discrimination, UNESCO's Department of Social Sciences solicited a number of short monographs from different authors to be compiled under the general title *The Racial Question in Modern Science*; they were to be written with the general public in mind, in lay language, and the purpose was to make known several scientific points directly related to the racial question.<sup>3</sup> Since 1951, 11 pamphlets have been published in this program. UNESCO also initiated the publication of two more monograph series on the racial problem, entitled *Race and Society* and *The Racial Question and Modern Thought*.

Several editions of the pamphlets included in the three series have been published since 1952 in various countries, languages, and presses, sometimes with the pamphlets of each series compiled into one volume. This is not the place to give more bibliographical details; let it simply be said that these are easily accessible publications. Later on we'll see whether they really achieved the objectives for which they were projected.

In order to fulfill Resolution No. 3.62 of the UNESCO program for 1957–1958, the Department of Social Sciences started to compile and diffuse a “document on interracial relations” in different geographical regions. This resulted in the publication of four contributions dealing with the problem in British East Africa, the German Federal Republic, Great Britain, and the United States, published in the *Bulletin International des Sciences Sociales* (Vol. 10, No. 3, Paris, 1958). Moreover, it was announced that similar studies were going to be published about the problem in South Africa, southeast and southern Asia, and Oceania. The corresponding study on interracial relations in Latin America appeared in 1961 (Comas 1961a). All these studies on social conduct among different population groups belonging to various “races” reached the same conclusion: that general prejudice and racial discrimination exist in all areas of the world where it was studied, with only qualitative or quantitative variations.

In order to comply with the third point of its working program set in 1950, UNESCO convened and sponsored a Commission of anthropologists and educators. They met in Paris, September 19–24, 1955, “to study and propose more adequate educational measures to combat prejudice and racial discrimination.”

That meeting resulted in a series of suggestions that, to the best of my knowledge, were never put into practice—with the exception of Cyril Bibby's book (1959) which was used in the schools in Great Britain as educational material to eliminate racial prejudice and discrimination.



Personally, and also under UNESCO sponsorship, I prepared a short essay suggesting ways in which the school system in Mexico could eliminate from children's minds all concepts of racial discrimination (Comas 1958).

In addition to the above (incomplete, of course) inventory of accomplishments at the international level, I must also name a series of 14 nongovernmental organizations that work specifically to combat racial discrimination: The International Committee on Science and Freedom (Manchester); Mouvement contre le racisme, l'antisemitisme et pour la paix (Paris); World Jewish Congress (London); Institute of Race Relations (London); International Academy of Human Rights (Zurich); International Society for the Scientific Study of Race Relations (Boston); National Association for the Advancement of Colored People (USA). I don't have concrete information relating to the activities of each of these organizations.

### **Results of the Antiracist Struggle in the Period 1950–1977**

If reality were in consonance with adopted legal norms and with the attempts to make them effective, the world right now would be free of racism. Unfortunately, this is not the case; the conflict between the "racially discriminated" groups and the politically and economically more powerful sectors of the population becomes more and more acute each day.

This situation is an imminent threat to the peace of individual nations, because many internal struggles may end up in civil war; it is also a threat to international peace because racial discrimination is an excellent smoke screen which nations may use to camouflage their true intentions of economic, political, and territorial conquest. Racial discrimination exists in almost all countries, to a greater or lesser extent, and in more or less disguised manifestations. Latin America, for example, is considered one of the world regions where there is no prejudice against Jews, and no "color" prejudice against Blacks, Mulattos, or Indians. Yet, a detailed analysis shows that discrimination is an active social phenomenon on this continent, though perhaps less virulent than in other regions of the world.

On the other hand, repudiation and segregation of the Whites by the Black populations of Africa is gaining ground each day. Due to racism, misunderstanding among human beings is reaching unsuspected and dangerous proportions—and nothing effective is being done about it.

The lack of positive outcomes from the struggle against racial discrimination is the result of several factors.

First, political leaders, both national and international, have failed to enforce the numerous resolutions against racial discrimination and segregation. In my opinion, the incumbent political organizations have *written* more "literature" than *done* any effective work: Declarations, Recommendations, Conventions, Suggestions . . . , but very little effective action. It is not my duty here to discuss the possibility or impossibility of imposing penalties for failure to fulfill international agreements. Even in those cases where compliance has been attempted, however, there have been obstacles which have minimized the end results.

For example, in 1949 the commission of experts named by UNESCO decided to publish short, heavily illustrated pamphlets that would reveal to the discriminated peoples the reasons for their condition, on the theory that this would help to eliminate it; the suggestion was to publish the pamphlets in the native languages of the populations most affected by segregation; moreover, there were to be enough copies (thousands or hundreds of thousands) to reach all. By the time the pamphlets appeared, they were published in several Western languages, and their content and size were no longer suitable for the masses, but for a minority of readers. It is possible that the

pamphlets in their present form have been useful, but their main purpose was to make the problem known to those most affected by it. Basically, this population does not consist of those who read French, English, Spanish, German, Italian, or Russian. I do not know who subverted the suggestions of the experts, but I believe it can be safely assumed that one or several of the government delegates to UNESCO thought that it would be inconvenient if propaganda against racial discrimination should reach their countries. They must have seen that such propaganda could have had repercussions inimical to the interests of those anxious to maintain political and economic power over natives and Blacks.

Next, consider again the fact that 18 years went by between the Universal Declaration of Human Rights (1948) and the approval in 1966 of the two international agreements which were the legal tools to enforce the 1948 document. Indolence? Forgetfulness? Or conscious and pre-meditated inaction to blunt the struggle against racial discrimination?

Finally, I recall an incident which took place at the World Seminar in Yaounde in 1971. We had obtained the collaboration of some experts to prepare documentation and background papers. The substantive part of those papers was accompanied by criticisms and suggestions on how to approach the question. The authors (i.e., the experts) were refused entry to the seminar; it seems that only diplomats from the member nations went to Yaounde and, of course, a team of functionaries from the world organization. The conclusions reached in Yaounde aside, does it make any sense to eliminate the participation of specialists who were invited to collaborate and whose papers were accepted as a basis for discussion in the seminar? Assuming that the diplomats and UN functionaries could have been well prepared on the matter under discussion, there's still the question of whether the presence of the experts would not have shed some light and would not have made more effective the results of such a meeting.

In some cases, expert testimony has been selected or ignored, with the result that discrepancies and even contradictions are found in official documents prepared by the UN and its specialized agencies. This tends to diminish the effectiveness of these documents and even provokes reactions contrary to the objective of fighting discrimination; for example, in the bulletin entitled *The Main Types and Causes of Discrimination* published by the UN, it says

Therefore, it is not possible to speak of races as distinct human groups, but only of differential characteristics as to the enumerated traits (United Nations 1949:17).

The assertion that it is impossible to talk about races as human groups represents the opinion of only a small number of anthropologists; the great majority accepts the concept of "human race," *when applied to populations and not to individuals*.

The same goes for paragraph 61 of the above mentioned document:

History, as well as actual observations and experimental psychology, also shows that it is not possible to affirm that mental superiority of any kind can be ascribed to any particular racial characteristics. The differences as to cultural achievements do not depend on racial characteristics but on historical factors, conditions and environment (United Nations 1949:17).

Barring an error in the interpretation, it seems to me that the above paragraph is confusing. It should state that *even though there is no intellectual superiority or inferiority of one human group (race) over another, such differences do, indeed, exist between individuals*,

*whatever their “race” might be*; and these differences are due as much to characteristics inherited genetically from one’s parents as they are to environmental factors and influences affecting the individual.

Bibby’s work (1959), prepared under the auspices of the UNESCO, was to serve as a guide for school teachers to use in antiprejudice and antidiscrimination campaigns. In February, 1961, UNESCO asked Professor Michael Banton (Department of Sociology, Bristol University) to do a pilot survey to determine how Bibby’s book might be utilized to eliminate racial prejudices. Survey results show some of the obstacles which—at least in Great Britain—seriously hinder attempts to struggle against racial prejudices. Banton points out that children’s knowledge and prejudices about other peoples stem, among other, from three sources: “mass media, the community in which the pupils live, and the teacher” (1962:738). The different types of media (i.e., movies, radio, television) seem to help “give children a more favourable image of other peoples” (Banton 1962:738). But, with respect to the influence of the local environment and the school, and referring to the teacher, the author says:

Even if he can keep the children tolerant in class and give them good reasons for remaining tolerant, a considerable proportion will learn prejudice as soon as they leave school. As long as there are acute tensions in the community no method of instruction can “eliminate race prejudice in the schools” to any worthwhile extent (1962:740).

Banton also points out that some teachers deem it inconvenient to deal with this subject at school because: (a) they consider that most of the available documentation refers to “matters of opinion rather than matters of fact” (1962:740); (b) race “as a biological phenomenon [is] only incidental to intergroup relations and that to emphasize the racial factor [is] often to create a problem where none existed before” (1962:740).

Actually, it is true that racial problems arise only from the moment in which individuals start being aware of the differences between groups. Banton says that “when pupils develop their contacts with the outside world, they almost invariably become ‘conscious’ of race as a socially significant attribute; to neglect it in school may make later adjustment the more difficult” (1962:741). By avoiding action, the school does not prevent racist attitudes from developing.

The psycho-biological differences between “races” and their supposed “correct” interpretation are offered as support for discrimination by racists. They group “racial” differences into three categories: anatomical, physiological, and psychological. Today, it is clear, even for laymen, that anatomical and physiological differences between races do not presuppose a qualitative hierarchy; nor do they justify the discrimination of one group by another. As a consequence, racists today focus their efforts on “intelligence” and “basic personality.”

Committees, associations and well-known scientific personalities in the fields of human biology, genetics, and physical anthropology have declared their stand on this. The *Declaration of Race* of 1964 categorically states that

Neither in the field of hereditary potentialities concerning the overall intelligence and the capacity for cultural development, nor in that of physical traits, is there any justification for the concept of ‘inferior’ and ‘superior’ races (UNESCO 1975:358).

The American Anthropological Association agreed in 1961 that it

repudiates statements now appearing in the United States that Negroes are biologically, and in innate mental ability, inferior to whites and reaffirms the fact that there is no scientifically established evidence to justify the exclusion of any race from the rights guaranteed by the Constitution of the United States (AAA 1961:1).

The American Association of Physical Anthropologists has also expressed itself; its last declaration says the following:

We, the members of the American Association of Physical Anthropologists professionally concerned with differences in man, deplore the misuse of science to advocate racism. We condemn such writings as “Race and Reason” that urge the denial of basic rights to human beings . . . and we affirm as we have in the past that there is nothing in science that justifies the denial of opportunities or rights to any group by virtue of race (AAPA 1963:402).

It would seem that these declarations were sufficiently explicit to nullify the biological arguments of racist propaganda. But the opposite is true; advocates of pseudoscientific racism, anxious to find support for their unjust social, political, and economic attitudes towards colored peoples, persist in interpreting facts erroneously. For example, we must reject one of the most deceitful of the racists’ arguments in order to avoid the confusion which they intentionally create. Namely, racists attribute constantly the concept of racial “egalitarianism” to those of us who fight against their discriminatory ideas. In fact, the struggle is for equality of rights, duties, possibilities, and opportunities for all men without distinction of “race” or “color.” But this social and moral egalitarianism bears no relation to a nonexistent biological egalitarianism. Men are *not* biologically equal; races are *not* biologically equal. But such differences do not mean (as they do to racists) that one group is superior while another is inferior. Granted, each human being, individually and independently of his race, possesses certain physical and mental characteristics which make him or her more or less competent, or more or less gifted for certain types of activities than other individuals. But this reality must not and cannot be taken as evidence for the false generalization that *all* those who comprise a race have “superior” or “inferior” characteristics with respect to another race.

In the 1960s H. E. Garret (1961), W. C. George (1962), and C. Putnam (1961, 1963, 1967) were the paladins of a racism which was camouflaged by pseudoscientific arguments. Numerous biologists, anthropologists, geneticists, and psychologists supported the antiracist thesis with serious arguments (Comas 1961b). But the theoretical racists have not abandoned their doctrinaire activities. They persist in falsifying information—consciously or from ignorance—and continue to support the practice of discrimination on the basis of “race” and “color.”

There have been several carefully conducted studies that show how fallacious the racist arguments are. Dr. Phillip V. Tobias, professor of anatomy at the University of Witwatersrand, recently carried out a comparative study on brain size and grey matter in Whites and Blacks; he says

From my little venture into the study of the brain, I have emerged with the conviction that vast claims have been based on insubstantial evidence. I conclude that there is no acceptable evidence for such structural differences in the brains of these two racial groups; and certainly nothing which provides a satisfactory anatomical basis for explaining any difference in I.Q. or in other mental and performance tests, in temperament or in behaviour (1970:22).

Other testimony comes from the conclusions of the study on intelligence and race (1970) authored by two professors of genetics, Drs. W. F. Bodmer (Oxford University) and Luigi L. Cavalli-Sforza (Pavia University). After defining the terms “heredity,” “intelligence,” and “race” they say that “the relative contributions of biological and cultural factors to complex characteristics such as behavioral differences, including those that distinguish one race from another, are exceedingly difficult to identify” (Bodmer and Cavalli-Sforza 1970:20). They conclude by saying

Surely innate differences in ability and other individual variations should be taken into account by our educational system. These differences must, however, be judged on the basis of the individual and not on the basis of race. To maintain otherwise indicates an inability to distinguish differences among individuals from differences among populations (1970:29).

One of the issues in the bio-psychological field that provoked much controversy was presented by Jensen (1969), who affirmed that intelligence is a basically innate factor; that a clear difference exists in the IQ of White and Black groups, in favor of the Whites. Literature for and against this thesis has proliferated between geneticists and environmentalists, without any signs of rapprochement of the two extreme positions. What is intelligence? What gauge or measure is used in tests? Of what significance is the IQ? These and other questions are passionately discussed and each group affirms that their data and analyses are objective. Jensen’s work (1975) is a reiteration with new data of his initial hypothesis. The following paragraph synthesizes his point of view:

In view of the failure of numerous environmental hypotheses to be borne out by evidence, the genetic hypothesis appears reasonable and highly likely, which is not to say that it is proven. But at least it is already established that genetic factors are the most important determinant of the IQ differences *within* the racial groups, and, in the absence of any compelling environmental explanation for the White-Negro intelligence difference, we would be scientifically remiss not to seriously consider the genetic hypothesis (Jensen 1975:106).

Other researchers presented well-reasoned arguments refuting this proposition. For example, Jensen’s experience based on monozygotic twins (1969) prompts him to affirm that differences in IQ among “White” individuals is due 80% to hereditary factors, and 20% to environmental action, “with negligible interaction.” Another one of Jensen’s phrases (Ebling 1975:124) is even more startling: “There are genetic differences between classes within each racial group.” This is not a quote out of context but rather a clear affirmation. We could ask at this point “What occurs to this deficient hereditary potential when the subgroup changes its

social class? Or is the immobility of classes now taken for granted?" A more ample view of the problem and its different approaches may be found in the publications of Brace et al. (1971), Kamin (1973, 1974), and Tobias (1970, 1974).

Decades may pass before the results of adequate experimentation in this complex biopsychological field are available, so that the various roles of intelligence, IQ, mental tests, and the inheritance-environmental interplay may be ascertained.

A final example of the activities of those who support racist doctrines is the creation in Edinburgh, Great Britain, in 1960 of the racist publication entitled *The Mankind Quarterly*; it continues to be published. It would be interesting to know the sources for the funds required to maintain and distribute it over a period of almost two decades.

In addition to the lack of action by the UN, and the pseudoscientific work of racists, we also must contend with mass propaganda and activities of racist organizations. The North American Nazi party, for example, published a manifesto in the Mexican press on December 3, 1977 which says:

We call for a White war against Jews and other non-Whites, and offer 5,000 dollars for each non- White killed in the struggle.

There have been confrontations in Chicago between Nazis and Jews.

Last November there was an anti-Semitic simulacrum in Hamburg, and another attack in Hannover.

A wave of discriminatory violence is being mounted against the aborigines in Australia.

Dr. Turner, a South African antifascist, was murdered (January, 1978).

Discriminatory measures based on color are being taken London schools and the Ku-Klux-Klan has been established in England.

Anti-Black attacks continue in Rhodesia, South Africa, and the United States (Ku-Klux-Klan, the Nazis, etc.).

The South African government has classified the population into four groups (Cuadra 1974:35): Whites (3,563,000), Bantus (12,750,000), Asians (561,000), and "coloreds" (1,859,000). Human rights and services—housing, health services, education, work, recreation, etc.—are all allotted on the basis of this scheme. And the electoral system is governed exclusively by the 3.5 million "Whites."

There have been modest, nongovernmental, attempts to counteract the danger: the World Council of Churches has tried to establish a special fund for the struggle against fascism, that is against racial discrimination; and the International Conference of Indigenous Peoples was held recently in Geneva. Its purpose was to let the voices and demands of native peoples be heard, to make effective the Universal Charter of Human Rights, and to liberate them from the tutelage and paternalism which protects illegitimate economic and political interests.

### **Suggestions for Future Antiracist Activities**

It is clear that the results of a systematic, organized, and scientifically based struggle against racial discrimination have been minimal; there has been a lot of talk, writing, and legislation, but they have been speculative and ineffective.

In my view the struggle against racism has to be started in the two following ways.

A. Short-Term Action. First the UN, through all available means, should make the governments of member states fulfill the legal agreements that they have signed and ratified since 1945. It seems useless to continue to multiply legal agreements when those in existence are not fulfilled.

A conference at the highest political level, held in Helsinki in 1975, offers an example of how international agreements contradict real life in the matter of human rights: principle 7 of the *Final Act*, subscribed to by representatives of 35 countries, explicitly mentions “the respect of human rights and fundamental liberties, including freedom of thought, conscience, religion or creed.”

A second conference (Belgrade, 1977) was planned to verify the extent to which the Agreements subscribed to in 1975 were being fulfilled. There were confrontations and aggressive accusations between two groups of government representatives, who reproached each other for racist and discriminatory acts committed in their respective territories. It got to the point where one of the so-called super-powers stated that trying to analyze the problem of human rights meant the “interference in the internal affairs of another country.” This is in flagrant violation of the solemn Agreements of Helsinki.

I offer the opinion of Rene Cassin, Nobel Peace Prize laureate and President of the International Institute for Human Rights, as a plausible explanation for such an attitude in this and other similar cases throughout the UN’s history; he attributes it to the fact that “the Member States don’t want to share their power to rule over their citizens’ rights” (Cassin 1974:405). This interpretation seems proper; extreme chauvinism is alien to the international spirit of collaboration, which is the reason for the UN’s existence.

I think that the monographs published by UNESCO in the series *Racism and Modern Science*, referred to before, as well as the pamphlets *Can You Name Them?* (American Committee for Democracy 1943), *The Human Races* (Benedict and Weltfish 1943), and *What Is Race?* (UNESCO 1952) should be brought up to date so they can serve as educational guides. I also believe that cycles of popular conferences, films, and newspaper articles similar in orientation to the ones published by the “UNESCO Courier” could also be vehicles for the kind of public education that would eventually abolish racial prejudice.

The peoples against whom discrimination is practiced are often illiterate or speak exotic languages which are not the official national language of the country in which they are citizens. For them, a different approach has to be taken. They must be made to understand the reasons for their subordinate situation, and know that there are “Whites” who are willing to fight with all available legal means to end discrimination. Here, more than in the former case, there is a need for the collaboration of experienced educators who can work with biologists and anthropologists to prepare short, clearly illustrated pamphlets, written in the native language of the group for which they are designed. Published by the hundreds of thousands, they must be made available to all interested persons, and serve as basic material for simple visual explanations that the teacher, school aide, or social worker must offer periodically.

The hardest group to change is the adult middle class. It is among them that prejudices are most deeply rooted. Furthermore, they have the most to lose if the status quo (exploitation of “inferior races”) were to be modified. I recognize the skepticism with which proposals to change this class’s attitudes will be met. Nevertheless, conferences should be organized and lectures and round-table discussions should be developed to challenge racist arguments.

The most productive arena for the antiracist struggle is the schools. The starting point for

such activity is the prior acceptance of certain postulates by the teachers: they must recognize that pernicious ideas and acts in the past and in the present have caused the slavery and exploitation of Blacks and Indians, and the genocide of the Jews; they must reject the principles of discrimination because they are convinced that all racial prejudices lack scientific basis; they must enthusiastically accept active participation in the attempt to free their students from these types of attitudes.

Schools could also combat racial prejudice and discrimination by other means: (1) establishing racially balanced classes; (2) hiring teachers who belong to minority groups; (3) putting social pressure on private schools to admit children from all races and to create scholarships for poor children; (4) campaigning so that the government will give equal educational facilities to communities of different racial origin; (5) fighting for the fulfillment of all legislative measures, both national and international, that proclaim equal opportunities for all persons, irrespective of the racial group they belong to.

Racial prejudices among children can be caused by many things. It is the task of the teacher to learn the personality of each child; in this way he or she can determine the deep origins of prejudices and fight against them. Teachers can establish friendly relations with the parents in order to better understand the family environment; they can try to learn from the children themselves about their extra-curricular activities, their preferences and aversions; they can give children psychological tests in collaboration with psychologists.

It is not necessary to burden school programs with special classes or courses on the racial problem; and in no case should there be “lessons” prepared on the issue, because this is not effective to combat deeply rooted attitudes; it is better to use constant and patient repetition and demonstration whenever the occasion arises and whatever the subject matter being treated in class. Each time the opportunity arises in a history, geography, biology, or ethics class the teacher should contribute something to “immunize” students against racial prejudice, explaining its origins. The works of Allport (1954) and Anzola (1949) are excellent orientation for teachers.

Native American, Black, or Mulatto teachers should head classes in White regions for a couple of months, and vice versa; the students would start to lose their color prejudice, seeing how these teachers are considered equal by their White colleagues.

**B. Long-Term Action.** In the struggle against racial discrimination it is indispensable to be able to determine, definitely and indisputably, the role played by hereditary and environmental factors in an individual’s life. This remains the most controversial question, because we lack controlled experiments, both on the genetic and on the environmental aspects of the problem.

For this reason, I suggest the creation of a boarding-school which, from birth on, would take orphans from different racial groups: Whites, Mongolians, Blacks, Native Americans, Mulattos, and Mestizos; there shouldn’t be less than 15 in each group.

A life shared from the beginning and under identical environmental conditions (physical, economic, familial, educational, cultural, hygienic, sanitary, etc.), with the same opportunities, would show that the differences with respect to aptitudes and final preparation for life at the end of the experiment (which should last 18 or 20 years) were due exclusively to heredity. The variations among the subjects under study would show whether there are really psychological characteristics and aptitudes common to all the individuals of a racial group, or whether the case is—as I believe—one of individual variation inimical to racist ideology.

The careful planning and execution of this project should be in the hands of a



commission of a small number of experienced scientists from different fields. They should be fully conscious of the great responsibility of their mission and, more importantly, they should be convinced of the import of the experiment in terms of the antiracist struggle.

I have made such a proposal on several occasions (Comas 1961b, 1964, 1972), but it has had no impact, much less acceptance, even in theory. Allport (1954) outlined the same idea, though more limited in scope; he referred to a possible educational experiment with White and Mongolian children. It is easy for any government to spend millions of dollars in arms; it is difficult—if not impossible—to convince politicians to invest a few hundreds of thousands in long-term human experiments.

The world has been living for years with the deadly threat, greater each day, of environmental contamination which, if not stopped, can end in the biological destruction of the human species. Side-by-side with the smog there is an even greater danger threatening us in the psychological arena: the contamination of racial prejudice, of discrimination; it will lead us inexorably to aggression, war, and destruction. The history of this century is evidence for this prediction.

I extend my most sincere wishes that anthropologists will collaborate in the struggle for coexistence and peace, bringing an end to the dangerous stereotype of bio-psychological “superiority” of some populations, and the “inferiority” of others.

### Notes

<sup>1</sup> This document preceded by several months the Universal Declaration of Human Rights prepared by the United Nations and proclaimed in San Francisco on December 10, 1948.

<sup>2</sup> Before the establishment of the United Nations the International Labour Organization was a specialized agency of the Society of Nations based in Geneva.

<sup>3</sup> In alphabetical order, the authors are: Comas, Dunn, Jahoda, Klineberg, Leiris, Levi-Strauss, Little, Morant, Arnold Rose, and H. L. Shapiro (UNESCO: 1961).

### References Cited

- Allport, Gordon W.  
1954    The Nature of Prejudice. Cambridge, Mass.: Addison-Wesley.
- American Committee for Democracy  
1943    Can You Name Them? New York: American Committee for Democracy and Intellectual Freedom.
- American Anthropological Association  
1961    Fellows Newsletter 2(10):1.
- American Association of Physical Anthropologists  
1963    American Journal of Physical Anthropology 21:402.
- Anzola Gómez, Gabriel  
1949    La educación contra los prejuicios y las discriminaciones. *In* Educación para la Paz. Washington, D.C.: OEA, Departamento de Asuntos Culturales.
- Ashley-Montagu, M. F.  
1951    Statement on Race. An Extended Discussion in Plain Language of the Unesco Statement by Experts on Race Problems. New York: Henry Shuman.

- Banton, Michael  
1962 Teaching Race Questions in British Schools. *International Social Science Journal* 14(4):732–742.
- Benedict, Ruth, and Gene Weltfish  
1943 Las razas humanas. Havana: Editorial Páginas.
- Bibby, Cyril  
1959 Race, Prejudice, and Education. London: Heinemano.
- Bodmer, Walter F., and Luigi Luca Cavalli-Sforza  
1970 Intelligence and Race. *Scientific American* 223(4):19–29.
- Brace, C. L., G. R. Gamble, and J. T. Bond, eds.  
1971 Race and Intelligence. Washington, D.C.: American Anthropological Association.
- Cassin, René  
1974 Protección nacional e internacional a los Derechos Humanos. *In Veinte Años de Evolución de los Derechos Humanos. Proceedings of the International Seminar on Human Rights, Mexico, 1968.* Mexico: Universidad Nacional Autónoma, Instituto de Investigaciones Jurídicas.
- Comas, Juan  
1958 La educación ante la discriminación racial. Suplemento del Seminario de Problemas Científicos y Filosóficos, No. 5. México: Universidad Nacional Autónoma.  
1961a Interracial Relations in Latin America: 1940–1960. *International Social Science Journal* 13:271–299.  
1961b Scientific Racism Again? *Current Anthropology* 2: 303–340; 3:284–302.  
1964 Combatir el racismo es defender la paz. *Cuadernos Americanos* 133:44–60.  
1972 Los males de la discriminación racial. *Cuadernos Amencanos* 182:67–93.
- Cuadra, Héctor  
1974 El apartheid y los instrumentos internacionales sobre derechos humanos. *In Veinte Años de Evolución de los Derechos Humanos. Proceedings of the International Seminar on Human Rights, Mexico, 1968.* Mexico: Universidad Nacional Autónoma, Instituto de Investigaciones Jurídicas.
- Ebling, F. John, ed.  
1975 Racial Variation in Man. Proceedings of a Symposium held at the Royal Geographical Society. London: The Institute of Biology.
- Garret, H. E.  
1961 The Equalitarian Dogma. *The Mankind Quarterly* 1(4):253–257.
- George, Wesley C.  
1962 The Biology of the Race Problem. Report commissioned by the Governor of Alabama.
- International Labour Office  
1953 Indigenous Peoples. Living and Working Conditions of Aboriginal Populations in Independent Countries. Geneva: ILO.
- Jensen, Arthur  
1969 How Much Can We Boost IQ and Scholastic Achievement? *Harvard Educational Review* 39(1):1–123.  
1975 Race and Mental Ability. *In Racial Variation in Man.* F. J. Ebling, ed. London: The Institute of Biology.
- Kamin, L. J.

- 1973 Heredity, Intelligence, Politics and Psychology. Paper presented at the 13th Congress of Genetics, Berkeley, California.
- 1974 Science and Politics of IQ. New York: John Wiley & Sons.
- Putnam, Carlton
- 1961 Race and Reason. A Yankee View. Washington, D.C.
- 1963 Three New Letters on Science and Race. New York: National Putnam Letters Committee.
- 1967 Race and Reality: A Search for Solutions. Washington, D.C.: Public Affairs Press.
- Tobias, Phillip V.
- 1970 Brain Size, Grey Matter, and Race. Fact or Fiction? American Journal of Physical Anthropology 32:3–25.
- 1974 IQ and the Nature-Nurture Controversy. Journal of Behavioural Science 2:5–24.
- United Nations
- 1949 The Main Types and Causes of Discrimination. Memorandum Submitted by the Secretary General. Lake Success, N.Y.: UN Commission on Human Rights.
- 1967 Human Rights. A Compilation of International Instruments of the United Nations. New York: UN.
- UNESCO
- 1952 What Is Race? Evidence from Scientists. Paris: UNESCO, Department of Mass Communications.
- 1953 Le concept de race. Resultats d'une enquête. Paris: UNESCO.
- 1961 Race and Science. The Race Question in Modern Science. New York: Columbia University Press.
- 1975 Race, Science, and Society. New York: Columbia University Press.

## **Chapter 9**

### **Laura Thompson: Culture, Personality, and Applied Anthropology**

**Thomas Weaver**

Laura Thompson, recipient of the Society for Applied Anthropology's Malinowski Award in 1979, met Malinowski only once, but like many other anthropologists, she was influenced by his emphasis on the value of studying social issues in relation to the changing native.<sup>1</sup> One of the few recipients of the Malinowski Award who worked as applied anthropologists for most of their careers, she held temporary academic posts at a dozen institutions. She considered case study accumulation and interdisciplinary research the basis for applied anthropology, reiterating a theme promoted by Edward Spicer, another Malinowski Award recipient (1976), in his edited text (1952). Holism, the emic approach, inferential or intuitive prediction, and cooperation and non-interference with the policy makers in a client-centered approach are elements in her conceptualization of application (Thompson 1979:117–118).

Thompson (1905–1999) was born in Honolulu and received a bachelor's degree in 1927 from Mills College in Oakland, California. She worked at the Bishop Museum examining archeological artifacts before her doctoral work. She received a Ph.D. from the University of California, Berkeley, in 1933 for a dissertation entitled "Trade in Southeastern New Guinea." She did postgraduate work at the University of Chicago in 1941. Immediately before the Second World War she was hired as a social scientist on Guam for the U.S. Navy. From 1941 to 1947 she held the position of coordinator of Indian education in the Bureau of Indian Affairs, during which time she worked on the pioneering Indian personality development project. She was a research consultant for the Institute of Ethnic Affairs in Washington, D.C. from 1946 to 1954 and for the Bureau of Indian Affairs and other agencies in the U.S. and Mexico for many years. She held teaching positions at the City University of New York, the University of North Carolina, North Carolina State University, University of Southern Illinois, and the University of California at San Francisco. While at North Carolina State she recorded half-hour television shows that became the basis for a general text on community studies (1969).

Thompson's most important publications include her book on Fiji, for which Malinowski wrote an introduction (1940). Other significant books include those on Guam (1947) and the Hopi (1945, 1950). She contributed more than one hundred articles to scientific journals. Although she worked primarily in the Pacific, much of what Thompson is known for is the decade or more spent with the Bureau of Indian Affairs, and for the studies of personality and change conducted among such groups as the Hopi and Navajo. Her 1945 book on the Hopi,

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written with Alice Joseph (a physician), was a product of the project on Indian Education, Personality, and Administration conducted under auspices of the University of Chicago and the Bureau of Indian Affairs. Anthropologists, psychologists, and psychiatrists studied personality development among the Navajo, Papago, Sioux, and Zuni, in addition to the Hopi, producing monographs on each group. The spouses of two other Malinowski Awardees, Alexander Leighton (1984) and Edward Spicer (1976), were involved in this project; Dorothea Leighton, a psychiatrist, worked with Clyde Kluckhohn in the Navajo study (Leighton and Kluckhohn 1948) and Rosamond Spicer, an anthropologist, with the Papago research with Alice Joseph (Joseph et al. 1949). Anthropologist Edward Dozier, a Pueblo Indian expert, hailed the Thompson and Joseph (1945) volume as an “excellent synthesis and analysis and superb account of Hopi socialization techniques, and Hopi social and ceremonial organization” (Dozier 1970:224).

Besides fieldwork on Fiji, Guam, and with the Hopi, Navajo, Papago, Zuni, and Sioux, Thompson conducted varying periods of research in China, Japan, Germany, and Iceland and with Andeans. Her 1969 book, *The Secret of Culture: Nine Community Studies*, was based on her first-hand experience with these groups. The book is an excellent summary of indigenous people, a term she used to refer to Icelandic and Germanic groups as well as to Native American and Pacific Island peoples. Using an adaptive definition of culture, she summarized environmental, geographical, archeological, social, and linguistic data on nine groups. With some updating, this book would still be an excellent reader for introductory courses.

Thompson’s Malinowski Award acceptance speech is entitled “The Challenge of Applied Anthropology” (1979). Her approach to applied science, shared by many then and now, might be dubbed “ethnographic generalizations in the service of administrators and policy makers.” Whether working with colonial administrators or with a client community in the postcolonial era after World War II, the applied anthropologist, Thompson noted, is often called upon to make predictions about future behavior and probable outcomes of alternative actions. Despite the doubts of some anthropologists, Thompson maintained that there are many examples of accurate predictions in anthropology. She cited Ruth Benedict’s *Chrysanthemum and the Sword* (1946) as an example of successful prediction and Benedict’s guidelines as a model for this approach:

[S]aturate yourself in the materials on a given culture, reading everything you can lay your hands on and interviewing as diverse a group of informants as possible; formulate some tentative hypotheses that seem to arise out of the data; then test these hypotheses by seeing whether you can predict what will come in new material obtained by reading, observation and interview; if prediction fails or is incomplete, reject or re-phrase the hypothesis and test the revision on still further material (Thompson 1979:116, quoting Kluckhohn 1949:18–19).

Laura Thompson was honored by her peers with the Bernice P. Bishop fellowship at Yale University in 1933. Interestingly, this was the same fellowship received by Malinowski only six years later. Thompson was also awarded the Rosenwald Fellowship at Hawaii and a distinguished professorship at Pennsylvania State University in 1961. She was one of the founders of the Society for Applied Anthropology and was the first woman to receive the Malinowski Award.

### Notes

<sup>1</sup>This annual award is given to a senior colleague in recognition of efforts to understand and serve the needs of the world through social science. See Thomas Weaver's "The Malinowski Award and the History of Applied Anthropology" (2002b) for a brief history of this award and an overview of the recipients and their work, and Weaver's "Malinowski as Applied Anthropologist" (2002a) for an introduction to Bronislaw Malinowski and his work.

### References Cited

- Benedict, Ruth  
1946     *The Chrysanthemum and the Sword: Patterns of Japanese Culture*. Boston: Houghton Mifflin.
- Dozier, Edward  
1970     *The Pueblo Indians of North America*. New York: Holt, Rinehart and Winston, Inc.
- Joseph, Alice, Rosamond Spicer, and Jane Chesky  
1949     *The Desert People, a Study of the Papago Indians of Southern Arizona*. Chicago: University of Chicago Press.
- Kluckhohn, Clyde  
1949     Ruth F. Benedict: A Memorial. New York: The Viking Fund.
- Leighton, Alexander  
1984     Then and Now: Some Notes on the Interaction of Person and Social Environment. *Human Organization* 43(3):189–197. (Republished at <<http://www.sfaa.net/>>.)
- Leighton, Dorothea, and Clyde Kluckhohn  
1948     *Children of the People: The Navajo Individual and his Development*. Cambridge: Harvard University Press.
- Spicer, Edward  
1976     Beyond Analysis and Explanation? Notes on the Life and Times of the Society for Applied Anthropology. *Human Organization* 35(4):335–343. (Republished at <<http://www.sfaa.net/>>.)
- Spicer, Edward H., ed.  
1952     *Human Problems in Technological Change: A Casebook*. New York: Russell Sage Foundation.
- Thompson, Laura  
1940     *Fijian Frontier*. San Francisco: American Council, Institute of Pacific Relations. New Edition. (Republished by Octagon Press in 1972.)  
1947     *Guam and its People*. San Francisco: American Council, Institute of Pacific Relations. (First published in 1942. Third Edition reprinted by Greenwood Press in 1969.)  
1950     *Culture in Crisis: A Study of the Hopi Indians*. New York: Harper. (Reprinted by Russell & Russell in 1973.)  
1969     *Secret of Culture: Nine Community Studies*. New York: Random House.  
1979     The Challenge of Applied Anthropology. *Human Organization* 38(2):114–199. (Republished at <<http://www.sfaa.net/>>.)
- Thompson, Laura, and Alice Joseph  
1945     *The Hopi Way*. Chicago: University of Chicago Press. (Reprinted by Russell & Russell in 1967.)

Weaver, Thomas

- 2002a Malinowski as Applied Anthropologist. *In* The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers. T. Weaver, ed., <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.
- 2002b The Malinowski Award and the History of Applied Anthropology. *In* The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers. T. Weaver, ed., <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.

## The Challenge of Applied Anthropology

**Laura Thompson**

I want to express deep appreciation for this singular honor. The Malinowski award has special significance for me. I greatly admire Malinowski's work. Although I did not study under him, and I met him only once, I consider myself to be his student. Under Malinowski's influence, I have been working for years on an empirically based general theory of human group behavior with special relevance to applied anthropology. I believe that we need a complex holistic approach toward human population units that can be applied by the group itself toward the solution of its own practical living problems.

To clarify this position permit me first to refer to the founding of the Society for Applied Anthropology. Many anthropologists who attended that historic meeting in 1941 had already been experimenting for several years with applications of their research findings to practical social problems. Some were working in industry primarily to improve work-group organization and individual adjustment to it (Chapple 1953), while others, myself included, were helping government administrators to implement their policies within the colonial system (Embree 1945). We were creating a niche for ourselves as troubleshooters and advisers to our colonial patrons, mainly in the Pacific area, Africa, and the U.S. Indian Bureau.

Thus, both industrial anthropologists and colonial anthropologists at this early date were under a certain degree of pressure from the larger society to develop a set of tools for engineering, manipulating, and managing people in small groups and ethnic enclaves toward utilitarian goals superimposed from without. Indeed, as Gunnar Myrdal (1953:214) noted, there was a trend toward state interventions, bigger business, and larger units of organization.

At the same time, hindsight reveals that a counter trend was developing away from authoritarianism, colonialism, and centralization of political power. As Malinowski (1961) pointed out long ago, practical concerns emphasized the dynamic nature of culture. Strange though it may now seem, this was a novel idea at the time. Our practical assignments forced us to study culture change and drew us toward the broad-gauged approach needed by government administrators in their concern with the day-by-day living problems of local communities in highly diverse habitats of the earth.

Now, traditional colonialism has almost disappeared and its interventionist methods have become dated. The post-colonial era faces new problems in a worldwide crisis. In this paper I suggest that this crisis is particularly acute among applied anthropologists because of the political implications of their findings and the experimental, predictive, and self-corrective nature of their research. In seeking heuristic approaches to the study of universal practical problems, applied anthropologists are formulating holistic theories and client-oriented, multidisciplinary methods that are helping to develop anthropology in its diverse dimensions toward a general humanistic science.

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We've all experienced political pressures, but I'd like to review two examples. When Hitler took control of Germany I noticed that it was the anthropologists who were ordered to rationalize Nazi Party dogmas. But the leading German anthropologists refused to prostitute their skills on behalf of the Third Reich. They stood together in an unwritten agreement not to surrender their intellectual integrity. They paid dearly for their stand in job loss, nervous exhaustion, and even fatal illness. K. T. Preuss, chairman of the anthropology department at the University of Berlin, was one of the casualties. His death (1938) came at the height of desperation in the profession. Richard Thurnwald managed to escape to Switzerland where he wrote several books published after the war.

A comparable draft occurred in this country after Pearl Harbor. Anthropologists were the first social scientists called from their ivory towers. In contrast to the Germans, however, Americans responded quickly to the national challenge. In this way academic anthropologists like Clyde Kluckhohn, Ruth Benedict, and Cora duBois were recruited into the applied field.

As is well known, anthropologists were assigned to intelligence activities and to survival, linguistic, and foreign service training missions, as well as to morale building, culture changing, War Relocation Authority, and occupied-area administration. One of the most difficult wartime assignments was that of inferring the behavior of people behind enemy lines and in enemy-held territories. Such tasks involved not only the expected behavior of the fighting forces under certain conditions but also that of national populations and locally rooted ethnic enclaves inaccessible to direct observation (Mead and Métraux 1953).

Most of these anthropologists had no training or experience in forecasting the expected behavior of specific groups. But there was an urgency and a clear-cut focus about war work that challenged creativeness and motivated cooperative effort. The welfare and even the lives of countless people depended on the skill and accuracy of these researches.

Many anthropologists believe that correctly hypothesizing expected group behavior under likely future conditions is impossible. To persist in this persuasion, however, one has to overlook many instances of successful predictive efforts. Doubters often dismiss these cases as inexplicable or unscientific. On the other hand, I'm sure you'll agree that a more appropriate approach would be to investigate how such positive results may have been achieved.

One way would be to analyze and compare cases of verified foresight. But most wartime experiments in prediction were classified. Unfortunately, a good many remain buried in government archives with the result that scholars are unaware of their contents. Hence, I beg your indulgence if to illustrate the point, I describe a personal experience.

In 1943, the U.S. armed forces were planning to retake Guam, the American dependency that had been occupied for more than two years by the Japanese. Just before the war I had made a six-months' field study of educational problems on the island for the naval governor. So I was asked to write an official statement regarding the behavior and attitudes to be expected of the Chamorro natives under Japanese occupation. Would they be loyal to the United States despite their repeated but unsuccessful prewar demands for American citizenship and representative government? Could they hold up under sustained anti-American propaganda, cruel treatment, and torture?

On the basis of Chamorro behavior during and after the Spanish conquest and under almost a half-century of American military rule, I knew that the people had developed impressive skills at subterfuge. While outwardly conforming to orders from above, they would covertly ridicule their rulers in their own language, for they loved to subvert foreign commands by innuendo and double-talk. It was a sort of game that they played with well-worn traditional

tricks. They improvised humorous, often insulting couplets to a catchy tune that had become a mode of secret communication.

While playing this game they complained loudly and persistently of the absence of democratic institutions under American naval government. But the Guamanians, I believed, were actually proud of their affiliation with the United States and appreciative of its benefits in schooling, medicine, and living standards. I concluded that under enemy occupation, the Guamanians would remain intensely and cunningly loyal to the United States. They would take pride in their skills at outwitting authority figures and use their resources to confuse and frustrate the Japanese.

It is now well documented that the people of Guam harbored and fed a U.S. Marine Corps radio technician through the entire Japanese occupation despite beatings, rape, and execution (compare to duBois 1960: xiv). A more astonishing fact—their revolt against the enemy—is virtually unknown. When the American warships approached Guam to retake the island, a few strong Chamorro men with their bare hands and one hand gun (which had been surreptitiously hidden for two-and-a-half years) overpowered their armed Japanese guards and freed some 800 natives of Merizo village from incarceration and almost certain death.<sup>1</sup> Then—you won't believe this but I checked it when I was in Guam in 1977—three village leaders set out from Merizo in an outrigger canoe to contact the American invasion fleet. They were picked up by a destroyer, interrogated, and escorted to the flagship. Thus, they succeeded in conveying valuable information to the American command that somewhat changed the invasion strategy and saved many lives (Camba 1965; Hutton 1962:15).<sup>2</sup>

In the strict sense, the Chamorro case did not involve the prediction of expected group behavior. When firsthand evidence was unavailable, it posed to the applied anthropologist a problem of inference on the basis of previous fieldwork. Hence it illustrates the use of hindsight as a basis for foresight, which, when eyewitness observation is impossible, may employ the same method and serve the same purpose.

I take the position that limited hypothesizing about the expected behavior of a specific cultural group under likely future conditions is a basic function of postcolonial administrative anthropologists. As we are all aware, when our findings are implemented by administrators, natural experiments in human relations and culture change are created that may be observed and recorded.

Let me review a familiar illustration. A careful rereading of *The Chrysanthemum and the Sword* reveals that Ruth Benedict (1946), in this postwar classic, described a natural experiment on a nationwide scale. U.S. occupation policy was announced in a detailed State-War-Navy directive. The Japanese, through their own institutions, including the emperor divested of divinity, were made responsible for the administration and reconstruction of their country. In one of the best examples of indirect rule on record, the Japanese were required to do their own “housecleaning” with Americans providing the specifications.

You may not have noticed that the last chapter of Benedict's book contains several predictions of Japanese group behavior (1946:303–316). For example, she stated that we Americans could not arbitrarily create a democratic Japan. The nation would experiment with democratic mechanisms, but Western forms would not be the trusted tools for building a better world that they were in the United States. Japan's real strength, she wrote, lay in her ability to switch her energies from a course of action that had failed to other channels. One-half of the national income before Pearl Harbor was spent on the military. If no rearmament provision was made in the state budget, she predicted, there would be an opportunity to raise the national

standard of living, and the Japanese would have a chance to build a healthy and prosperous economy.

During the 30 years since these forecasts were made, many field studies and reports have been written about postwar Japanese behavior. Thus we can test the success of Benedict's diagnosis not only by the responses of the people themselves or by the reactions of colleagues, previously considered to be the only tests available to anthropologists. We can verify the forecast by comparing it to historical events as they have occurred through time.

By this test, I believe you will agree, Benedict rates a high score. The question is: how did she do it? As she explained her method to Clyde Kluckhohn (1949:18–19):

saturate yourself in the materials on a given culture, reading everything you can lay your hands on and interviewing as diverse a group of informants as possible; formulate some tentative hypotheses that seem to arise out of the data; then test these hypotheses by seeing whether you can predict what will come in new material obtained by reading, observation, and interview; if prediction fails or is incomplete, reject or re-phrase the hypothesis and test the revision on still further material.

The empirical test, by highlighting not only our successes but also our failures, puts applied anthropologists directly on the firing line. It also gives us a crucial advantage over our more academic colleagues because we are able to verify our findings, to learn from experience, and to correct our errors in the light of historical facts. According to the present thesis, this breakthrough is of utmost significance to the development of anthropology as a whole. For anthropologists are now in a position to create and validate a genuine science of humankind, a science with its own mode of verification. I have discussed this point in a recent paper (Thompson 1979) so I shall not dwell on it here.

To answer the question of how this kind of forecasting may be successfully accomplished, I urge those who are interested to collect cases for analysis and comparison in order to sharpen our perception of the problem and learn the operations needed to resolve it. For example, the tentative predictions of Cornelius Osgood (1975:III, 1148–1151) regarding the Lung Chau people of Hong Kong may be tested historically as time marches on.<sup>3</sup>

The investigator should also be alert to relevant cases of prediction in diverse guises and disguises. To illustrate, Leonard Mason (1978:4) describes a successful example of forecasting that he witnessed some years ago. In voting two-to-one under American auspices to resettle on Kilo island, the displaced Bikini people took on functions that traditionally were performed by their chief. Mason's informant, himself a Micronesian paramount chief, said at the time that the Bikinians, like guests at a feast who got sick from overeating, had taken on too big a decision without their chief's guidance. They would regret their decision, he predicted, and "regurgitate what they had done." Subsequent events, according to Mason, have proved the high chief was right.

Here it may be noted that many correct forecasts are so disturbing as to be rejected or ignored. On the other hand, not all predictions are verified by historical events. We all make mistakes, especially in an unfamiliar situation. But it is very important to analyze our failures for what we can learn from them (Foster 1962:231–232).

Comparison of cases of the accurate prediction of group behavior reveals that they cannot be explained on the basis of extrapolation, a method rooted in the untenable hypothesis that past

group behavior will be reproduced with mathematical precision in the future. However, all these forecasts have one characteristic in common. Each is based on knowledge in depth, not only of the human condition but also of the specific culture involved. Such insight is not gleaned solely from observation of human behavior and its productions. Rather it reveals deep, emic-type understanding of a culture's covert structure in relation to human needs and values. This kind of understanding probably also characterizes the consciousness mode of many seers, prophets, and diviners (see Jules-Rosette 1978).

Thus, insight into structure seems to provide the key ingredient in a mix of aptitudes and skills used to understand a culture in depth and to forecast in a tentative, limited way the expected behavior of the social group that expresses it. Often incorrectly called intuition, structural insight is apparently based on a complex, holistic mental approach to problem solving similar to that diagnosed by the Rorschach test. Recent research on the two hemispheres of the human brain has thrown significant light on the problem (Orstein 1975:65–89; Paredes and Hepburn 1976). This approach evidently utilizes the right hemisphere through which the subject suddenly grasps a complex holistic form with all of its parts related to the whole.

Structural insight does not depend on a mechanical, computer-like approach to problem solving characteristic of the brain's left hemisphere. It does not operate by logical linear analysis of items that can be observed directly, verbalized, counted, and mathematically coded. By contrast, structural insight, as here conceived, apprehends reality as a totality directly from experience. It depends on the sudden, nonverbal, analogical grasp of a structured whole, an approach known to characterize the consciousness mode of the brain's right hemisphere. Since, as a rule, structural insight also uses information obtained through functions of the left hemisphere, I suggest that it may express an asymmetrically balanced product of both cerebral hemispheres.

Proceeding from these inferences, I am seeking to develop a general anthropology that closes the gap between social, biological, and applied anthropology and reflects its own built-in code of ethics.<sup>4</sup> It perceives a human community as a complex, whole event with definite spatial dimensions in a specific earthly habitat. The community is also viewed in terms of its durational dimension; that is, it has a past and a present that contains the seeds of its probable future. Community members create and tailor their culture as their main adaptive, problem-solving device (Montagu 1968; Thompson 1969). In any problematic situation the alternative choices available to them are limited by the present constitution of the community in its total existential situation (see also Foster 1962:265–269). An understanding description of such a social unit in the predictive sense requires the integral use of several related disciplines. These include not only archaeology, ethnohistory, and ethnography, but also ecology, population genetics, biocultural anthropology, and the psychological disciplines.<sup>5</sup>

Such a population unit, it should be emphasized, is not viewed as a type or an ideal. Rather, it is conceptualized as a human macroevent consisting of a number of related human organisms behaving in transaction with one another and with the real natural and social world in space and time. In this reality domain, precise numerical predictions in the scientific mode are impossible because of the nature of organic life. No two organisms or organic groups are exactly alike (see, e.g., Brown 1975:119). Each expresses not only its species biogram (see Count 1973:xi, 98–106), but also a growth and developmental history that is different from any other, depending on very complex genetic, biological, historical, cultural, and situational contingencies that are unique and cannot be replicated (Simpson 1967:369–370).

Thus, in terms of method, a group behavioral forecast is particularistic. Its applicability is confined to the actual social unit under consideration as a unique historical process in an ongoing, earth-rooted, biophysical world. The kind of hypothesizing under discussion is therefore strictly limited—a probability projection; an analysis of possible alternatives with an “if . . . then” grammar. It has nothing to do with phases of a human-invented cultural scheme by which modern sociocultural evolutionists type a given community and pigeonhole it into a predetermined evolutionary progression toward an ideal state. Exponents of the latter persuasion have no need for the predictive skills under discussion, or, for that matter, for client-centered anthropology. A generalized forecast for all human societies is built into their hypothesis, which holds that cultural evolution is essentially unaffected by human attempts to modify or otherwise affect its course (Harris 1977:xii, 195).

By contrast, the realistic clinical approach to inferential forecasting under discussion calls for emphasis, not on human-fabricated, armchair-projected formal schemes of lineal cultural evolution, but on the eyewitness, fact-based, culture-change processes stressed by Malinowski. There is no short cut. In holistic social forecasting each focal unit of research must be studied and analyzed substantively as a unique, complex whole event.

According to this postcolonial school of thought, I suggest that an anthropologist who is hired by a client community as a professional consultant needs two basic qualifications. Such a consultant ideally should be equipped with a complex holistic mental approach (the right hemisphere of the brain developed in complementary relation to the left). Secondly, he or she should possess, or have the means to acquire, knowledge of the client community’s covert culture and psychological structure (including its value system) in an effective environmental and historical setting. Such knowledge in depth can be used as the basis for inferential predictions of the probable alternatives the group is likely to face in the near future in trying to resolve its practical living problems.

According to this thesis, the choices that the client group makes are up to the members themselves or their designated leaders. These decisions are not the responsibility of the consulting anthropologist. The latter should exercise great care not to take on the functions of an administrator or social engineer (see Barnett 1956:87–88). In the ideology of today’s postcolonial era, such activities are considered to be the prerogatives of the group itself or its representatives who must be free to make up their own minds, whether or not they use the information and inferences that the consultant provides. The consultant’s approval or disapproval of the client’s selections is irrelevant. This school of thought holds that he or she should forego all intervention or meddling activities and should allow clients full opportunity to make their own decisions and develop their options in their own ways (Thompson 1976; compare to Arensberg and Niehoff 1964).

Finally, what kind of general theory and method regarding group behavior contains the client-centered predictive potential? Although we may not have such a theory with every constituent concept in place and related to every other, I believe there are indications regarding the directions in which to move to achieve a relevant working hypothesis and multidisciplinary method (Thompson 1961, 1967). The directions to avoid are also clear. We should shun theories that contain a built-in long-range generalized prediction of future human behavior and also those that foster human engineering methods aimed at changing a culture in the direction of superimposed goals.

The results of research in applied anthropology<sup>6</sup> indicate the specifications for a general, factually based, predictive theory of human group behavior and suggest four basic principles as necessary foundations:

1. It should be biologically based. That is, it should accommodate the natural processes regarding the organic, evolving human species that are accepted by modern biologists, including recent research on human genetics, variability, and behavior; that on the human biogram; and findings on the human brain with its two hemispheres and its potential for balanced development.

2. Such a theory should be situationally based. In other words, it should focus on living population units as territorial and sociocultural events in ongoing temporal perspective on the planet earth. It should seek to give space depth to human populations. That is, it should accommodate natural processes and facts accepted by modern ecologists concerning the behavior of all organic species.

3. It should be historically oriented. Specifically, it should seek to give temporal depth to an ongoing human group by means of the approach and findings of paleontologists, archaeologists, ethnohistorians, and modern historians.

4. The theory should afford means of exploring and accommodating the hidden dimensions of a group's psyche. In other words, it should be cognizant of the methods and results of well-designed and well-executed research on group consciousness, personality structure, development, and variability.

If, despite political pressures, we can produce, validate, and implement such a complex holistic theory and a multidisciplinary methodology, surely anthropologists, both "pure" and applied, will be well on the way toward resolution of the present discipline-wide crisis. The blurred image of applied anthropology as an integral branch of the discipline as a whole will be clarified, its conflict of interests and ethics resolved, and its difficult professional role spelled out. The discipline of anthropology as a whole may then move confidently on track toward a general science of humankind that can be applied by client-oriented methods to urgent practical problems. For in the words of Malinowski (1961:151), ". . . the most important thing is the clear realization that all really sound theory is bound in the long run to be practical."

### Notes

<sup>1</sup> Chamorro men impressed the early voyagers with their great strength and agility. Many Chamorros still retain this trait.

<sup>2</sup> Thus, ironically, intelligence was transferred to the American command not by the professional radio technician, for whom the Chamorros risked their lives daily and suffered torture, rape, and execution, but by the so-called "mighty men of Merizo"—Jesus Barcinas, Jose M. Torres, and Juan C. Cruz.

<sup>3</sup> An example of the "if . . . then" type of prediction to be tested by time is found in Bronowski (1973:437): ". . . fifty years from now, if an understanding of man's origins, his evolution, his history, his progress is not the commonplace of the schoolbooks, we shall not exist." See also Foster (in Potter et al. 1967:12) who predicted that peasant society as a genuine unique social form is on the way out. It represents a cultural lag tied to the preindustrial city. If this disappears, then peasants will disappear.

<sup>4</sup> For a different point of view, see Foster (1962:256).

<sup>5</sup> It may be argued that complete ethnographic reports according to the proposed model have not yet been achieved. On the other hand, I suggest that Elkin's work on the Australian aborigines, Verrier Elwin's on the aboriginal tribes of India, and Kluckhohn's on the Navaho illustrate the point. So far as the data were available, I attempted to describe nine communities in Oceania, the American southwest, and Europe according to the model (Thompson 1969).

<sup>6</sup> For example, see Kluckhohn and Leighton (1946) and Lewis (1951).

### References Cited

- Arensberg, Conrad M., and Arthur H. Niehoff  
1964     Introducing Social Change: A Manual for Americans Overseas. Chicago: Aldine.
- Barnett, Homer O.  
1956     Anthropology in Administration. New York: Harper and Row.
- Benedict, Ruth  
1946     The Chrysanthemum and the Sword: Patterns of Japanese Culture. Boston: Houghton Mifflin.
- Bronowski, J.  
1973     The Ascent of Man. Boston: Little, Brown.
- Brown, Barbara B.  
1975     New Mind, New Body. Bio-feedback: New Directions for the Mind. New York: Bantam.
- Camba, A. B.  
1965     Merizo Massacre. Pacific Profile (July).
- Chapple, Eliot D.  
1953     Applied Anthropology in Industry. *In Anthropology Today*. A. L. Kroeber, ed. Chicago: University of Chicago Press.
- Count, Earl W.  
1973     Being and Becoming Human: Essays on the Biogram New York: van Nostrand.
- duBois, Cora  
1960     The People of Alor: A Social-Psychological Study of an East Indian Island. Revised ed. Cambridge, Massachusetts: Harvard University Press.
- Embree, John  
1945     Applied Anthropology and its Relationship to Anthropologists. *American Anthropologist* 47:635–637.
- Foster, George  
1962     Traditional Cultures and the Impact of Technological Change. New York: Harper Bros.
- Harris, Marvin  
1977     Cannibals and Kings. The Origins of Cultures. New York: Random House.
- Hutton, Shirley  
1962     I Like Guam. *Territorial Sun* (February 18).
- Jules-Rosette, Bennetta  
1978     The Veil of Objectivity: Prophecy, Divination, and Social Inquiry. *American Anthropologist* 80:549–570.
- Kluckhohn, Clyde  
1949     Ruth F. Benedict: A Memorial. New York: The Viking Fund.
- Kluckhohn, Clyde, and Dorothea Leighton  
1946     The Navaho. Cambridge, Massachusetts: Harvard University Press.
- Lewis, Oscar  
1951     Life in a Mexican Village: Tepoztlan Restudied. Urbana: University of Illinois Press.

- Malinowski, Bronislaw  
 1961 The Dynamics of Culture Change: An Inquiry Into Race Relations in Africa. P. M. Kaberry, ed. New Haven: Yale University Press.
- Mason, Leonard  
 1978 Reflections on a Recent Visit to Micronesia. *Micronesian Perspective: A Contemporary Review* 1(6):4–7.
- Mead, Margaret, and Rhoda Métraux, eds.  
 1953 The Study of Culture at a Distance. Chicago: University of Chicago Press.
- Montagu, Ashley, ed.  
 1968 Culture: Man's Adaptive Dimension. London: Oxford University Press.
- Myrdal, Gunnar  
 1953 The Relation Between Social Theory and Social Policy. *British Journal of Sociology* 4:210–242.
- Orstein, Robert E.  
 1975 The Psychology of Consciousness. Penguin Books.
- Osgood, Cornelius  
 1975 The Chinese: A Study of a Hong Kong Community. 3 Vols. Tucson: University of Arizona Press.
- Paredes, J. A., and M. J. Hepburn  
 1976 The Split-brain and the Culture-and-Cognition Paradox. *Current Anthropology* 17(1):121–127.
- Potter, Jack N., et al., eds.  
 1967 Peasant Society: A Reader. Boston: Little, Brown.
- Simpson, George G.  
 1967 The Crisis in Biology. *The American Scholar* 36(3):363–377.
- Thompson, Laura  
 1961 Toward a Science of Mankind. New York: Macmillan.  
 1967 Steps Toward a Unified Anthropology. *Current Anthropology* 8(1–2):67–77;86–88.  
 1969 The Secret of Culture. *Nine Community Studies*. New York: Random House.  
 1976 An Appropriate Role for Postcolonial Applied Anthropologists. *Human Organization* 35(1):1–7.  
 1979 Validation in Ethnology. *Humanitas* 15(1):59–75.



## Chapter 10

### Fei Xiaotong: Policy in the Service of Minorities

Thomas Weaver

Fei Xiaotong, the 1980 recipient of the Society for Applied Anthropology's Malinowski Award, was a student of Bronislaw Malinowski from 1936 to 1938.<sup>1</sup> His motive for going to Great Britain to study with Malinowski was to understand his country and to help improve living conditions. Most immediately, he wanted to learn from Malinowski how to put his research data based on participant observation in a Chinese village into manageable form, and he wrote his first book, *Peasant Life in China* (1939), under Malinowski's guidance. Malinowski praised this book "as a landmark in the development of anthropological field-work and theory" (Freedman 1963:1) and saw Fei's work as pushing the frontiers of Chinese anthropology beyond the boundaries set by the evolutionists (Freedman 1963:1).

Fei (1910–) received a doctorate from the London School of Economics, and, on his return to China from Britain, he served as director of the Sociological Research Station of Yenching University. Later, he conducted research and study at Harvard, the University of Chicago, and, at the invitation of the U.S. State Department, at the Institute of Pacific Relations. He was then appointed professor of anthropology at Tsinghua University. Fei's early work, including *Earthbound China* (1945), a study of rural economy in three villages, as well as his 1939 book, provided models for community studies in China and elsewhere. According to Art Gallaher,

Fei's peasant village study [is] a classic analysis supporting [Malinowski's functional premise] . . . He demonstrated, for example, how silk prices fluctuating in New York and London, as well as mechanization of the silk industry in Japan, impacted local marriage customs, division of labor, etc., in a small, seemingly quite parochial, Chinese village on the other side of the world (e-mail to author, March 25, 1998).

Fei lived during a period of radical political change in China, with broad swings during which intellectuals were given little or uncertain freedom. In April 1979, Fei renewed his contacts with American social scientists as a member of the first delegation to the U.S. from the Chinese Academy of Social Sciences. Nevertheless, at the time of the presentation of the Malinowski Award, Fei had been largely out of touch with Western scholarship for 30 years. The reader must keep in mind Freedman's analysis of Fei's career in the context of these political

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changes and the political tensions between China and the West: “Fei first butted his head against the political wall under the Kuomintang and then, as far as anthropology is concerned, succumbed to the alternating blandishments and severities of the Communists” (Freedman 1963:2).

In his Malinowski Award lecture, entitled “Toward a People’s Anthropology” (1980), Fei contrasted the anthropology practiced in Chinese colonial times, presumably meaning before 1949, with the use of anthropology by a political system to better the lives of poor and disadvantaged peoples. Fei believed that anthropology, as an applied science, must act as a conduit for facts about how people actually live in order to provide suggestions to policy makers to benefit the people (Fei 1980:116, 120).

The research in which I participated answered the demand of the people living in the national minority areas and was geared to the government’s need to carry out its work for nationality affairs. The minority peoples . . . wanted to end the backwardness left by history and were anxious to develop their economy and culture. But these reforms had to start from the stage reached by these nationalities in social development, and had to be carried out by themselves of their own accord. And this was where scientific research work came in (Fei 1980:118-119).

Fei expressed concerns similar to those of U.S. applied anthropologists regarding the link between theory and practice and the relations of anthropologists to the people studied.

[O]ur knowledge of comparative sociology and the theories about the general laws of social development were useful in analyzing these societies. . . . We did not try to understand things for the sake of understanding them, nor to study a theory just for the sake of advancing a theory; we tried to understand things and study theories for practical reasons, namely, for the purpose of producing scientific, factual bases for the minority peoples to carry out social reforms. . . . This was a good example of applied anthropology (Fei 1980:119).

Under these circumstances, the anthropologist is no longer looked upon as a self-serving researcher, but as helping people help themselves in reaching self-prescribed goals.

Fei’s aims were similar to those of fellow Malinowski Award winner, Sol Tax, who pioneered action anthropology in the United States (Tax 1977). To the question of who is using the anthropologist’s work and to what purpose, Fei answered that anthropology, science, and politics must be unified for the betterment of people: “Only when the truthfulness of theories is being constantly examined in practice can we steadily push research work in a scientific direction and make it a prime mover of social progress” (Fei 1980:119).

Fei also believed that science was a constituent of culture and that anthropology, particularly, was connected with national politics and economics (Fei 1980:117). As if speaking of himself, he continued:

Accordingly, when appraising a particular scholar, we must take care not to overlook the time in which he lives and his position in society. We must evaluate the part he plays in facilitating the march of time in light of the specific environs. .

. . We must, at the same time, take into account the limitations imposed on him by the times (Fei 1980:117).

At the time Fei received the Malinowski Award he was president of the Chinese Sociological Association and Deputy Director and Professor at the Institute of Nationality Studies of the Chinese Academy of Social Sciences in Beijing (Fei 1980:115).

### Notes

<sup>1</sup>This annual award is given to a senior colleague in recognition of efforts to understand and serve the needs of the world through social science. See Thomas Weaver's "The Malinowski Award and the History of Applied Anthropology" (2002b) for a brief history of this award and an overview of the recipients and their work, and Weaver's "Malinowski as Applied Anthropologist" (2002a) for an introduction to Bronislaw Malinowski and his work.

### References Cited

- Fei, Xiaotong  
 1939 Peasant Life in China. London: G. Routledge & Sons.  
 1980 Toward a People's Anthropology. *Human Organization* 39(2):115–120. (Republished at <<http://www.sfaa.net/>>.)
- Fei, Xiaotong (H. T.), and Chih-I (T. Y.) Chang  
 1945 Earthbound China: A Study of Rural Economy in Yunnan. Chicago: University of Chicago Press.
- Freedman, Maurice  
 1963 A Chinese Phase in Social Anthropology. *British Journal of Sociology* 14(1):1–19.
- Tax, Sol  
 1977 Anthropology for the World of the Future: Thirteen Professions and Three Proposals. *Human Organization* 36(3):225–234. (Republished at <<http://www.sfaa.net/>>.)
- Weaver, Thomas  
 2002a Malinowski as Applied Anthropologist. *In* *The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers*. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.  
 2002b The Malinowski Award and the History of Applied Anthropology. *In* *The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers*. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.

## Toward a People's Anthropology

Fei Xiaotong

Having traveled thousands of miles over the Pacific and across the Rockies to Denver to receive this year's Malinowski Memorial Award, my sentiments go beyond delight and gratitude. Preparing my remarks for this occasion stirred up memories of a scene that took place 42 years ago when I bade farewell to my mentor, a man who left a deep imprint on my lifelong academic pursuits. I still remember his advice to me: "By all means go on with your study of Chinese society and its culture." He was a great admirer of Chinese culture and had deep sympathy for the Chinese people. I knew very well his solicitude for Chinese students because I was one of them. He was untiring in his efforts to help and enlighten us. He had the high hope that his creative method of research into social anthropology would also be of use to social sciences in China.

But have I lived up to my mentor's expectations? The Society for Applied Anthropology has conferred on me an honor linked to his name, and my conscience is greatly disturbed. You may recall that not long after my teacher passed away, in the vicissitudes of life, I lost contact with my colleagues abroad. Today, as we share each other's joy in a reunion after a prolonged separation, I am not quite sure if there is anything at all I can offer you, you who are my old friends. This makes me feel ill at ease. But if you'll allow me to say a few words for the sake of the old days, I will be very grateful to you indeed, and I'm quite ready to shake off the reserve which is hard to avoid on such an occasion. What I am saying is that I wish to share with you my personal experience and reflections on years in pursuit of studies in social anthropology, or sociology. This will be an informal chat with familiar friends, the aim of which is to dredge up, so to speak, a long-silted channel of thoughts to prepare the way for our future academic exchanges.

You must remember that for 30 years I have been out of touch with Western scholarship. I have not had a chance to use Malinowski's published diaries or recent historical research in the preparation of this address. What I have to say is based on my sincere and earnest impression of Malinowski as a teacher and what I learned from him. If Malinowski showed a racist mentality in unguarded moments of his fieldwork, he did not communicate it as a teacher. Malinowski's instructional manner did not contradict his humanitarian and egalitarian image. He inspired us toward a more human relationship between the investigator and the investigated.

I studied under Professor Malinowski for only two years, between 1936 and 1938. It was no accident that I became one of his students. First and foremost, I very much wanted to study social anthropology. This is explained in the foreword to my book, *Earthbound China* (1945). As a Chinese youth in the 1930s, with my country and Chinese nationhood at the crossroad of survival or extinction, I knew that my life was closely bound up with Chinese society. I realized that as long as the future of my country and my people was in doubt, so was my own. I also realized that a young man like me must first have a good understanding of the society in which I lived, if I wished to do something meaningful and do it with a clean conscience in that era of

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unprecedented upheaval. Hence, the desire and the decision to probe for a scientific way of examining and studying Chinese society.

I think perhaps it is proper and necessary for me to say something first about the starting point of my lifelong academic endeavor. Present at this gathering are friends from all parts of the world, all devoted to the study of applied anthropology. Applied anthropology, as I understand it, is a science concerned with the transformation of human society through the knowledge of social sciences. And wasn't this what I was after in the early years of my life? Science should serve people and is needed by people to help them survive and prosper. There is no need to cover up or lie about the pragmatic stand I take. The question is, for whose benefit and for what purpose should social anthropology be used? If it is for the good of the people, for the common security and prosperity of the great majority, and hopefully even for humanity as a whole, if it is used for meeting their growing need for an ever-better material and social life, then, and only then, can science be made to play its proper role in human history.

With such a goal in mind, young people like me who wished to acquire some kind of know-how in transforming society and serving the people, naturally could not be content with book knowledge only, the knowledge of Chinese society that was being taught in the classroom. So we walked out of our schools, left our textbooks behind, and went down to the countryside, to communities in cities and townships to observe and experience society as it was.

Social life itself, in the last analysis, is the fountainhead of all knowledge about society. Once young people had understood the point, academic activities, known at that time in China as "community studies," came into full swing. These activities were part of an effort to understand Chinese society through on-the-spot observation and personal experience in social life.

This method of studying Chinese society attracted our young people at that time, and I myself took an active part in promoting and using this method. But there was still the problem of how to collate, analyze, and interpret the mountains of materials gathered in on-the-spot observation. It was with such a problem in mind that I knocked on the door of Bronislaw Malinowski for help. Under his tutorship, I was able to compile a book that drew heavily on the data I had collected in a village community in my native country before I left for England. *Peasant Life in China* (1939) was of course only an experiment with the fieldwork and observation mentioned above.

My tutor's proposition of studying human society in the midst of humanity's throbbing social life was well known. It had such great appeal that I traveled all the way to him for his wise counsel. In his 1926 article, "Myth in Primitive Psychology," written in memory of his teacher, Sir James Fraser, Malinowski issued the call, and I quote: "I shall invite my readers to step outside the closed study of the theorists into the open air of the anthropological field." And this open air of the anthropological field he referred to was none other than the islet of Trobriand where he had lived for many years among the local inhabitants. Trobriand society was just another stage setting for the drama of human life and like those of many other societies, fraught with soul-stirring, dramatic moments of sorrow and joy, of separation and reunion. I never asked my tutor what had made him break away from the tradition of the classical school, the tradition of his precursors who confined themselves to their studies and got lost in meditation. Nor did I ever ask him what made him develop his seemingly unorthodox school of thought that was sure to raise the eyebrows of his contemporaries.

To me, a seeker of scientific social knowledge that would help to transform society, it seemed that the rationale for his propositions was self-evident. So I just took it for granted. This, however, led to my failure in the distant past to have a full assessment of the significance of his

epoch-making contribution to the process of knowing human social life. It was only recently when I reread some of his earlier works that I came to see the great significance of his break with the traditional standpoint then dominating the realm of anthropology. I now realize that his was an exemplary deed that certainly commands our respect and admiration. The significance of his position is that it was the breakthrough that laid the foundations for applied anthropology. So by 1938, Professor Malinowski was able to declare emphatically, "Anthropology must become an applied science."

And when he called on anthropologists to go to primitive societies and live with people hitherto considered savages having nothing in common with civilized people, he presented us not only with a new approach to the science of humanity but also with a desire to restore to the so-called subhuman savages the dignity and status of humanity—a desire that was stronger than any other. With his literary talent, combined with his scientific proficiency, he convincingly led his readers to see that there is basic unity in people's collective life, however diversified its forms may be. Despite their different geographical and historical background, despite difference in their economic and cultural development and ways of life, all human inhabitants of the earth remain a single species. They all have the same ability to create and invent things. They all have emotions, common sense, and a mind to produce thoughts which are characteristic of human beings. The differentiation of people and nations into categories qualitatively different from one another, which thus sets up an insurmountable barrier among them, is entirely the making of some people, either out of prejudice or speculation, or with an ulterior motive. What they have done is not in conformity with the facts of real life and is, therefore, unscientific.

Unfortunately, the science of anthropology of the old tradition was once permeated with such prejudice. Moreover, it was hidden behind a scholarly garment that called for reverence. It took courage and ingenuity to break free from this unscientific tradition. What I find comforting is that Professor Malinowski, who possessed such courage and genius, lived to see the anthropology that once provided the theoretical basis for many despotic rulers to slaughter, bully, exploit, and oppress the peoples in the colonies, begin to change into a branch of science serving the purpose of establishing a world order under which all nations are equal, and into a science working for the advance and progress of the peoples. We owe our thanks to him for this.

It may not be superfluous here at this gathering of scholars of applied anthropology to recount the turning point in the history of this branch of science. Anthropologists of the new generation may say that the basic unity of human social culture is there for all to see and that all nations of the world are bound to achieve common prosperity in the end. If they do, let me remind them that this belief now accepted as a matter of course was not easily established. So let us set great store by this hard-won concept and fully assess the achievements that our precursors have passed on to us so that we may take up the new assignment history has prescribed for us. In the fulfillment of the task of building a world where scientific knowledge is entirely in the service of the people, we will probably need still greater courage and ingenuity than our predecessors.

It must be noted that science itself is a constituent of a given culture. It at once propels and checks the advance of the other elements in the culture and vice versa. Anthropology, including the humanities and other social sciences, is more closely connected with the politics and economics of a given place at a given time. Accordingly, when appraising a particular scholar, we must take care not to overlook the time in which he lives and his position in society. We must evaluate the part he plays in facilitating the march of time in light of the specific environs around him. We must, at the same time, take into account the limitations imposed on him by the times.

Speaking of the time factor, there are both similarities and dissimilarities between the days my teacher lived in and the present. Malinowski was a man who came to the fore after World War I and we are living in an era more than 30 years after the conclusion of World War II, with a half a century in between. We are, it is true, still living under the shadow of a new world war, in a world where hegemonic powers of all descriptions are still very much in existence, in a world where most nations are still suffering from poverty and hunger. But it can no longer be justified in our era that colonies, like those divided up among the victors of World War I as war booty, should officially appear on the political map of the world today. Let us not deny that this is a change of considerable importance to this scientific field of ours. We must not, therefore, forget the imprint imposed on the science by the colonial system of the time when Malinowski was doing his research work.

The fieldwork conducted by anthropologists of that time was invariably restricted to nations under colonial rule. As we see it today, we might call it an irony of the anthropologists' own making that the science of humanity was reduced to a science of "barbarians," albeit the fact that then, only a generation ago, this very idea was accepted *ipso facto*. So, under the influence of tradition, our young hero, who called on others to step outside their reading rooms in order to study people, could only choose a colony under alien rule as the site of his fieldwork.

Worse still, in the eyes of the inhabitants in the clutches of colonialism, this inquisitive outsider who came to live with them and make observations was yet another member of the race who ruled them. Under the colonial system the relationship between rulers and ruled, between Whites and the local inhabitants, inevitably placed limitations on anthropologists doing fieldwork, which made scientific observation more difficult. Under the circumstances, there was no way for an investigator to overcome such limitations. In other words, it was impossible for the investigator and the investigated, or for an observer and those under observation, to trust each other with real candidness. And this sets a limitation on the truthfulness and penetration of the materials about the social phenomena gathered during the investigation and observation.

Many anthropologists who personally suffered these limitations felt that there was nothing they could do about it, so they might as well forget it. But this very situation they were in was a harsh reality and it was sure to be reflected in one way or another in their heart of hearts. Take my tutor for instance. He was noted for being able to get along well with the native inhabitants, but, when we read between the lines, it is not difficult to tell from his works that his fact-finding activities aroused strong feelings among them. Of course, I never talked to my tutor about the question of what was going on in the innermost being of an investigator when carrying out his fieldwork. But I had the intuition—maybe I was being oversensitive—that he had all along been frustrated by the fact that he as an investigator was kept at a distance by those he investigated. He discovered this distance in his scientific endeavors and found it difficult to remove, considering the times he lived in and the position he was in. I concluded this when he repeatedly told me to value the advantage I had, the advantage of being a Chinese studying Chinese society. On these occasions he even used the word "enviable" to show how he felt.

To try to make a guess at what was in the mind of my tutor whom I respect would be at once improper and imprudent. People belonging to different eras each have their own frustrations. This we all know. But I often like to put myself into the position of my predecessors and try to figure out what was behind their frustrations. It was all very well that many anthropologists by that time had already freed themselves from the national prejudice of feeling superior to the indigenous peoples, full of sympathy and goodwill for them, but what I want to know is whether or not their investigations meant anything at all to the peoples under

investigation. What, after all, was the effect of their investigations on the local inhabitants they investigated? Those who treat the people they investigate as mere objects of study in a laboratory, of course, can well dismiss these questions as asked by people who are only annoying themselves and can therefore refuse to think about them. But a scholar who treasures human dignity more than anything else will agree that the annoyance arising from these questions is certainly not caused by those who ask them but by the social system of the time and place. On some occasions, after finishing reading my tutor's books, I suddenly ask myself what has become of these Trobriand islanders. Has any one of them ever read these books analyzing their social life? If there are people who have, what do they think of their own life? What action or actions are they to take against the society of theirs?

All these random thoughts have brought me nothing but distraction and disappointment, because it seems that what our anthropologists were interested in was merely the story of these people as told by my tutor, not the actual people themselves. It appears that these living people were long gone from the minds of our anthropologists and what they knew or what they kept talking about was, if I may say so, the mere shadow of these people dancing under my tutor's pen nib. Sometimes I could not but feel sorry for my predecessors. The knowledge they found from the local inhabitants through hard work never found its way back to the local inhabitants or served to bring them a better life. Sometimes I wonder if this state of affairs, which I deplore, has already changed by now. It is true that the understanding in anthropology to treat the object of study like a natural resource to be freely tapped to serve selfish ends has been rightly denounced by our contemporary anthropologists. But whether the very thing that gives rise to such activity, and the limitations of the time, have been eliminated at all is still a question that deserves our attention.

I am saying all this with my tutor in mind. But it is also a reflection of the many contradictions I have encountered in my own work in the anthropological field since I left him. Although I started my fieldwork in rural areas with a long-cherished desire to improve the life of the peasants there, and although I was making investigations in my own country, a thing my tutor envied, for some time I still suffered limitations imposed on me by the social conditions of that era.

Take my earliest investigations in Kaihsienkung village. I was brought up in the county where the village is located, I spoke the local dialect, and my elder sister had worked in the village for many years teaching peasants the art of sericulture. One might think there would not be any distance between the local inhabitants and me. But in actual fact, things were not that simple. Within Chinese society in those days there were different social classes whose interests clashed, and during that particular period the contradictions between classes were becoming increasingly acute. I myself belonged to that social structure, so my viewpoint and my social relations with the local peasants were still subject to limitations. These limitations found expression in the priority I gave to the things I wanted to observe and the people I wanted to approach. Although I had warned myself beforehand against any subjective preference, I found afterwards that such limitations nevertheless existed. Judging from my own experience, there is every reason to believe that the limitations encountered by the White people in their fieldwork in colonies must be far more serious than those I myself encountered in my interactions with the peasants in my native place.

If I were to conclude my speech here and now, I would merely have reviewed in the presence of my friends the contradictions you all have more or less experienced. They make us feel depressed and helpless. But surely such a mood is not appropriate to our reunion after such a



long separation. The reason I have boldly spoken out about the frustrations of anthropologists in bygone days is that I have something else to tell you, something about what I experienced in the years when I was not with you. I hope it is the kind of experience that will help free us from the frustrations mentioned above.

So what I am going to tell you is some of my experiences in my academic pursuits after the liberation of the Chinese people in 1949.

The founding of People's China was a great historic change for us Chinese. It has made an impact on every individual and every thing in the country. As I have changed, so has our science of social anthropology. Of course, the last 30 years itself has been a period of constant changes. I have personally gone through tremendous twists and turns, ups and downs, and so has the scientific branch in which I work. I am not going to talk much about what happened in these years. I just want to share with you my experiences and reflections as related to the questions I mentioned above. These reflections cover three aspects: One, how do we decide on the thing we are to investigate? Two, how do we investigators get along with the people we investigate? Three, what should be an investigator's attitude toward the effects of his or her investigations?

Before liberation, as I have said, the motivation of my investigations was the desire for national salvation. The enemy had set foot on our soil, so what should we do? We were seeking a way out for our country, for the Chinese nation. And this became the theme of our investigation.

After the victory of the people's revolution, we put an end to China's colonial status, freed the country from the ruthless grip of foreign imperialism and the feudal class at home. The issue then confronting the Chinese people was how to rapidly turn China into a modern socialist country. Scientific knowledge was needed to tackle the problem. That established the general orientation for social sciences in post-liberation China.

I myself undertook research work on China's minority peoples immediately after liberation. Ours is a unified, multinational country where national oppression once prevailed. After liberation, people of all nationalities in the country, without exception, wanted to put an end to this unjust state of affairs. They demanded equality among all nationalities. All nationalities were to send their deputies to sit in the people's congresses at all levels and national regional autonomy was to be instituted wherever a minority people lived in a compact community. Each nationality was entitled to use a language, written and oral, of its own, and its customs, habits, and religious beliefs would be respected. These were the basic measures taken to bring about equality among all nationalities. But before these measures could be adopted, we needed information on many specific things about these nationalities. For instance, how many nationalities were there in China? How big was each of them? What about their geographical distribution? Because of prolonged national oppression, we were not very clear about these basic facts at the time of liberation. So the task of finding them out quickly fell on the shoulders of researchers on nationality affairs. It was a matter of course that those who had previously studied social anthropology should take part in this work. And this time my feelings were quite different from what I felt during my previous investigations. This was because the aim of the work was well defined and I knew well the significance of the work. So my subjective wish was identified with the objective demand. Doing work like this, I must say, made me feel genuinely happy.

The research in which I participated answered the demand of the people living in the national minority areas and was geared to the government's need to carry out its work for nationality affairs. The minority peoples had pressed for social reforms because they wanted to end the backwardness left by history and were anxious to develop their economy and culture. But these reforms had to start from the stage reached by these nationalities in social development,

and had to be carried out by themselves of their own accord. And this was where scientific research work came in. We needed to analyze the societies of these nationalities precisely as they were to determine which stage of social development they had reached, or, to use our language, what social form they belonged to—in other words, were they living under the slave system or the feudal system, etc. Our knowledge of social development in social anthropology was very useful to the new research work. It goes without saying that we studied the social history of these minority peoples with a view to promoting their progress. In the course of our research, our knowledge of comparative sociology and the theories about the general laws of social development were useful in analyzing these societies. In other words, theory was linked with practice. We did not try to understand things for the sake of understanding them, nor to study a theory just for the sake of advancing a theory; we tried to understand things and study theories for practical reasons, namely, for the purpose of producing scientific, factual bases for the minority peoples to carry out social reforms, for the purpose of making suggestions that were in the interest of the minority peoples. This was a good example of applied anthropology.

It must be noted that although such scientific research was in the service of the state's political work at that time, it was neither a part of political work nor a substitute for political work. Our politics is politics in the service of the masses; it must operate in accordance with the objective socioeconomic laws and the specific conditions of each nationality. So it must be based on scientific investigations. The aim of these investigations was to find out things as they were, and in no way were they to be based on the will of any private individual. They were to serve objective needs, not the subjective and therefore unrealistic whims of a particular administrator. Although these investigations were carried out to serve political needs, the investigators merely provided materials about what had happened or what was happening. All decisions leading to the solution of actual problems were to be made by political workers. We scientific workers were not supposed to, nor were we in a position to, make such decisions on behalf of the political workers. We gave the political workers only the facts and our opinions.

Our way of investigation radically changed the relations between investigators and the people under investigation. We carried out these investigations entirely for the purpose of finding the way to bring about equality among nationalities and help the minority peoples forge ahead. This was also what the people we investigated were asking for. So we could be quite frank with them about the purpose of our investigation, and the people investigated clearly understood what these investigations were for and found them acceptable.

This often reminds me of how frustrated I was when I undertook investigations in the countryside before liberation. I was frustrated then because the people I investigated could not really understand why I made these investigations and what good the investigations could do for them. Many people might have conceded that I meant no harm and so put up with my inquisitiveness, but there were also people who grew suspicious and disliked my activities. Compared with my former experience, I found myself warmly received when I carried out my fieldwork among minority people after liberation. These people were sincere with me and I had the feeling that I was talking to my kinfolk. There was really nothing mysterious in this. It was simply because the people I studied knew and believed that I meant to help, to help solve their problems so that what they dreamed of could come true. The terms “researchers” or “investigators” and the “objects of study” or “the investigated” thus were no longer suitable. For both parties were actually working hand in hand to observe and explain truthfully the existing social phenomena and problems thereby enriching and deepening people's understanding of these phenomena and problems. Through my personal experience I have discovered a new

horizon for social sciences, namely, that investigations and research work in social sciences can help humanity shake off its blindness and passivity in the effort to transform society and help individuals take initiative into their own hands in the transformation of society in a scientific way.

Of course, in our fieldwork, we sometimes also faced difficulties growing out of the distrust of minorities who still had not gotten over the wounds caused by Han racial discrimination. But such difficulties will be overcome in time, because our social system has dedicated itself to equality among nationalities. We can gain their confidence through our own work by genuine sympathy and affection. So the new horizon for social sciences, as I visualize it, is within reach.

This way of handling investigations and research, however, also offers a challenge to the investigators, that is, a challenge to their sense of responsibility for the consequences arising from these investigations. True, this new relationship between the investigator and the investigated provides the conditions enabling the former to gather materials that more truthfully reflect the actual state of affairs, but human beings' scientific knowledge of their own social life is still rudimentary. At present, there is a gap between their knowledge of nature and their knowledge of society—and the contrast is glaring. As things stand, in the effort to transform society, people invariably find that they are quite helpless because, much as they want to transform it, their ability to do so is limited. Anthropologists of the early generation generally did not concern themselves with the consequences of their investigations or their effects on the investigated and so the question of being responsible to the investigated did not arise. A few who did care were doing it only from the moral point of view. As to who was making use of one's materials, for what purpose were these materials being used, and what would be the effects on the investigated, these questions seemed to be of no concern to academic circles. Of course, in a social system where theory is divorced from practice, and learning from politics, this is understandable, because it is impractical to institute an investigation and hold the scientific worker responsible for the social effects his or her work produces. But under our social system, theory is linked with practice; in a society where science is made to serve politics, it is necessary for scientific workers to estimate the effects of their work on society. This is not just a question of personal morality but also a question of what is good or bad for the majority of the people. Only when the truthfulness of theories is being constantly examined in practice can we steadily push research work in a scientific direction and make it a prime mover of social progress.

By making a contrast between the two phases of my academic work from the three foregoing aspects, I wish to point out that some of my experience in social investigations and research work can be helpful to the development of applied anthropology. But it must also be made clear that we have not yet found in China a systematic applied social anthropology because, as we were moving in this direction, our work was interfered with and we met with obstacles. The road we have traversed has zigged and zagged. This was particularly true during the period in which a reverse current emerged in New China. The country still suffers from the serious damage done by the Gang of Four's feudal fascism. In that fairly long period, we have had some sad and painful experiences from which many lessons can be drawn. One of the lessons is that at that time, the aim of social investigations ran counter to the interests of the masses. These investigations were carried out to serve a number of reactionaries who had seized a part of the state power. Then social investigations degenerated to the point of exacting statements from the investigated and the "materials" gathered were used against political opponents, who were thus implicated in plots and persecuted. The so-called social investigations

of that period often turned out to be fabrications and fictitious things. The result was untold suffering to the country and the people. The history of that period has borne out a truth: any social investigation conducted in a scientific way and useful to the people must be in keeping with the interests of the masses. In other words, genuine applied anthropology must be a science serving the interests of the masses. That is what is meant by people's anthropology, which is the topic of my speech today.

I am here to meet my colleagues and join them in commemorating my teacher, Professor Malinowski. I have taken this opportunity to say what I want to say after all these years of separation. As we are here together to review the development of anthropology in the last 50 years, we certainly want to pay our respects and express our gratitude to this outstanding pioneer of applied anthropology. He deserves to be held in high esteem as the founder of modern anthropology. Among his countless academic contributions recognized by his successors is his fundamental principle established in his scientific endeavors, namely, the principle that each nationality should have respect for its own culture and give equal treatment to cultures of the other nationalities. As one who has experienced the life of the people in a semicolon, I personally want to thank him for this. To the various nations of the contemporary world, his insight is a prerequisite for mutual help and common progress.

During the past 30 to 40 years when I was out of contact with my colleagues abroad, I have come to realize from my experience—both negative and positive—that the suffering masses are in need of the knowledge of a social science which reflects objective reality and which will help them build a society where there is peace, equality, and prosperity. And it is the duty of the anthropologists, whose objective is to study human society and culture, to meet the imperative needs of the people and to blaze a new path for anthropology in their service. The purpose of this branch of study is, if I may be allowed to look into the not-too-distant future, to make the broad masses understand full well the society they live in, to organize their collective life in accordance with existing social laws, and to help satisfy their ever-growing needs. At present this remains a goal which a number of our colleagues are fighting to achieve. I will devote the rest of my life to the cause of people's anthropology and will stride ahead, alongside those colleagues who share my view.

Thank you.

#### References Cited

- Fei, Xiaotong  
1939     Peasant Life in China. London: G. Routledge & Sons.
- Fei, Xiaotong (H. T.), and Chih-I (T. Y.) Chang  
1945     Earthbound China: A Study of Rural Economy in Yunnan. Chicago: University of Chicago Press.
- Malinowski, Bronislaw  
1926     Myth in Primitive Psychology. *Psyche Minatures*, General Series, No. 6. London: K. Paul, Trench, Trubner & Co., Ltd.  
1938     The Anthropology of Changing African Culture. *In* *Methods and Study of Culture Contact in Africa*. L. P. Mair, ed. International Institute of African Languages and Cultures. Memorandum XV. Oxford: Oxford University Press.

# **Chapter 11**

## **Raymond Firth: A Theoretician in King Arthur's Court**

**Thomas Weaver**

Raymond Firth, like Gunnar Myrdal before him (Myrdal 1975), was surprised at being selected for the Society for Applied Anthropology's Malinowski Award in 1981.<sup>1</sup> "Though on occasion I have tried to make practical use of my anthropology, I have never considered myself an 'applied anthropologist'" (Firth 1981:193). As a New Zealander, Firth was aware of the Maori's diminished lands, threatened language, and experiences as a minority population. In part as a result of this early awareness, he was "concerned with the broad problems of how to orient anthropological research more directly towards the solution of significant social issues, and how to get the findings of anthropology more effectively into the process of public education" (Firth 1981:193). He did not become directly involved in finding solutions because of an unease with the nature of industrial society and with who would ultimately profit from anthropology's efforts (Firth 1981:197).

Raymond Firth (1901–2002) spent his professional life in Great Britain. He received early degrees from Auckland University in New Zealand and then studied at the London School of Economics (LSE) with Bronislaw Malinowski beginning in 1924. He received a doctorate from LSE in 1927. After a short period in Australia at the University of Sydney he returned to LSE and ended his career at the University of London. He was a visiting professor at the Universities of Chicago, Hawaii, and British Columbia, and at Cornell University, City University of New York, and the University of California at Berkeley. Most of his fieldwork and publications were on the Tikopia and Pacific peoples, with additional fieldwork conducted in West Africa, Malaya, and New Guinea. Subjects of his writing include social organization (1936, 1951), economy (1939), change, religion, and symbolism (1973). Malinowski lauded his book on Tikopia kinship in the preface as "a model of anthropological research" (Malinowski 1936:vii).

In his Malinowski Award acceptance address, entitled "Engagement and Detachment: Reflections on Applying Social Anthropology to Social Affairs" (1981), Firth addressed the decision of each anthropologist to "work within the system or to oppose it" as an individual matter. "Far more than in theoretical anthropology, in applied anthropology, the personality, temperament, and operations skills of the individual are immediately relevant to the interpretations of issues and implementation of proposals for improvement. Putting it crudely, it is an anthropologist, not an anthropology, that is applied" (Firth 1981:197).

As a perceptive observer and eloquent spokesperson, Firth addressed many of the recurring problems in applied anthropology. Among the unresolved or continuing issues he

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identified is the need to educate the public about the value of anthropology for resolving social issues. The “obscurity in the public mind about what anthropology is and what it may be able to do,” he argued, has led to a lack of public support and funding (Firth 1981:195). Another problem he addressed is the lack of an adequate career structure in applied anthropology—that is, the lack of permanent employment, security and advancement, and assurance of long-term commitments. Lack of a career path limits the roles of applied anthropologists while reinforcing the status of academia and teaching as primary career tracks for applied as well as other anthropologists, an issue also discussed in later years by George Foster (1982) and Michael Cernea (1995) in their Malinowski Award addresses.

Like other Malinowski awardees (Thompson 1979; Fei 1980; Foster 1982; Leighton 1984; Colson 1985; Arensberg in 1991[Weaver 2002a]; Frankenberg 1995; Robert and Beverly Hackenberg 1999), Firth addressed the significance and interconnection of fieldwork, theory, and application in his address. In indicating the importance of theory for application Firth quoted Radcliffe-Brown: “[W]ithout a sound basis in theory applied anthropology must deteriorate and become not applied science but merely empirical practice” (Radcliffe-Brown 1944, as quoted in Firth 1981:200). In support of the importance of fieldwork for theory, he cited C. G. Seligman when applying for funds to support Malinowski’s fieldwork: “Field research in anthropology is what the blood of the martyrs is to the Church.” Firth continued, “The research role remains central in applied as in other types of anthropology, not least also for the contributions it can make to general theory” (Firth 1981:193). Like many anthropologists of his era and ours, Firth saw academia as the best place to produce theory for practical purposes. “For an anthropologist too, an ivory tower can be a retreat for refreshment, thought, and reemergence; a vantage point for survey of the field in perspective and without emotion; and a fortress in which to store a stock of the resources of theory for inspiration and defense of more practical interests” (Firth 1981:200).

Firth defined applied anthropology broadly, in terms of the services that anthropologists may be called upon to provide. One type is episodic assistance to the population the anthropologist has studied, especially during times of disaster or emergency. Another is academic publication, which, if it is consistent with views expressed by native observers, can have a positive role in interpreting native people to an outside world. A third type of service, client-oriented research, he considered “the most testing exercise of anthropological skills” (Firth 1981:198). Problems include the desire of clients for quick solutions and the reluctance or inability of anthropologists to provide them; the unwillingness of anthropologists to adopt the client’s perspective; and the perhaps excessive concern of anthropologists “with disturbance to the few to envisage the benefits to the many” (Firth 1981:198). A key role of the anthropologist in such research is to call attention to potential difficulties, conflicts of interest, and contradictions, contributing to the risk that anthropology may become “the uncomfortable science” in the eyes of the public. Firth added, “But if we cannot become popular we may at least gain respect” (Firth 1981:198).

If one were to depend upon Firth’s Malinowski Award address alone, one would not understand the close and respectful relationship between Malinowski and Firth. Malinowski advised and taught Firth as a student, and Firth was the editor of a volume and the author of an introductory essay that made a balanced assessment of Malinowski and his works (Firth 1957). In that introductory essay, Firth also discussed Malinowski’s difficult personality, egotism, and abruptness that created enemies (Firth 1957:1). There he lauded Malinowski’s contributions to fieldwork, methodology, and teaching, but at other times Firth also praised Malinowski’s rival, A. R. Radcliffe-Brown. Firth noted, for example, that Radcliffe-Brown may have been the first

to use the term “applied anthropology” in his 1931 publication (Firth 1981:194). In that article Radcliffe-Brown proposed that “the art of government in the future will have to rest more and more on applied anthropology” (Radcliffe-Brown 1931, as quoted in Firth 1981:194). Firth concluded, however, that Radcliffe-Brown believed that the best anthropology could offer administrators were ethnographic case studies, teaching, and theory, while Malinowski was clearly the practitioner in matters of applied anthropology, fieldwork, and educating the public on the value of anthropology.

Sir Raymond Firth was also honored as Distinguished Lecturer by the American Anthropological Association. He was a fellow in the Center for Advanced Studies at Stanford; received the Viking Fund Medal, the Huxley Memorial Medal, and numerous honorary degrees; and was knighted by Queen Elizabeth.

### Notes

<sup>1</sup>This annual award is given to a senior colleague in recognition of efforts to understand and serve the needs of the world through social science. See Thomas Weaver’s “The Malinowski Award and the History of Applied Anthropology” (2002c) for a brief history of this award and an overview of the recipients and their work, and Weaver’s “Malinowski as Applied Anthropologist” (2002b) for an introduction to Bronislaw Malinowski and his work.

### References Cited

- Cernea, Michael M.  
 1995 Social Organization and Development Anthropology. *Human Organization* 54(3):340–352. (Republished at <<http://www.sfaa.net/>>.)
- Colson, Elizabeth  
 1985 Using Anthropology in a World on the Move. *Human Organization* 44(3):191–196. (Republished at <<http://www.sfaa.net/>>.)
- Fei, Xiaotong  
 1981 Toward a People’s Anthropology. *Human Organization* 39(2):115–120. (Republished at <<http://www.sfaa.net/>>.)
- Firth, Raymond  
 1936 *We, The Tikopia: Kinship in Primitive Polynesia*. London: George Allen & Unwin Ltd. (Abridged new edition published by Beacon Press in 1963.)  
 1939 *Primitive Polynesian Economy*. London: Routledge & Sons. (Second edition published by Anchor Books in 1965.)  
 1951 *Elements of Social Organization*. London: Watts & Company.  
 1957 Introduction: Malinowski as Scientist and as Man. *In* *Man and Culture: An Evaluation of the Work of Bronislaw Malinowski*. R. Firth, ed. Pp. 1–14. London: Routledge & Kegan Paul, Ltd.  
 1973 *Symbols: Public and Private*. Ithaca: Cornell University Press.  
 1981 Engagement and Detachment: Reflections on Applying Social Anthropology to Social Affairs. *Human Organization* 40(3):193–201. (Republished at <<http://www.sfaa.net/>>.)
- Foster, George M.  
 1982 Applied Anthropology and International Health: Retrospect and Prospect. *Human Organization* 41(3):189–197. (Republished at <<http://www.sfaa.net/>>.)

Frankenberg, Ronald

- 1995 Learning from AIDS: The Future of Anthropology. *In* The Future of Anthropology: Its Relevance to the Contemporary World. A. S. Ahmed and C. N. Shore, eds. Pp. 110–133. London: Athlone. (Republished at <<http://www.sfaa.net/>>.)

Hackenberg Robert A. and Beverly H. Hackenberg

- 1999 You CAN Do Something! Forming Policy from Applied Projects, Then and Now. Malinowski Award Lecture. *Human Organization* 58(1):1–15. (Republished at <<http://www.sfaa.net/>>.)

Leighton, Alexander H.

- 1984 Then and Now: Some Notes on the Interaction of Person and Social Environment. *Human Organization* 43(3):189–197. (Republished at <<http://www.sfaa.net/>>.)

Malinowski, Bronislaw

- 1936 Preface. *In* We, The Tikopia: Kinship in Primitive Polynesia. R. Firth. Pp. vii–xi. London: George Allen & Unwin Ltd. Abridged New Edition published in 1963, Boston: Beacon Press.

Myrdal, Gunnar

- 1975 Unity of the Social Sciences. *Human Organization* 34(4):327–331. (Republished at <<http://www.sfaa.net/>>.)

Radcliffe-Brown, A. R.

- 1944 Meaning and Scope of Social Anthropology. *Nature* 154(3904):257–260.  
1931 Applied Anthropology. *Proceedings of the Australian and New Zealand Association for the Advancement of Science* 1930:267–280.

Thompson, Laura

- 1979 The Challenge of Applied Anthropology. *Human Organization* 38(2):114–199. (Republished at <<http://www.sfaa.net/>>.)

Weaver, Thomas

- 2002a Conrad Arensberg: Economic and Industrial Anthropologist. *In* The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.  
2002b Malinowski as Applied Anthropologist. *In* The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.  
2002c The Malinowski Award and the History of Applied Anthropology. *In* The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.



## **Engagement and Detachment: Reflections on Applying Social Anthropology to Social Affairs**

**Raymond Firth**

To be the recipient of the Malinowski Award of the Society for Applied Anthropology is a moving experience. My surprise in being selected to receive it is perhaps equaled only by the surprise of some of my colleagues who have been engaged in grappling with practical problems in public affairs much more intensively than I have been. Though on occasion I have tried to make practical use of my anthropology, I have never considered myself an “applied anthropologist.” But I have been concerned with the broad problems of how to orient anthropological research more directly towards the solution of significant social issues, and how to get the findings of anthropology more effectively into the process of public education. These were issues to which Malinowski made a fundamental contribution, and I am honored to be associated with his name in this award.

Looking back over 60 years in anthropology, I am impressed by the advances made in the practical field—the range of issues being tackled, the sophistication in formulation of problems, the depth of penetration into institutional organizations, the extent of cooperation with specialists in other disciplines. The very call for this international conference, stressing the need for reevaluation of methods and findings of applied social research, is evidence of the progress made, and also of a lively awareness of the scientific implications of changing circumstances. But it is also obvious that great sectors of the world outside still have to be convinced that anthropology has anything very useful to offer, either in the solution of practical questions or as general comment on the human condition. So I feel sure, as most of you must, that while there is continued need for presenting our general arguments about validity to the world outside, these arguments will lack great force until we have many more apt and clear illustrations of how anthropology has been used in actual cases. Fifty years ago I wrote, “Much has been written on what anthropology can do, little has been shown of what anthropology has done” (1931:2). While this is not as true now as it was then, there is still need for more solid demonstration effect. So I am acutely conscious of the basic contributions being made by you, my colleagues working directly on practical problems, in confrontation with technicians in other fields, doctors and patients, shop-floor mechanics, farmers and politicians, and administrators of all kinds. On you falls the burden of matching theory with practice; of coping with ignorance, apathy, mistrust, or hostility; of brooding over finance; of deciding where the line must fall between principle and expediency, and between dispassionate analysis and personal commitment. It is the activities of you field-workers that test the truth of our claims. From your experiences we must get some of our most valuable insights. At this meeting we will learn far more from you than from any “keynote” address, however inspired. From you too, we hope, will speedily roll out the publications that we need to build up the record and show to anyone interested. As C. G. Seligman wrote about seventy years ago when seeking funds to send Malinowski to the field,

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“Field research in anthropology is what the blood of the martyrs is to the Church.” The research role remains central in applied as in other types of anthropology, not least also for the contribution it can make to general theory.

But there is still room for reexamination of many issues in the applied field. I had thought originally to outline for this conference the history of the subject in Britain. But even there the field has become so diversified in the last few decades that I have limited myself to a brief statement on the early British period, mainly before World War II, and have focused instead on a few general themes that seem to run through most of our work. If I make little reference to the massive, often brilliant American and other work in applied anthropology, much of it available in *Human Organization* and other publications, it must be remembered to stand as a backcloth to all my discussion.

Nowadays, so many aspects of applied anthropology in Britain are new that we are apt to forget that the subject does have a long history. The pioneering formulations of Lucy Mair go back more than 40 years (e.g., *The Anthropologist's Approach to Native Education*. . . of 1935 [Mair 1957] ), as do the more specific studies of Audrey Richards on nutrition, Schapera on customary law, Fortes on education and on food, Margaret Read on migrant labor—all in Africa. Malinowski's contribution to the concept of anthropology as public service goes back at least to 1918 in his evidence to an Australian government enquiry on labor conditions in the Pacific (Hogbin 1957). But even if Malinowski is recognized as the patron saint of applied anthropology, he was not its first prophet. On the linguistic side, for instance—in an area of my own interest—James Howison, M.D., a member of the Asiatic Society, introduced his *Dictionary of the Malay Tongue* in 1801 by writing that trade “to the east of Hindoostan” had become of such consequence as to render every means of facilitating communication between us and the Malays a matter of national importance. Later, the practical value of even a study of proverbs for understanding national character, modes of thought, and motives for action was emphasized by writers on the Malays, the Maori, and the peoples of India. Explorers, administrators, missionaries, and members of learned and philanthropic societies promoted the collection of ethnographic data in a complex and subtle interrelation between scientific interest, moral values, and their own special practical concerns. As the 19th century advanced, many people interacting with alien communities became proto-anthropologists almost without knowing it. In the British tradition, for example, records of the New Zealand Native Land Court, with their immense amount of genealogical and sociological information on Maori custom, were forerunners to the later investigations of anthropologists into land tenure in Africa and elsewhere. In a classic judgment of 1869, for instance, Judge Fenton, sitting with a Maori assessor, evaluated the Maori genealogies as an anthropologist might have done. “Even if these pedigrees are all ‘a mass of invention,’ and the names therein are purely fictitious, their general value is not in my judgment affected, if all natives agree to have their claims, to a certain extent, decided by them” (1879:60). What may be called the “pragmatic potential” of ritual and mystical beliefs in regulating family and property relations, authority and morality also began to emerge. At the theoretical level, this had been apparent in the work of Lewis H. Morgan (1851) and Fustel de Coulanges (1864). But for a British audience it was brought out more generally by Henry Maine, Herbert Spencer, and Sir James Frazer.<sup>1</sup> Allied to this, a fresh perspective on Greek and Roman beliefs and institutions may have been promoted also by Gilbert Murray, Jane Harrison, J. L. Myres, and others who set material from the classics alongside ethnographic data from Asia, Africa, and the South Seas. Since Greek and Roman classics were treated as the most proper model in the education of the public school men who formed the backbone of the Indian Civil Service and other colonial

services, these men were offered the idea that the customs of the peoples they were set to rule had some comparable status and should be respected.

The argument about how far British social anthropology was founded on colonialism is well-known. The history is complex, but two relevant points are sometimes overlooked. One is that the antecedents of applied anthropology go back much earlier than the overt colonialist scramble for territory in many parts of Africa, the Far East, or the Pacific, and these antecedents embodied elements of a scientific and philanthropic order often opposed to the exploitation of indigenous peoples by alien economic or political interests (Stocking 1971). A second point is that when in the field social anthropologists reflected on the human conditions of the peoples they were studying, they were apt to be quite critical of aspects of the colonialist position in the use of land and labor, and toward educational policy, which disregarded customary norms of the people concerned.<sup>2</sup>

Be that as it may, by 1904 the situation had crystallized so far that Sir Richard Temple, an administrator of great Indian experience, was able to give an address to the Antiquarian Society of Cambridge University with the title, “On the Practical Value of Anthropology” (Temple 1914:57–90). Temple’s chief target was the administrative needs of the British Empire. His dream of a school of applied anthropology to be set up at Birmingham or one of the older universities was never realized. But as a result of this and other initiatives, some training in anthropology was made available for overseas government officers (though not officially for the Indian Civil Service, who preferred their own avenues to knowledge of the peoples they ruled). This move culminated in the “Devonshire” courses after World War II, though these had but a brief vogue as the tide of independence surged through the British colonial territories. Another development in the early days of this century was the preparation of anthropological reports by administrative officers—as in the massive Ethnographic Survey of India, which still continues in a more modern framework, on a very large scale. Important, too, were monographic publications, such as the tribal studies from Assam or the Papers on Malay Subjects issued by direction of the government of the Federated Malay States. Of special interest was the appointment in a few territories of government anthropologists, among the best-known being R. S. Rattray in Ashanti, C. K. Meek in Nigeria, F. E. Williams in Papua, and E. W. P. Chinnery in the Mandated Territory of New Guinea. Later Verrier Elwin and Indian colleagues filled analogous positions with distinction in India.

Between the two world wars, on the academic side, general statements on applied anthropology were developing, if in a cautious way. C. G. Seligman had championed the professionals by challenging the claims of practical “men on the spot” to know much about the people with whom they were in contact. Even the most sympathetic administrator or missionary, after years, he said, was apt to know less than some one thinking along anthropological lines could discover in a few weeks (Temple 1914:44). In 1921, A. C. Haddon pointed out how knowledge of local customs could avoid administrative difficulty. He used Chinnery’s striking example of how he was able to get a wild boar’s head substituted for a human head as a trophy of courage among tribes of the Owen Stanley range in New Guinea, thus allowing the young men to marry without being embarrassed by an Australian government that disapproved of homicide. Radcliffe-Brown discussed the practical applications of social anthropology both in Cape Town and in Sydney.<sup>3</sup> In 1923 he claimed “immediately practical results” to be derived from anthropology in tempering missionary opposition to *lobola* marriage payments and to witchcraft, and his lectures on “Economic Aspects of the Native Problem in South Africa,” afterwards published in the *Cape Times*, attracted some public attention. In Australia, in 1930, at a science

congress in Brisbane, he gave an address on “applied anthropology” (perhaps the first time the term was used in print), stressing how anthropology might provide a plan for a novel administrative and educational policy, especially in regard to the Australian responsibility for New Guinea. He concluded: “The art of government in the future will have to rest more and more on applied anthropology.”

But by then an issue of persistent significance was beginning to emerge. Radcliffe-Brown argued that it was not the business of social anthropologists to attempt the solution of such practical problems. Rather, they should keep themselves as free as possible from considerations of application, “and particularly so in a region of problems that are the subject of heated and often prejudiced discussion” (1923:143). In the training of government officials in anthropology, whether in Britain or Australia, there was always a basic dilemma—how to combine an effective knowledge of the general principles of anthropology (without which much of the application could not be properly understood) with not only the detailed ethnography demanded by the officers to fit each of them for their local area, but also some sensible priming for the practical court work and other judgments they had to exercise—and all in a limited period of a few months or even weeks. Radcliffe-Brown’s solution at Sydney was austere:

We try to suit these courses to the needs of administrative officers, not, however, by dealing with actual problems of administration, but by giving a systematic training in theoretical anthropology as a basis for the understanding of the native culture with which they have to deal. [1931:277]

As a meal for the hungry, this was rather dry bread. But similar problems plagued us in the “Devonshire” courses here in Britain after World War II. My guess is that they may even come up, though in different form, in the question of how to train people for modern work in applied anthropology.

The problem of detachment or engagement in applied anthropology was illustrated by some contrast between the approaches of Radcliffe-Brown and of Malinowski. Like Radcliffe-Brown, Malinowski saw that beneath the surface adaptation of indigenous people to colonial rule, there were great areas of maladjustment, some real conflicts of interest, and a continuing flow of independent local activity and belief, with important ritualistic and symbolic elements. He too held that the modern theory of social anthropology could throw light on the processes involved. But Malinowski committed himself to much more trenchant critical statement than did Radcliffe-Brown. It was not a blind partisanship. He did not accuse the governing powers directly. Using his Polish background as a kind of ironic foil, Malinowski praised the adaptability, tolerance, and genuine interest in the welfare of the “natives” shown by the British colonial system. Like Henry Maine writing about India more than half a century earlier (1887:27–28), Malinowski saw the destruction of indigenous institutions as due much less to British repression than to the unintended consequences of well-meaning power moves. But he did not shrink from a positive controversial stance. His blistering attack on the education of Africans by colonial powers (1936a) showed up the miserable inadequacy of a schooling system not backed up by effective social and economic opportunity, but which nevertheless tended to separate young people from the stable institutions and values of their traditional society. In his later writing particularly, Malinowski held that ethnographers who have studied culture contact and assessed its potential and dangers have the right and duty to formulate their conclusions in a way in which they can be seriously considered by those who frame policy and carry it out. They

also have the duty to speak as the natives' advocate. But, Malinowski argued too that ethnographers must recognize their limitations—decisions and handling of practical affairs are outside their competence. Their primary duties are to present facts, develop concepts, destroy fictions and empty phrases, and so reveal relevant, active forces (1940:26).

Argument about the degree of engagement needed to secure an informed opinion, let alone to adopt a proper moral position, is endemic in the anthropological approach. Malinowski's outspokenness may have been facilitated in part by the fact that he was less dependent than was Radcliffe-Brown on direct government support for his teaching activities, but I think it was largely a matter of temperament. Malinowski's idea of anthropologist as expert adviser, like Radcliffe-Brown's idea of discovering scientific laws in the control of social phenomena for practical ends, has turned out to be not very realistic. But each saw an important if refractory set of problems, and outlined some possible approaches to them. In retrospect, their influence seems to have been diffuse rather than specific, of little direct impact on the pressing problems of the day. Yet my guess is that without the inspiration of Radcliffe-Brown, the very fruitful efforts of Elkin, Stanner, and others on behalf of the Australian Aborigines would not have been so pointed; likewise, without the sympathetic teaching of Malinowski, public figures as diverse as Jomo Kenyatta and Fei Xiaotong might have lacked some of their intellectual weapons.

Malinowski's contribution was noteworthy also to the degree to which he applied his anthropology to problems of contemporary Western society. His very frank and provocative statements for the British Social Hygiene Council, advocating freer use of contraceptives and easier divorce as measures to strengthen the stability of marriage and family relations were in advance of most public thinking at the time, but have been overtaken by events, perhaps not quite as he anticipated. His defense of intellectual freedom and his consciousness of the terrible dangers posed for civilization by modern war have now an even more urgent ring. What he called in 1936 (in a little-known article in *The Atlantic Monthly*, the result of a talk given at the Phi Beta Kappa exercises of the Harvard Tercentenary) "the deadly issue," was set out long before nuclear fission had demonstrated all too horribly how far modern technology and nationalist aggression have put human society at risk (see also Malinowski 1947).

I refer to this early history not just to show that an interest in application of anthropology to practical affairs goes far back into the 19<sup>th</sup> century; I draw from it several other general points leading up to the present position.

The first point is that the slow development of applied anthropology was not just because anthropologists were uninterested or imperceptive about practical issues; important parameters were lacking. There is still much obscurity in the public mind about what anthropology is and what it may be able to do. But appreciation of it is far clearer than it was a couple of generations ago, when even in intellectual circles, through a linguistic transference, anthropology was apt to be confused with the study of apes. Now, anthropologists have begun to appear as reputable characters in novels. Though readers may not know what precise branch of knowledge is represented, an anthropologist seems to be regarded as someone who has something meaningful to say about people. A university degree in the subject is now as acceptable (or at least as harmless) as a degree in history or psychology. Formerly there was almost no official backing for anthropological research, in any clear-cut institutional form. Between the two world wars the Rockefeller Foundation supported both the Australian National Research Council and the International African Institute in their anthropological work. But even with bodies such as League of Nations FAO and WHO, there was nothing like the pervasive modern system of

highly financed, articulate bureaucratic organizations directly concerned with development and welfare of so-called Third World and more wealthy countries, and providing channels for anthropological opinion and enquiry, of applied as well as of theoretical kind. This lack of support, either from public acceptance or from institutional agencies, did much to inhibit an earlier development of applied anthropology, and the change in these parameters has given us useful modern assets.

But another characteristic of the earlier period still remains—there is still no adequate career structure in applied anthropology. Between World Wars I and II, most social anthropologists at some point found administrators, missionaries, or business people interested to discuss practical implications of their research. A few even carried out studies especially commissioned by governments, as Schapera studied Tswana customary law, or Margaret Read studied the effects on Ngoni and Chewa village life of the emigration of their male labor to mines and towns. But most anthropologists who took up a practical topic, as Audrey Richards did with Bemba nutrition, did so as a spin-off from their more general studies. During World War II, some anthropologists trained in the British tradition were able to put their experience to good use, but this was largely an accident of geography; they were usually appointed as specialists in a local region, not as anthropological experts. Even the postwar overseas colonial research institutes offered little permanent employment. Nowadays, anthropologists are diffused widely through government departments, institutions, and industrial concerns, but still on a rather haphazard basis, and often not *as* anthropologists. The name is not important, but the nature of their work and its prospects are. I stress the importance of a career structure for applied anthropologists because I think the subject will never attain proper status without it. And while I realize I am raising a very debatable issue, I think the question of training for work in applied anthropology is closely linked with career prospects. I remember about forty years ago being asked by an official in the Colonial Office what the chances were of getting six anthropologists for applied work in Kenya. My reply was, at that moment, almost nil. People would have to be trained for the jobs and with no assurance of the posts, let alone of any future employment comparable to that of an ordinary colonial civil servant, no experienced anthropologist would be easily attracted by such an uncertain offer. (In any event, World War II intervened and the idea came to nothing.) The importance of a continued career structure for applied anthropologists, however, is not primarily to provide jobs, though security and advancement prospects for a research worker are powerful stabilizers. Scientifically, the importance of a career structure is that without assurance of reasonably long commitment anthropologists cannot plan for a proper analysis of a situation; they can't be sure that they will have a sound empirical basis for any practical suggestions they may make for improvement. Anthropological work is essentially long-term. Of course, short-term contracts sometimes have to be accepted for utilitarian reasons, and they may make good sense if an anthropologist is already familiar with the general field. But any public relations drive to promote applied anthropology must make clear the necessity of a long-term approach if solid work is to be done. If months or years can be spent on analysis of strength of materials in mechanical engineering, time should not be begrudged in analyzing the strength of relations in human engineering.

Until about thirty years ago, British applied anthropology showed a dichotomy of interests. In the field of general education it was concerned, as Malinowski was, with broad problems—ranging from more rational sex behavior to ethnic relations, intellectual freedom, and the dangers of war—applicable to contemporary Western conditions, as well as to those of Africa or New Guinea. But the study of specific problems of a practical order was conducted

largely on matters of administrative interest, in countries undergoing rapid change under Western pressures. The important work of the African and West Indian research institutes was much concerned with general welfare of the people, but much of it had government needs in mind. Recently, applied anthropology has focused its interests more sharply and diversified its range immensely. Western as well as Asian, African, or Oceanic societies have been brought into the field. Reasons for this include the shock given by World War II to many conventional Western values; more highly focused moral views about underprivileged sectors of society; a dawning recognition of the importance of social factors in conditioning the rate and quality of technical progress; some skepticism as to the predictive powers of leading social sciences such as economics; a transference of responsibility from a notion of evil as a quality of individuals to evil as an outcome of some features of social structure. A result has been a more acute identification of problem in the anthropology of crisis and disaster, development anthropology, ethnic relations anthropology, industrial anthropology, medical anthropology, nutritional anthropology, and the range of other diverse interests covered by this impressive conference. I am not competent to comment on many of these developments, but perhaps I may refer briefly to my own personal experience with some of them, before making a few more general points.

My own reaction to problems of applied anthropology has not been one of detachment. But it has been one of engagement rather than of commitment—a conviction of the importance of the problems rather than assurance as to the nature of the solutions or of my own role in relation to them. Probably this is partly a matter of temperament, partly a result of early environment. New Zealand, when I knew it as a young man, was in a late colonial phase—it had recently become an autonomous dominion with an established White population well spread through the country, but with a significant Maori social, economic, and political presence. New Zealanders were used to seeing Maoris in positions of authority—elected members of Parliament, Ministers of the Crown, and for academics like myself, having as a leading anthropologist and most able public speaker Te Rangihiroa or Peter Buck. (This man, son of an Irish father and Maori mother, was a medical man, a superb technologist of Polynesian material culture and later Director of the Bernice P. Bishop Museum in Honolulu.) But it did not take much sensitivity to become aware that behind this thin line of public figures stood a very vulnerable Maori community, in minority status, with greatly diminished lands and difficulties in land use, threats to their language from within and without, and tribal jealousies hampering or channeling their cultic and nationalistic movements. So when I became an anthropologist I was already conditioned to looking at the effects of Western technology, economic enterprise, and political pressures on traditional communities. I could see the dynamic possibilities of reaction among these, in a largely Western type society. In fieldwork in Tikopia and later with Rosemary Firth in Malaya, as well as in brief survey visits to West Africa, southeast Asia and New Guinea (e.g., Firth 1947, 1948a), I was impressed by the importance of questions of adaptation and viability of communities in modern conditions. Conferences and committees of United Nations agencies, the Rockefeller-sponsored Agricultural Development Council, the British Medical Research Council, and secretaryship of the Colonial Social Science Research Council, reinforced my interest in relating anthropology to practical problems, both in teaching and in research.

But I have long felt that there can be different degrees of absorption in a problem. In my own case, while I made some tentative observations on work situations, abroad and in this country (1948b), soon after World War II I found it hard to pursue this issue. This was mainly because of pressure of other interests. But it was partly because of some unease about the nature

of the industrial structure in which one would be involved—was one supposed to be making a contribution to industrial welfare or to profits? Or, putting it another way, how validly could one accept a reassurance that they coincided? But people have different ways of perceiving such ambiguities, evaluating their importance, and resolving them. I remember my surprise on an occasion now passed into history, when a group of social scientists were discussing possibilities of research with representatives of the Medical Research Council at a conference called by the sympathetic secretary of the MRC, Sir Harold Himsworth. Then a leading anthropologist said he thought he could cooperate more easily with industrialists than with medical men. The immediate effect, in the medical context, was disconcerting. Yet this was Max Gluckman, no defender of capitalism, and in any event, with his stimulus a series of very successful studies of British industrial conditions emerged. But I thought then, as I think now, that whatever be the hierarchical or other institutional attitudes of some of the medical profession, the ultimate values of relief of human physical and mental suffering are shared with medicine by anthropology. My experience has shown that there are always enough mavericks among the great group of doctors to invite help from and offer warm cooperation to anthropologists. After what Loudon has called “those distinguished and formidable trail-blazers”—Audry Richards and Margaret Read, with Margaret Field—it was not hard for Rosemary Firth (1978) to help Malay peasant women take advantage of clinical services, or for me to collaborate with John Brotherston and Margot Jeffreys in seminars on anthropology and public health, or to take part in WHO conferences on population genetics or child mental health. But in all this I have felt that it was unrealistic to expect every anthropologist to look at these practical issues in the same way, to be prepared to adopt the same assumptions, and to supply a similar input of energy. One must expect a range of individual commitment, just as one expects variation in skill in analyzing and evaluating situations, or in putting forward solutions to difficulties. Far more than in theoretical anthropology, in applied anthropology the personality, temperament, and operational skills of the individual are immediately relevant to the interpretation of issues and implementation of proposals for improvement. Putting it crudely, it is an anthropologist, not anthropology, that is applied. I think this is a crucial point to stress in an operational context.

But to get back to a more abstract level, if we look at applied anthropology not in terms of its branches and subject matter but in terms of major functions to be fulfilled, there are several types of tasks that historically have been performed.

One type of task, episodic, and a product of peculiar local conditions, is the performance of some special service to a people who are the subject of anthropological study. This may be of a material kind, even spectacular—such as reporting and analyzing a situation of disaster and helping to ameliorate its effects. I think here of a time in 1952 when I and my colleague James Spillius notified the government of the Solomon Islands by radio of the near-famine condition of the Tikopia after a hurricane, and at government request undertook the distribution of relief supplies of food in a way which took account of Tikopia values and social structure by consultation of local opinion. Aid to societies in crisis, as the Royal Anthropological Institute has realized, offers an important action role for an anthropologist, and a worthwhile opportunity for public service. But as I realized from my Tikopia experience, it can impose great strains on the anthropologist in witnessing human misery, which can only be partly alleviated, and personal factors again are relevant here. Moreover, such activities alone, if seen by the outside world as romantic or sensational, may distort the anthropological picture. At a more mundane level, many anthropologists have performed other useful service for a community—as Spillius (1957) did for the Tikopia in acting as mediator among them, the government, and plantation interests when



they wanted to take fuller advantage of the labor market. Aspects of development anthropology and industrial anthropology often fall within this specific service category.

Much more diffuse, but still, I think, an undoubted service as interpreted by the people themselves, is the way in which anthropological publications about a community have helped to interpret them to the world outside, to promote their cultural significance, to explain to others a people's own concepts of their history, their social structure, their cherished values and symbols. There may be drawbacks to this. If an anthropologist is insensitive, he or she may not be merely explaining but actually creating a cultural identity for the people externally, just as internally he or she may be furnishing a novel alien type of historical and cultural record. Crystallization of disparate oral traditions into a unitary written account may be a disservice to some sectors of the community. Moreover, the anthropologist's record of the people's culture, valuable as they estimate it, may have to be balanced against a long-term need for their own ethnographers and historians, so that the people feel that they themselves have an integral part to play in the definition of their own past and future. Criticisms of this kind, accompanied by accusations of distortion of evidence and exploitation of informants, have been made by members of some societies studied by anthropologists, and one effect of this should be to make anthropologists more keenly aware of their responsibilities to the people among whom they work. And anthropologists who test their findings against the opinions of the people studied would do well not to expect gratitude. The perennial question stands: Which gives more accurate results, local knowledge with local bias, or alien observation without deep experience in the situation but possibility of more detachment? Whatever the answer may be, I think it no accident that, for example, Phyllis Kaberry was given a title of honorary Queen Mother by the Fon of Nso,<sup>4</sup> and Schapera has had two streets named after him in Gaberones, the Tswana capital. But this may mean a long-term, perhaps a lifetime engagement with the people.

The most testing exercise of anthropological skill is the conduct of a client-oriented piece of research, usually marked by the isolation of a specific problem and the production of specific recommendations on how to deal with it. Many examples, in different fields, and of different grades of intensity, have been produced during the last decade or so, and some are before us at this meeting. To cite only a few would be invidious. But here we get to the core of the argument about applied anthropology—can it give straight answers to blunt questions of the order, What do I *do* now? Rarely, I think, can an answer be given without qualification, and this may be disappointing, even irritating to someone who hoped for a simple clear guide to action. Lord Halley said ruefully many years ago, "When you ask an anthropologist for guidance on land tenure, he is likely to give you a lecture on lineage systems." Analogous situations have been commonplace in the experience of many of us. Of course some anthropologists have been too doctrinaire—unwilling to learn the language of the technicians among whom they work, too rigid to see the problem from the practical person's angle as something demanding a decision and action, perhaps too concerned with disturbance to the few to envisage the benefits to the many.

Granted all this, working anthropologists know that while the descriptive and analytical part of a job is usually well within their competence if they are given adequate time, any business of making positive suggestions about how to get people to change their behavior is much more chancy. In the most prestigious of the social sciences, economists have discovered this by now, and are apt to be quite cautious in going beyond the controlled area of model construction. The economists' interest in what I believe is called "rational expectations theory" is pretty abstract, and it seems to be admitted that it is not known how far this theory really describes any actual human behavior, and if so, whether such behavior is general to all human beings or is fairly

closely culturally determined (Simon 1980). Charting the formation of expectations in actual life in a given society, and how this bears on economic decisions in particular social circumstances is a field for applied anthropology—one of the most recent exercises is Mary Douglas’s study of consumer patterns in *The World of Goods* (1979). But while many projects in recent years have mentioned what they refer to vaguely as human or social factors, far too few of them have realized just how difficult social technology is, as compared to material technology.<sup>5</sup> That anthropologists tend to identify problems rather than propound solutions has often been pointed out, by ourselves as well as by our critics. I think we must accept this position, not apologetically but firmly; that we should say straightforwardly that an important part of an anthropologist’s job is to expose the difficulties, the contradictions, the conflicts of interest in a situation in order that false hopes of easy solutions should not mislead. This caution may not increase our popularity. As in times past, economics was sometimes called “the dismal science,” so anthropology may become “the uncomfortable science” if it identifies human factors in ways people do not expect. But if we cannot become popular we may at least gain respect.

I can illustrate now what I mean by the uncomfortable science. A field in which anthropologists have had much experience in cultures other than their own is what I might call anthropology of paradox—the field of incompatibilities, between ends and means, between what is said to be the rule and what is done, between contrasting conduct in different contexts. It is a field that anthropologists can cultivate in this country. Fifty years ago a cultured, witty Hollander, Dr. G. J. Renier, published an ironical study of contradictions in the English national character under the title “*The English: Are They Human?*” His general answer as far as English men of the middle class were concerned, with their ritualistic conceptions of life, was rather negative, though he thought that the new freedoms of the 1930s gave some hope that they might soon join humanity. Renier ranged over law, religion, blood-sports, public schools, and attitudes to sex, all of which have contradictions that could be subjects for anthropological enquiry. But for me one of the clearest cases for “paradoxical anthropology” is one that Renier did not touch: the consumption of alcohol, in what is euphemistically called “drinking.”

The ravages of alcohol among Polynesian populations in the past century are well-known, and have recently begun to be apparent even in such a remote society as Tikopia. But alcohol consumption also presents many problems in our modern Western society, indeed almost everywhere in the world except the stricter Muslim countries. The consumption of alcohol in its various decorative forms can be esthetically gratifying, relaxing, a social emollient, a great solidarity factor. Its production is a major industry with international connections, a great employer of labor, a huge spender in advertising and public relations, an influential lobby in the political field. At the ritual level, alcohol is an element in the Christian communion of all but the more rigorous nonconformist sects. Yet alcohol is a drug, and one for which most people have very limited tolerance. Taken in excess it is highly dangerous to health, sapping the physique and responsible for or at least implicated in a range of social disorders, from family disruption and football hooliganism to injury and death of thousands of people in what are weakly called “accidents” on the roads. Is it not a logical absurdity that ethyl alcohol is not included under the Misuse of Drugs Act of 1971? Is it not odd that tobacco, which damages primarily only the smoker, is labeled officially in Britain with health warnings (in advertisements of bizarre invitation) while alcohol, much more widely lethal, bears no public message of its menace? Years ago, J. N. Morris, himself with great achievements in social medicine, said to me “Why don’t you anthropologists study smoking?” Now I am glad to see sessions of this conference devoted to the subject of the use and abuse of alcohol. The problem lies far deeper than abuse—it

lies in the complex defense mechanisms put forward by large sections of the alcohol-using population who may rarely if ever drink to excess, to justify what is socially speaking a balancing tightrope kind of activity which distracts attention from the more tragic performances. But one thing emerges very clearly here. Whatever motivations of sociability, male virility, ritual license, and consequences of social disruption and unhappiness may be exposed by the systematic enquiry of an anthropologist, little of this by itself is likely to alter British alcohol drinking behavior. One reason for this is the general if vague notions held by many people that the situation does not call for modification—that alcohol-linked broken homes and deaths on the roads are part of the price we pay (or others pay) for the right to choose our pleasures and receive the satisfactions of those who do not drink to excess. Many anthropologists may share this view. Another reason is the compartmentalism whereby most of us live only in the positive, benign area of the “social glow” of alcohol consumption, and never encounter the negative areas which I have mentioned. So in what is admittedly a set of very serious social problems (even more so in socialist Russia it appears than in capitalist Britain), the nature of the problem is such that no general solutions are likely to be secured from anthropology, however highly applied. But a good old-fashioned functional study would still be illuminating.

Certain inferences are suggested by this. One is that an important function of applied anthropology may be just to help to form public opinion on a subject. By dispassionate analysis we try to get people to recognize the difficulties, inconsistencies, and verbal smoke-screens in a situation and interpret them in terms of intelligible social relations and values. Twenty years ago Audrey Richards wrote that in Africa the creation of a climate of opinion was probably a more important contribution to government policy than any series of answers to specific questions. I think that the work anthropologists are doing within and outside Britain in regard to health care is a worthwhile effort of this kind. One would like to hope too that anthropological studies of ethnic situations in Britain, from the days of Kenneth Little’s pioneering book, *Negroes in Britain*, may in time help to produce a more enlightened climate of opinion. Though we may sometimes get despondent, anthropological attention to social problems is being helped forward in this country, through semipopular books, radio, TV, and films. The Royal Anthropological Institute in particular has devoted much care to organizing approaches of this kind, including the very difficult question of teaching anthropology in schools.

But whether we look to solve specific problems or to generate a climate of opinion about a subject, there are features of the situation that cannot be avoided. One is that some immediate parameters to a problem may be beyond anthropological control. A clear case in point is presented by the predatory economic and political forces that are destroying the Amazonian forest, and that severely limit any anthropological efforts to save the cultures, perhaps even the lives, of the Amazonian Indians. A more specific case, which I heard of recently, was a proposed massive irrigation scheme in Malaysia, involving movement of population and opening up of new farm land. Who would get the new land was clearly going to be arranged not on anthropological advice but at a high political level where the interests of an old aristocracy, modern men of wealth, bureaucrats, and party politicians would jostle one another, with those of the landless peasantry low down the list. If anthropologists should protest in situations where politics are dominant, they may alienate themselves from influence of any kind. We know that issues of power-holding and privilege may crop up in any type of problem, whether it be the operation of a hospital, a factory, or a welfare scheme in an urban neighborhood, and in any type of society, whether one is working in a capitalist milieu or that of a socialist state. And this means that at some time anthropologists will have to decide whether to work within the system

or to oppose it. There are no ready-made solutions—at least none that an analytical social anthropologist should accept without question. Again I come back to the burden of individual decision that lies upon so much engagement in applied anthropology. I think it cannot be avoided, but its importance should not be muffled.

A resolution of such issues in a way that accords with certain temperamental leanings is to adopt the concept that there is no single best solution of a practical problem, but only some compromise between conflicting interests. I have hinted at this in various contexts (Firth 1938:511–12; 1964 [1949]:143, 1964 [1955]:74, 80, 86); Nadel said it clearly after his war-time experiences in North Africa (1953:10); and Liza Peattie’s analysis of “advocacy planning” (1968) states it very frankly. Peattie points out how a plan can be conceived in terms of accommodation between pluralistic partisan interests of particular groups, and not as a search for general welfare. This seems to me to be a healthily skeptical approach, even if its skepticism may make too little allowance for some symbolic sacrifice by some elements in the situation.

Even this view allows for some detachment by the analyst. It is assumed, in my view correctly, that the training and motivation of an anthropologist does not lead him or her to look at a piece of research simply as an extension of self-interest and professional advantage. There is some element of projection of the issues in wider terms of public interest, however this may be conceived. Applied anthropologists must be engaged with the problem, must think it worthwhile, nontrivial in social terms, and not just an intellectual game. But they must also view it with enough detachment to be willing to trace out the implications of their findings even if these prove unpalatable to their expectations or prejudices—or to the views of those who employ them. And at the back of their mind must always be the old question of the lawyers: *Cui bono?*—for whose good is the research ultimately being conducted?

Here a link with the more theoretical aspects of our discipline may help to keep up standards. It has sometimes been said that anthropologists are living in an ivory tower, with no connection with the world of reality. Even if the idea has some basis, the metaphor is misconceived. The original ivory tower reference, as I trace it, was made by the French literary critic C.-A. Sainte Beuve nearly 150 years ago, in a poem of friendship with some lines addressed to Alfred de Vigny. Referring to the invigorating effect of Victor Hugo, de Vigny, and others on French poetry of the day, Sainte Beuve described Hugo as powerful and strong, a tough partisan, and Vigny as careful and subtle—“plus secret, comme en son tour d’ivoire, avant midi, rentré” (1869[1844]:286–87). In a later collected edition of his poems Sainte Beuve, evidently proud of his phrase, noted that this “ivory tower” idea had become inseparable from the name of Vigny. But if Vigny did retreat as into his ivory tower, it was not to shut himself off forever from the world and its problems; on the contrary, it was to reequip himself for struggle on the literary and political scene. Often deceived in his political ambitions, Vigny was deeply interested in social and political questions, in ideas of romantic suffering and sacrifice, in symbolic justification of the role of the unquiet man in modern times. He denounced the malaise of modern society, he saw in the poet an opposition between concepts of power and of art, and he regarded contemplative intelligence as ultimately in the service of humanity. He sought solitude, but never ceased to interest himself with passion in the problems that affected the future of people of his kind (Castex 1952:7 etc.). For an anthropologist too, an ivory tower can be a retreat for refreshment, thought, and reemergence; a vantage point for survey of the field in perspective and without emotion; and a fortress in which to store a stock of the resources of theory for inspiration and defense of more practical interests.<sup>6</sup>

Even that apparently remote “ivory tower” subject, the theory of kinship, may have significance for practical work. Many studies in modern British society have shown the supportive role given to people by kin outside their nuclear family, and the way in which practical decisions can be affected by kin ties. In situations as diverse as the birth of children, the care of geriatric or chronically ill patients, the choice of a job location, or the response to earning incentives, kin factors may be significant, and the modern theory of cognatic kinship may suggest fresh lines of enquiry.

I conclude with a few very broad remarks. It is increasingly clear that anthropology, like other intellectual disciplines, is being called upon more overtly to accept a notion of social responsibility. If anthropologists are to be supported from public funds they must expect to be asked for some recognizable contribution to public benefit. The intellectual climate of knowledge is changing, perhaps irreversibly, moving from the assumption that scientific enquiry can be justified as an esthetic pursuit for its own sake.

While we are convinced that our studies can ultimately be of public benefit, albeit in diffuse ways, we find it hard to get this generally recognized. So, as has often been emphasized, we need to focus our work more on social problems, and on communication with those already engaged on such problems, as well as with the general public.

But the very nature of anthropology as an inquisitive, challenging, uncomfortable discipline, questioning established positions and proclaimed values, peering into underlying interests, and if not destroying fictions and empty phrases as Malinowski said, at least exposing them—all this poses difficulty for its application to practical problems. I think we have to accept this. Blaming the apathy of the general public, the ignorance of the client, or the remoteness of some of our colleagues will not help us. Nor will we get automatic salvation through the adoption of slogans like “working for the people.” Tough questions are involved. How do we present an issue to the public in terms that are imaginative, relevant, and memorable without distortion or sensationalism? What structural overburden of custom can be removed from a people’s life to improve their lot without destroying much of what makes their lives worth living? In the state of society in which we work, how legitimate do we think the authority structure is? What personal engagement is feasible and tolerable if we are to preserve our intellectual standards of judgment? Through all this run questions that we as individuals have to face on practical, scientific, and moral grounds.

This talk suggests no easy way out of our dilemmas. I have had no new research results to offer, no new methods to promote. If I have a message it is that the title of this conference should apply not just to a periodic reunion but to a continuing process. In the changing conditions of the modern world we have to keep rethinking our basic issues all the time.

### Notes

<sup>1</sup> In a general address of this kind I have thought it appropriate to limit specific references to literature.

<sup>2</sup> See the commentary from various angles by Loizos et al. (1977) and Firth (1972:26–27; 1977).

<sup>3</sup> Since Radcliffe-Brown was one of the founders of social anthropology and possibly the first to use publicly the term “applied anthropology,” it is worth noting that the centenary of his birth occurred this year, on January 17.

<sup>4</sup> Mrs. E. M. Chilver has told me that after she communicated to the Fon of Nso the news of the death of Phyllis Kaberry—Ya-o-Kof, the Lady of the Forest of the burial place of kings—an elaborate funeral commemoration was held. As part of this, the traditional masks of the Nso regulatory society, the princes’ society, and the war society (including the rarely seen royal “terror-mask” symbolizing the forces of nature) all “came out” in mourning.

<sup>5</sup> In a recent discussion of “social econometrics” Richard Stone has pointed out how difficulties have arisen from inconsistency of data available on human stocks, and total lack of data on human flows (1980:731). More understanding of “human flows,” e.g. the dynamics of social mobility, of marriage choices, of job selection, may be possible through anthropological research.

<sup>6</sup> In a communication to *Nature* from Sao Paulo, Brazil, in 1944 Radcliffe-Brown wrote: “without a sound basis in theory applied anthropology must deteriorate and become not applied science but merely empirical practice.”

### References Cited

Castex, Pierre-George

1952 Vigny: L’Homme et L’Oeuvre. Paris: Boivin et Cie.

Douglas, Mary, and Baron Isherwood

1979 The World of Goods. New York: Basic Books.

Fenton, F. D.

1879 Important Judgments Delivered in the Compensation Court and Native Land Court 1866–1879. Auckland: Chief Judge, Native Land Court.

Firth, Raymond

1931 Anthropology and Native Administration. *Oceania* 2:1–8.

1938 Human Types. London: Thomas Nelson and Sons.

1947 Social Problems and Research in British West Africa. *Africa* 17:77–91, 170–180.

1948a Report on Social Science Research in Malaya. Singapore: V. C. G. Gatrell, Government Printer.

1948b Anthropological Background to Work. *Occupational Psychology* 22:94–104.

1964 Essays on Social Organization and Values. London School of Economics Monographs on Social Anthropology no. 28. London: Athlone Press.

1972 The Skeptical Anthropologist? Social Anthropology and Marxist Views on Society. London: The British Academy.

1977 Whose Frame of Reference? One Anthropologist’s Experience. In *Anthropological Research in British Colonies: Some Personal Accounts*. P. Loizos et al., eds. Anthropological Forum.

Firth, Rosemary

1978 Social Anthropology and Medicine—A Personal Perspective. *Social Science and Medicine* 12B:237–245.

Fustel de Coulanges

1873[1864] The Ancient City. A Study on the Religion, Laws, and Institutions of Greece and Rome. Willard Small, trans. Garden City, N.Y.: Doubleday.

Haddon, A. C.

1921 The Practical Value of Ethnology. London: Watts & Co.

Hogbin, H. Ian

1957 Anthropology as Public Service and Malinowski’s Contribution to it. In *Man and Culture: An Evaluation of the Work of Bronislaw Malinowski*. R. Firth, ed. London: Routledge & Kegan Paul.

Howison, James

1801 A Dictionary of the Malay Tongue. London: Arabic and Persian Press.

Little, K. L.

1947 Negroes in Britain: A Study of Racial Relations in English Society. London: Kegan Paul, Trench, Trubner.

- Loizos, Peter, et al.  
1977 Anthropological Research in British Colonies: Some Personal Accounts. *Anthropological Forum* 4:137–248.
- Maine, Henry Sumner  
1887[1876] *Village-Communities in the East and West*. London: John Murray.
- Mair, Lucy  
1957 *Studies in Applied Anthropology*. London School of Economics Monographs on Social Anthropology no. 16. London: Athlone Press.
- Malinowski, Bronislaw  
1936a Native Education and Culture Contact. *International Review of Missions* 25:480–517.  
1936b The Deadly Issue. *Atlantic Monthly*. Dec.: 659–69. 1940 *Modern Anthropology and European Rule in Africa*. Atti dell' VIII Convegno 'Volta' 4–11 Oct., 1938. Reale Accademia D'Italia 18:6–26.  
1947 *Freedom and Civilization*. London: Allen & Unwin.
- Morgan, Lewis H.  
1851 *League of the Ho-Dé-No-Sau-Nee, Iroquois*. Rochester: Sage & Brother.
- Nadel, S. F.  
1953 *Anthropology and Modern Life*. Canberra: Australian National University.
- Peattie, Lisa  
1968 Reflections of an Advocate Planner. *Journal of the American Institute of Planners* 34:80–88.
- Radcliffe-Brown, A. R.  
1923 The Methods of Ethnology and Social Anthropology. *South African Journal of Science* 20:124–147.  
1931 Applied Anthropology. *Proceedings of the Australian and New Zealand for the Advancement of Science*, 20<sup>th</sup> meeting, Brisbane 1930:267–280.  
1944 Meaning and Scope of Social Anthropology. *Nature* 154 (3904):257–260.
- Renier, Dr. G. J.  
1945[1931] *The English: Are They Human?* London: Williams & Norgate.
- Sainte Beuve, C.-A.  
1869 *Pensées d'août* (collected 1837, rev. 1844). In *Oeuvres de C.-A. Sainte Beuve: Poésies Complètes* tom. 2. Paris: Alphonse Lemerre.
- Simon, Herbert A.  
1980 Testimony at National Science Foundation Hearings, Items 34:6–7. New York: Social Science Research Council.
- Spillius, James  
1957 Polynesian Experiment. *Tikopia Islanders as Plantation Labour*. *Progress (Magazine of Unilever)* 46:91–96.
- Stocking, George W., Jr.  
1971 What's in a name? The origins of the Royal Anthropological Institute (1837–71). *Man N.S.* 6:369–390.
- Stone, Richard  
1980 Political Economy, Economics and Beyond. *The Economic Journal* 90:719–736.

Temple, Sir Richard C. Bt.

1914     Anthropology as a Practical Science. London: G. Bell & Sons.



## **Chapter 12**

### **George M. Foster: Medical Anthropology in the Post-World War II Years**

**Thomas Weaver**

After the Second World War anthropologists were very much concerned with culture history and topical interests such as religion and kinship, acculturation, functionalism, and community studies, and most followed the “four fields” approach (advocating training in the four subfields of cultural anthropology, physical anthropology, archaeology, and linguistics). In spite of the valuable contributions made by anthropologists in the Bureau of Indian Affairs under John Collier (1932–1945) and during the Second World War, applied anthropology was not considered a viable career option. Application has always been considered less prestigious than basic ethnological and theoretical work, and most anthropologists were drawn to academic posts by increased employment opportunities in the rapidly expanding American university system. In this context, it is clear why the Institute of Social Anthropology, of which George Foster, recipient of the Society for Applied Anthropology’s Malinowski Award in 1982, was director from 1946 to 1952, was not concerned with application.<sup>1</sup>

Foster (1913–) received a bachelor’s degree in 1935 from Northwestern University where he studied with Melville Herskovits and first met Bronislaw Malinowski. After receiving his Ph.D. from the University of California at Berkeley in 1941, Foster taught at Syracuse University and the University of California at Los Angeles. In 1943 he went to work for the Institute of Inter-American Affairs (IIAA) for four months and then for almost ten years for the Institute of Social Anthropology (ISA). The IIAA was the model and forerunner of USAID, and the ISA was part of the U.S. government’s Latin American cultural relations program. The first director and founder of the ISA, Julian Steward, understood its goals to be “training Latin American social scientists and providing sociocultural information for Latin American governments to use in their postwar development programs” (Foster 1982:191). As director of the ISA, Foster formed ties with the IIAA’s Health Division. Although ISA staff members were not primarily concerned with application, they carried out cooperative studies with IIAA personnel that significantly influenced the design of international health programs. During this period Foster also taught at the Escuela Nacional de Antropología e Historia and began his fifty-some years of research in Tzintzuntzan.

Foster’s initial interest in the application of anthropology was driven by economic need rather than the desire to serve society. In his Malinowski Award lecture, entitled “Applied Anthropology and International Health: Retrospect and Prospect” (1982), he related how his principal teachers—Herskovits, Alfred Kroeber, and Robert Lowie—“had successfully

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inoculated me against the germ of applied work.” By 1950, however, the U.S. government had lost interest in Latin America, and Foster received word that the State Department would no longer fund the ISA. “This news constituted my first lesson in directed culture change: nothing encourages one to reexamine fundamental premises more rapidly than the threat of economic disaster” (Foster 1982:191 and personal communication, February 8 and July 7, 1998.). He focused the final year of the ISA on surveying health problems in Central America for the Institute of Inter-American Affairs. After the ISA was terminated in 1952, Foster joined the University of California at Berkeley as a faculty member and became one of the early contributors to applied (1969, 1973) and medical anthropology (1978). His major publications also include longitudinal studies of Tzintzuntzan, as well as studies of peasants (1973) and the classic “image of limited good” (1965).

Foster’s Malinowski Award address constitutes a superb overview of the development and status of international health programs and an outline of the history of early medical anthropology. In spite of advances, Foster lamented the small number of anthropologists involved in international health programs in 1982. This has changed dramatically in recent decades, but some of the problems he identified remain. A continuing problem is the lack of institutionalization of medical anthropology in international agencies. Anthropologists may be hired on short-term contracts, but they are not included as long-term employees of the agencies. This is in part due to, and in turn contributes to, the absence of sponsors or brokers in agencies who call attention to the importance and potential application of anthropology and become instrumental in hiring anthropologists. This is a role performed by Louis Miniclier in USAID in the 1950s and 1960s (Langley 1980–81) and by Michael Cernea, another Malinowski Award winner (1995) at the World Bank in recent decades. The importance of having sponsors for anthropologists within agencies has also been recognized in international agriculture programs (Rhoades 1986).

One impediment to more frequent employment of anthropologists in international agencies is the generally low status and reputation of anthropology in the eyes of government and international agencies. Foster was convinced, however, that the role of anthropology could grow.

[I]nternational health organizations largely view the role of anthropologists as that of short [term] consultants for specific projects, as committee members, and as workshop participants. Less frequently, they participate in teaching programs, in the evaluation of health behavioral science resources in Third World countries, and in organizing meetings to consider behavioral science research on health problems. It is on tenuous bases such as these that we must build if we wish to play a more important role in international health programs (Foster 1982:193).

Foster provided some interesting observations on what international agencies expect of anthropology and what anthropologists can offer in turn.

In his acceptance address, Foster also commented on the constant accusation that applied anthropology does not contribute to anthropological theory. He countered by observing that much of culture change theory has come from the materials collected through applied anthropology. He also argued that applied anthropology has provided, if not the grand theory with which anthropology has been so obsessed, a “middle range” theory specific to the medical field.

George Foster has been president of the American Anthropological Association and recipient of its Distinguished Service Award. He is a member of the National Academy of Sciences and of the American Academy of Arts and Sciences. He was also a Guggenheim Fellow and a fellow of the Center for Advanced Study in the Behavioral Sciences, and he has received an honorary doctorate from Southern Methodist University.

### Notes

<sup>1</sup>This annual award is given to a senior colleague in recognition of efforts to understand and serve the needs of the world through social science. See Thomas Weaver's "The Malinowski Award and the History of Applied Anthropology" (2002b) for a brief history of this award and an overview of the recipients and their work, and Weaver's "Malinowski as Applied Anthropologist" (2002a) for an introduction to Bronislaw Malinowski and his work.

### References Cited

- Cernea, Michael  
 1995 Social Organization and Development Anthropology. *Human Organization* 54(3):340–352. (Republished at <<http://www.sfaa.net/>>.)
- Foster, George M.  
 1965 Peasant Society and the Image of Limited Good. *American Anthropologist* 67:293–315.  
 1969 *Applied Anthropology*. Boston: Little, Brown and Company.  
 1973 *Traditional Societies and Technological Change*. Second Edition. New York: Harper & Row, Publishers.  
 1983 *Applied Anthropology and International Health: Retrospect and Prospect*. *Human Organization* 41(3):189–197. (Republished at <<http://www.sfaa.net/>>.)
- Foster, George M., and Barbara G. Anderson  
 1978 *Medical Anthropology*. New York: John Wiley.
- Langley, Grace  
 1980–81 The Uses of Social Scientists in the United States' Foreign Aid Programs: A Retrospective. *Practicing Anthropology* 3(2):9–10, 54–55. (Republished in *Classics in Practicing Anthropology: 1978–98*. P. J. Higgins and J. A. Paredes, eds. Pp. 51–55. Oklahoma City: Society for Applied Anthropology, 2000.)
- Rhoades, Robert  
 1986 Breaking New Ground: Agricultural Anthropology. *In Practicing Development*. E. C. Greed, ed. Pp. 22–66. Boulder: Westview Press.
- Weaver, Thomas  
 2002a Malinowski as Applied Anthropologist. *In The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers*. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.  
 2002b The Malinowski Award and the History of Applied Anthropology. *In The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers*. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.

## **Applied Anthropology and International Health: Retrospect and Prospect**

**George M. Foster**

It is a high honor indeed to be the recipient of the Tenth Malinowski Award, thus to be associated with the names of Bronislaw Malinowski and of my nine distinguished predecessor recipients. When Peter New advised me of my selection, I experienced mixed emotions: first, pleasure and appreciation, and second, concern as to my topic. Fortunately, colleagues came to my rescue; several suggested that I talk about the involvement of anthropologists in international health programs, a field to which I have been exposed for more than 30 years. Hence, the title of this address. Specifically, I propose to note the early events that drew anthropologists to the international health field, to take stock of where we are today, and to suggest the opportunities and problems we may encounter in the future development of the field.<sup>1</sup> To limit my remarks to reasonable length I exclude mental health and family planning, and restrict myself largely to American anthropology.

I use the term “international health programs” to denote those national and international efforts designed to improve the health care services of peoples who, until recent years, have relied largely or entirely on indigenous medical resources. These include most of the people, both urban and rural, in developing countries, and substantial numbers of minority ethnic groups in some industrialized countries. The common characteristic of these programs is that they are cross-cultural: historically, health care providers and health care recipients have represented distinct cultural, social or ethnic groups. Whatever the setting, the basic problems associated with planning and delivery of health care, and utilization of services, are essentially the same.

A generation ago physicians and anthropologists alike defined the problem of providing better health care for traditional peoples as how to persuade them to accept scientific medicine. As anthropologists, our fundamental research task was to learn those things about the health beliefs and practices of these peoples that would enable health care providers to convince them of the superiority of modern medicine over their traditional therapies. Our practical goal was to further effective cross-cultural communication which, we assumed, would lead to scientifically acceptable health beliefs and behavior.

Today the goals of international health programs are more realistic, namely to determine and develop appropriate forms of health care that will meet the diverse needs and expectations of the world's peoples. While it is still assumed in medical circles that scientific medicine will be the cornerstone of such programs, it is now respectable—even fashionable—to voice opinions no one would have dared utter a generation ago. Particularly in the context of primary health care programs, the roles of traditional therapists and therapies are being seriously discussed and, in one medical subspecialty—midwifery—major use is made of traditional birth attendants in many national programs. Almost without exception, World Health Organization and Agency for International Development reports stress the need to understand the sociocultural aspects of

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health and illness behavior, to learn about the dynamics of “community participation” in health care programs, and to incorporate behavioral scientists into health research projects. On the surface, at least, it looks as if the time is propitious for anthropologists to play an increasingly important role in international health programs. Time will tell whether this will prove to be the case.

### **Some Origins of International Health Programs**

International health programs long antedate applied anthropology. By the middle of the 19th century European and American medical missionaries had begun to cater to the needs of body and soul of non-Western peoples. Most missionary work has been curative rather than preventive. In the beginning this failure to include preventive measures may have reflected the embryonic status of public health in the last century. Except for smallpox vaccination, effective means to prevent most illnesses were not developed until about three generations ago. But it is also tempting to speculate that just as it is more satisfying to “save” a repentant sinner than to prevent someone from falling into sin, so is it more gratifying to save a seriously sick patient than to prevent that patient from falling ill.

At a government-to-government level, international health concerns were first reflected on a major scale in the International Sanitary Conferences, the first of which was held in Paris in 1851. In contrast to missionary activities, the concern of European governments—the principal organizers of the Conferences—was preventive, to control the cholera epidemics which, beginning early in the 19th century, periodically threatened European port cities (Howard-Jones 1975). In the Americas the major international health threat was yellow fever. It was the desire to control this disease that led to the establishment of the first continuous international health organization, the International Sanitary Bureau, later the Pan American Sanitary Bureau, in Washington, in 1902 (Howard-Jones 1980). Prevention by means of environmental sanitation was the rationale that underlaid the United States government’s successful effort to eliminate yellow fever in Havana in 1901, and shortly thereafter in Panama, Rio de Janeiro, and other tropical port cities. Later the Rockefeller Foundation joined in international yellow fever and hookworm control efforts. Needless to say, anthropologists played no role in these programs, although, as a historical oddity, one may note the 1921 article by Philip Ainsworth Means on “Sociological Background of Sanitation Work in Peru” in the *American Journal of Public Health* (Means 1921).

### **Nutrition Programs**

The earliest role of anthropologists in the international health field appears to have been a function of colonial concern in the 1930s and 1940s with indigenous diet and health. By the mid-1930s both anthropologists and administrators in Africa realized that under the impact of wage labor and migration, indigenous diets had deteriorated while, at the same time, hard and continuous mine and plantation labor required a level of nutrition not always realized under preexisting conditions. As early as 1934 Sir Raymond Firth wrote on “The Sociological Study of Native Diet,” suggesting “a possible systematic field approach to the situation” (Firth 1934:402). In 1935 the International Institute of African Languages and Cultures established an interdisciplinary Diet Committee, whose members included Sir Raymond and Audrey Richards. The latter’s *Land, Labour and Diet in Northern Rhodesia* (A. Richards 1939) was one outcome

of the work of the committee. Although it is hardly a guide to improving indigenous diets, it was written in the context of developmental health problems and, in the author's words, had a "practical bent." Concern with dietary problems is shown in the work of other British anthropologists of the period, including Bronislaw Malinowski, who devotes an entire chapter to "Problems of Native Diet in the Economic Setting" in *The Dynamics of Culture Change* (Malinowski 1945).

In the United States the National Research Council set up a Committee on Food Habits in 1940; its initial members included Ruth Benedict, John Cooper, and John Province. Shortly thereafter, Carl Guthe joined as chairman and Margaret Mead as executive secretary. Although the immediate goal of the committee was to provide guidance on how best to nourish an America at war, the applied and international implications for nutrition policy were early recognized, particularly the problem of meeting food shortages in other countries after the war. The introduction to the committee's 1945 *Manual for the Study of Food Habits* makes this clear: "The study of food habits is an applied science to which many different sciences have to contribute in order to find answers to the questions that come up continually in our attempts to improve the nutrition and living habits of the peoples of the world" (National Research Council 1945:13). The applied science of food habits was seen, not as a static description of what people eat, but as a dynamic problem the goal of which was to learn how actively to intervene to change food habits in directions believed to promote better nutrition (National Research Council 1943:20). Although the committee sponsored no major cross-cultural research, and hence cannot properly be said to have been involved in international health, it made explicit the central theme of contemporary anthropological participation in international health programs: the process of directed culture change.

Oddly, this promising early collaboration between nutrition specialists and British and American anthropologists had little influence on succeeding developments involving anthropologists in international health programs. Writing in 1964, Margaret Mead said that after the war, the newly-established international organizations concerned with health—WHO, UNICEF and FAO—made only minor use of what anthropologists had learned about the problems of changing food habits (Mead 1964:4).

### **The Navaho Laboratory**

Health research and medical practice among the Navaho Indians played a much more important role than nutrition studies in clarifying problem conceptualization and in providing research guidelines for the budding medical anthropologists of the postwar period. Alexander and Dorothea Leighton began their research in 1940, and in 1944 published *The Navaho Door* (Leighton and Leighton 1944). This was the first major study to show in detail, and with abundant examples, the medical and behavioral problems inherent in the cross-cultural practice of medicine. This book profoundly influenced those of us who began research on international health programs in the early 1950s, as did Alice Joseph's pioneering study of White physicians and Navaho patients (Joseph 1942), the first to deal with how the cross-cultural practice of medicine affects the physician.

The Navajo-Cornell Field Health Project at Many Farms, Arizona, begun in 1955, went beyond these earlier studies in that it combined research and medical intervention to provide culturally more appropriate health care services (Adair and Deuschle 1958; McDermott et al. 1960; C. Richards 1960). Particularly innovative was the creation of the role of "health visitor," a

Navaho paramedic and health educator who may be viewed as the prototype of the “village health worker,” the mainstay in the delivery of primary health care in developing countries today (Adair 1960).

### Early Institutional Ties

Significant anthropological participation in international health work requires a formal tie to an organization whose primary responsibilities are in the international (or cross-cultural) field. It was not until such organizations—or more precisely, individuals in such organizations—began to hire anthropologists that the base was established for anthropology’s involvement in international health activities. It is from the first use of anthropologists in institutional contexts such as these that we must date this branch of medical anthropology. I believe Cora DuBois was the first American anthropologist to be hired by an international health organization. She joined the World Health Organization in 1950 for about a year. “I saw my role,” she has written me, “[as that] of an observer and consultant.” In other words, she did not plan to recommend specific courses of action. DuBois’s assignment was plagued by the problem that has afflicted many subsequent anthropological consultants: neither she, nor the hiring organization, really knew what she should do. Shortly after joining WHO she left for India and Southeast Asia where, in the regional office, her reception was unenthusiastic. “I was left to make my own plans and schedules and I was more than a little perplexed as to what was expected of me. . . . Much the same perplexity about my role obtained when I returned to Geneva” (communicated by C. DuBois).

Three separate and unrelated events that proved to be more propitious for anthropologists in international health took place shortly thereafter. Edward Wellin was hired by the Rockefeller Foundation to work in Peru, Benjamin Paul was hired by the Harvard School of Public Health, and Richard Adams, Charles Erasmus, George Foster, Isabel Kelly, Ozzie Simmons, and Kalervo Oberg, all of the Smithsonian Institution’s Institute of Social Anthropology (ISA) began their Latin American research for the Institute of Inter-American Affairs (IIAA). These developments came about as follows:

John L. Hydrick, an American public health physician, had spent some years in the Netherlands East Indies in the 1930s engaged in health education and rural hygiene work. He wrote what is probably the first “how-to-do-it” manual describing how the members of a traditional community should be approached by health workers, and how their interest in changing traditional health behavior might be elicited (Hydrick 1942). In 1950 he was working under Rockefeller auspices in Peru, where he found many of the same problems encountered in the Indies. At his request the foundation hired Edward Wellin, then a graduate student at Harvard. Wellin arrived in Peru in late 1950 and remained until 1954. His classic article on water boiling in the Paul casebook is one of the products of this Peruvian work (Wellin 1955; also 1953a, 1953b, 1958).

The antecedents of the Paul appointment are more complex. In 1930 Esther Lucile Brown, a recent Ph.D. in anthropology at Yale, signed on with the Russell Sage Foundation for a six-month assignment, an appointment that was to continue for more than 30 years. Foundation concerns at that time dealt with the professions, and it was this interest that led to Brown’s pioneering research on nursing (Brown 1936). In 1948 Donald Young, a distinguished sociologist, became president of the Russell Sage Foundation and launched a new applied program whose goal was to determine what anthropology, sociology, and social psychology

could offer to and learn from “social process” programs. Because of her experience in the field, he asked Brown to begin with health. Her assignment was to find out if schools of public health, nursing, and medicine would be interested in having behavioral scientists on their faculties. If such schools could be found, the foundation would help them locate appropriate scholars and initially pay their salaries with the understanding that the institutions gradually assume financial responsibility. The University of Colorado’s School of Medicine was the first to take advantage of this offer, with the appointment of Lyle Saunders, a sociologist, to the Department of Preventive Medicine and Public Health. The outcome of this appointment was *Cultural Difference and Medical Care* (Saunders 1954), a book that describes the problems inherent in the cross-cultural practice of medicine and recommends action to bridge the “cultural chasm” separating Anglo health care providers and Spanish American health care recipients (communicated by E. L. Brown).

Paul’s appointment was made possible by the same program. In the spring of 1951 he was approached by Hugh Leavell, Professor of Public Health Practice at the Harvard School of Public Health who, with Russell Sage support, asked him to join his faculty to introduce social science concepts and methods to graduate students. Paul’s association with the Harvard School of Public Health lasted from 1951 until 1962. The major product of this collaboration was, of course, *Health, Culture and Community* (Paul 1955), the single most widely read and influential book in all of medical anthropology. Although Paul did not see his appointment as specifically international, his own research experience in Guatemala, and the large number of foreign students at the Harvard School of Public Health, ensured that major emphasis would be placed on this aspect of health, as is evidenced by the fact that 12 of the 16 cases in *Health, Culture and Community* describe programs and conditions in foreign countries (communicated by B. D. Paul).

The entry of Institute of Social Anthropology anthropologists into international health came about in still another fashion. The institute, which had been established by Julian Steward in 1942 as a part of the United States government’s Latin American cultural relations policy, was financed by the Department of State. Steward’s justification for the institute was its potential applied value in training Latin American social scientists and providing sociocultural information for Latin American governments to use in their postwar developmental programs. Steward saw the roles of his staff members as teaching, conducting research with Latin American colleagues and students, and publication. But, in keeping with prevailing views about applied anthropology, he did not envisage the participation of staff members in specific development programs, nor were they to make policy recommendations.

I joined the ISA in 1943, spent two years in Mexico, and returned to Washington in 1946 to succeed Steward as institute director. Initially I left unchanged his policies and philosophy. My principal teachers—Herskovits, Kroeber, and Lowie—had successfully inoculated me against the germ of applied work and I was content to continue in a traditional course. But by 1950 the U.S. government had largely lost interest in Latin America in favor of new concerns in Africa and Asia. I was advised that 1951 would be the last year the institute would enjoy State Department funding. This news constituted my first lesson in directed change: nothing encourages one to reexamine fundamental premises more rapidly than the threat of economic disaster. So, after consulting my colleagues in Latin America, in January 1951 I approached officials of the Institute of Inter-American Affairs. The IIAA, the first American technical aid program of modern type, and the forerunner of AID, was established by Nelson Rockefeller in 1942 to carry out bilateral developmental projects in Latin American countries in health,



agriculture, and education. I proposed that we make analyses of some of the social and cultural aspects of the institute's health programs, especially of the barriers being encountered in introducing preventive medicine in traditional communities. Permission received, in the spring of 1951 Kelly, Erasmus, Simmons, and Oberg carried out analyses of eight health centers in Mexico, Colombia, Peru, and Brazil. In Guatemala, Adams simultaneously began research that led to "A Nutritional Research Program in Guatemala" in the Paul case book (Adams 1955a).

The findings of this research were issued in mimeographed form in July 1951 (Foster, ed. 1951), and they aroused a good deal of interest among professional public health personnel, including Henry van Zile Hyde, chief of the IIAA Health Division. Recognizing the potential contribution of anthropology to the solution of the problems his division was encountering, he asked ISA staff members to participate in a United States Public Health Service evaluation then being planned of the first ten years of IIAA bilateral health programs in Latin America. This evaluation, with the participation of Erasmus, Foster, Kelly, Oberg, and Simmons, was carried out from October 1951 to March 1952. As part of the agreement, the IIAA also financed the ISA during all of 1952, thus solving our budgetary worries for that year. The IIAA found the anthropological contributions to its programs to be so useful that it asked the ISA to become one of its divisions, a move that would have entailed leaving the Smithsonian Institution. This action was not taken, but Erasmus, Kelly, Oberg, and Simmons transferred to the IIAA as direct-hire U.S. government employees where they continued in developmental research for periods of from two to ten years. Some of the early seminal papers on anthropological aspects of international health programs resulted from these associations (e.g., Erasmus 1952, 1954; Foster 1952, 1953a, 1953b; Kelly 1955, 1957; Garcia Manzanedo and Kelly 1955; Oberg and Rios 1955; Simmons 1955a, 1955b, 1957; Jenny and Simmons 1954).

After the Institute of Social Anthropology program in Guatemala was terminated in mid-1951, Adams continued health research, first on a State Department grant under the auspices of the Instituto de Nutrición de Centro-América y Panamá (INCAP), and from 1953 to 1956 as a Pan American Sanitary Bureau staff member. Several pioneering publications came from this work (Adams 1951, 1953, 1955a, 1955b). Nancie Gonzalez's association with INCAP began in 1955, and during the following decade her Guatemalan research contributed significantly to our understanding of international health problems (Solien and Scrimshaw 1957; Solien de Gonzalez 1963a, 1963b). Also noteworthy were the extensive medical research and intervention programs among Mexico's Indian groups carried out in the 1950s by that country's National Indian Institute under the direction of Gonzalo Aguirre Beltrán (Aguirre Beltrán and Pozas A. 1954; Aguirre Beltrán 1955), who, you will recall, delivered the first Malinowski lecture nine years ago.

In comparing the disappointments of DuBois's assignment with the positive results of the work of those who shortly followed her, a critical difference in the institutional setting stands out. No one in WHO was interested in DuBois's work. In contrast, the work of Wellin, Paul, the ISA anthropologists, Adams, and Gonzalez was successful in large part because of the interest and support of strategically placed culturally sensitive public health physicians: Hydrick in Peru, Leavell at Harvard, and Hyde in the IIAA. And without the support of Nevin Scrimshaw, medical doctor and nutritionist, then director of INCAP, the work of Adams and Gonzalez in all likelihood would not have prospered.

## **Anthropologists in International Health Programs: The Current Picture**

Many, if not all, of the details of the involvement of American anthropologists in international health programs since the 1950s are described in a series of reviews of medical anthropology, especially Caudill (1953), Polgar (1962), Scotch (1963), Fabrega (1972), Lieban (1973), and Colson and Selby (1974). In some ways it is an impressive story. At the same time, the institutionalization of anthropology in international health organizations—a development I would confidently have predicted 30 years ago—has not taken place. Although medical anthropology increasingly is institutionalized in schools of public health, nursing, psychiatry, and even medicine, most international work continues to be done on the same ad hoc basis as in the 1950s, and assignments depend on the personal networks that link anthropologists and health personnel. In considerable part the failure to institutionalize our international health roles must be laid at our own doorstep. In the 1950s we were enthusiastically welcomed as colleagues by health policy makers and planners; we enjoyed their respect and confidence, and they were anxious to engage our services.

In part because of the low status accorded applied anthropology—and international health work was viewed as “applied”—and in part because of the abundance of openings in American universities, anthropologists largely rejected the proffered hand of our public health colleagues. We accepted academic rather than applied appointments. In retrospect, we muffed our greatest applied opportunity. With the interest and support of foundations, and government and international agencies, we had the opportunity to create a developmental science that might significantly have altered the pattern of international aid programs. Insofar as international health programs are concerned, we have made progress since the 1950s, but less than we should have.

To the best of my knowledge there is no inventory of anthropologists who work or have worked in international health programs. One thing seems certain, however. Whereas in the 1950s international and cross-cultural health activities constituted the largest part of medical anthropology, today these activities are but a small part of the field. Paradoxically, at a time when international health work attracts anthropologists, the national and international organizations that are our obvious allies and employers show little interest in us. The situation of 25 years ago is completely reversed.

Among the major organizations employing anthropologists to work on health problems, AID continues, as in the past, to lead the field. Four “direct-hire” anthropologists are based in Washington, and at least two more are members of field missions. A much larger, fluctuating number work under long- and short-term contractual arrangements, while a few serve as “consultants” for not to exceed six months a year. Still others are employed indirectly via university, Population Council, American Public Health Association, and private consulting firm contracts (communicated by Barbara Pillsbury).

In contrast, the World Health Organization makes relatively little use of anthropologists. At headquarters in Geneva there is but one full-time anthropologist, and he is scheduled to complete his assignment this year. Anthropologists serve, however, as short-term consultants, as “expert committee” and “expert panel” members for specific projects, and they participate in workshops dealing with specific health problems. The regional offices of WHO also make fairly limited use of anthropologists, largely in the capacity of consultants. None, I believe, has a full-time staff member. The World Bank has assigned an anthropologist to problems of population and nutrition and has sponsored contract research on appropriate technologies for water supply systems. UNICEF appears to have made the least use of anthropologists of all the major

organizations. This may change, however. “Given UNICEF’s mandate for children and women, the future of women anthropologists (who tend to include women, or gender issues in their research concerns) looms more favorably in UNICEF than that of men” (communicated by Mary R. Hollnsteiner).

Apart from AID, then, it appears that international health organizations largely view the role of anthropologists as that of short consultants for specific projects, as committee members, and as workshop participants. Less frequently, they participate in teaching programs, in the evaluation of health behavioral science resources in Third World countries, and in organizing meetings to consider behavioral science research on health problems. It is on tenuous bases such as these that we must build if we wish to play a more important role in international health programs.

### **International Health Personnel: What They Want of Anthropology**

I now turn to the kinds of help international health personnel would like to have from anthropologists, to what I think we can in fact offer them, and to the problems that make it difficult to realize the hopes of both groups. I draw largely on my experiences in recent years as WHO consultant in South and Southeast Asian countries. Since 1975, WHO’s primary strategy in providing health care and disease control has been through primary health care, an approach in which, at least in theory, the community plays a major role in defining health priorities, marshalling resources, and providing services. The primary health care philosophy is, of course, a restatement specific to health of the community development movement of the 1950s, and programs are encountering many of the same problems that surfaced a generation ago, particularly obtaining community participation. The principal programs where community participation is deemed essential include the control of a group of tropical diseases (e.g., malaria, schistosomiasis, filariasis, leprosy, yellow fever, and infant diarrheal diseases), immunization programs against infectious childhood diseases, and installation and maintenance of water supply systems and latrines.

Not surprisingly, then, the question I am most frequently asked is: “How can we elicit community participation in our programs?” Just as among community development workers a generation ago, so today among health personnel there is a hopeful assumption that there is a right “key” which, if only anthropologists can discover it, will unlock the door to wholehearted community cooperation in primary health care activities. It is difficult to convince WHO personnel that the problem is not one of hitting upon a clear way to communicate ideas, to motivate community members to action, but that there are basic structural and historical factors having to do with economics, caste and class, religion, politics, land tenure, and the like that frequently militate against genuine community cooperative efforts in solving health problems.

A second question has to do with “early case finding” and “case holding.” With leprosy, for example, early identification of symptoms usually makes possible rendering the patient noncontagious in as little as 24 hours, and regular treatment prevents disfigurement. Yet leprosy patients are reluctant to be so identified, and usually they come to the attention of physicians only after a long period in which they have threatened the health of others, and when disfigurement can no longer be concealed. Even with early identification, treatment must continue for many years, sometimes throughout the patient’s life. Here is the problem of case holding: in the absence of symptoms, people who feel well see no need to continue with a treatment which, in all likelihood, they do not understand. Early case finding and case holding

are equally important problems in the control of tuberculosis and other chronic infectious diseases. Case holding is also a major problem in immunization programs: in the usual series of three visits to the health center there is a rapid drop-off after the first and second injections, so that many children do not receive complete protection. How can parents be persuaded not only to begin immunizations, but to complete the prescribed series? That is the question we are asked. Other common questions have to do with the difficulties encountered in persuading people to build and use latrines, and to cooperate in the maintenance of community water supply systems.

The striking thing about these questions is that almost all assume that effective health care can be achieved only when members of traditional communities change their health behavior. Rarely if ever is the question asked, “How can anthropologists help to change bureaucratic behavior that inhibits the design and operation of the best possible health care systems?”

### **Anthropologists: What We Can Offer International Health Programs**

In trying to answer this question, we must distinguish between what we, as anthropologists, would *like* to do, and what realistically we can *hope* to achieve. For example, we would *like* to study health bureaucracies, attempt to determine ways in which their structure and operations might be changed to offer more effective services, and communicate to health personnel the urgency of making these changes. Most—but not all—of the health personnel with whom I have worked in recent years are genuinely surprised when it is suggested that changes in their beliefs and behavior are just as critical as changes in community members’ beliefs and behavior, if adequate health care is to be provided. And those who do recognize the importance of bureaucratic factors in health care delivery feel—realistically, I suspect—that this is a constant about which little can be done. If changing behavior will result in effective primary health care, it must be community, not bureaucratic behavior, that changes.

A more hopeful area in which we as anthropologists can contribute to health care programs is health research. What international health planners want, reasonably enough, is operational research, research whose results can be incorporated into program planning and operations. As a step in this direction, in the fall of 1981 the Southeast Asian Regional Office (SEARO) of WHO hosted a six-country workshop in New Delhi entitled “Appropriate Technologies for Behavioural Science Research on Health Problems.” Workshop participants, who included anthropologists, sociologists, nutritionists, medical doctors, and health educators, concluded that a principal reason why so little health research in their countries has been operational is the near-exclusive reliance on survey techniques. Survey research is emphasized in part because some researchers feel it is more “scientific” than observational techniques, and in part because medical doctors prefer it. The latter are so imbued with the biomedical research model that qualitative information on human behavior is suspect to them.

In spite of these problems, workshop participants felt that it is realistic to anticipate much improved behavioral science research on health problems in their countries. They concluded that a “comparative case study” design combining participant-observation with structured and unstructured interviews is most apt to provide the needed information.

A third important opportunity for anthropologists in international health programs is to provide convincing evidence that primary health care must take into consideration the prevailing health beliefs and practices of the population to be served. It is anomalous indeed that although WHO has a major program in traditional medicine (Bannerman 1977), and WHO staff members

continually speak of the importance of incorporating traditional therapies and healers into primary health care programs, almost no one thinks about how to research ethnomedicine and, after the results are in, to apply these findings to ongoing programs. Nevertheless I am optimistic about progress in this area. With patience, and with good examples, I believe we can put across the idea that effective health care must reflect knowledge of what community members believe and do.

## **The Problem of Theory**

I now turn to a more general question, namely, What does anthropology gain from research on international health programs? Like most new fields, medical anthropology, including international health, has been charged with failure to develop theory. Twenty years ago Caudill, in commenting on Polgar's survey, "Health and Human Behavior" (Polgar 1962), noted that anthropologists had "done a considerable amount of careful descriptive reporting" but that "we have not, as yet, made the conceptual and theoretical contributions to this field of which we are capable" (Caudill 1962:181). In the same commentary, Kundstadter noted that the literature reviewed by Polgar "is more impressive for its bulk than for its importance in relation to general theories of human behavior. It is a pity that a field with such apparent practical significance has received so little theoretical consideration" (Kundstadter 1962:185).

It is probably the fate of every new anthropological field to be judged initially as showing limited theoretical potential. In reviewing literature on peasants in the same year, Geertz, while noting its "undoubted descriptive value," felt that "it is not, at the moment, a very likely place to look for ideas, and certainly not for systems of ideas" (Geertz 1962:18). Yet by now it is clear that like peasant studies, medical anthropological studies have made important contributions to theory. This is because, as Scotch pointed out long ago, "what comes out of research is not dependent on the *nature* of the problem to be studied but rather on the *way* the problem is studied" (Scotch 1963:32). The concern of the anthropologist is to study human behavior. *All* human behavior is grist for our mill, and all good research data—whatever the context in which they are gathered—have theoretical potential. The study of a health clinic in Thailand is potentially as productive of theory as is the study of a hill tribe or peasant village in the same country.

In speaking of theoretical contributions, it is important to distinguish between two levels: (1) general anthropological theory, and (2) theory specific to medical anthropology and to international health.

With respect to general theory, the main contribution to emerge from international health research is in the area of sociocultural change, and particularly the factors that promote and discourage change. I believe a large part of current anthropological theory about the dynamics of culture change has come from the applied field: work in agriculture, community development, and health programs. A particularly important theoretical point emerges from the growing literature on the "hierarchy of resort" in seeking medical care (Schwartz 1969), the study of the sociopsychological forces that lead patients to choose between alternative therapies and therapists. A general theory to explain how people make up their minds when faced with viable choices will, of course, go well beyond the boundaries of anthropology itself.

International health research has also contributed to our understanding of the relationship between belief and behavior. Behavior, it is often assumed, is a function of belief. Logically, then, a change in behavior must be preceded by a change in belief. At least until recently this has

been the rationale for health education—that improved health habits will follow educational efforts that change traditional beliefs about health and illness. Yet it is now clear that health behavior is not so much a variable dependent on belief as dependent on the perception of advantage that comes from changing behavior. Thirty years ago, Erasmus found among postparturient women in a hospital in Quito that their traditional birth beliefs remained intact while they happily conformed to a maternity ward regimen that, in many respects, ran counter to traditional practices. Mothers found they regained their strength more rapidly in the hospital than at home, and they felt that their infants had a better start on life than when delivered by a midwife (Erasmus 1952). Subsequent health research has confirmed Erasmus's conclusions: people are pragmatic, and will adopt new behavior forms if they perceive advantage in so doing. Conversely, we now know, the acquisition of new knowledge will not automatically lead to changes in behavior; individual priorities and personal preferences override strict rationality.

Still another point on which, I believe, international health data have contributed to general change theory is that of the importance of social costs. Planners often assume that economic costs are the basic determinants of change. These are important, but international health research reveals that social costs may also be very important. A comprehensive theory of change must take into account both economic and social costs to the individual and the community.

With respect to comprehensive theory specific to medical anthropology and international health, I am less certain about what we may expect. The field of medical anthropology is so broad, and so diverse, and has ties to so many biological and social disciplines, that it seems unreasonable to expect any general theory applicable to all of medical anthropology. But I believe we already have a very substantial body of “middle-range” theory that is specific to human behavior in health contexts. I take a reasonably good explanation of a particular form of behavior to be a theoretical statement. By “reasonably good” I mean an explanation that has general, if not absolute, predictive value. In this sense we have good theories about the behavior of leprosy patients: they tend to avoid having their cases diagnosed because they fear ostracism. We have theories that predict the probable sequence of events when antimalarial DDT spray teams enter a community. And we can explain why people in traditional communities often are uninterested in building latrines, and why, once built, they may not be used. We can predict, in the international health field, that preventive medicine will be more easily introduced into a traditional community if the curative “felt needs” of the population are already being met. Many of these middle-range theories, I am confident, will with the passage of time become incorporated into more general theories.

## **Prospects for the Future**

In 1961 Louis Miniclier, then head of the Community Development Division of AID, whose office employed more anthropologists than any other branch of government, noted three problems he had encountered in using anthropologists: defining their roles, producing evidence demonstrating to administrators that anthropologists contribute to foreign aid objectives, and finding anthropologists willing to serve overseas for two or more years at a stretch (Miniclier 1964:187). These are the problems that bedevil us today. Satisfactory long-term roles for anthropologists in international health programs are almost as elusive today as when Cora DuBois struggled to decide what she should do. Short-term assignments as consultants, service on international health committees, and even research projects of a year or two are, of course,

“roles,” but they are not institutionalized roles understood by others in medical hierarchies, in the same way that the roles of doctors, nurses, and health planners are understood. We can continue on the same ad hoc basis that has marked most international medical anthropological work during past decades, but until we fill institutionally recognized roles our work will be marked by improvisation. To work effectively we still need medical “godfathers”—a Hydrick, Leavell, Hyde, or Scrimshaw—sympathetic to anthropologists and knowledgeable as to how to use them. But the age of our godfathers appears to have passed.

To those of us gathered here tonight, anthropologists have abundantly demonstrated their ability to contribute to international health programs. But it is well to remember that what we regard as convincing evidence is not so regarded by all members of the medical profession: after all, our evidence is not based on hard data. It is not enough to make specific recommendations for recognized problems. We have a job of selling, of selling ourselves and our skills to those who determine international health policies and who decide who will be employed to carry out these policies.

It is only with respect to Miniclier’s third point—the need for long-term commitment by anthropologists—that the picture has improved significantly. It is now acceptable to aspire to a nonacademic, goal-oriented career. With the medical anthropological training now offered in a number of universities, we are preparing anthropologists with the specific skills needed for international health work; and many of them feel a long-term commitment to the international field to be a desirable choice. It is up to us who, in any capacity, are involved with international health programs to do our best to create the institutionalized roles that will enable them to realize their aspirations.

### Notes

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### References Cited

- Adair, John  
1960 The Indian Health Worker in the Cornell-Navaho Project. *Human Organization* 19:59–63.
- Adair, John, and Kurt Deuschle  
1958 Some Problems of the Physicians on the Navajo Reservation. *Human Organization* 16(4):19–23.
- Adams, Richard N.  
1951 Un Análisis de las Enfermedades y sus Curaciones en una Población Indígena de Guatemala: Con Sugerencias Relacionadas con la Práctica de Medicina en el Area Maya. Guatemala City: Instituto de Nutrición de Centro América y Panamá. (Mimeo.)  
1953 Notes on the Application of Anthropology. *Human Organization* 12(2):10–14.  
1955a A Nutritional Research Program in Guatemala. *In* *Health, Culture and Community*. B. D. Paul, ed. Pp. 435–458. New York: Russell Sage Foundation.  
1955b On the Effective Use of Anthropology in Public Health Programs. *Human Organization* 13(4):5–15.
- Aguirre Beltrán, Gonzalo  
1955 Los Programas de Salud Interpretados en la Situación Intercultural. México, D.F.: Instituto Nacional Indigenista, Serie Mimeográfica No. 10.

- Aguirre Beltrán, Gonzalo, and Ricardo Pozas A.  
 1954 *Medicina y Salubridad. In Métodos y Resultados de la Política Indigenista en México.* Alfonso Caso et al. México, D.F.: Memorias del Instituto Nacional Indigenista 6:231–244.
- Bannerman, R. H.  
 1977 WHO's Programme in Traditional Medicine. *WHO Chronicle* 31:427–428.
- Brown, Esther Lucile  
 1936 *Nursing as a Profession.* New York: Russell Sage Foundation.
- Caudill, William  
 1953 *Applied Anthropology in Medicine. In Anthropology Today.* A. L. Kroeber, Chairman. Pp. 771–804. Chicago: University of Chicago Press.  
 1962 Comments [on Polgar 1962]. *Current Anthropology* 3:181–182.
- Colson, Anthony C., and Karen F. Selby  
 1974 *Medical Anthropology. In Annual Review of Anthropology.* B. J. Siegel et al., eds. Pp. 245–262. Palo Alto, Calif.: Annual Reviews.
- Erasmus, Charles J.  
 1952 Changing Folk Beliefs and the Relativity of Empirical Knowledge. *Southwestern Journal of Anthropology* 8:411–428.  
 1954 An Anthropologist Views Technical Assistance. *The Scientific Monthly* 78:147–158.
- Fabrega, Horacio, Jr.  
 1972 *Medical Anthropology. In Biennial Review of Anthropology 1971.* B. J. Siegel, ed. Pp. 167–229. Stanford: Stanford University Press.
- Firth, Raymond  
 1934 The Sociological Study of Native Diet. *Africa* 7:401–414.
- Foster, George M.  
 1952 Relationships between Theoretical and Applied Anthropology: A Public Health Program Analysis. *Human Organization* 11(3):5–16.  
 1953a Changing Health Concepts in Latin America. *The Health Education Journal* 11:94–103.  
 1953b Use of Anthropological Methods and Data in Planning and Operation [IIAA 10-Year Evaluation]. *Public Health Reports* 68:841–857.
- Foster, George M., ed.  
 1951 *A Cross-Cultural Anthropological Analysis of a Technical Aid Program.* Washington: Smithsonian Institution. (Mimeo.)
- García Manzanedo, Héctor, and Isabel Kelly  
 1955 *Comentarios al Proyecto de la Campaña para la Eradicación del Paludismo en México.* México, D.F.: Institute of Inter-American Affairs. (Mimeo.)
- Geertz, Clifford  
 1962 *Studies in Peasant Life: Community and Society. In Biennial Review of Anthropology 1961.* B. J. Siegel, ed. Pp. 1–41. Stanford: Stanford University Press.
- Howard-Jones, Norman  
 1975 *The Scientific Background of the International Sanitary Conferences 1851–1938. History of International Public Health, No. 1.* Geneva: World Health Organization.  
 1980 *The Pan American Health Organization: Origins and Evolution.* *WHO Chronicle* 34:367–375, 419–426.



- Hydrick, John L.  
1942[1937] *Intensive Rural Hygiene Work in the Netherlands East Indies*. New York: Netherlands Information Bureau.
- Jenny, E. Ross, and Ozzie G. Simmons  
1954 *Human Relations and Technical Assistance in Public Health*. *The Scientific Monthly* 78:365–371.
- Joseph, Alice  
1942 *Physician and Patient: Some Aspects of Inter-Personal Relations between Physicians and Patients, with Special Regard to the Relationship between White Physicians and Indian Patients*. *Applied Anthropology* 1(4):1–6.
- Kelly, Isabel  
1955 *El Adiestramiento de Parteras en México, Desde el Punto de Vista Antropológico*. *América Indígena* 15:109–115.  
1957 *Notas Acerca del Diseño de una Hidrante*. México, D.F.: Ingeniería Sanitaria 2(1).
- Kunstadter, Peter  
1962 *Comments [on Polgar 1962]*. *Current Anthropology* 3:185.
- Leighton, Alexander H., and Dorothea C. Leighton  
1944 *The Navaho Door: An Introduction to Navaho Life*. Cambridge: Harvard University Press.
- Lieban, Richard W.  
1973 *Medical Anthropology*. In *Handbook of Social and Cultural Anthropology*. J. J. Honigman, ed. Pp. 1031–1072. Chicago: Rand McNally.
- Malinowski, Bronislaw  
1945 *The Dynamics of Culture Change: An Inquiry into Race Relations in Africa*. New Haven: Yale University Press.
- McDermott, Walsh, Kurt Deuschle, John Adair, Hugh Fulmer, and Bernice Loughlin  
1960 *Introducing Modern Medicine in a Navajo Community*. *Science* 131(22):197–205, 280–287.
- Mead, Margaret  
1964 *Food Habits Research: Problems of the 1960's*. Washington: National Academy of Sciences-National Research Council, Pub. 1225.
- Means, Philip Ainsworth  
1921 *Sociological Background of Sanitation Work in Peru*. *American Journal of Public Health* 11:17–20.
- Miniclier, Louis  
1964 *The Use of Anthropologists in the Foreign Aid Program*. *Human Organization* 23:187–189.
- National Research Council (NRC)  
1943 *The Problem of Changing Food Habits: Report of the Committee on Food Habits 1941–43*. Washington: National Academy of Sciences-National Research Council, Bulletin 108.  
1945 *Manual for the Study of Food Habits: Report of the Committee on Food Habits*. Washington: National Academy of Sciences-National Research Council, Bulletin 111.
- Oberg, Kalervo, and Jose Arthur Rios  
1955 *A Community Improvement Project in Brazil*. In *Health, Culture and Community*. B. D. Paul, ed. Pp. 349–376. New York: Russell Sage Foundation.

- Paul, Benjamin D., ed.  
 1955 Health, Culture and Community: Case Studies of Public Reactions to Health Programs. New York: Russell Sage Foundation.
- Polgar, Steven  
 1962 Health and Human Behavior: Areas of Interest Common to the Social and Medical Sciences. *Current Anthropology* 3: 159–179.
- Richards, Audrey I.  
 1939 Land, Labour and Diet in Northern Rhodesia: An Economic Study of the Bemba Tribe. London: Oxford University Press for the International African Institute.
- Richards, Cara E.  
 1960 Cooperation between Anthropologist and Medical Personnel. *Human Organization* 19:64–67.
- Saunders, Lyle  
 1954 Cultural Difference and Medical Care: The Case of the Spanish-Speaking People of the Southwest. New York: Russell Sage Foundation.
- Schwartz, Lola Romanucci  
 1969 The Hierarchy of Resort in Curative Practices: The Admiralty Islands, Melanesia. *Journal of Health and Social Behavior* 10:201–209.
- Scotch, Norman A.  
 1963 Medical Anthropology. *In* Biennial Review of Anthropology 1963. B. J. Siegel, ed. Pp. 30–68. Stanford: Stanford University Press.
- Simmons, Ozzie G.  
 1955a The Clinical Team in a Chilean Health Center. *In* Health, Culture and Community. D. B. Paul, ed. Pp. 325–348. New York: Russell Sage Foundation.  
 1955b Popular and Modern Medicine in Mestizo Communities of Coastal Peru and Chile. *Journal of American Folklore* 68:57–71.  
 1957 Implications of Social Class for Public Health. *Human Organization* 16(3):7–10.
- Solien, Nancie L., and Nevin S. Scrimshaw  
 1957 Public Health Significance of Child Feeding Practices Observed in a Guatemalan Village. *The Journal of Tropical Pediatrics* 3:99–104.
- Solien de Gonzalez, Nancie L. I  
 1963a Breast-Feeding, Weaning and Acculturation. *The Journal of Pediatrics* 62:577–581.  
 1963b Creencias Médicas y Nutricionales en un Grupo SocioEconómico Bajo, de la Ciudad de Guatemala. *Revista del Colegio Médico de Guatemala* 14:1–3.
- Wellin, Edward  
 1953a Anthropological Analysis of a Typhus Control Program in Southern Peru. Washington: Report to the Pan American Sanitary Bureau, WHO. (Mimeo.)  
 1953b Pregnancy, Childbirth and Midwifery in the Valley of Ica, Peru. Cuzco: World Health Organization. (Mimeo.)  
 1955 Water Boiling in a Peruvian Town. *In* Health, Culture and Community. D. B. Paul, ed. Pp. 71–103. New York: Russell Sage Foundation.  
 1958 Implications of Local Culture for Public Health. *Human Organization* 16(4):16–18.

## **Chapter 13**

### **Omer C. Stewart:**

### **Advocate for Indian Religion and Land Claims**

**Thomas Weaver**

Omer Stewart, recipient of the Society for Applied Anthropology's Malinowski Award in 1983, was a life-long advocate for the American Indian.<sup>1</sup> Perhaps his most important contribution to applied anthropology was his persistent work to validate peyotism as a legitimate Indian religion (1972, 1987). In his Malinowski Award acceptance speech, entitled "Historical Notes about Applied Anthropology in the United States" (1983), he traced some of the history of the campaign to prevent government interference with the religious use of peyote and the contributions made by other anthropologists, going back to James Mooney's testimony in 1918 against legislation proposed to prohibit its use. Another of Stewart's important contributions to practicing anthropology and to American Indians was his testimony as expert witness for the Indian Land Claims Commission. Some fifty anthropologists provided expert testimony as ethnological witnesses during the thirty years of commission hearings, often on opposite sides of the issue. Stewart served on the side of the Indian in ten cases, with other anthropologists supporting the Department of Justice's case against Indian land claims.

Omer Stewart (1908–1991) was trained in anthropology at the University of Utah and the University of California at Berkeley where he received a doctorate in 1939. Much of his field experience was with indigenous groups in the western United States. Like other Malinowski Award recipients educated in the early 1930s, Stewart was a third generation Boasian historicist. Alfred Kroeber, Robert Lowie, and Carl Sauer at Berkeley and Julian Steward at Utah were among his teachers. During World War II Stewart was attached to the Office of the Chief of Staff in the War Department. He was founder and first chair of the Department of Anthropology at the University of Colorado where he stayed from 1944 until his death. In addition to his work on American Indian religion and land claims, he is well known for his study of early American Indians' practice of setting fires to encourage better forage for animals they hunted. He published twelve articles on this subject.

Stewart's Malinowski Award address, like many others, was a commentary on the history and development of applied anthropology interspersed with personal examples of applied work. He provided a more extensive synopsis than others had of the prior century of applied anthropology. Like many Malinowski Award winners, Stewart was not strongly influenced by Malinowski's vision of applied anthropology. In fact, he admitted only reading Malinowski as an applied anthropologist, along with back issues of *Human Organization*, in preparation for his acceptance speech.

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Stewart observed that there was no training in applied anthropology at the University of California at Berkeley or elsewhere in the 1930s, and that one practiced applied anthropology according to one's "own natural tendency." He quoted Kroeber, writing in 1959, as an example of the prevailing attitude about the field: "If I do not say more about applied anthropology, it is for lack of personal experience. I have never practiced nor even dabbled in it" (Kroeber 1959, quoted in Stewart 1983:190). Stewart cited two instances, however, in which Kroeber had practiced anthropology, one in 1937 as an expert witness concerning a United States Senate bill meant to outlaw peyote and the second Kroeber's two years of work in the 1950s as an expert witness in the Indian Claims case for Indians of California (Stewart 1983:190). Kroeber, like most anthropologists until the 1970s, did not see his work as an expert witness as applied anthropology. Many anthropologists of his generation espoused the idea that applied anthropology was not sufficiently theoretical or ethnographic, which they considered the main objectives of anthropological work.<sup>2</sup>

Stewart concluded his address with the following reflection:

[L]et me recall the statement of Kroeber that applied anthropologists in general were following their own natural tendency, rather than being specially trained. No professor, so far as I can remember, suggested to me that I should emphasize the practical application of the insights I gained by my study of anthropology. I just did what came naturally (Stewart 1983:193).

He continued advocacy work until the end of his life. (See, for example, his 1984 publication with Martha C. Knack based on his testimony in a water case.) He was also active in bettering minority relations and improving Indian education at the University of Colorado and in the city of Boulder.

Stewart was president of the Society for Applied Anthropology in 1966. He received the Distinguished Scholarship Award from the Southwestern Anthropological Association, the Merit of Honor from the Alumni Association of the University of Utah, and the CT Hurst Award from the Colorado Archaeological Society. The High Plains Society for Applied Anthropology named an award in Omer Stewart's honor

### Notes

<sup>1</sup>This annual award is given to a senior colleague in recognition of efforts to understand and serve the needs of the world through social science. See Thomas Weaver's "The Malinowski Award and the History of Applied Anthropology" (2002b) for a brief history of this award and an overview of the recipients and their work, and Weaver's "Malinowski as Applied Anthropologist" (2002a) for an introduction to Bronislaw Malinowski and his work.

<sup>2</sup>This attitude had not changed in the early 1960s when I was a graduate student at Berkeley. My prior fieldwork in medical anthropology was not considered important except for the fieldwork experience it provided. At least by this time it was a subject discussed. George Foster conducted an upper-division course in applied anthropology in which he gave prominence to medical anthropology.

### References Cited

- Knack, Martha C. and Omer C. Stewart  
 1984 As Long as the River Shall Run: An Ethnohistory of Pyramid Lake Indian Reservation. Berkeley: University of California Press.
- Kroeber, Alfred L.  
 1959 A History of the Personality of Anthropology. *American Anthropologist* 61(3):398–404.
- Stewart, Omer C.  
 1972 The Native American Church and the Law. *In* The Emergent Native Americans: A Reader in Culture Contact. D. E. Walker, Jr., ed. Pp. 382–397. Boston: Little, Brown.  
 1983 Historical Notes about Applied Anthropology in the United States. *Human Organization* 42(3):189–194. (Republished at <<http://www.sfaa.net/>>.)  
 1987 *Peyote Religion: A History*. Norman: University of Oklahoma Press.
- Weaver, Thomas  
 2002a Malinowski as Applied Anthropologist. *In* The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.  
 2002b The Malinowski Award and the History of Applied Anthropology. *In* The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.

## Historical Notes about Applied Anthropology in the United States

Omer C. Stewart

I feel greatly honored to receive the Malinowski Award. To begin with, it has stimulated me to read about Malinowski and applied anthropology in general, resulting in many new insights. For one thing, I have discovered a broader Malinowski which had escaped me because I had been satisfied to limit my study to his numerous monographs on the Trobriand Islanders. Furthermore, since the telephone call in October from G. Alexander Moore, chairman of the Malinowski Award Committee, checking to see whether I was “alive, willing, and able to deliver an address” in San Diego in March, I have reviewed many publications of the Society for Applied Anthropology, examined several recent books with applied anthropology in their titles, and have given a great deal of thought to defining applied anthropology and my role in it. The ten addresses and articles from previous senior anthropologists who were awarded the Malinowski Award have provided material from scholars and teachers concerning the history of applied anthropology. I will try to add something to their contributions.

Conrad Reining (Clifton 1970:9) found statements from Tyler in 1881 and from Flower in 1885 to the effect that anthropologists could and should make practical application of anthropology, and Peake proposed in 1921 that a “School of Applied Anthropology” should be established in Great Britain. However, most writers consider the first teachers of applied anthropology to have been Radcliffe-Brown and Malinowski. The first reference I have on Radcliffe-Brown is that in 1930 he gave a lecture entitled “Applied Anthropology” in Brisbane (Firth 1981:201).

A professorship for Malinowski at the London School of Economics was established in 1928, and soon appeared titles by him concerned with applied anthropology. The first I came across was “Practical Anthropology” (1929). Later, “Culture Contact” was a title he used. He gave a paper in 1938 entitled “The Scientific Basis of Applied Anthropology” (1940). “Culture Change,” “Acculturation,” “Action Anthropology,” “Study of Social Issues” were other titles. His seminars at the London School of Economics were held from 1928 to 1938. After 1938, he lived in the United States, mostly at Yale, until his death in May 1942.

Of his seminars Phyllis M. Kaberry wrote in the introduction to Malinowski’s *The Dynamics of Culture Change* (1945:1) which she edited shortly after his death: “His seminars at the London School of Economics were unique, not only because they were attended by administrative officials, missionaries, and specialists in anthropology and allied subjects, but also because he had his own particular methods of instruction.”

The training of practical anthropologists for colonial administration did not start with Malinowski. In Britain some earlier ones Lowie used to cite were Rattray for Ashanti, Meek for Nigeria, and Williams for Papua. It has been intriguing to seek to explain why anthropology was not applied in a practical way in the early administration of the “colonial” peoples in the United States, and there appear to be a number of reasons. Until 1849, the Indians were under the

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supervision of the War Department. Early interaction between immigrants and native Americans often involved military confrontation. The Indians were legally dependent nations even though in 1924 they were accorded full U.S. citizenship. Federal administrators, Congress, and the American people acted on the assumption that the first and most important job of the federal administration was to convert the aborigines to Christianity and to pressure them into adopting the ways of free enterprise and rugged individualism. The obligation to make Christians of native Americans was assumed by the first European discoverers and explorers, starting with the Spanish. It is remarkable to me that that obligation persisted in the United States until the election of Franklin D. Roosevelt in 1932, bringing the appointments of Harold L. Ickes as Secretary of the Interior and John Collier as Commissioner of Indian Affairs.

The first dedicated amateur ethnologist in the United States was certainly an applied anthropologist. Henry Rowe Schoolcraft (1793–1864) met unconquered Indians of the Plains in Missouri in 1817 while on a trip to investigate lead mines for his glassmaker father. The Indians interested him and he remained in the vicinity of the Great Lakes as Indian agent to the Chippewa. He married a Chippewa and recorded Indian lore and served as interpreter for a number of treaty councils with tribes around the Great Lakes. In 1851, he published his *Personal Memoirs of a Residence of Thirty Years with the Indian Tribes on the American Frontiers, A.D. 1812–1842*. Moreover, from 1851 to 1857 he published a six-volume study financed by a direct appropriation from Congress: “Historical and Statistical Information Respecting the History, Condition, and Prospects of the Indian Tribes of the United States.” In addition to his own data from the Chippewa he collected reports from travelers, Indian agents, and others.

Another American amateur ethnologist was Lewis Henry Morgan (1818–81), who began as an applied anthropologist. Morgan grew up near Aurora, New York, surrounded by Seneca Indians. His first interest was to help the Seneca protect their land and culture. With White friends he organized a young men’s secret society and tried to pattern it after Iroquois social structures. Morgan’s skill as an interviewer and observer was matched by his writing skill in his *League of the Ho = de = no = sau = nee or Iroquois* (1851). Two large monographs for the Smithsonian and his popular and controversial theoretical book *Ancient Society* (1877) have caused most scholars to overlook his original work as an applied anthropologist.

In their book *The Golden Age of American Anthropology* (1960:227) Mead and Bunzel declare Alice Cunningham Fletcher (1838–1923) “America’s first applied anthropologist” because in addition to her monograph “The Omaha Tribe,” for the Smithsonian, she worked for years as a Bureau of Indian Affairs allotment agent among the Omaha, Winnebago, and Nez Perce.

Because of their practical work as U.S. government officials, both John Wesley Powell and James Mooney should be called applied anthropologists. Powell was the first director of the Bureau of Ethnology and hired both Mooney and Alice Fletcher. In 1874, Powell made a survey of Great Basin tribes and, besides identifying their political organizations, made recommendations for their supervision. James Mooney (1861–1921) is most famous for his BAE monograph “The Ghost Dance and the Sioux Outbreak of 1890,” which was a descriptive record of recent events, not to be classed as applied anthropology. But during his research on the Ghost Dance (1891–96) he discovered the peyote religion being practiced by Kiowa and Comanche in Oklahoma. He was fascinated by the peyote ceremony and became the first American participant-observer and reporter of that ritual. Mooney found that the Bureau of Indian Affairs and the missionaries to the Indians had decided that peyote was a dangerous habit-forming drug and that the complex all-night ceremony was a cover for orgies. His own research, however, had

convinced him that peyote was not habit forming and that the ritual was a sincere religious ceremony by which supernatural healing and other help was received. His article in the *Handbook of American Indians*, Part 2 (1910), was a forthright defense of peyote. The Indians in Oklahoma asked for his help and in 1918 he advised and supervised the legal incorporation of the Native American Church under the laws of Oklahoma with provision for the use of peyote as a sacrament.

Those opposed, officials of the Indian Bureau and united missionary groups, decided a federal prohibition was required to stop Peyotism and a full-dress congressional hearing was arranged. Mooney and Indian leaders of the Native American Church from Nebraska and Oklahoma testified against the legislation and it was not passed. But Mooney was insulted during the hearings and was denied permission to again visit and study tribes in Oklahoma. His attempt to bring facts about peyote to Congress in order to protect the religious freedom of peyotist tribes was applied anthropology of the most direct kind.

An important reason why professional anthropologists in the United States have been slow to engage in applied anthropology may be due to academic teaching and tradition. Professional anthropology in the United States is generally thought to begin with Franz Boas (1858–1942), whose life span paralleled that of Malinowski (1884–1942). He came to the United States in 1887, was first hired as a writer for *Science* magazine, 1887–88, and then began teaching at Clark University, 1889–92. For nearly ten years he was at the American Museum of Natural History with a lectureship at Columbia University. Finally, a professorship at Columbia in 1899 continued until 1937. During that more than a third of a century Boas was the major teacher of professional anthropologists, and his students filled most of the major teaching posts in the United States until World War II. Only regarding race relations was Boas an applied anthropologist. As Partridge and Eddy wrote (1978:13), “. . . the major portion of Boas’ energy was devoted to abstract anthropology.”

Most of Boas’s students demonstrated even less interest in applied anthropology than Boas. A quotation from A. L. Kroeber (1876–1960), the first and most famous of Boas’s students, and the one usually characterized as carrying on the Boas tradition, will illustrate:

If I do not say more about applied anthropology, it is for lack of personal experience. I have never practiced nor even dabbled in it. I am not trying to deny it a place. We are certainly all of us grateful that the scheme of the universe allows the applied art of scientific medicine to flourish alongside the fundamental sciences of physiology and biochemistry, and engineering beside physics. It is by its fruits that applied anthropology should be appraised. But I would be a poor appraiser. There are some individuals who can in their own person make notable contributions in both approaches: Pasteur, for instance, and Virchow, and our own Margaret Mead. But there are others whose bent is one way or the other, and who can produce more successfully by keeping that bent (1959; reprinted Darnell 1974:327).

In 1958, at age 82, when he wrote the above, Kroeber can be forgiven for not remembering two important occasions when he had performed as an applied anthropologist. The first was connected with the introduction into the U.S. Senate of a bill to outlaw peyote. Early in 1937 Dr. Skudder Mekeel, an anthropological advisor to Commissioner John Collier, proposed approaching a number of anthropologists who might be willing to express opinions against the



proposed legislation. Eight anthropologists, a botanist and an Osage submitted letters which became part of BIA opposition to the legislation. It was a rather surprising list of anthropologists who responded to the invitation by the commissioner to help the Indians protect their religious freedom. They were Boas, Kroeber, Hrdlicka, J. P. Harrington, M. R. Harrington, La Barre, Petrullo, Elna Smith; plus botanist Schultes, and Osage Chief Fred Lookout. Although it was never published, the BIA distributed hundreds of copies of the mimeographed report "Documents on Peyote," May 18, 1937.

Kroeber's second foray into applied anthropology occurred in 1953–54 when he devoted nearly two years to research and participation as a witness on the side of the Indians in the U.S. Indian Claims Case for the Indians of California. I was not a witness myself, but the attorneys for the Indians hired me for two summers to be understudy for Kroeber. I was paid to sit and listen to all of his testimony so that I could replace him in the event he had to withdraw from the case. This was to satisfy the legal rule that I could read and interpret the transcript of the case if I had heard it being delivered. The second summer I replaced Kroeber when he could not be in court to consult with the attorney directing the cross-examination of Department of Justices witnesses—who were Steward, Strong, Beals, Driver, Halpern, Ermaine Voeglin, and Goldschmidt. I have found no mention of this application of anthropology by Kroeber in any of the several obituaries written for him. Perhaps it was decided his few acts of applied anthropology were so uncharacteristic they were not worth mentioning.

Considering Kroeber's assertion that he had no interest in applied anthropology, a view shared by Lowie, Olson, Gifford, and Julian Steward—then the faculty at Berkeley when I enrolled there as a graduate student—the number and stature of applied anthropologists who received their Ph.D.'s from Berkeley is amazing. I consider it a compliment to that faculty that they really wished to produce independent scholars, not disciples. It started, of course, before I arrived in Berkeley in 1933, and two California graduates, W. Lloyd Warner (b. 1898) and Laura Thompson (b. 1905), warrant special attention because of their early roles in the Society of Applied Anthropology.

Warner received an A.B. at Berkeley in 1925, studying especially with Lowie, but in 1926 he went to Australia and studied with Radcliffe-Brown, who was a professor of anthropology at the University of Sydney (1926–31). Warner did research on the Murngin and wrote his famous book *Black Civilization*. When he returned to Berkeley in 1929 it was only to visit with Lowie and Kroeber; as rumor had it, he said there were no faculty at Berkeley knowledgeable enough to examine him. In the fall of 1929 he was hired as an instructor at Harvard. His first project was his research of a Connecticut city he called "Yankee City" (1931–36), during which he hired and directed Conrad Arensberg, Eliot Chappel, Allison Davis, Burleigh Gardner, Solon Kimball, and other Harvard anthropology graduate students. In 1936, he moved to the University of Chicago where Radcliffe-Brown was a professor from 1931 to 1937. With Radcliffe-Brown and Lloyd Warner at Chicago and Malinowski at Yale, the stage was set for the founding of the Society of Applied Anthropology in October 1941.

Laura Thompson, Malinowski Awardee in 1979, was a native of Honolulu and worked at the Bishop Museum before she received her Ph.D. from Berkeley in 1933. Immediately thereafter, she departed for Fiji to do the research that led to her first major publication "Fijian Frontier" for which Malinowski wrote the introduction when it was published in 1940. Her work on Guam, 1938–39 prepared her for her research in applied anthropology for the U.S. Navy during World War II. Both Laura Thompson and Malinowski were participants in the organizational meeting at Harvard when the Society for Applied Anthropology was founded.

In the thirties another event contributed to interest in applied anthropology. The election of Franklin D. Roosevelt and the beginning of the New Deal offered a number of anthropologists federal employment. One can give credit to the two British functionalists for the beginning of applied anthropology in academia in the United States, but for nonacademic employment of applied anthropologists, Secretary of Interior Harold L. Ickes and Secretary of Agriculture Henry A. Wallace must be given much credit. In 1933 anthropologists were hired to give practical help to the National Park Service, the Bureau of Indian Affairs, Soil Conservation Service, and the Bureau of Agricultural Economics. A few continued in government service 20 to 25 years, or until they earned a full retirement. To name a few: Ruth Underhill received her Ph.D. from Boas in 1934 and immediately accepted government employment; John Provinse, a Ph.D. from Chicago in 1934, was in government service in 1936; Gordon MacGregor, a Harvard Ph.D. in 1935, became an ethnologist with the Bureau of Indian Affairs in 1933, before his degree; Edward A. Kennard, a Ph.D. from Columbia in 1938, entered government service the same year.

Unfortunately I have not had available to me a list of the organizers or founding members of the Society. Laura Thompson stated she was an organizer, and *Human Organization* (9[3]:1, 1950) reported that Malinowski was also. The first issue of *Applied Anthropology* (1[1], 1941), listed an Advisory Council: John D. Black, A. V. Kidder, Clarence Pickett, and M. L. Wilson. The editor was Eliot D. Chappel; the book review editor was Conrad M. Arensberg; and the treasurer was F. L. W. Richardson, Jr. Two of the founders, Chappel and Arensberg, had been with Lloyd Warner at Yankee City. (The name of the journal for the Society of Applied Anthropology was changed to *Human Organization* with Volume 8, Number 1, 1949, with Conrad Arensberg as managing editor and Eliot D. Chappel as editor.) The articles in Volume 1, Number 1 were by Eliot D. Chappel about industry, Froelich Rainey about natives of Alaska, William F. Whyte about settlement houses, Margaret Mead about national morale, F. L. W. Richardson, Jr. about a depressed coal region, and Conrad M. Arensberg about management. Volume 1, Number 2, January-March 1942, published Chappel and Lindeman, Arensberg and MacGregor, Douglas Oliver, Stonborough, and Charles P. Loomis and D. Ensminger. Volume 1, Number 3, April-June 1942, announced the second annual meeting of the Society for Applied Anthropology for October 9, 10, 11, 1942, Washington, D.C., called by an Executive Committee of C. M. Arensberg, E. D. Chappel, D. W. Lockard, D. L. Oliver, J. Provinse, and F. L. W. Richardson, Jr. The volume contained an article, "Applied Anthropology in 1860," about the Northwest Coast, by Homer G. Barnett, a fellow graduate student with me at Berkeley for several years. *Applied Anthropology* Volume 2, Number 1 (October-December 1942) reported John H. Provinse was elected president for 1943-44, but no replacement was named until George Murdock was elected for 1946-48. The next few presidents were Charles Loomis, Margaret Mead, Everett Hughes, F. L. W. Richardson, Jr., and Solon Kimball. Kimball thanked the Society for moving an annual meeting "way out west" to Chicago in 1953. Homer Barnett was president for the annual meeting in Kansas City in 1962, I was president for an annual meeting in Milwaukee in 1966, and Margaret Lantis was president in 1974 for the annual meeting in Boston. On the basis of articles and listings as Fellows, the following other Berkeley Ph.D.'s have been active in the Society: Ralph Beals, Alan Beals, James F. Downs, Philip Drucker, George Foster, Walter R. Goldschmidt, Jane Richardson Hanks, Thomas McCorkle, Marion Pearsall, John A. Price, Katherine Luomala, Julian H. Steward, Demitri Shimkin, Stanley and Ruth Freed, Gordon W. Hewes, and undoubtedly some I do not know. Again, considering that the Berkeley Department of Anthropology had no professor who identified himself as a

functionalist or as an applied anthropologist until after the 1950s, I find the number of Berkeley students who became active applied anthropologists, at least from time to time, remarkable.

During World War II anthropologists did not object to working for OSS, the forerunner of the CIA, or for Army or Navy Intelligence, as Boas had objected to scientists working as spies during World War I (and being censured by a majority of anthropologists for his complaint) (see Weaver 1973:51–52). Using one's ability, training, and knowledge in the national war effort seemed the proper thing to do. Complaints were voiced only when someone was barred from military activity, such as was Clyde Kluckhohn due to the rheumatic fever he had had as a teenager. Demetri Shimkin and I were in the Office of Chief of Staff, G-2. Ed Norbeck and Ted McCown were in a code-breaking unit; Carlton Coon, Cora DuBois, Gordon Hewes, and others were in OSS. Cora was on Montbatten's staff in Ceylon. George Devereaux and Weston La Barre prepared to parachute into Indo-China behind enemy lines until the commander of the Asiatic Theater vetoed the drop and put them on staff duty in Chungking.

Phileo Nash, later to be Commissioner of Indian Affairs, was assigned to the White House and worked on the order by President Truman which desegregated the armed forces. Margaret Mead worked on trying to change national food habits to fit the food available. Ruth Benedict advised on treatment of the Japanese. George Murdock and Clelland Ford were commissioned in naval intelligence to analyze the Human Relation Area Files for data regarding the Pacific Islands. Laura Thompson wrote reports about Guam to help the navy.

Soon after the attack on Pearl Harbor by the Japanese, President Roosevelt ordered all California Japanese into internment camps, and applied anthropologists were soon hired in all of the camps. A special office was created under the president of the Society of Applied Anthropology, John Provinsé, to help ease the problems of administration of the Japanese Americans.

Immediately after the surrender of the Japanese, numerous anthropologists were hired as special advisors to naval units in the Pacific area formerly under the Japanese, and most considered their work applied anthropology. Among those directly involved were George Murdock, Edward T. Hall, Jr., John Useem, Ed Galahue, William Bascomb, Leonard Mason, Douglas Oliver, Homer Barnett, Philip Drucker, Thomas Gladwin, John L. Fischer, Frank J. Mahony, William A. Lessa, and probably others.

In 1950, it was the Point IV Program of the U.S. government to aid Third World nations, as well as a speech of mine on the subject, that were reported in *Human Organization* (9[3]:26–27, 1950). About that time the Society for Applied Anthropology published a *Clearinghouse Bulletin* in order to give short notices of the many applied anthropology projects. In 1951 36 projects were described, involving 59 research personnel. The *Clearinghouse Bulletin* continued about four years.

One of the best examples of applied anthropology has occurred when anthropologists have testified before congressional committees or in courts regarding ethnic groups. The most important of these opportunities came about because of the U.S. Indian Claims Commission Act of 1945. At first anthropologists were conspicuous by their absence. In 1945 when congressional committees held extended hearings before passage of the Indian Claims Commission Act, neither Indian tribes lobbying for passage of the act nor their attorneys invited anthropologists to testify. The only anthropological input I could find was a letter from the two anthropologists representing Indians on the board of the American Civil Liberties Union, who wrote supporting the legislation. They were A. V. Kidder and Gene Weltfish. However, during the next 30 years approximately 50 anthropologists have engaged in research and have testified as expert

ethnological witnesses in the nearly 400 cases adjudicated by the Indian Claims Commission. The Indians were awarded \$818,172,606.64 (Final Report, U.S. Indian Claims Commission, 1978). I testified in 10 cases and worked as the understudy for A. L. Kroeber in the California case.

Finally, I wish to speak about my own role as an applied anthropologist. I think you might say that I am one of those who have a natural bent toward applied anthropology. In 1935 my first old Pomo informant told me that Indians had intentionally set fire to all the fields and forests to keep down the brush so that they could more easily travel and could see game better and produce more abundant vegetable food. My little monograph on the ethnogeography of the Pomo, which was an important document in the California claims case, started me on a research program to popularize Indian foresting methods and led in time to 12 articles on the subject from 1951 to 1982, one in the *Encyclopedia Britannica* in 1959. The information was useful in each of the claims cases I worked on as evidence of the Indians' understanding and activity in modifying the environment to benefit themselves as hunting and gathering peoples.

Possibly my most important contribution as an applied anthropologist has been to help Indians protect themselves from unjust state laws which outlawed peyote, an essential substance for the practice of the peyote religion. Again, I might point out the reaction of a natural-born applied anthropologist as compared to an anthropologist solely dedicated to recording precontact Indian culture and to developing cultural theories. Robert Lowie spent years doing research and writing on Crow religion. In a dozen articles and books on religion including references to peyote in several, he never mentioned Crow peyotism until his last writing, which was an autobiography. In that he said that although his favorite interpreter had urged him to record the history and theology of Crow peyotism, he did not do so because his interpreter was not a reliable source, he was a "marginal" man (Lowie 1959:19). Another anthropologist, Julian H. Steward, did two Culture Element Surveys of the Shoshone of Nevada and Idaho, 1935–36. He visited and recorded the presence or absence of over 3,000 culture elements, but failed to record the presence of the peyote religion which had been well-established among the Gosiute, the White Knife Shoshoni of the Duck Valley Reservation, and the Shoshoni-Bannock of Fort Hall. On the other hand, whatever I found that concerned the Indians, whether ancient or current, has always seemed equally relevant to me. In 1936, I recorded in a footnote that a Northern Paiute attended peyote ceremonies in Idaho and Oklahoma, although none were being held among the Northern Paiute at that time. In 1937, I found peyotism among the Gosiute and Ute and arranged to be invited to be a participant-observer at three Ute peyote rituals. The experiences I had among the Ute and Gosiute made me aware that peyotists were simultaneously active members of Christian denominations; and while the non-Indians in the vicinity of the reservation had no experience with peyote, they all condemned it.

In January 1938, when I returned to Berkeley, I began a lifelong effort to bring truth about the peyote religion and justice to devotees of that religion. It started in February 1938 with an interview over an NBC radio network in a program called "The University Explorer" when I explained the sincere beliefs of the Native American Church (Peyote) and deplored the public misunderstanding and the many improper state laws prohibiting peyote. This led to a half-page interview with photographs in the *Oakland Post-Enquirer* and a favorable editorial in the *Los Angeles Evening Herald-Express*. Since then I have published about 20 articles, 1939–82, and two monographs on peyotism and have given dozens of public lectures. Most important are the eight state cases from 1960 to 1982 when I have been qualified as an expert witness and have helped Indians to practice the Native American Church with use of peyote as the sacrament.

The most important hearing took place in 1962 in San Bernardino, California, where I testified at the request of the president of the Native American Church and his attorneys to gain freedom for Navajo peyotist section hands, Jack Woody, Dan Dee Nez, and Leon B. Anderson, to provide information which helped the Supreme Court of California rule that there should not be a California state law prohibiting the use of peyote in bona fide religious services. The respect held for the written opinion of the California Supreme Court has been important in the amendments of nearly all state laws to conform to that opinion. Now known as *Woody et al.*, this opinion is now also accepted by the federal government. Other anthropologists who have used their knowledge to help peyotists are Weston La Barre, Carling Malouf, J. Gilbert McAllister, David F. Aberle, and J. Sidney Slotkin.

In comparison with my effort on behalf of freedom of religion for native Americans, the few years I was active in Boulder, Colorado, to improve race relations may appear rather insignificant, but perhaps they deserve mentioning. Immediately after World War II I became a member of a university committee on minority relations at the University of Colorado, and, on my own initiative, organized the Boulder Unity Council. As executive secretary for about ten years I arranged local public meetings advocating equality for Blacks and all people; I obtained from the Anti-Defamation League in New York City copies of subway car cards promoting better intergroup understanding and arranged to put these same cards in Boulder's four buses; and I had copies of radio programs sent from New York to Boulder where they were aired as a public service. Like most communities in the United States in 1945, Blacks in Boulder suffered all kinds of discrimination: in barber shops, eating places, movie houses, hotels, etc. I am glad to say the climate of opinion has improved in Boulder to the extent that a Black attorney has been elected mayor, and a Black woman was appointed president of the University of Colorado.

Another area of applied anthropology in which I was active was Indian education, by being treasurer and contact with the University of Colorado for special summer workshops for American Indian college students under the direction of American Indian Development, Inc. (AID), organized by D'Arcy McNickle and Sol Tax. Some of the instructors were Fred Gearing, Rosalie and Murray Wax, Robert Thomas, and Deward Walker.

While I was active with AID I sponsored and was successful in having the University of Colorado award honorary degrees to Indians Helen Peterson and D'Arcy McNickle and to a long-time applied anthropologist, Ruth Underhill.

Besides my continuing role as expert witness for the Native American Church, in recent years I have served as expert witness for the Native American Rights Fund, whose headquarters is in Boulder, and for the Pyramid Lake Paiute water case, for which I testified and prepared a 450-page memorandum for the attorneys. This became the basis for a book by my research assistant Martha Knack which has been accepted for publication by the University of California Press with authors listed as Knack and Stewart. In Nevada I have testified on a Northern Paiute hunting case, and on a Western Shoshone homestead case. A year ago I testified for the Wind River Shoshones and Northern Arapahoes on another water case.

In conclusion, let me recall the statement of Kroeber that applied anthropologists in general were following their own natural tendency, rather than being specially trained. My own life is testimony to that idea. No professor, so far as I can remember, suggested to me that I should emphasize the practical application of the insights I gained by my study of anthropology. I just did what came naturally.

### References Cited

- Barnett, H. G.  
1942     Applied Anthropology in 1860. *Applied Anthropology* 1(3):19–32.
- Boas, Franz  
1919     Letter to the Editor: Scientists as Spies. *The Nation* 109(2842):797. (*In* T. Weaver, 1973, Pp. 51–52.)
- Clearinghouse Bulletin  
1951–55   Human Organization. Society for Applied Anthropology. 3 volumes.
- Clifton, James A., ed.  
1970     Applied Anthropology: Readings in the Uses of the Science of Man. Boston: Houghton Mifflin.
- Darnell, Regna  
1974     Readings in the History of Anthropology. New York: Harper & Row.
- Eddy, Elizabeth M., and William L. Partridge, eds.  
1978     Applied Anthropology in America. New York: Columbia University Press.
- Firth, Raymond  
1981     Engagement and Detachment: Reflections on Applying Social Anthropology on Social Affairs. *Human Organization* 40(3):193–201.
- Herskovits, Melville I.  
1953     Franz Boas. New York: Charles Scribner's Sons.
- Kroeber, Alfred L.  
1959     A History of the Personality of Anthropology. *American Anthropologist* 61(3):398–404. (*In* Readings in the History of Anthropology. Regna Darnell. New York: Harper & Row, 1974.)
- Lowie, Robert H.  
1959     Robert H. Lowie, Ethnologist: A Personal Record. Berkeley: University of California Press.
- Malinowski, Bronislaw  
1929     Practical Anthropology. *Africa*. 2(1):22–38.  
1940     The Scientific Basis of Applied Anthropology. *Reale Accademia d'Italia, Fondazione Alessandro Volta. Estratto dagli Atti dell'VIII Convegno, Rome, 1938*. Pp. 18–19.  
1945     The Dynamics of Culture Change: An Inquiry into Race Relations in Africa. P. M. Kaberry, ed. New Haven: Yale University Press.
- Mead, Margaret, and Ruth L. Bunzel  
1960     The Golden Age of American Anthropology. New York: Braziller.
- Meek, C. K.  
1931     Tribal Studies in Northern Nigeria. London: Kegan Paul, Trench, Trubner and Co.
- Mooney, James  
1892     Eating the Mescal (Peyote). *Augusta (Georgia) Chronicle*, January 24:11.  
1896     The Ghost Dance Religion and the Sioux Outbreak of 1890. *Annual Report of the Bureau of American Ethnology* (1892–93) 14, Pt. 2.  
1910     Peyote. *Handbook of American Indians. Bulletin of the Bureau of American Ethnology* 30, Pt. 2:237.  
1915     Statement Regarding Peyote. Hearing before House Committee on Indian Affairs. Washington, D.C.

- Morgan, Lewis H.  
 1850 Report upon Articles Furnished the Indian Collection. Albany: 3rd Annual Report, Regents University of State of New York, 1849.  
 1851 League of the Ho=de=no=sau=nee or Iroquois. 2 vols. Rochester, N.Y.: Sage; New York: M.H. Newman.  
 1877 Ancient Society. New York: Henry Holt.
- Partridge, W. L., and Elizabeth Eddy  
 1978 The Development of Applied Anthropology in America. *In* Applied Anthropology in America. E. M. Eddy and W. L. Partridge, eds. New York: Columbia University Press.
- Peake, N. J. E.  
 1921 Discussion. *Man* 21(103):174.
- Powell, John W., and G. W. Ingalls  
 1874 Report . . . on the Condition of the Utes . . . Pact-Utes Go-si Utes . . . and Shoshoni. . . Washington, 1874.
- Radcliffe-Brown, A. R.  
 1931 Applied Anthropology. Proceedings of the Australian and New Zealand Association for the Advancement of Science 20th Meeting, Brisbane, 1930. Pp. 267–280.
- Rattray, R. S.  
 1923 Ashanti. Oxford: Clarendon Press.
- Reining, Conrad C.  
 1962 A Lost Period of Applied Anthropology. *American Anthropologist* 64:593–600. (*In* Applied Anthropology: Readings in Uses of the Science of Man. James A. Clifton, ed. Boston: Houghton Mifflin, 1970.)
- Schoolcraft, Henry Rowe  
 1851 Personal Memoirs of a Residence of Thirty Years with the Indian Tribes on the American Frontiers A.D. 1812–1842. Philadelphia: Lippincott, Grambo and Co.  
 1851–57 Historical and Statistical Information Respecting the History, Condition, and Prospects of the Indian Tribes of the United States. 6 vols. Philadelphia: Lippincott, Grambo and Co.
- Stewart, Omer C.  
 1950 Social Scientists and the Point IV Program. *Human Organization* 9(3):26–27.  
 1955 Teen-age Ute Indians and Spanish and Anglo Americans: Analysis of a Tri-Cultural Community. *Human Organization Clearinghouse Bulletin* 3(3):21.  
 1972 The Native American Church and the Law. *In* The Emergent Native Americans: A Reader in Culture Contact. D. E. Walker, Jr., ed. Pp. 382–397. Boston: Little, Brown.
- Thompson, Laura  
 1940 Fijian Frontier. Studies of the Pacific, No. 4. American Council, Institute of Pacific Relations, San Francisco, New York, and Honolulu.
- Warner, W. Lloyd  
 1937 A Black Civilization. New York: Harper & Row.
- Weaver, Thomas, ed.  
 1973 To See Ourselves: Anthropology and Modern Social Issues. Glenview, Ill.: Scott, Foresman.
- Williams, F. D.  
 1923 The Vailala Madness and the Destruction of Native Ceremonies in the Gulf Division. Anthropological Report No. 4.

## **Chapter 14**

### **Alexander H. Leighton: Interdisciplinary Research and Personality Theory**

**Thomas Weaver**

Alexander Leighton (1908–), perhaps more than any other social scientist, deserves to be characterized as truly serving the interdisciplinary agenda considered for so many years the core of applied anthropology. Recipient of the Society for Applied Anthropology's Malinowski Award in 1984, Leighton is a physician with a doctorate in anthropology from Johns Hopkins University, which he received in 1936.<sup>1</sup> He was professor of anthropology and sociology at Cornell from 1944 to 1966 and later of psychiatry at Harvard. He held visiting professorships at various universities. He was a consultant to the World Health Organization, the Bureau of Indian Affairs (BIA), the Milbank Memorial Fund, the U.S. Surgeon General, and the Peace Corps and served on a panel set up by the President's Scientific Advisory Committee. Groups that were the focus of his fieldwork include the Japanese, Navajo, St. Lawrence Island Yupik, Yoruba, and Nova Scotians.

During the Second World War, Leighton, along with a number of other anthropologists, including fellow Malinowski Award winners Edward Spicer and Elizabeth Colson, worked with Japanese Americans in the War Relocation Authority (WRA) concentration camps. (See Colson 1985 and Spicer 1976.) The employment of anthropologists at the ten relocation centers set up in the west and mid-west was facilitated by John Collier, then Commissioner of Indian Affairs, who believed that the enterprise should be directed by research and observation and the use of applied psychology and anthropology. Leighton's 1945 book, *The Governing of Men*, describes one of these centers, and an appendix written with Edward Spicer, who influenced Leighton's theoretical outlook, details the history, methodology, and research techniques used in their studies. Out of this labor came Leighton's unique and refreshing perspective on personality under conditions of stress.

At the time of his work at the WRA camp, Leighton was a Lieutenant Commander in the Medical Corps of the U.S. Navy. The Navy subsequently assigned him to the Office of War Information where he formed the Foreign Morale Analysis Division. Among the social scientist members of the team were prominent anthropologists Clyde Kluckhohn, Ruth Benedict, Dorothea Leighton (a physician-anthropologist), and Morris Opler. When the war ended, the Foreign Morale Analysis Division was dissolved and Leighton was assigned to the Army's U.S. Strategic Bombing Survey. He was sent to Hiroshima to study the feelings and attitudes of the survivors of the nuclear explosion. The resulting publication, *Human Relations in a Changing World: Observations on the Use of the Social Sciences* (1949), examines Japanese morale and

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draws out the implications of an applied social science approach to the wartime analysis of morale. In another applied endeavor Leighton wrote *The Navajo Door* (1944) with Dorothea Leighton, a book whose goal was educating non-Navajo BIA employees about Navajo culture.

In his Malinowski Award acceptance speech, entitled “Then and Now: Some Notes on the Interaction of Person and Social Environment” (1984), Leighton identified types of perceived predicaments and reactions to what he called “sociological earthquakes”—the disruption of institutional support systems for the individual, as experienced, for example, by Japanese Americans placed in the WRA concentration camps (Leighton 1984:192). He addressed the problem of intercultural variability in individual forms and responses, uniquely phrased as consisting of intracultural variability and individual variability, as well as culture specific influences, all based on a common core of human striving (1984:191). Leighton, therefore, rejected a strict following of Ruth Benedict’s premise of the overwhelming importance of culture in forming personality (Benedict 1934).

Leighton also noted in his speech some interesting conclusions that came from cross-cultural studies of mental illness conducted with the Yupik and Yoruba and the people of Nova Scotia, New York City, and Vietnam (1959). Among these was the discovery of a rate of mental illness of 20 percent (give or take 5 percent) in most populations regardless of state of development or other factors. “No culture appears to have the ability to protect its people from mental illness and none appears to foster inadvertently much higher rates than others” (Leighton 1984:195). A strong association was found between high rates of mental illness and the malfunctioning of social institutions, however. Leighton concluded that “the associations being demonstrated by psychiatric epidemiology call for a continuing, problem-oriented, comprehensive interdisciplinary approach—something able to encompass what Bronfenbrenner (1979) has so aptly called ‘experiments by nature and design’” (Leighton 1984:195-196; see also Leighton 1959).

The underlying premise of Leighton’s observations is the existence of what he calls the “bio-psycho-social” core of human strivings. Although Leighton knew Bronislaw Malinowski in the final two years before his death in 1942 and cites many concepts and interests pioneered by Malinowski, he does not mention the very similar biological needs basis of Malinowski’s theory of functionalism. Discussions with Malinowski had boosted Leighton’s interest in researching the relationship between culture and personality, but in his work Leighton seems to have followed more the functional and institutional framework of A. R. Radcliffe-Brown and Spicer; a more Boasian concept of culture derived through association in fieldwork with Kluckhohn and Spicer; and a culture and personality framework influenced by Abraham Kardiner, Ralph Linton, Kluckhohn, and Margaret Mead. Leighton failed to mention Malinowski’s contribution to discussions of the oedipal complex because, as he later explained, he was not “a subscriber to psychoanalytic theory . . . and not impressed with Malinowski’s ideas about the Oedipal Complex” (personal communication April 25 and July 4, 1998).

Leighton, while a devotee of interdisciplinary cooperation, astutely pointed out some of the problems that would be encountered in dismantling the separate social science approaches to the study of human behavior. Lack of a shared technical language and theory is one of the major obstacles to achieving this unity. Culture, anxiety, and psychoneurosis are only the most prominent concepts that lack a consensus definition among the social sciences. The problem of theory is encumbered by the common reliance of psychiatrists on “intuition as criteria of validity, something strictly forbidden in other scientific fields” and cultural anthropologists’ “tendency to gather observations to illustrate theory rather than critically examine, probe, and test” (Leighton

1984:190). This problem is exacerbated by social scientists' "tendency to confuse speculative fantasies with verified processes in nature. . . . [and their] efforts to construct complete explanatory systems within the arbitrary boundaries of one discipline" (Leighton 1984:193).

Leighton was a member of numerous professional societies in Canada, the U.S., and other countries, testifying to the broad scope of his research. He was a fellow at the Center for Advanced Study in the Behavioral Sciences, Thomas W. Salmon Lecturer for the New York Academy of Medicine, Honorary Fellow in the Royal College of Psychiatrists of the United Kingdom, and a member of the American Philosophical Society. He was awarded an Social Science Research Council fellowship, and he also received an honorary doctorate from Harvard, the Rema Lapouse Award from the American Public Health Association, and the McAlpin Award from the American Association for Mental Health.

### Notes

<sup>1</sup>This annual award is given to a senior colleague in recognition of efforts to understand and serve the needs of the world through social science. See Thomas Weaver's "The Malinowski Award and the History of Applied Anthropology" (2002b) for a brief history of the award and an overview of the recipients and their work, and Weaver's "Malinowski as Applied Anthropologist" (2002a) for an introduction to Bronislaw Malinowski and his work.

### References Cited

- Benedict, Ruth  
1934    Patterns of Culture. New York: Houghton-Mifflin.
- Brofenbrenner, U.  
1979    The Ecology of Human Development. Cambridge, Mass.: Harvard University Press.
- Colson, Elizabeth  
1985    Using Anthropology in a World on the Move. *Human Organization* 44(3):191–196. (Republished at <<http://www.sfaa.net/>>.)
- Leighton, Alexander H.  
1945    The Governing of Men. Princeton, N.J.: Princeton University Press.  
1949    Human Relations in a Changing World: Observations on the Use of the Social Sciences. New York: E.P. Dutton & Company, Incorporated.  
1959    My Name is Legion: Foundations for a Theory of Man in Relation to Culture. Stirling County Study of Psychiatric Disorder and Sociocultural Environment, volume 1. New York: Basic Books.  
1984    Then and Now: Some Notes on the Interaction of Person and Social Environment. *Human Organization* 43(3):189–197. (Republished at <<http://www.sfaa.net/>>.)
- Leighton, Alexander H. and Dorothea Leighton  
1944    The Navajo Door. Cambridge: Harvard University Press.
- Spicer, Edward  
1976    Beyond Analysis and Explanation? Notes on the Life and Times of the Society for Applied Anthropology. *Human Organization* 35(4):335–343. (Republished at <<http://www.sfaa.net/>>.)

Weaver, Thomas

- 2002a Malinowski as Applied Anthropologist. *In* The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.
- 2002b The Malinowski Award and the History of Applied Anthropology. *In* The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.

## **Then and Now: Some Notes on the Interaction of Person and Social Environment**

**Alexander Leighton**

One of the things that makes me extremely sensible of the honor of being asked to give this lecture is the fact that I had the privilege of knowing Malinowski during the last few years of his life. As I look back now I feel immensely indebted to his personal counsel as well as to his writings.

Our meeting was in the autumn of 1937 or 1938 when he came to give a talk in the Department of Psychiatry at Johns Hopkins. I was then a resident in psychiatry and delegated to greet him at the station. I saw him, therefore, for the first time early one morning as he approached along the platform from his Pullman, through steam and frosty air, waving a black, rolled up, Chamberlain-type umbrella. "I hope," he said with a characteristic, engaging and energetic laugh, "I hope you will pardon this symbol of British acculturation."

Malinowski's visit was an opportunity to explain to him a question regarding which I hoped to begin some research. It was approximately this: What influences do environmental factors have on the origin, course and outcome of mental illnesses? This question was located within a particular orientation, called "psychobiology," that deserves a few words of explanation, since it differs from the better known orientation of psychoanalysis. As the British psychiatrist Michael Shepherd has recently written, psychobiology came from "the broad central tradition of psychiatric theory and practice which originated on the European mainland, was transported by Adolf Meyer to North America and returned to Europe via the United Kingdom, where its preeminent representative has been Sir Aubrey Lewis. At its core is an adherence to principles of scientific inquiry in clinical and basic research, with due acknowledgment of the role played by social and psychological investigation as well as by the natural sciences" (Shepherd and Zangwill 1983:ix).

This approach owes allegiance to no grand theory, except in a general way to the notion of organic evolution, yet it is hospitable to many theories when these open possibilities for investigation. It keeps such theories, however, in holding pens labelled "tentative" until opportunity comes for pulling one or another out and subjecting it to some manner of testing. The orientation shows a preference for taking theory in small bites with frequent appeal to observations in nature. "Clinical Baconism" is perhaps an appropriate descriptive term, especially if one cools somewhat Francis Bacon's overconfidence in an automatic operation of his method.

As indicated in the quotation from Michael Shepherd, Adolf Meyer was a major proponent of this tradition. He was also head of the Department of Psychiatry at Johns Hopkins, where he made it the foundation of the Department's teaching, patient treatment, and research. In contrast to most psychoanalytic theories of the time, Meyer thought that all the post-infancy periods of life—adolescence, youth, maturity, and senescence—were times when important

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influences occurred affecting the origins, as well as the outcomes, of mental disorders. Because of this he emphasized the importance of looking into such common “settings” as family, job and community. He thought that these contexts of the individual had great power to influence, for better or for worse, the course of mental illnesses in individuals. These ideas might, in Robert Merton’s terms, be called “theories of the middle range” (Merton 1968:39–72) in contrast to grand theory such as that of Freud. It may be of interest to note that W. I. Thomas acknowledged Meyer as a major influence in the development of his thinking, particularly as reflected in the book on the Polish peasant (Leighton 1952:xxii).

Aside from psychobiology, the framing of the research question presented to Malinowski was also under the influence of my pre-psychiatric training. This had included not only biomedical training with specialization in neurophysiology, but also studies of animal behavior in natural settings (Leighton 1932, 1933, 1935).

After careful listening to my research question, Malinowski said that in speaking of environmental factors, I was talking about American culture, and that the cognitive frame I needed in order to ask nature the right questions was the concept of culture. “But,” he added, “you are too close to American culture, too swimming in it to see and to understand it. To prepare yourself for the research you want to do you must go and study another culture first, then you can return equipped to study your own.”

This advice helped steer me toward academic studies in cultural anthropology and then, in the winter of 1940, to living in a Navaho hogan among the sage in the high country of northern New Mexico; and the following summer, to trudging the Alaskan tundra in the company of Yupik-speaking Eskimos. These studies were made possible by a joint fellowship granted to D. C. Leighton and myself from the Social Science Research Council.

In this talk tonight my plan is to select out of these and other experiences that followed later, certain notions which I think have bearing on social psychiatry today, and to some extent also on other problems of the human predicament. The thread on which I shall string these sets of ideas is largely autobiographical, and I hope therefore that the story-form will add a large enough measure of interest to balance things if I trespass too much on good taste.

## Person in Culture

The specific task chosen, in order to give focus during the field experiences among Navahos and Yupik-speaking Eskimos, was to make personality studies of selected individuals. This seemed an appropriate target, given that I wished to explore the axis that connects person to culture.

The operational scheme for implementing this was an adaptation of one developed by Meyer as part of the training he provided to medical students and residents. The individual’s life story was the backbone of this method, with emphasis on the experiences he or she had found most significant, and on his or her perceptions and interpretations of self and others. In the adaptation utilized in the field work there was also emphasis on observing the individual in the current round of daily activities. No *a priori* theory of personality was involved, but there were topical headings that served as information-gathering guides. These included relationships in the family of origin, education, means of subsistence, marriage, children, religious activities, and experiences with sickness and death (Leighton and Leighton 1949).

My selection of subjects for the personality studies was based on the Baconian notion that contrasts could be revealing. This is, of course, what led to choosing both Navahos and Yupik

Eskimos in the first place, but it also influenced the selection of individuals within each cultural group.

The general anthropological studies in preparation for the field were primarily under the auspices of Ralph Linton, but with supplemental guidance from Ruth Benedict, Scudder McKeel, and Margaret Mead. In addition and more specifically, it was also necessary to absorb what had been so far written about Navahos and Yupiks. The Yupik material turned out to be easy because there was so little of it, but the Navahos were a different matter, and Clyde Kluckhohn became an enormously helpful coach and mentor. Particularly formative also was the opportunity to attend the then famous Kardiner-Linton seminars on culture and personality (Kardiner 1939; Kardiner et al. 1945).

During these preparatory years, I found I was starting up coveys of questions in my mind about how to apply the scientific process to all the phenomena, concepts and theories that were coming into view. One such covey pertained to psychiatry, and arose from realizing that there was in this field a very loose relationship between theory and systematic data, a looseness not tolerated in the biomedical sciences in which I had been previously trained. The classical theories of Freud and the neoanalytic theories of Kardiner, Harry Stack Sullivan and Karen Horney, for example, were treated by many people as though they were discoveries about nature rather than as interesting speculations that might possibly apply to nature. Intuition played a role in devising psychiatric theory, just as it does in all sciences, but intuitive feelings were also used as criteria of validity, something strictly forbidden in other scientific fields.

A similar covey of problems arose at the cultural end of the person-in-culture axis. Ruth Benedict and Margaret Mead were at this time the main exponents of culture as the principal determinant of human behavior, but they had support from virtually every leading cultural anthropologist. The issues of interest to them were about *how* culture molds personality, not *whether*, and there was a tendency to gather observations to illustrate theory rather than critically examine, probe, and test. Again, intuition could be seen playing a validating role.

Nevertheless, I began the field work much impressed with the possibility that each culture was a distinct, self-sufficient whole and that different cultures would very likely turn out very different kinds of persons. This is the first of the sets of ideas I mentioned in the introduction, and from the point of view of its impingement on the individual, we may call it *inter-cultural variability*.

In the course of the field work a host of things were taught to me by the Navahos and Eskimos I got to know. These were, indeed, so many and so rich that I am still, more than 40 years later, digesting them. For our purposes here tonight, however, I shall limit myself to three conclusions that make up three more of the sets of ideas.

1) The first is *intra-cultural variability*. Although intercultural variability was evident, the patterns displayed by the Navahos and Eskimos to whose life stories I listened also showed a wide range of intra-cultural variation. Values, practices, rituals, symbols, interpretations, and myths were far less standard or consistently compelling than one would have gathered from ethnographic accounts. Patterns of family life, child care, gaining a living and managing sickness varied considerably from time to time, place to place, hogan to hogan, ningloo to ningloo, and family to family, as well as from individual to individual. Each culture comprised a *range of ways* of perceiving, feeling, and doing things, and was by no means the same for each individual (Sankoff 1972).

The fact that most ethnographic accounts create in the reader an unrealistic impression of uniformity and consistency is, I suspect, not so much due to faulty observation as to the

requirements of writing a general description. The serious problem begins when people get seduced by this artifact and treat it as if it were a reality. This happens inadvertently to psychiatrists and other non-anthropologists who read anthropological writings, but it is also encouraged by many anthropologists whose theoretical orientation leads them to exaggerate the global wholeness and uniqueness of each culture and to do so at the expense of attention to variations within.

None of this denies the existence of group-wide cultural patterns, nor their tremendous influence over individual perceptions and behavior. What my field experience did was to draw attention to the more and less quality of this, and to suggest that while the notion of culture as a whole and of inter-cultural differences might be useful for many purposes, so far as personality formation was concerned it was important also to pay attention to intra-cultural variability.

2) The second conclusion from the field work is that there exists a substratum in each individual's behavior which may be called the *common core of human striving* (Leighton 1959: 133–187). The attempt to use the topical headings I have mentioned in the personality studies revealed that they functioned very well among both Navahos and Eskimos, just as they had in Baltimore among people of my own culture. This seemed to show that as far as these three cultures were concerned there were some common processes at work (not easily explained by acculturation) constituting for each individual a core of activities upon which the influences of culture played. If this were true across all cultures, it meant that there were limits to cultural determinism and relativity, and it pointed to the importance of trying to define and identify the components of the core.

To some extent, of course, there was no argument about this. Even the strongest proponents of cultural relativity recognized a biological base that disposed people to strive for food, shelter, sex, and child-care. They rejected as biological, however, such processes as striving for love, social acceptance, prestige, opportunity to express aggression and numerous others. To me it seemed that, although there were differences in certain specifics, there was nonetheless a bundle of strivings that was common to the people of the three cultures that were under consideration—my own and the two in which the personality studies were being done. Because these cultures were so different from each other, it seemed that a common core of human strivings might be characteristic of mankind, even if at present only very dimly perceived in the social and life sciences, and poorly sorted out. At least this possibility was one that needed attention and clarification, and it appeared unwise for anyone to banish it from consideration by assuming that culture was what accounted for these psychological and social phenomena in their entirety.

3) The third set of ideas from the field work is *person variability* as another influence affecting each individual's common core of strivings. Within both the Navaho and Eskimo cultural groups there were marked variations of person functioning from one individual to another. This held across all observable behaviors, whether matters of opinion, attitude and sentiment, or matters of temperament such as alertness, fearfulness, optimism, steadiness, and torpor, or cognitive attributes such as perceptivity, memory and reasoning. Some people were quick and others were not, some were jolly while others were habitually sad, some had short fuses while others seemed to have infinite patience, some had a need to conform to social expectations, while others had a need to be different, and so on (Thomas and Chess 1977). Within each cultural group the range of personality patterns was very great.

Reflecting on the four sets of ideas and considering them together led to visualizing the behavior of persons as based on a bio-psycho-social core of strivings that varied under the

impact of two different but intermingling streams of influences. One was composed of constitutional, temperamental, and cognitive characteristics and the other of cultural factors. Both of these, however, varied a great deal. Individuals differed widely from each other in person characteristics that influenced their response to cultural factors, while within the globe of each culture there were marked intra-cultural variations. The whole picture turned out to be a lot more complicated than the notion—now widely prevalent—that people do things because their culture tells them to.

This perspective on human behavior had both theoretic and pragmatic implications. On the theoretic side it made ideas such as Jones' assertion that the oedipal situation was the cause of culture appear one-sided and not very much worth pursuing (Malinowski 1927:135–178) and the case was similar with the notion that culture could cause personality in the manner of Benedict's *Patterns of Culture* (Benedict 1934) or as in the later national character studies (Mead 1953).

With regard to the pragmatic implications, these are perhaps best presented by means of an example, and for this we must move forward in time to the later years of World War II (Leighton 1949:43–57). The problem was the refusal of Japanese military units to surrender even when it was plain to them that their position was hopeless. They seemed to prefer death to surrender and this was costly for the Allies. Could anything be done to turn such behavior around? The official and prevailing view was that nothing could be done to induce the Japanese troops to surrender. Many cultural anthropologists supported this and considered the behavior of the soldiers to be an illustration of the deterministic power of culture. Biological determinists also considered the behavior to be unchangeable, but based their opinion on race rather than on culture.

My colleagues and I, however, viewed the situation as an interplay among the factors of intra-cultural variability, person variability and common core strivings. After studying interviews with Japanese prisoners and the diaries kept by Japanese soldiers (which were often picked up on the battle field), we concluded that there were likely many among the Japanese troops who wanted to survive and some among these who would surrender. The problem was to facilitate this by appealing to those value elements within Japanese culture that would encourage the transition from hold-out to surrender. As you can see, this notion was like an intuitive form of what is now called “catastrophe” theory (Zeeman 1976). The upshot was that hold-out units were addressed through loudspeakers by prisoners who had surrendered and who could argue in Japanese cultural terms in favor of the move. The surrender of the units then began to occur first as a trickle, but soon became general. I do not wish to create the impression that our recommendations were the only factor involved in policy change. The point is that it represented a conclusion very different from those who leaned heavily on cultural determinism, and equally from that of those who relied on biological determinism.

## **Person in Society**

I would like now to turn from considering the domain of person-in-culture to the domain of person-in-society, or, more specifically, in social institutions, or as some might prefer, in societal systems. For illustration this means turning from Navahos and Eskimos and focusing on the Japanese in North America during a critical event in their history.

The event occurred in 1942 when the Japanese in North America suffered a sudden, massive interruption and destruction of their social institutions. It came about following Pearl



Harbor, when the U.S. and Canadian governments decided to move all Japanese people and all people of Japanese ancestry away from the West Coast. A hundred and twenty-four thousand people were thus forced to move to hurriedly constructed systems for feeding, sheltering and governing them (Leighton 1945).

I was in the Navy at this time and was borrowed by the Department of the Interior to make a study of the problems that might arise later on during military government of people of Japanese culture. This novel idea came from John Collier who, as head of the Bureau of Indian Affairs, had administrative responsibility for the first of the concentration camps. Containing more than 9,000 individuals, it was located near the town of Poston in the Colorado River Valley on the Mojave Reservation at the boundary between Arizona and California, a region that has some of the hottest summer weather in North America. During my year at Poston the temperature reached as high as 126° Fahrenheit in the shade.

It was very difficult at first to think about making a systematic study of the internment camp; the anguish of the people was numbing to everything except responsive feelings. It was also an exceedingly confusing picture. I was, however, fortunate in having very shortly the collaboration of two superb anthropologists—E. H. Spicer and Elizabeth Colson—and then a magnificent team of field workers, secretaries and typists recruited from among the residents of the camp. The story of how the team operated and cooperated could be an interesting one in itself, and so too might be an account of how our interdisciplinary methodologies were developed. That would, however, be another report from the one I have embarked upon, and I must move now to the central point I wish to make.

The notion of a common core of human striving together with inter-cultural, intra-cultural and person variation was vividly reinforced by what went on at Poston. Indeed, so far as person variation was concerned, there were times when I thought that every character in literature from Don Quixote to the Little Shepherd of Kingdom Come must have been herded together there.

But there was something more. The disaster of evacuation and internment, by the interruption and destruction it brought to social institutions, laid bare the existence of these systems and highlighted the multiple ways and different degrees by which they affected individuals. In particular the consequences of having institutions function badly or not at all became vividly apparent. In many ways the situation was like a sociological earthquake that not only slowed and stopped the movement of traffic, but also stripped away walls and showed what had been going on within. Brought sharply to view was the fact that many of these social institutions were what would now be called *support systems* upon which individuals depended in order to carry out and satisfy their common cores of human strivings (Caplan and Killilea 1976; Leighton 1959:133–187; Thoits 1982). The camp was filled with the enormous business of evacuees trying to make the place physically livable, but there appeared to be an even greater business about mending, rebuilding and replacing the social systems that had been disrupted, and a tremendous emotional reaction to the disturbance and destruction caused by their malfunction. One could not help being reminded of a broken anthill.

These facts brought to our attention another powerful variable, a fourth to be laid alongside the three I have previously mentioned: inter-cultural variability, intra-cultural variability, and person variability. It may be called the *functional variability of social institutions*, meaning by this the degree of effectiveness social systems have for helping the members of a population meet, through social means, the needs encompassed in the common core of human strivings—the strivings for survival, love, recognition, prestige and so on.

Retrospection made it evident that I had actually seen much of this before in a chronic rather than acute form among Navahos and Eskimos. One aspect was what in Western cultural terms would be called “poverty,” and in terms of their own cultures something like “not enough to eat,” “no game,” and “no power.” In both cultural groups the social institutions concerned with supplying food, shelter and protection from disease functioned poorly and uncertainly, so that malnutrition and starvation peeped over everyone’s shoulder. From this institutional malfunction, secondary effects radiated that adversely affected many other social institutions besides those concerned with food and shelter—for example, marriage practices, child rearing, kinship customs, curing, and the processes of group decision-making.

In coming to appreciate social institutions as functional variables, I was greatly influenced by Spicer, who in turn had studied with Radcliffe-Brown. Although Radcliffe-Brown and Malinowski had often been at odds with each other in matters of theory, in my thinking the social institutional perspective was not so much a break with Malinowski as an extension into new conceptual territory that was highly congenial to the psychobiological orientation, and full of promise as a framework for exploring the relationships between individual phenomena and societal processes.

For example, in the study of the concentration camp, one of our main efforts was to try to understand how the malfunctioning of social institutions appeared to the individuals involved. Obviously they did not say to each other, “My God, my social institutions have been shot out from under me.” Instead, they were conscious of a multitude of concrete dangers, threats, disappointments, deprivations and hurts, largely composed of distorted and broken human bonds. Despite their protean and manifold character, these can be classified into four major types of perceived predicament that were individually excruciating and collectively overwhelming.

Type 1 represents the classic situation of sign and symbol confusion. The sign-posts to “the good life” on which the person has been depending give contradictory messages, and the individual doesn’t know which way to go.

Type 2 represents the idea that because the sign-posts are unintelligible, almost all strivings bring frustrations and pain.

Type 3 consists of the concept that contradictory sign-posts combined with other contradictory pressures from incompetent social systems make the individual feel pulled in opposite directions against his will by ineluctable forces.

Type 4 indicates that the confusion of the sign-posts and the defeat of coping efforts result in complete and terrifying disorientation.

This way of classifying the psychological impact of malfunctional social institutions came primarily from observations in the concentration camp, but I was helped toward their formulation by previous acquaintance with two other types of stress situations. One of these was delirium, a condition in which disorientation plays a major and terrifying role. The other was observing Gantt’s experimental studies of neurotic-like behavior in dogs (Gantt 1944). These were derived from Pavlov’s original research and indicated how devastating and long-lasting the consequences could be of having to distinguish between signals that had become indistinguishable. It seemed to me that this might, in terms of process, have something in common with Type 1, the contradictory messages. Since those days, new work has thrown out further hints regarding how malfunctioning social institutions may have prolonged effects on individual behavior. Hebb’s work on the effects of sensory deprivation (Solomon et al. 1961) and Seligman’s studies of learned helplessness may well be highly relevant (Abramson 1978; Keen 1979).

In the study of the concentration camp, having noted the four types of perception described above, we further thought we could discern four patterns of reaction to these perceptions, especially when they were intense and lasting.

1) The first pattern of reaction may be described as rational and pragmatic. This does not mean that strong fearful emotion is necessarily absent in such people, but it is controlled in the interests of planful coping.

2) The second type of reaction consists in withdrawal, apathy, and indifference, constituting at times isolation from the malfunctioning social institutions. A refusal to enter into any kind of self-help often goes along with this type.

3) The third is a welling-up of hostility and aggression that can and does lead to violent action wholly inappropriate to the circumstances of the individual. Sometimes agents of the offending social institutions are attacked. More often available substitutes and scapegoats are utilized.

4) The fourth consists in unbridled fear leading to an inability to distinguish between real and imagined dangers. Delusions and even hallucinations are reported by some individuals.

To someone with interests such as mine in the relationships between sociocultural factors and mental illnesses, it was of interest to note that many of the reactions just listed, or combinations of them, had the appearance of being clinical syndromes, such as severe depression and paranoid schizophrenia. It was plausible to think, therefore, that disasters such as the Japanese evacuation might cause or at least precipitate clinical conditions, but we did not then have the methodology to find out. Even if we had been able to ascertain the rate of mental illness among the evacuees in 1942 or 1943, we would not have known how to interpret that figure because we would have had practically nothing with which to compare it. We would still not have known what the rate was among the evacuees before the war, nor what the rate was among the rest of the American population or in Japan. It was becoming more and more apparent that the field of mental illness and environment was sadly lacking in factual anchors.

## **Reassessment**

The end of the war was an opportunity to take stock before a new beginning. The general research issue remained as it had been: "What influences do sociocultural factors have on the origin, course, and outcome of mental illnesses?" The application of the social sciences to the war effort had brought about a number of advances that could be useful in tackling a question of this kind. One of the most important among these was the application of interdisciplinary cooperation in problem solving. This had begun for North America during the depression years, with a focus on rehabilitation and on rural community development, but it was greatly expanded at the time of the war (Sanders et al. 1949; Spicer 1952). Anthropologists, psychiatrists, psychologists, social psychologists, and sociologists in varying assortments worked together in trying to solve problems of military morale, both allied and enemy (Stouffer et al. 1949; Leighton 1949). There was also work on problems of industrial productivity (Whyte 1946). In addition there were interdisciplinary programs aimed at post-war planning, one of the most notable being a study of the cultural, social psychological, and educational needs of Amerind children (Joseph et al. 1949; D. C. Leighton and Kluckhohn 1947; Macgregor 1946; Thompson and Joseph 1944). Independently of these experiences among the social sciences, there was also the advent of operational research, the successes of which helped foster a climate of opinion in favor of problem solving through interdisciplinary cooperation (Weiss 1970). For many of us the

potential in all this was not simply pragmatics, but a promise of ultimate heuristic advance of considerable magnitude.

There remained, however, large obstacles, the seriousness of which has been confirmed in subsequent years. Let me mention just two: technical language and theory. By technical language I mean terms that are clearly defined and consistently employed. These did not exist for cross-disciplinary problem solving, and even within disciplines were largely absent. Kroeber and Kluckhohn (1952), as you may remember, produced a monograph showing that the term “culture” had 164 meanings. The same could have been done with words like “anxiety” and “psychoneurosis,” for their meanings not only shifted from author to author but, at this time, from page to page in the work of the same author. As a result, the concepts to which these words were supposed to refer were, to say the least, slippery.

The problems with theory encompassed these terminological and conceptual inconsistencies, but also a great deal more, including the reliance by researchers on appeal to intuitive feelings rather than scientific validity, and the tendency to confuse speculative fantasies with verified processes in nature—both mentioned earlier. To these may now be added efforts to construct complete explanatory systems within the arbitrary boundaries of one discipline.

In saying all this I am, of course, being somewhat unfair to the social science disciplines because I am looking at them from the perspective of my research problem and not in terms of their own orientation and goals. Things look different depending on whether your aim is to create a theory of sociology, culturology, or metapsychology. Such academic purposes, however, do not mesh very well with the needs inherent in many of the problems in medicine that involve better understanding of human behavior. Nor was I alone in feeling this way. Donald Young, a sociologist and at one time President of the Social Science Research Council, and later head of the Russell Sage Foundation, wrote: “The related disciplines of sociology, social psychology, and social anthropology have been inadequate in concept, research method, and substantive content for the purposes of medical research and practice” (Reiser 1980:212).

Isolationist tendencies of the social sciences had been in abeyance during the war, but were prompt to reassert themselves afterward, and those who crossed disciplinary lines began to be regarded with disapproval. This was troubling because it appeared by now unlikely in the extreme that any discipline could hold a key-in-lock relationship to the kind of research issue under discussion.

Nevertheless, Young was more harsh than I was willing to be, for it seemed to me there were some useful methods and substantive findings which could help build a research program. Grand theories were certainly very problematic, whether in psychiatry or in the social sciences, but theories of the middle range looked as if they could be helpful in guiding advance on my central question for research. For me middle-range theories included the cultural perspective, Meyer’s psychobiology, the functional concepts of both Malinowski and Radcliffe-Brown, some of Durkheim’s ideas (especially as they were being developed and reformulated by Robert Merton), Linton’s thoughts on role and status, and McDougal’s theory of sentiments (McDougal 1936) together with bits and pieces of learning theory.

In sum, my post-war stock-taking suggested that it would be desirable at this time to steer away from large theories and to focus on generating data, on careful picking and shovelling to uncover empirical detail that would enable us to measure differences unequivocally and by this means begin separating the more from the less likely theories. Taking the four major sets of variables as guides, one approach seemed to be to try to re-do the concentration camp study in a normal population, something like a replay in slow motion. One could expect that the major

sociocultural processes would be there and at work, but they would be moving less swiftly and dramatically, more in fiduciary equilibrium, yet with many lapses which could be identified and their effects studied. This time it should be possible to go much further in the matter of taking measurements.

### **Person in Sociocultural Processes**

The result of these deliberations was a study of a population in Nova Scotia that my colleagues and I named the Stirling County Study (Murphy 1980). The focus was on 20,000 people who constituted an ecological unit marked off by natural boundaries of sea, forest, and rivers. It was a rural area with some 97 named settlements, and divided into English- and French-speaking culture moieties. There was also a range of occupations and socioeconomic levels across both cultural groups.

The initial steps of the study consisted in three parts: 1) to describe the two cultures and all the main social institutions characteristic of this geographically defined population; 2) to find and diagnose by means of objective criteria every case of mental illness in the population; and 3) to identify the location of each such case in relation to the two cultures and to the social institutions. In other words, make a sociocultural map of the county and then locate on it the position of every discovered case. Through these activities we hoped to get information bearing on three questions: 1) How much mental illness is there in the population? 2) Of what kinds are they? and 3) How are they distributed in relation to the main demographic, economic, cultural, and social institutional features (Leighton 1959:3).

The methods employed for studying the county were developed as a series of successive approximations. First came an ethnographic description of the whole based on participant observation and interviewing key informants. This was followed by questionnaire surveys, the analyses of census and other documentary data, intensive studies of selected communities, photographic surveys, studies of occupational roles, of folkways, of sentiments, and of selected personalities (Hughes et al. 1960).

The methods employed for case finding were based on the Hopkins-Maudsley principles mentioned earlier in the quotation from Michael Shepherd. At first we thought it would be possible to find all persons suffering from a mental illness, but it became apparent that the number was too great to allow this, and so we turned to the use of probability samples. To each such sample we applied several techniques in order to identify every individual suffering from a mental illness—and in addition the type of illness—regardless of whether he or she had ever had any previous diagnosis or treatment from a medical or psychological service. The results obtained were subject to numerous tests of reliability and validity (Murphy 1980; D. C. Leighton et al. 1963; A. H. Leighton et al. 1966).

The time point of reference for the prevalence study was 1952, but we shortly afterward established this as a base-line for a longitudinal study that continued by means of intermittent resurveys until 1970. The purpose of this was to discover time trends in those relationships that had been found at the base-line.

From the beginning there were questions about the generalizability of findings made within the ramparts of Stirling County. Checking these led to studies in other parts of Nova Scotia, among the Eskimos (Murphy 1976b), in New York City (Benfari et al. 1974), in southern France (Brunetti 1964), in Nigeria (A. H. Leighton et al. 1963), and in Sweden (D. C. Leighton et al. 1971). After 1970 comparative work was also done in Senegal (Beiser et al. 1974) and in

Vietnam, where the focus was on civilian populations under the severe stress of war conditions (Murphy 1977).

Both before and during the time that our research in Stirling County was in progress, other studies focusing on mental illness rates in general populations came into existence. In the beginning these projects were few and often conducted in isolation from each other and from the mainstream in psychiatry and the social sciences. With time, however, there has been a considerable increase in the number, variety, and level of sophistication of studies, and progressively more sharing in a common frame of reference so that the findings by one group of researchers can more easily be related to the findings of others. There is also worldwide geographic spread that includes not only this continent, the United Kingdom, and northern Europe, but also Africa, Australia, both Chinas, Japan, parts of Latin America, Southeast Asia, and Spain. In some ways this psychiatric epidemiologic research resembles the geographically scattered paleontological work on hominids that is gradually, through many specific details, beginning to reveal a synoptic picture.

The most startling finding in all of this is the magnitude of the prevalence rate of clinically definable mental illnesses. For all kinds of disorders lumped together, the rate runs at about 20% in most natural populations, give or take 5% (Regier et al. 1978:691; Dobrenwend et al. 1980; Leighton 1982:13). This figure by itself tells us nothing about social environmental factors, but it nevertheless has some meaning for applied anthropology. This is because it points to the need for radical alterations in the way mental illnesses are conceptualized, in the way psychiatry is taught in medical schools, and in the policies, priorities, and administrative organization of governmental departments of health and welfare. The skills and knowledge of applied anthropologists will be much needed in the design and management of such changes.

A second point is that cultures as wholes, when compared one with another do not show a very powerful relationship to the prevalence rates of mental illnesses. No culture appears to have the ability to protect its people from mental illness and none appears to foster inadvertently much higher rates than others. For example, Jane Murphy (1972, 1976b) has found that psychotic disorders are remarkably similar across societies that have cultures as fundamentally different as Eskimo, Yoruba, and Nova Scotian. The expectations of Ruth Benedict have not, on the whole, been fulfilled. None of this, of course, diminishes the significance of culture in spheres other than mental illness, and it does not obviate the possibility that important subtle and so-far undetected influences on psychopathology do exist.

Third, intra-cultural variation as such has not as yet been given very much attention in epidemiologic studies, except insofar as it may be a component in socioeconomic class levels. It remains a topic deserving of a good deal more research attention than it has so far secured.

Fourth, with regard to *person variability*, perhaps the most interesting feature that has come to light is that some individuals appear to be extraordinarily resistant to developing mental illness. If, for instance, adverse circumstances play a part in precipitating clinical depression and anxiety, it would appear that there are some individuals who do not react with these clinical syndromes—nor in any other disordered way—even under the most extreme and prolonged situations of stress. The most stressed population we have studied consisted of civilians from a village in Vietnam who over a five-year period experienced almost every kind of abuse a war can impose on individuals short of extermination (Murphy 1977). Even so, some of these people did not, according to our methods, manifest anything that could be called clinical anxiety or clinical depression.

To date epidemiologic studies have given most of their attention to the ill and to the symptomatic. This is doubtless because of the pragmatic orientation of public health and the focus on illness characteristic of all clinical work. Yet from a heuristic point of view, the people who stand out because of their persistent freedom from symptoms are of major interest and ought to be the subjects of intensive and extensive investigation aimed at understanding the nature of the factors that enable them to be this way. This seems to me to constitute a most important target for interdisciplinary teamwork, but so far it has hardly been touched. It is a foothold for beginning comparative studies of wellness, and it is vital to expanding knowledge regarding the impact of sociocultural factors as well as of those that may be more constitutional in their nature (Leighton 1979).

Finally, the malfunctioning of social institutions and high prevalence rates of mental illnesses is one of the strongest associations that has so far been uncovered in psychiatric epidemiology (Dohrenwend and Dohrenwend 1969; Hollingshead and Redlich 1958; D. C. Leighton et al. 1963; Srole et al. 1962; Langner and Michael 1963; Taylor and Chave 1964; Leighton 1974). It seems to operate everywhere regardless of culture, with poverty areas being the most outstanding examples of where badly functioning social institutions and people with mental illnesses congregate together. It is also a fact fraught with immense implications for the future of mankind, whether considered from the point of view of controlling mental illness, poverty, civil disturbance, or wars. The association of illusions, delusions and irrationality with social incompetence strongly suggests the presence of one or more vicious circles that we need to understand better than we do.

Unfortunately it is proving extremely difficult to identify and determine the direction of causal trends in this association of mental illness rates and the malfunctioning of social institutions. One set of theories would recall types 1 through 4 and say that these kinds of stresses, when severe, induce mental illnesses in susceptible people, and further that most natural populations contain large numbers of such susceptibles (Murphy 1976a).

Another set of theories puts emphasis on what Dunham has called “social selection,” whereby mentally ill individuals both select for themselves and are pushed into the less structured, less demanding, and less desired parts of society (Dunham 1974). Here they pile up and by their presence contribute heavily to the malfunctioning of the social institutions. Brenner’s studies regarding the delayed impact of economic cycles and how they shift people vertically in society hold promise of illuminating this process (Brenner 1980).

At least indirectly relevant to both these theoretical sets are research efforts such as those of Asch, Milgram, Sherif, Zimbardo (Bronfenbrenner 1979:86–102), and others, that have shown the tremendous impact even very temporary social situations can have on the perceptions, orientation, values, behavior, and emotional stability of individuals and of small groups.

Putting all this together, one may conclude that the associations being demonstrated by psychiatric epidemiology call for a continuing, problem-oriented, comprehensive interdisciplinary approach—something able to encompass what Bronfenbrenner (1979) has so aptly called “experiments by nature and design.” One of the themes that runs through the history and traditions of anthropology is just this kind of comprehensiveness. It takes shape from the *experience* of studying all aspects of life in a given island or tribe from economics to psychological traits; and from the *ideal* of understanding the whole man in his total environment. Epidemiological problems open an important field for this kind of anthropological perspective and synoptic talent.

The talent, however, must include an interest in working with multiple factors that interact with each other and therefore often mask each other's effects. The presence of mental illnesses in populations is emerging into view as something like a table top held up by two dozen legs. This means that if you tamper with a leg or even remove one—or indeed several—the table will continue to stand there. This is what multifactoriality often means.

Magicians have a standard operating procedure when doing illusions before small groups. If you are watching one and detect how he did a particular card trick and you challenge him with your discovery, he will make you think you are wrong by doing the trick again, getting exactly the same effect—but by a different method. Nature is like that, and this is why she has to be observed synoptically, from many disciplinary angles and repeatedly across time.

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### References Cited

- Abramson, L. Y. et al.  
1978     Learned Helplessness in Humans: Critique and Reformulation. *Journal of Abnormal Psychology* 87(1):49–74.
- Benedict, R.  
1934     *Patterns of Culture*. New York: Houghton-Mifflin.
- Benfari, R. C., M. Beiser, A. H. Leighton, J. M. Murphy, and C. J. Mertons  
1974     The Manifestation of Types of Psychological States in an Urban Sample. *Journal of Clinical Psychology* 30(4):471–483.
- Beiser, M., W. A. Burr, H. Collomb, and J. L. Ravel  
1974     Pobough Lang in Senegal. *Social Psychiatry* 9:123–129.
- Brenner, M. H.  
1980     Industrialization and Economic Growth: Estimates of Their Effects on the Health of Populations. *In Assessing the Contributions of the Social Sciences to Health*. M. Brenner, A. Mooney and T. J. Nagy, eds. Pp. 65–115. Boulder, CO: Westview Press.
- Bronfenbrenner, U.  
1979     *The Ecology of Human Development*. Cambridge, MA: Harvard University Press.
- Brunetti, P. M.  
1964     A Prevalence Survey of Mental Disorders in a Rural Commune in Vaucluse: Methodologic Considerations. *Acta Psychiatrica Scandinavica* 40:323–358.
- Caplan, B., and M. Killilea  
1976     *Support Systems and Mutual Help*. New York: Behavioural Publications.
- Dohrenwend, B. P., and B. S. Dohrenwend  
1969     *Social Status and Psychological Disorder*. New York: Wiley.



- Dohrenwend, B. P., B. S. Dohrenwend, M. S. Gould, et al.  
1980     Mental Illness in the United States. New York: Praeger.
- Dunham, W. H.  
1974     Social Causation and Selection Theories in Schizophrenia. *Social Psychiatry*, Vol. I. J. H. Masserman and J. I. Schwab, eds. New York: Grune and Stratton.
- Gantt, W. H.  
1944     Experimental Basis for Neurotic Behavior. New York: Hoeber.
- Hollingshead, A. B., and F. C. Redlich  
1958     Social Class and Mental Illness. New York: Wiley.
- Hughes, C. C., M. A. Tremblay, R. N. Rapoport, and A. H. Leighton  
1960     People of Cove and Woodlot. New York: Basic Books.
- Joseph, A., R. Spicer, and J. Chesky  
1949     The Desert People. Chicago: University of Chicago Press.
- Kardiner, A.  
1939     The Individual and His Society. New York: Columbia University Press.
- Kardiner, A., R. Linton, C. DuBois, and J. West  
1945     The Psychological Frontiers of Society. New York: Columbia University Press.
- Keen, J. D., ed.  
1979     Psychopathology in Animals. New York: Academic Press.
- Kroeber, A. L., and C. Kluckhohn  
1952     Culture: A Critical Review of Concepts and Definitions. *Peabody Museum Papers* 47(1), Harvard University.
- Langner, T. S., and S. T. Michael  
1963     Life Stresses and Mental Health: The Midtown Manhattan Study. Vol. II. London: Free Press of Glencoe, Collier-Macmillan.
- Leighton, A. H.  
1932     Notes on the Beaver's Individuality and Mental Characteristics. *The Journal of Mammalogy* 13(2):117–126.  
1933     Notes on the Relations of Beavers to One Another and to the Muskrat. *The Journal of Mammalogy* 14(1):27–35.  
1935     Notes on the Behaviour of Norwegian Beaver. *The Journal of Mammalogy* 16(3):189–191.  
1945     The Governing of Men. Princeton, NJ: Princeton University Press.  
1949     Human Relations in a Changing World. New York: E. P. Dutton.  
1952     Introduction to: The Collected Papers of Adolf Meyer. E. E. Winters, ed. Vol. IV, Mental Hygiene. Baltimore: Johns Hopkins Press.  
1959     My Name Is Legion. New York: Basic Books.  
1974     The Erosion of Norms. *Australian and New Zealand Journal of Psychiatry* 8:223–227.  
1979     Research Directions in Psychiatric Epidemiology. *Psychological Medicine* 9:235–247.  
1982     Caring for Mentally Ill People: Psychological and Social Barriers in Historical Context. New York: Cambridge University Press.
- Leighton, A. H., and D. C. Leighton, with Catherine Opler  
1949     Gregorio the Hand Trembler: A Psychobiological Personality Study of a Navaho Indian. *Peabody Museum Papers* 40(1), Harvard University.

- Leighton, A. H., T. A. Lambo, C. C. Hughes, D. C. Leighton, J. M. Murphy, and D. B. Macklin  
1963     Psychiatric Disorder among the Yoruba. Ithaca, NY: Cornell University Press.
- Leighton, A. H., D. C. Leighton, and R. A. Danley  
1966     Validity in Mental Health Surveys. *Canadian Psychiatric Association Journal* 11(3):167–178.
- Leighton, D. C., and C. Kluckhohn  
1947     The Children of the People. Cambridge, MA: Harvard University Press.
- Leighton, D. C., J. S. Harding, D. B. Macklin, A. M. Macmillan, and A. H. Leighton  
1963     The Character of Danger. New York: Basic Books.
- Leighton, D. C., A. H. Leighton, O. Hagnell, I. S. Harding, S. R. Kellert, and R. A. Danley  
1971     Psychiatric Disorder in a Swedish and a Canadian Community: An Exploratory Study. *Social Science and Medicine* 5:189–209.
- Macgregor, G.  
1946     Warriors without Weapons. Chicago: University of Chicago Press.
- Malinowski, B.  
1927     Sex and Repression in Savage Society. London: Kegan Paul.
- McDougal, W.  
1936     An Introduction to Social Psychology. London: Methuen.
- Mead, M.  
1953     National Character. *In Anthropology Today*. A. L. Kroeber, ed. Chicago: University of Chicago Press.
- Merton, R. K.  
1968     Social Theory and Social Structure (1968 enlarged edition). New York: Free Press.
- Murphy, I. M.  
1972     A Cross-Cultural Comparison of Psychiatric Disorder: Eskimos of Alaska, Yorubas of Nigeria, and Nova Scotians of Canada. *In Transcultural Research in Mental Health*. William P. Lebra, ed. Pp. 213–226. Hawaii: The University Press of Hawaii.  
1976a     Social Causes: The Independent Variables. *In Further Explorations in Social Psychiatry*. B. H. Kaplan, R. N. Wilson, and A. H. Leighton, eds. Pp. 386–406. New York: Basic Books.  
1976b     Psychiatric Labelling in Cross-Cultural Perspective. *Science* 9:1019–1028.  
1977     War Stress and Civilian Vietnamese: A Study of Psychological Effects. *Acta Psychiatrica Scandinavica* 56:92–108.  
1980     Continuities in Community-Based Psychiatric Epidemiology. *Archives of General Psychiatry* 37:1215–1223.
- Regier, D. A., I. D. Goldberg, and C. A. Taube  
1978     The de facto U.S. Mental Health Service System: A Public Health Perspective. *Archives of General Psychiatry* 35:685–693.
- Reiser, S. T.  
1980     The Bearing of Social Science Knowledge on Medical Education and Practice. *In Assessing the Contributions of the Social Sciences to Health*. M. H. Brenner, A. Mooney, and T. J. Nagy, eds. Pp. 209–216. Boulder, CO: Westview Press.
- Sanders, I. T., et al.  
1949     Experience with Human Factors in Agricultural Areas of the World. Washington, D.C.: Extension Science and Office of Foreign Agricultural Relations, U.S. Department of Agriculture.

- Sankoff, G.  
1972 Cognitive Variability and New Guinea Social Organization: The Buang Dgwa. *American Anthropologist* 74(3):555–566.
- Sheperd, M., and O. L. Zangwill, eds.  
1983 *Handbook of Psychiatry 1, General Psychopathology*. Cambridge: Cambridge University Press.
- Solomon, P. et al., eds.  
1961 *Sensory Deprivation*. Cambridge, MA: Harvard University Press.
- Spicer, E. H., ed.  
1952 *Human Problems in Technological Change*. New York: Russell Sage Foundation.
- Srole, L., T. S. Langner, S. T. Michael, M. K. Opler, and T. A. C. Rennie  
1962 *Mental Health in the Metropolis: The Midtown Manhattan Study*. New York: McGraw-Hill.
- Stouffer, A. S., A. A. Lumsdaine, M. H. Lumsdaine, et al.  
1949 *The American Soldier: Combat and its Aftermaths*. Princeton: Princeton University Press.
- Taylor, L., and S. Chave  
1964 *Mental Health and Environment*. London: Longmans, Green.
- Thoits, P. A.  
1982 Conceptual, Methodological and Theoretical Problems in Studying Social Support as a Buffer against Life Stress. *Journal of Health and Social Behavior* 23:145–159.
- Thomas, A., and S. Chess  
1977 *Temperament and Development*. New York: Brunner/Mazel.
- Thompson, L., and A. Joseph  
1944 *The Hopi Way*. Chicago: University of Chicago Press.
- Weiss, J. M. A.  
1970 (introductory editorial). *Journal of Operational Psychiatry* 1(1).
- Whyte, W. F.  
1946 *Industry and Society*. New York: McGraw-Hill.
- Zeeman, E. C.  
1976 Catastrophe Theory. *Scientific American* 234(4):65–83.

## **Chapter 15**

### **Elizabeth Colson:**

### **American Africanist and Resettlement Theory**

**Thomas Weaver**

The projects that best define the applied anthropology of Elizabeth Colson, recipient of the Society for Applied Anthropology's Malinowski Award in 1985, are those completed at a War Relocation Authority Japanese internment camp during the Second World War, others completed at the Rhodes-Livingstone Institute in Africa, and her work on resettlement.<sup>1</sup> Among those who influenced her ideas about applied anthropology were Edward Spicer and Alexander Leighton, fellow Malinowski Award winners and colleagues in the Japanese internment episode. (See Spicer 1976 and Leighton 1984.) This experience alerted her to the problems of relocated people and led to what may be her most important applied contribution, the work on resettlement with Thayer Scudder, also a Malinowski Award winner (Scudder 1999). Other important teachers or colleagues included Wilson and Ruth Wallis, Bernard and Ethel Aginsky (for field training), and Clyde Kluckhohn. Colson's work came during the ambivalence toward applied anthropology that characterized the immediate post-World War II period.

Colson (1917–) received bachelor's and master's degrees from the University of Minnesota in 1938 and 1940. Her doctorate was completed at Radcliffe College in 1945 and involved a study of the Makah, a North American Indian tribe (Colson 1953). Before completing her doctorate, she also did fieldwork among the Pomo and at the Poston Camp of the War Relocation Authority. In 1945, she accepted a position with the Rhodes-Livingstone Institute in Africa where she came in contact with British anthropologists taught by A. R. Radcliffe-Brown and Bronislaw Malinowski. Among these were Max Gluckman, Godfrey and Monica Wilson, Audrey Richards, Lucy Mair, and fellow Malinowski Award winner Raymond Firth (1981). Colson was director of the institute from 1948 to 1951. Later she conducted fieldwork with the Plateau Tonga of Zambia, in Australia, and among the Navajo. She taught at a number of universities including Manchester, Goucher, Boston, and Northwestern before going to the University of California at Berkeley in 1964.

Colson's theoretical orientation in her study of the Makah Indians of Washington was rooted in the field of culture and personality, but the study concluded with an "assimilationist" perspective. Her work became more functional and applied as she began to focus on the solution of social issues at the Rhodes-Livingstone Institute. There she examined the impact of cash economy, labor migration, and cash cropping on family and systems of social control. The influence of Max Gluckman, director of the Rhodes-Livingstone Institute, and others she worked with led to an academic career, but one that was sympathetic to applied anthropology (Colson

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e-mail to author, 1998). This is reflected in her long-term interests in the Gwembe Tonga (1962, 1988), work begun in 1946 that resulted in a theoretical framework developed with Thayer Scudder for dealing with resettlement issues (Scudder and Colson 1982). This framework, along with other studies by anthropologists, influenced the development of resettlement policies for the World Bank by Michael Cernea (1995), a later recipient of the Malinowski Award.

Colson's Malinowski Award acceptance speech, entitled "Using Anthropology in a World on the Move" (1985), provides an interesting summary of applied projects and of the anthropologists who influenced her. Spicer and Gluckman, both students of Radcliffe-Brown, inspired her to look for the broader institutional context of social issues in the political system. Accepting such a framework for understanding social problems, however, is not sufficient, she maintained. It is also important to understand that policy influence operates in the context of power and politics.

Malinowski and the founders of the Society for Applied Anthropology, and the neophytes among us today, appear to work with the assumption that societies are integrated structures based on a commonality of interest, rather than arenas within which those with different interests test their power and attempt to impose their will upon others, admittedly sometimes with the best of intentions for the general good, as they define it (Colson 1985:192).

Colson suggested that one solution to the problem created by power concentration is decentralized management, that is, the transfer of decision-making power to the local community and more direct involvement by anthropologists in the policy process (1985:194). Decentralization runs against the power and politics of the central bureaucracy and government driven as they are by the need to protect their interests and prohibit opposition groups from gaining power. Colson maintained that "... much of what happens in the transmission from the center to the periphery involves political maintenance rather than technical advance or any real expectation that life chances of those at the bottom will be affected" (Colson 1985:194). Further, she argued that moaning over the problems and constraints in policies and poorly directed projects does little good. Postmortems on failed projects are not effective. We must "become better political animals within the arenas in which we now operate . . . [without] becoming absorbed by the system and accepting its premises." And again, later, she asserted that "to be heard . . . [we] must become part of a system which has its own dynamic" (Colson 1985:195).

Elizabeth Colson has received many honors in addition to the Malinowski Award. Among these are memberships in the National Academy of Sciences and the American Academy of Arts and Sciences. The American Anthropological Association honored her as Distinguished Lecturer, and the Royal Anthropological Institute made her an Honorary Foreign Fellow. She was a Morgan Lecturer at the University of Rochester, a Fairchild Lecturer at the California Institute of Technology, a Harvey Lecturer at the University of New Mexico, and a Faculty Research Lecturer at the University of California, Berkeley. She has received honorary doctorates from Brown University, the University of Rochester, and the University of Zambia.

## Notes

<sup>1</sup>This annual award is given to a senior colleague in recognition of efforts to understand and serve the needs of the world through social science. See Thomas Weaver's "The Malinowski Award and the History of Applied Anthropology" (2002b) for a brief history of this award and an overview of the recipients and their work, and Weaver's "Malinowski as Applied Anthropologist" (2002a) for an introduction to Bronislaw Malinowski and his work.

## References Cited

- Cernea, Michael  
 1995 Social Organization and Development Anthropology. *Human Organization* 54(3):340–352. (Republished at <<http://www.sfaa.net/>>.)
- Colson, Elizabeth  
 1953 *The Makah Indians: A Study of an Indian Tribe in Modern American Society*. Manchester: Manchester University Press.  
 1962 *The Plateau Tonga of Northern Rhodesia, Social and Religious Studies*. Manchester: Manchester University Press.  
 1985 Using Anthropology in a World on the Move. *Human Organization* 44(3):191–196. (Republished at <<http://www.sfaa.net/>>.)
- Firth, Raymond  
 1981 Engagement and Detachment: Reflections on Applying Social Anthropology to Social Affairs. *Human Organization* 40(3):193–201. (Republished at <<http://www.sfaa.net/>>.)
- Leighton, Alexander  
 1984 Then and Now: Some Notes on the Interaction of Person and Social Environment. *Human Organization* 43(3):189–197. (Republished at <<http://www.sfaa.net/>>.)
- Scudder, Thayer  
 1999 The Emerging Global Crisis and Development Anthropology: Can We Have an Impact? *Human Organization* 58(4):351–364. (Republished at <<http://www.sfaa.net/>>.)
- Scudder, Thayer and Elizabeth Colson  
 1982 From Welfare to Development: A Conceptual Framework for the Analysis of Dislocated People. *In* *Involuntary Migration and Resettlement: The Problems and Responses of Dislocated People*. A. Hansen and A. Oliver-Smith, eds. Pp. 267–287. Boulder, CO: Westview Press.  
 1988 For Prayer and Profit: The Ritual, Economic, and Social Importance of Beer in Gwembe District, Zambia, 1950–1982. Stanford: Stanford University Press.
- Spicer, Edward  
 1976 Beyond Analysis and Explanation? Notes on the Life and Times of the Society for Applied Anthropology. *Human Organization* 35(4):335–343. (Republished at <<http://www.sfaa.net/>>.)
- Weaver, Thomas  
 2002a Malinowski as Applied Anthropologist. *In* *The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers*. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.  
 2002b The Malinowski Award and the History of Applied Anthropology. *In* *The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers*. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.

## Using Anthropology in a World on the Move

Elizabeth Colson

The Society for Applied Anthropology does me great honor in its gift of the Malinowski Award for 1985. It is humbling to find myself associated with the previous recipients of the award who have done so much to demonstrate that anthropology is more than an academic discipline or a literary conceit. It is some indication of how close-knit the company of anthropologists has been that I have known, at least slightly, all save two of the previous recipients. A number played a major role in shaping my own career and so perforce that of students who later worked with me.

With Edward Spicer (1976) and Alexander Leighton (1984) I shared the experience of Poston, one of the camps where Japanese-Americans and their parents were interned during World War II, the beginning of my concern for the indecency of forced resettlement, whether this be for what is known as development or because of the even greater indecency of war. That concern led to my involvement in a major study of the Gwembe Tonga who occupied what was to become the Lake Kariba Basin and were sacrificed so that a hydro-electric plant could be built to empower industrialization within Central Africa. From that study my colleague, Thayer Scudder, has gone on to create a general theory dealing with reactions to uprooting and stages of readjustment (Scudder and Colson 1982). His work as a consultant advising on the problems associated with resettlement brought him recognition at the 1984 meetings of the American Anthropological Association when he became the first recipient of its Solon Kimball Prize for Public and Applied Anthropology. A further spin-off of that Poston experience may be the recent founding of a Center for the Study of Migration and Resettlement, the work of a group of graduate students at Berkeley who are concerned with what is happening to Laotian, Vietnamese, and Central American refugees in Thailand, France, and the United States, and the experiences of others who have voluntarily sought for new opportunities in a new land. Since this group may include the recipient of the Malinowski Award for 2020 I shall give you their names: Oekan Abdoellah, Giselle Bousquet, Jonathan Habarad, Mutsu Hsu, Eun-Young Kim, Scott Morgan, Marcello Orozco-Suarez, and Louisa Schein.

It was at Poston, too, that I learned from Spicer how crucial it is to look at the institutional organization that frames action and to see this as a political system. I suspect that he learned this as a student of Radcliffe-Brown rather than through any contact with Malinowski; at least I found the same approach basic to the work of Max Gluckman, another student of Radcliffe-Brown. I have come to see it as crucial for all those who are concerned with change. Wherever the approach came from, it stood me in good stead in 1948 when I presented a paper based on my first year of research among Plateau Tonga. This was in the famous seminar at the London School of Economics, founded by Malinowski, but then and for many years presided over by Raymond Firth (1981). The paper examined the contemporary Tonga political system as an artifact created by the colonial system which contained it, to be understood against the

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This address was previously published in *Human Organization* 44,3(1985):191–196.

expectations and power of the British colonial officials and the competing demands of the local population which had its own views of the proper role of government and the meaning of authority. In a sense, that seminar under the direction of Firth was examining the question that haunts all of applied anthropology: who can do what with whom and under what circumstances.

My colleague, George Foster, who received the Malinowski Award in 1982, used much the same approach when he looked back upon his involvement with public health workers and explicated the limitations on the role of technical consultants whose advice is given to actors who operate within what is in effect a political arena (1982). From his own long-term research in Tzintzuntzan, Foster also has illuminated the rapidity with which people respond to shifts in perceived political and economic opportunities as the national and international climate changes, and so expectations and values feed on one another and those now being championed may not guide future action. Thayer Scudder and I have found the same rapid switch in expectations and attitudes among Gwembe Tonga, whom we have followed since 1956 as they have faced the fears and rigors of resettlement, the ebullience associated with the coming of independence, the rapid expansion of opportunity in the 1960s and early 1970s, and then the decline in the Zambian economy since 1973 (Scudder 1983, 1984; Colson 1984). Such transformations make more difficult the task of the applied anthropologist who must extrapolate from old experience in seeking to forecast the future.

I have stressed my links with those who have helped to extend the anthropologist's involvement with the world out there, for on an occasion such as this we need to be reminded that we form a community with a tradition. At the same time, today there is a break with the past, for I think I am the first to receive the award to belong to the generation which neither knew Malinowski in the flesh, nor took part in the founding and early work of the Society for Applied Anthropology. I can add nothing to the reminiscences of Malinowski given to you by his students Fei Xiaotong (1980) and Raymond Firth (1981), though I can speak to the importance of his books to my generation of students who learned from them that anthropology properly deals with people who are very much alive and have a future: too much of our reading was in the standard ethnographic monographs which enshrined what old men and women could remember about a past seen as irrevocably over. Malinowski invoked a cast of Trobriand Islanders who drew upon the past as they planned for future action: they were going to plant gardens and make *urigubu* gifts and go on *kula* expeditions and celebrate the return of the dead, and they bent their actions to achieve these ends, calculating both social and material resources as they looked to the future.

Although it is now fashionable to decry Malinowski's functionalism, that too was a contribution, for in practice it meant that we were trained to look for interconnections across fields of action in a systematic fashion and to ask "If this changed, what else would happen?," no bad directive whether one is an applied or an academic anthropologist. In fact, while functionalism never was very much as theory it provided a good working method. Today, of course, one would use other terms since each generation needs its own vocabulary—and so we might claim to be developing working models, or writing thick description, or adopting a holistic approach, rather than undertaking a functional analysis. The old term, however, meant something more in the context of applied anthropology, for if something can function it also can fail to function, and this calls for investigation of what has gone wrong; and if something is working, one needs to ask what will happen if one tries to tinker with a working system by introducing change. In a time and place that honored the technician and engineer, the term "functionalism" invoked associations that made a place for the anthropologist as technician. In that time and



place such terms as “thick description” or “holism” would have gotten applied anthropology nowhere. I suspect they would not get it very far today. “Model,” on the other hand, is a good term now, so that even the archaeologists have adopted it.

Edward Spicer in his Malinowski Award lecture (1976) told something of Malinowski’s direct contribution to the development of applied anthropology through his claim that we are trying to understand functioning systems. Lawrence Kelly (1985), still more recently, has demonstrated that it was functionalism, as preached by Malinowski and Radcliffe-Brown and practiced by William Lloyd Warner, that helped to legitimate anthropology among the social sciences and so gave it a claim on the \$55 million dollars expended by the Laura Spelman Rockefeller Foundation between 1922 and 1929 in enlisting social scientists in the solution of current social problems. It was this that provided the critical mass which led to the foundation of the Society in 1941 and from the beginning associated it with a problem-solving mission: those anthropologists who looked at industrial settings or at rural communities in the depth of the depression did so with an intent to do more than describe. They hoped to change the future and had a sense of high adventure.

I well remember the stir around Peabody Museum in the spring of 1941 when various well-known figures, including Eliot Chapple and Frederick Richardson, appeared in its halls and smoking room and the rumor spread that a new association was being created by anthropologists who had skills for social engineering. The importance of the occasion was impressed upon us by the fact that we graduate students were told to keep our distance, that as yet we had nothing to offer to such an association.

Before I was senior enough to be welcomed into membership, I had left the United States for Northern Rhodesia to work at the Rhodes-Livingstone Institute which had been formed in 1937 as another reflex of the belief that the social sciences could and should contribute to solutions of the issues of the day. But there the social scientists were expected only to supply accurate information. According to Max Gluckman, its second director, under whom I worked, the Institute was founded with three chief aims. I quote them because they give some idea of the innocence with which we faced the world in those days, working on the assumption that it is possible to know the truth and the truth will prevail. Those appointed to the Institute were: “(1) to analyse scientifically the social life of modern man, White and Black, in Central Africa; (2) to provide accurate scientific information on the social life of man for Governments and other persons working with human beings in this area; and (3) to disseminate this accurate information as widely as possible to the public” (Gluckman 1944a:5). I, and the other officers appointed in 1945, were instructed to examine the impact upon rural life, especially upon familial relationships and systems of social control, of increasing involvement in the cash economy through labor migration or cash cropping. The underlying assumption was that accurate information would make for good policy and good policy would lead to right action.

Godfrey Wilson, the first director of the Institute, had based his own work upon this assumption. According to Gluckman, “He believed that men, once they could be shown the reason for social ills and the scientific methods of eliminating these, would accept the conclusions of the sociologist” (Gluckman 1944b: 1). What Wilson failed to recognize, and what many another anthropologist has failed to recognize since that time, was that he was operating in a charged political arena where the accuracy of information would be assessed by the degree to which it supported established positions. He and his wife, Monica Wilson, undertook a study among Africans working in the mining town of Broken Hill. They came to the conclusion, backed by a wealth of evidence, that some of the workers had already become urbanized and

ought to be regarded as permanent residents of the town and dependent upon its industry for their livelihood. They also concluded that the current policy backed by government and the mining companies, which insisted that laborers circulate between the towns and rural homes, favored European interests over African, and was destructive in that it led to a neglect of the development of rural areas and the drawing off of much of its labor force to the towns where they received inadequate provision and wages. If the policy was continued, Wilson said, it would lead to increasing rural hunger and recurrent urban riots (G. Wilson 1941:70).

It is not surprising that with the publication of the first portion of the report Wilson was denied further access to the mining quarters, that the government refused to back his further research, that the Chief Secretary of Northern Rhodesia made a speech denying that urbanization was happening, and that Wilson resigned. What is surprising is that the Institute proceeded to publish the second portion of the report (G. Wilson 1942). Monica Wilson many years later summed up the experience when she wrote, "Similar sorts of conflict continually beset workers in the social field, whatever the economic organization or government of the day, for facts are potentially explosive" (M. Wilson 1977).

Wilson, of course, had produced an analysis and a prediction as well as called attention to the inequities of the existing system. South Africa which has clung to a policy of circulatory labor, neglect of rural areas, and a denial of the right of Africans to permanent settlement in economically favored areas, has seen increasing rural hunger and recurrent urban riots. Successive investigators of the African scene have also pointed to the continued imbalance between urban and rural investment as a major cause of massive population movements, the growth of urban slums, and the decline in agricultural production. Small-scale projects which seek to ameliorate conditions in the countryside exist within a context which largely stultifies their efforts. Generations of consultants have continued to echo Wilson's original diagnosis, but not his optimism that the contradictions produced by the imbalances will work themselves out into a new and more just arrangement.

The Wilson Broken Hill study is a landmark for its work on labor migration, urbanization, and the interdependencies within a region. It is also a remarkable achievement for applied anthropology in its enduring relevance. At the same time it points to how uneasy our relationship with those we seek to influence becomes when diagnosis challenges policy or we speak as experts in situations where interests diverge. Malinowski and the original founders of the Society for Applied Anthropology, and the neophytes among us today, appear to work with the assumption that societies are integrated structures based on a commonality of interests, rather than arenas within which those with different interests test their power and attempt to impose their will upon others, admittedly sometimes with the best of intentions for the general good, as they define it.

Wilson was neither the first nor the last anthropologist to become the center of controversy or to be denied research access. He might well have given thought to the experience of James Mooney, perhaps the greatest of American ethnologists, though one who worked outside of academia and so left no students to sing his praises. Mooney was censured in 1918 when he testified before a congressional committee in defense of the right of Native Americans to practice the Peyote Religion. His belief that this religion was explicable as a reaction to defeat and impoverishment and that it was a reasonable alternative to mainline Christianity was anathema to missionaries, Indian Bureau agents, and Native Americans who opposed the spread of peyote. They denigrated his expertise and accused him of fomenting trouble on the reservations. Then they succeeded in having him banned from the Oklahoma reservations where so much of

his research centered (Moses 1984:206–217). Yet here again further investigation has confirmed Mooney's analysis.

Wilson and Mooney faced the common dilemma of the field anthropologist who falls into the trap of what Delmos Jones has called "taking sides without reflection" (1980:103). I think each man on reflection would have made the same choice and taken the consequences. Max Gluckman, at least, felt no surprise when he was banned from returning to Northern Rhodesia for further work in Barotseland after he assisted African leaders to put their case against the formation of the Central African Federation before the British public, knowing full well that it clashed with official policy. He predicted that pursuit of federation would only antagonize Africans who had good reason for seeing that it was not in their best interests, and he foresaw that forcing federation against their wishes would lead to increasing resistance and the impossibility of governance. All this was played out over the next ten years.

It is a common charge that the social sciences, including anthropology, are unable to produce results in the form of generalizable principles that can be applied to particular cases. In fact, this has not been our primary problem. Wilson and Gluckman explicated the dynamics of what was happening in Central Africa. Mooney pointed to the conditions under which new religions are likely to arise and the form they are likely to take. Our problem arises rather from the fact that our research challenges what others want to believe; our problem lies in obtaining an audience that will listen when the information is not palatable.

Let me illustrate. Frances D'Souza has reported that in the years immediately after 1973, workers with the International Disaster Institute established the stages through which people moved in response to food shortages, the sequence, if allowed to run its course, eventually leading to disaster. They published reports showing what indicators to watch for. By 1982 observer reports from Ethiopia gave evidence that the indicators were appearing, yet the international agencies were not prepared to act until they found themselves faced with a full-scale disaster. D'Souza finds that the major humanitarian organizations which ought to be able to respond to early signals "are not themselves geared to act on the basis of advance social information, but rather swing into action only when it becomes politically expedient or seemingly practical to do so" (1985:18).

Again Jonathan Kozol (1985) has written that one third of all Americans are now illiterate. This is known and the consequences for their lives and that of their children can be spelled out. Yet the administration lobbies to reduce funds for education and the National Commission on Excellence lectures parents, many of whom are illiterate, on their duty to read to their children. Marian Wright Edelman of the Children's Defense Fund points to the voluminous research funded over the last 20 years on the causes of infant mortality, yet when the last annual report "Health United States" provided evidence that black infants are still about twice as likely to die as white infants, the Secretary for Health and Human Services responded by announcing her intention to create yet one more task force to study the problem. As Edelman said, "The problem is not unknown facts, but inaction" (1984). In comparable circumstances, no doubt other agencies would call for a series of conferences or institute yet another training program.

The social sciences may not be science in the same fashion as the natural sciences since they deal with humans who have the marked ability to change conditions and so to come up with the unexpected. It is certainly true that all kinds of planning "deal with an intractable future, unpredictable people and unreliable resources" and that we fool ourselves if we believe in the "infallibility of techniques" or that policy tools are neutral (Robertson 1984:88). On the other hand, we already have a good deal of banked knowledge that is applicable to the issues at hand,

and frequently it is good enough to allow us to predict what is likely to happen and what needs to be done, as Alasdair MacIntyre affirms in his recent critique of the claims of the social sciences (1984). Some of this banked knowledge may only be common sense, but then it is also common sense that has been formulated for testing and has stood up to the test.

When Lenore Ralston, James Anderson, and I explored the development literature some years ago to see what was known about the possibilities for decentralizing development activity to the local level, we found no absence of generalization about what could be expected to happen when another project was initiated (Ralston et al. 1983). What seemed inexplicable was that so much knowledge led to so little application, that the same kind of studies continued to be called for, and the same kinds of mistakes continued to be made (Colson 1982).

We know, for instance, that it is relatively easy to persuade national governments and local communities to accept new programs financed by bilateral and multilateral aid, while maintenance is another matter. Yet donor agencies continue to institute new programs with the happy expectation that somehow or other recipients will have the organization and material resources to maintain them. In fact, most resources are monopolized by the center and the center usually has no desire to see strong local organizations arise to provide a base for political rivals with alternative programs. That new projects are likely to join the graveyard of old projects can be predicted from the beginning, but there is little evidence that those who plan them systematically undertake to work out with members of the local community in advance such mundane matters as monthly costs in cash and labor per head of population that will be required over the years. Only if this is understood do the local people have any way of saying whether they can or will afford the additional effort. Such planning appears to be even less likely to happen today than in the years when development organizations were in their infancy and feeling their way. Judith Justice (1981) has explored the ways in which knowledge of local conditions becomes sidetracked before it can affect either planning or implementation of projects largely initiated from outside, and Robert Chambers (1980) suggests that the gulf between planners and villagers is steadily widening so that local people are increasingly less likely to have input into what is done in their name. I suspect that, if given the option, most governments and local communities would opt for support to existing programs rather than to another set of false-starts. Or would they?

After all, much that happens in the transmission from the center to the periphery involves political maintenance rather than technical advance or any real expectation that life chances of those at the bottom will be affected. A Zambian official was being realistic when, to my objection that the road they planned to build was irrelevant to the area's particular need, he responded with the practical comment, "True, but now they will never be able to say that we did nothing for them." He was well aware that his government could do nothing with respect to the real issue, but he recognized that a government that appears unconcerned with the welfare of its people cannot expect them to be concerned to support it. We need to recognize that many projects serve a variety of ends not specified in the formal plan. It is in this spirit that Stepick and Murphy have asked if some of the self-help housing programs which so obviously are beyond the means of the targeted poor may not be instruments "designed to pacify the urban poor by utilizing their otherwise excess labor capacity in owner-constructed housing" (1980:342).

If this is the case, the applied anthropologist goes wrong when he or she fails to recognize the real agenda or agendas and believes that advice is being sought for planning or implementing a program designed to make a difference at the technical level. We then have situations such as that described by Corrine Glesne in her heartfelt article (1984) based on experience on St.

Vincent, where international consultants, officials in the ministries, and local people irritate one another, find themselves increasingly suspicious of the whole process of consultation, and grow doubtful that it can result in anything more than further visits of foreign consultants.

It does not need to be this way, but it is difficult to know how to break out from the established routines. We are unlikely to advance our theoretical understanding of the conditions under which development, which we could agree *is* development, will take place by further assessment of projects that have failed. Lucy Mair, who has kept her mind on what is happening in applied anthropology longer than any of us, says that “postmortems on failed projects get you nowhere. Nobody reads them” (1985:20). What may be most useful is to examine the context within which we work, discover ways of converting anthropological knowledge into a form that can be digested by those who need to read it, and finally become better political animals within the arenas in which we now compete.

In venturing into new fields of inquiry, we run risks. Some of you are now working within government organizations and other agencies where you have the opportunity to bring knowledge won through anthropology into the early stages of the planning process where it has a chance to influence results. You run the risk of becoming absorbed by the system and accepting its premises. I suspect you find colleagues invoking “cultural conservatism” or “tradition” as the reason why projects fail. When I was in touch with various agencies involved in development I came to cringe when I heard either word, for it always placed the blame for failure elsewhere. It is worth insisting that cultural conservatism is more characteristic of bureaucracies than it is of people who are trying to make a living from agriculture, fishing, or small industry. *Homo sapiens* is notorious for both curiosity and mobility, and is capable of colonizing and exploiting the most unlikely environments. Since we originated as a species we have been moving ourselves around the world to take advantage of new opportunities, transforming ourselves as we became dissatisfied with what we were, experimenting with new techniques, sometimes cautiously and sometimes with disastrous daring. Thayer Scudder and I found Gwembe Tonga villagers to be frequent innovators, although now one and then another takes the lead, and only a few are known for their consistent ability to seize the moment and profit from it. Elsewhere, other workers have found this same willingness at the grass roots to try something new. The timeless village is a figment of our imagination.

The onus, then, is on us to explain why what we offer should provide more opportunity and less hardship than what they have. I am in complete agreement with A. F. Robertson when he writes, “While society is certainly a structure it is also a process, with the relentless cycle of birth, procreation and death as its mainspring. Maintaining community can be an arduous and very active business: the sleepy demeanour of the village may be very misleading. . . . poor people have to work very hard to sustain an unimpressive level of welfare, and to preserve some necessary but unimpressive social institutions. In such circumstances the invitation to innovate may be received with scepticism, but to assume that this response is apathy is an insult born of ignorance. All communities depend on activity, not passivity, in their relentless struggle with the seasons, with demographic growth and in the long term with historical change” (1984:147–148).

To understand the structures within which applied anthropology may continue to play a role we need to examine the wider context that impinges on all of us. Our primary contribution in the past has been derived from experience in villages, slums, factories, and other situations where people are involved in coming to terms with each other as they work toward some common end or disparate ends. We have also used our understanding of organizations and the way in which symbolic systems are manipulated in practice to throw light on the working of

bureaucracies. Robertson now has undertaken to examine the growth of the planning industry with which so many social scientists are presently engaged. In reading his recent book I was struck with the way the experience of those recruited into the society of planners echoed the experience of migrants. Those who arrived first found themselves isolated among strangers and had to reach out and make a quick adaptation to what was, after all, a foreign community. With increasing numbers and the establishment of sub-communities in capitals throughout the world, the migrants became increasingly encapsulated, rarely talking with those who did not share their training or their goals. The highly mobile community had outposts everywhere. In Zambia, Nepal, and Bangladesh one could find professional kin. And with this the gap between those who plan and seek to apply knowledge and those for whom planning is done widened, for when one is habituated to speaking with those who respond easily to the nuances of vocabulary and style it becomes difficult to communicate with others. The formalities associated with planning then become ends in themselves, and timetables are dictated not by seasons of research but by the need to meet another kind of schedule.

Applied anthropologists and applied social scientists of all kinds face this dilemma. To be heard they must become part of a system which has its own dynamic. But this is at the expense of continued involvement in that which has supplied anthropology with its own dynamic: close contact with those living people out there who are concerned about their own futures and trying to plan their lives in relationship to it, drawing upon their past experience to predict the future and to appraise what is desirable. Lucy Mair wrote me that she believes that Malinowski became disillusioned with the contribution that anthropology could make to the betterment of human life, for he had come to see that little of what anthropologists said was ever heeded. Raymond Firth, in his Malinowski Award Lecture (1981), had his own queries, for he saw that by and large what we have is uncomfortable knowledge, the kind of knowledge that challenges established clichés and puts in question accepted solutions, and so those who champion them.

It is not easy to be an applied anthropologist today, but then it never was. It is challenging, as it always has been. It is challenging precisely because it involves us in the future, our own as well as that of others. But it is this involvement with the future which always has distinguished applied anthropology from other kinds of anthropology which look rather to the past or to the present. Like those who founded the Society for Applied Anthropology in 1941, you are betting on our ability to look at today in relation to something that lies ahead, that is not yet determined and that is subject to human thought and planning. This calls for a sense of responsibility and a spirit of adventure; both are part of your tradition.

### References Cited

- Chambers, Robert  
1980 Rural Development: Putting the Last First. London: Longmans.
- Colson, Elizabeth  
1982 Planned Change: The Creation of a New Community. Berkeley: Institute of International Studies, University of California.  
1984 The Reordering of Experience: Anthropological Involvement with Time. *Journal of Anthropological Research* 40:1-13.
- D'Souza, Frances  
1985 Anthropology and Disasters. *Anthropology Today* 1:18.

- Edelman, Marian Wright  
1984 We Already Know Why Black Babies Die. *Washington Post*, February 19.
- Fei Xiaotong  
1980 Towards a People's Anthropology. *Human Organization* 39:115–120. (Republished at <http://www.sfaa.net/>.)
- Firth, Raymond  
1981 Engagement and Detachment: Reflections on Applying Social Anthropology to Social Affairs. *Human Organization* 40(3):193–201. (Republished at <http://www.sfaa.net/>.)
- Foster, George  
1982 Applied Anthropology and International Health: Retrospect and Prospect. *Human Organization* 41:189–197. (Republished at <http://www.sfaa.net/>.)
- Glesne, Corinne  
1984 The Impossibility of Doing Good. *Culture and Agriculture* 23:1–6.
- Gluckman, Max  
1944a The Rhodes-Livingstone Institute and Museum. *Rhodes-Livingstone Journal* 1:4–9.  
1944b Godfrey Baldwin Wilson. *Rhodes-Livingstone Journal* 1:1–3.
- Jones, Delmas  
1980 Accountability and the Politics of Urban Research. *Human Organization* 39:99–104.
- Justice, Judith  
1981 International Planning and Health: An Anthropological Case Study of Nepal. Ann Arbor: Dissertation Microfilms.
- Kelly, Lawrence C.  
1985 Why Applied Anthropology Developed When it Did: A Commentary on People, Money, and Changing Times, 1930–1945. In *Social Contexts of American Ethnology, 1840–1984*. J. Helm, ed. Pp. 122–138. Washington, DC: American Ethnological Society.
- Kozol, Jonathan  
1985 The Crippling Inheritance. *New York Times Book Review*, March 3:1, 26–27.
- Leighton, Alexander  
1984 Then and Now: Some Notes on the Interaction of Person and Social Environment. *Human Organization* 43:189–197. (Republished at <http://www.sfaa.net/>.)
- MacIntyre, Alasdair  
1984 After Virtue. (2nd ed.) Notre Dame: University of Notre Dame Press.
- Mair, Lucy  
1985 Development Anthropology. *Anthropology Today* 1:19–20.
- Moses, L. G.  
1984 The Indian Man: A Biography of James Mooney. Urbana and Chicago: University of Illinois Press.
- Ralston, Lenore, James Anderson, and Elizabeth Colson  
1983 Voluntary Efforts in Decentralized Management. Berkeley: Institute of International Studies, University of California.

- Robertson, A. F.  
 1984 *People and the State: An Anthropology of Planned Development*. Cambridge: Cambridge University Press.
- Scudder, Thayer  
 1983 Economic Downturn and Community Unraveling. *Culture and Agriculture* 18:16–19.  
 1984 Economic Downturn and Community Unraveling, Revisited. *Culture and Agriculture* 23:6–10.
- Scudder, Thayer, and Elizabeth Colson  
 1982 *From Welfare to Development: A Conceptual Framework for the Analysis of Dislocated People. In Involuntary Migration and Resettlement: The Problems and Responses of Dislocated People.* Art Hansen and Anthony Oliver-Smith, eds. Pp. 267–287. Boulder, CO: Westview Press.
- Spicer, Edward  
 1976 *Beyond Analysis and Explanation: Notes on the Life and Times of the Society for Applied Anthropology*. *Human Organization* 35:335–344. (Republished at <<http://www.sfaa.net/>>.)
- Stepick, Alex, and Arthur D. Murphy  
 1980 Comparing Squatter Settlements and Government Self-Help Projects as Housing Solutions in Oaxaca, Mexico. *Human Organization* 39:339–343.
- Wilson, Godfrey  
 1941 An Essay on the Economics of Detribalization in Northern Rhodesia, Pt. 1. Rhodes -Livingstone Papers 5.  
 1942 An Essay on the Economics of Detribalization in Northern Rhodesia, Pt. 2. Rhodes-Livingstone Papers 6.
- Wilson, Monica  
 1977 The First Three Years, 1938–41. *African Social Research* 24:279–283.



## **Chapter 16**

### **Philleo Nash:**

### **Policy and Equality within the Halls of Government**

**Thomas Weaver**

The career of Philleo Nash, recipient of the Society for Applied Anthropology's Malinowski Award in 1986, reflects a pursuit of racial and ethnic equality—a goal that most anthropologists share.<sup>1</sup> The difference between Nash and other anthropologists is that he was a master at “working the system” in pursuit of his goal. In this he presents a model for the applied anthropologist. He served effectively as a policy anthropologist in government for many years, including seven years on the administrative staffs of Presidents Roosevelt and Truman and five years as Commissioner of Indian Affairs.

Philleo Nash (1909–1987) received a bachelor's degree from the University of Wisconsin in 1932 and a Ph.D. from the University of Chicago in 1937. His major fields of interest were applied anthropology, acculturation, and functionalism. Ralph Linton was his major professor at Wisconsin, while at Chicago he studied with A. R. Radcliffe-Brown and Robert Redfield. He became familiar with applied anthropology through Redfield's descriptions of Manuel Gamio's work. Leslie Spier was his fieldwork advisor, and Harold Lasswell served as a member of his dissertation committee. His earliest published work was in the archaeology of the mounds in Wisconsin (1933), while his doctoral work was on religious revivalism on the Klamath reservation (1937).

In his Malinowski Award presentation, “Science, Politics, and Human Values: A Memoir” (1986), Nash traced his career in public, governmental, and “Indian” service and discussed some of the history of applied anthropology. He described the conditions of the Klamath and other Indians as not much changed from the 1870s to the 1930s, the time of his doctoral research, when he vowed to do better for the Indians. In 1942 he went to work for the Office of Facts and Figures and its research arm, the Bureau of Intelligence, later known as the Office of War Information. This work, as well as service on the staffs of Presidents Roosevelt and Truman, provided valuable knowledge on how to apply anthropology in government (Nash 1979). In private life, Nash and his wife founded a parent-directed cooperative school that was one of the earliest integrated schools in Washington, D.C. Nash also worked at the Royal Ontario Museum and taught at the Universities of Toronto and Wisconsin before returning to the family-owned cranberry farm. Later he was a consultant to the State Department on a visit to India and with the Phillips Petroleum Company. He became a faculty member at American University in 1971.

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Nash's government service involved networking, drafting reports and executive orders, and serving on task forces, commissions, and committees. Most of Nash's work was accomplished in what he called the "committee and report approach," and it meant producing reports anonymously, a practice alien to the academic anthropologist who works under the dictum of "publish or perish." Much of the substance of his work on the White House staff related to monitoring and reducing racial tensions in U.S. urban areas. He helped create the Mayors' Committee on Unity and the American Council on Race Relations (Officer 1988). Another of his concerns was the elimination of discrimination and segregation in governmental agencies and the armed forces.

During the Truman administration Nash contributed to the development of and report the report for the President's Committee on Civil Rights (Nash 1984), and he wrote another report that was an important addition to President Truman's 1948 address to Congress on civil rights. Nash also reported on discrimination in the civil service system, fair employment practices, and segregation in the armed forces. He was successful in preventing riots that would interfere with the war effort, desegregating the armed forces, and placing anti-discriminatory rules in the Civil Service Commission. He was active in the origination of the Commission on Civil Rights and the production of a report that laid important groundwork for the civil rights movement of the 1960s.

After this period of government service, Nash became involved in Wisconsin politics, serving as chair of the state Democratic Party from 1955 to 1957 and as Lieutenant Governor for the state from 1958 to 1960. In 1961 he returned to federal government service in the Department of the Interior on a task force for the investigation of problems in the Bureau of Indian Affairs (BIA). James Officer, another anthropologist, served with him on this task force. The report made broad-ranging recommendations for change in the BIA and led to Nash's nomination to head the agency. After much critical examination by various governmental committees he was approved as Commissioner of Indian Affairs. John O. Crow, an Oklahoma Cherokee, was appointed deputy in charge of daily operations, and James Officer was named associate commissioner with responsibility for policy and tribal operations.

Nash served as Commissioner of Indian Affairs for five years and was one of the most effective commissioners in history, bringing about many reforms. He was especially effective in advancing economic development and education funding and programs (Officer 1988). He made it a point to meet often with Indians on their reservations; to socialize with them in powwows, dances, and other traditional activities into the "wee hours of the morning;" to seek the views of bureau employees; and to make public presentations about Indian affairs. The goals of his administration were Indian self-determination within the protective framework of trusteeship; social participation and economic growth in the context of the outside society; and the protection, improvement, and beneficial use of tribal property (Officer 1988). His philosophy as commissioner reflected an astute knowledge and broad experience in national politics. Eventually, the enemies he made in Senate and House committees forced him out as commissioner, but he retired as a friend to Indians and bureau personnel.

Nash was treasurer of the American Anthropological Association (AAA), secretary of Section H (Anthropology) of the American Association for the Advancement of Science, and president of the Anthropological Society of Washington. He was a recipient of the AAA Distinguished Service Award, and in 1989 the Institute for Intercultural Studies, founded by Margaret Mead, presented an award to Nash and his wife, Edith Nash, called "In the Spirit of

Margaret Mead” in recognition of their contributions to anthropology and education. Nash served as president of the Society for Applied Anthropology from 1970 to 1971.

### Notes

<sup>1</sup>This annual award is given to a senior colleague in recognition of efforts to understand and serve the needs of the world through social science. See Thomas Weaver’s “The Malinowski Award and the History of Applied Anthropology” (2002b) for a brief history of the award and an overview of the recipients and their work, and Weaver’s “Malinowski as Applied Anthropologist” (2002a) for an introduction to Bronislaw Malinowski and his work.

### References Cited

Nash, Philleo

- 1933 The Excavation of Ross Mound Group I. *Bulletin of the Public Museum of the City of Milwaukee* 16(1), December 20, 1933. Milwaukee: Board of Trustees, Public Museum of the City of Milwaukee.
- 1937 The Role of Religious Revivalism in the Formation of the Intercultural Community of Klamath Reservation. *In Social Anthropology of North American Tribes: Essays in Social Organization, Law, and Religion*. F. Eggan, ed. Pp. 377–442. Chicago: University of Chicago Press. Reissued in 1955.
- 1979 Anthropologist in the White House. *Practicing Anthropology* 1(3):3, 23–24. (Reprinted in *Classics of Practicing Anthropology: 1978–1998*. P. J. Higgins and J. A. Paredes, eds. Pp. 31–35. Oklahoma City: Society for Applied Anthropology, 2000.)
- 1984 Memorandum: President Truman and Civil Rights. Unpublished manuscript. Prepared for Truman Centennial Committee. Clark M. Clifford, Chairman.
- 1986 Science, Politics, and Human Values: A Memoir. *Human Organization* 45(3):189–201. (Republished at <<http://www.sfaa.net/>>.)

Officer, James E.

- 1988 Philleo Nash. Unpublished manuscript.

Weaver, Thomas

- 2002a Malinowski as Applied Anthropologist. *In The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers*. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.
- 2002b The Malinowski Award and the History of Applied Anthropology. *In The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers*. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.

## Science, Politics, and Human Values: A Memoir

Philleo Nash

Preparing the traditional lecture for this award has given me, as it gave my predecessors, an opportunity to review the evolution of our profession during our lifetimes.

My own professional life came of age with “acculturation” and “applied anthropology.” I was lucky enough to have been one of Ralph Linton’s first students at the University of Wisconsin; Lauriston Sharp and Sol Tax were just ahead of me. Linton had brought Charlotte Gower Chapman into the Department at Madison and she had just completed the manuscript of *Milocca: A Sicilian Village* (Chapman 1971). We had opportunities to discuss the material of this pioneer community study in a seminar which Linton often attended. This course was called “Acculturation,” although the committee of the Social Science Research Council consisting of Redfield, Linton, and Herskovits had yet to introduce the term “acculturation” to modern anthropological usage (Redfield et al. 1936:149–152). Precedence in the use of this term goes, I believe, to John Wesley Powell, chief of the U.S. Geological Survey and Bureau of American Ethnology (Powell 1881:xxviii), who combined geographic exploration with tribal negotiations on behalf of the President of the United States.

I was also fortunate in my arrival at Chicago in 1932. It was disappointing that Sapir had just left for Yale, but with Robert Redfield to introduce me further into the intricacies of acculturation and Radcliffe-Brown to introduce functionalism and the promise of comparative study of social systems, I was in sixth—if not seventh—heaven.

At Chicago, under Redfield, I learned the kind of applied anthropology that he and Manuel Gamio practiced: the method of anthropology as a whole applied to major social problems. Gamio had been engaged in this activity since 1918 in an effort to use anthropology to make real the promise of the Mexican Revolution of 1910.

At this time a different approach was coming into being at Harvard where, in the School of Business Administration, factories and assembly lines were being looked at as interactive systems. The Hawthorne effect (that as work forces become organized into a group they increase their productivity) is now returning to our shores disguised as a Japanese import.<sup>1</sup>

Bronislaw Malinowski, a Pole, returned from Melanesia to initiate the systematic application of anthropology to the administration of the African colonies acquired by Great Britain in WWI, and taught many of our colleagues in America. We honor him more than any of us when we use his name and memory for this great award.

Like Malinowski, A. R. Radcliffe-Brown was in the field when WWI broke out and spent the duration on Tonga where he was an advisor to the Queen, practicing applied anthropology.<sup>2</sup>

Our Society and its journal *Human Organization* owe their origin to the group at Harvard which had been studying industrial organization. Margaret Mead, Ruth Benedict, and Gregory Bateson were concerned about the wartime role of anthropologists and created the Institute for Intercultural Studies in 1944 (Dillon 1974).

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It would seem that the field of applied anthropology is not an American, or even a New World invention, but a truly international and interdisciplinary enterprise.

From this multi-faceted beginning our discipline was guided in its early days by twin concepts: that society could be improved by changing forms of social organization; and that sick societies could be healed by social physicians. Both concepts called on anthropologists to make themselves central agents of change in association with other social science disciplines. Both assumed that the correctness of their views, based on scientific observation and tested hypotheses, would prevail in the marketplace and that anthropologists would be called on for advice and public service.

Most anthropologists of my generation came out of the Depression with research and travel funds limited. We found it easiest to continue the Boasian tradition and get our first field experience on the Indian reservations of the American West. We owe a great deal to the Indian people who were our trainers and colleagues, often unrecognized. The New Deal Commissioner of Indian Affairs, John Collier, had formulated a major reform which was adopted, although extensively modified, by the U.S. Congress as the Indian Reorganization Act of 1934. Despite opposition, the reforms went forward and brought major changes to the reservations. Economic development, increased self-government, tribal elections, more power for tribal attorneys, soil conservation, and Indian preference in employment were just a few of the programs that came to dominate reservation life (Collier 1949, 1963; Philp 1977; Cohen 1982; Brophy and Aberle 1966).

This was the state of affairs in 1934 when I arrived on Klamath Reservation for my first field training under Leslie Spier. The tribes residing on the Klamath Reservation owned a magnificent forest: several billion board feet of ponderosa pine, much of it still awaiting the virgin cut. I was a run-of-the-mill young anthropologist and the dismal anthropological view of the Bureau of Indian Affairs which prevailed then, and often prevails today, was consistent with what I saw. The lumber operations had very few Indian workers. The schools offered no courses that would prepare their Indian students for work in the woods or mills. The non-Indian communities were enclaves within the reservation, non-participating and uncaring (Stem 1965; Spier 1930).

The past interested me the most. It was then only 60 years since the Modoc War of the 1870s. There were plenty of men and women who remembered those days and the religious revival that went with it. They remembered their dreams and face paintings and could sing the songs that went with them. They could tell—a few of them—about the battles in the lava beds and what it was like to hide out in the caves and crevices (Dubois 1939; Nash 1937). Wade Crawford, one of Collier's early appointments of an Indian person as a Reservation Superintendent, was friendly and helpful and thought no more highly of the Indian Bureau than I did. He opened the records of the reservation to me.

There were no Archives of the United States at that time. The manuscript reports and correspondence were stored in the hayloft of the Agency barn where feed for horses had been kept only a few years before. As winter came on and it got too cold to work there, Superintendent Crawford let me move the first ten years of records into the Agency office and gave me the use of a typewriter to copy what I wanted. He also recognized the importance of the documents and thereafter kept them in the Agency vault.

As I read the reports of the schoolteacher, the wagon and plow maker, the farmer, and the policeman, it seemed to me there was little difference between the Agency of the 1870s and the 1930s. As I got into the written history of the Modoc War and realized that the United States

government had spent more than one million dollars; had used one thousand soldiers and one hundred Indian scouts (recruited from traditional enemies of the Klamath and Modoc) to subdue a band of little more than one hundred men, women, and children, I felt a sense of outrage and a determination to do it better some day.

In the coming together of history and field experience, I was supported by theory. Chicago, in the 1930s, was a place of ferment and inter-disciplinary communication. The Department of Anthropology, with Fay-Cooper Cole as its chairman, was remarkably tolerant of dissent and intellectual vagabondage. When I found a political scientist who had worked closely with Edward Sapir and the Chicago Institute of Psychoanalysis, the Department of Anthropology made no objection to his close monitoring of my dissertation.

The political scientist was Harold Lasswell, who, at the time, was much concerned with a psychological framework for movements of social protest. He referred many symbols of protest to the “balance of indulgence and deprivation” (Lasswell 1930, 1935a, 1935b). David Aberle (1982:315) has since referred to this framework as the theory of relative deprivation and attributes it equally to Lasswell and myself, but the theory is really Lasswell’s. He developed it from the work of the many psychoanalysts and their analysands who have struggled to make sense out of frustration, inadequacy, and helplessness by looking introspectively at their own balances of deprivation and indulgence together with their own behavioral response.

This framework seemed made to order as I thought about the three tribes thrust together on the Klamath Reservation; about the attempts to make farmers out of them; the flight from the reservation of the little band of malcontents; the rich symbolism of their apocalyptic revival; the test of magic against the soldiers’ bullets; and the decline into individual dreaming and passive acceptance of the new order (Spier 1930; Nash 1937; Stern 1965).

In the government response to the flight of the Modocs I thought I could see more than indifference and hostility to Indian culture. I saw panic, misinformation, and arrogance.

But I was also arrogant and misinformed. Despite my burrowings into the early history of the reservations I had little knowledge or understanding of the complexities of the decision-making process in Indian Affairs. In 1935 I knew quite a bit more about the relationship between Modoc chiefs and shamans in 1870 than I did about the Superintendent and the Tribal Chairman. In this respect I was very much in the anthropological groove of my time. I will always remember asking Leslie Spier, my first field instructor, what was going on in the white house with the old barn at Klamath Agency. He told me not to go in there; if I did the Indians wouldn’t talk to me.

At this point, given the sound academic training I had received, I might have pursued my theory in an academic setting. The revival of the 1870s was clearly related to the passive acceptance of reservation life that followed in the 1880s. Other Plateau reservations beckoned, and so did other nativistic movements and syncretic religions. But I had little opportunity for such refinements. My first academic job opportunity called on my archaeological background and I deferred indefinitely further research into acculturation on Klamath Reservation. Nevertheless, I was convinced of the rightness of what I had been thinking. I waited only for the opportunity to help make decisions and better the chance for social justice (Loram and McIlwraith 1943:331–335).

The opportunity was not long in coming. Pearl Harbor changed my life as it did many others. Wartime Washington—and much of the rest of the country—was unsure about the loyalty and the willingness of Black Americans to fight. Ugly rumors about Blacks had wide circulation, and sporadic violence between ethnic Whites and migrant Blacks was turning up, especially in New Deal housing projects.

On a recommendation from Lasswell, I was invited to join the specialists in the Office of Facts and Figures (OFF); its research arm was called the Bureau of Intelligence. Among its numerous service functions were sophisticated opinion and media content analysis, a roster of groups and organizations pertinent to wartime information, and a small network of field offices. Monitoring rumors was one of the numerous functions of the field offices. Some of the rumors they reported were hostile fantasies about groups, especially Blacks and Hispanics.

Analysis of these rumors led to close examination of the tensions between groups, and the geographic pinpointing of tensions. Among the prime tension centers were Washington, D.C., and Detroit, Michigan. Numerous memoranda went out from OFF to all the War Agencies naming these and other cities where sporadic violence was occurring and where it seemed likely to grow.

Many social scientists were involved; the political scientist and writer, Leo Rosten, was one of them. I took it on myself to take some of these memoranda around the government and to try to get something done about them. In my brashness, I went almost everywhere; and in my innocence, I was unaware that “race relations,” as Black-White relations were then called, were something everyone talked about, but government tried to avoid.

I found that Jonathan Daniels was in the process of moving to the White House, where he was to become one of the Administrative Assistants to the President. Daniels was editor of the Raleigh, North Carolina, *News and Observer*, a liberal Southern paper, and the author of several books about the South. I was familiar with one of them which I had used in teaching at the University of Toronto (Daniels 1938).

In Daniels I found a willing listener and teacher. He patiently explained to me how and why “race relations” were a no-man’s land. It was political. Anything to do with Blacks was in the domain of the states. If the Federal Government intervened, the agency involved would be slapped on the wrist, or worse, at appropriations time by the defenders of states’ rights in the Congress (Daniels 1975).

My meeting with Daniels was most serendipitous. I had no way of knowing that he had been attempting to get the Office of Civilian Defense (OCD) to take notice of mounting inter-racial tension for several months and had moved out of OCD in frustration (Eagles 1982:83–120).

By the summer of 1943, race tension was at the breaking point in Detroit. Both Blacks and Whites had moved into war jobs and everything was in short supply, from housing to recreation. The gasoline shortage had caused Whites to return to an urban park, close to home, that had been taken over by Blacks. On a Sunday night in August, rumors swept through the crowds. Blacks said a Black baby had been thrown from a bridge; Whites said it was a White baby. There were fights, but they eased up late in the afternoon. After dark, serious violence developed, not in the park, but in the vicinity of a popular night club in the Black area. Fighting went on all night and in the morning, carloads of Blacks, on their way to the war production plants, were attacked by gangs of angry Whites.

The Governor was out of the state. The police asked for help, and some state guards attempted to restore order, but were inadequate. A battalion of military police was situated in Detroit, camped in a race track unused because of the war. They had practiced drills, being aware—from their own sources—of Detroit tension, but their drills had been to deploy their forces around City Hall, which was in no danger. An officer from the Fifth Service Command flew from Chicago to Detroit, reading en route, we were later told, the Constitution and laws regarding the use of Federal troops in domestic emergencies. Thirty lives were lost and property

was damaged to the amount of five million dollars before the proper procedures could be sorted out, the necessary Presidential proclamation be issued, and the full force of Federal troops be used to restore order.

Throughout the day Mr. Daniels, acting for the President of the United States, found that the fastest information on the rapidly changing situation came from the teleprinters of the Office of War Information (OWI), which I was watching for him. OWI was the successor agency to the OFF. We operated as before, but with a greatly reduced research staff. As a courtesy to the OWI, all the major news networks provided us with news printers which were arranged in a bank in the OWI press room. Jonathan Daniels was a newsman, and having reporters keep him up to date appealed to him. Their information proved to be accurate as well as fast. It was a matter of pride to me, of course, to be ahead of the numerous intelligence agencies, both military and civilian, that were also called on by the White House.

Needless to say, the government agencies were not the only ones to be aware of the growing tension in the cities. Edwin Embree of the Rosenwald Fund and Marshall Field, publisher and philanthropist, were seeking White House approval for a national program to help the cities along the lines of the Mayor's Committee they had helped create in Chicago. President Roosevelt was not open to this suggestion, but Mrs. Roosevelt invited both Mr. Embree and Mr. Field to dinner at the White House. Mr. Daniels thought it unwise to attend, probably in view of FDR's disapproval of any overt national government effort in race relations. However, he saw to it that I was invited to the same dinner and that very night the decision was made, around the family dining table but without FDR, to create a private organization to assist mayors through the formation of Mayors' Committees on Unity. It was to be private, though informally blessed by the White House, and was to be initially funded by grants from the Rosenwald and Field Foundations.

My researches into the post-World War I riots had convinced me that we must look forward to a repeat of those disasters. There were no prospects for a Federal agency, so I thought it important for the private effort to get an early start. In the fall of 1944 I took temporary leave from the Federal Government to help get the new organization started. It was called the American Council on Race Relations, and I spent three months with it before I returned to Washington, dissatisfied with the scale of the effort in the face of a large national problem (Johnson 1943).

After order had been restored in Detroit, Daniels asked the Pentagon to set up an organization that could receive emergency information from mayors and governors, particularly of those states and cities that were on the watch list of our race tension analysis, but, of course, not limited to them. Daniels was determined that delay and lack of information should not again be the cause of death, damage, and lost war production. But what good are military police if the goal—continuous production in the factories—could not be reached? War workers had to go safely to their jobs, and any outbreak of violence in a dozen different cities would make that impossible.

I proposed a committee of a dozen or more federal agencies that were concerned with the human needs in short supply in the war production centers: housing, health, recreation, education, transportation, and many others. At this point I learned the limits of both theory and practice. Various files in Daniels' office, to which I was given access, showed that both Attorney General Biddle and Interior Department Secretary Ickes had recommended the creation of a wartime body to cope with the mounting racial problems, but that the plan had never been approved. President Roosevelt had decided that "tinkering" with racial matters during the war



would endanger the war effort. Nothing must endanger us to the point of diverting the effort it would take to win it (Eagles 1982:105).

How then to coordinate the necessary effort to prevent violence, not by police action after it had occurred, but by anticipating it and dealing with the root causes where possible? Jonathan Daniels provided the answer. The committee didn't actually have to meet to be a committee. The designated agencies could each appoint a representative and they would answer to Daniels, but they would never meet in one room or with each other. Nowadays we call this "networking."

I drew up the list and Daniels took a letter signed by himself, addressed to each Department or Agency Head, for the President's approval. He brought it back and in the margin was the notation in FDR's handwriting, "OK FDR." That was the last I ever heard about it. It was also as close as I ever got to FDR. The program we set up by this means lasted until his death in 1945, but I never worked with him face-to-face. Charles Eagles, who saw the letter long after I did, says the notation was at the bottom of the letter and that it was in handwriting "J.D.O.K.F.D.R." (1982:105).

The letter was the real program. It called on each agency head to appoint someone to keep track of "developing racial situations" which might interfere with war production and to report to Daniels about them.

One of the recipients was Domestic Counterintelligence in the Department of the Army, and this agency provided a weekly summary of events. But without a meeting and without guidance, the usefulness of its reports was greatly diminished. I reviewed these reports weekly for two years and there was always something useful to be learned from them; but the best source of information continued to be the OWI tickers (Daniels 1975:204, 205).

My method of operation was simplicity itself. I scanned the tickers each day for news of tension-revealing incidents in a city, a military camp, or a war production plant. If it was of a scale that seemed threatening, I attempted to get all the information possible over the telephone. These calls were made from the Office of War Information. I never said, "The White House wants to know . . ."

The so-called Black press, the Negro Weeklies, were helpful. Their Washington representatives all knew what I was doing and why. An invaluable ally was the head of the Negro News Desk in the Domestic Branch of OWI, Theodore R. Poston. A superb newsman and a close friend of mine until his death at a regrettably early age, he was known to all Washington and New York journalists as "Ted." Almost always Ted could think of someone who would know or who could find out what really happened in one of these incidents. Sometimes it was a Black newsman or publisher, sometimes a Black community leader, often a clergyman. They provided the essential balance to our White newsmen.

If no mention had been made in the weekly report from Counterintelligence, I called my opposite number there and asked for details. Usually that took too long, for our theory required that we cope with small incidents while they were still small. Coping was, of course, more difficult than finding out about them. Once we had pinpointed a problem—and Ted and I usually reported to Daniels together—Daniels could call the agency representative if he wished and ask for action (Daniels 1975:207).

The typical problem was a small one: discrimination in assigning parking spaces; racially separate and unequal toilets; pay differentials; discrimination in hiring, promotions, and leave; and many others too numerous to mention. Of course, there were others too big to handle, but the fact that they were being worked on was often enough to reduce tension. From the time FDR put the notation in the margin of that first letter, there were no more Detroit's, although there were a

number of close calls (Johnson 1943).

Roosevelt was re-elected to a fourth term in 1944, but on April 12, 1945, he died and was succeeded by Harry S. Truman. VE Day and VJ Day soon followed, the War-Powers Act expired, and the rules of the game had changed. The future of our race tension program seemed uncertain.

Nevertheless, there was satisfaction in the fact that our mission had succeeded. I felt deprivation theory had been adequate and that “preventive politics” had been good applied anthropology. To be sure, under the limitations imposed by FDR’s policy of limiting our activities to situations interfering with war production, the social structure of caste and class continued unchanged. That was not consistent with the human values of anthropology; but the situation was about to change at the hands of an improbable person, the “uncommon common man,” Harry S. Truman.

In 1919—one of the first post-war years after the first World War—our country had been plagued by a series of violent race riots. I felt there was every reason to expect the same immediately after the second World War. Some of those who knew what I was doing made great fun of my “hot weather” theory of race riots. After WWI and up to 1943 in WWII, most riots occurred in the summer, especially in August, and most frequently in places associated with crowds. The crowding, the heat, and the competition for space seemed to have a bad chemistry. The 1919 riots were also associated with returning servicemen.

All of these fears of a repetition of 1919 were borne out in 1946, the first year after WWII. There was little we could do about it. The riots did not occur in plants but in public places. We could ask the FBI to investigate, but in some flagrant cases of daytime violence no one was willing to testify, though there must have been dozens of witnesses. The medium-to-small towns of the South seemed to offer the highest risk.

The military in WWII was not the best place in the world for Black servicemen, but when they returned to the rural and suburban South, they acted too assertively and violence often erupted. Black people were no longer willing to stand aside in such instances, and violence quickly became major.

Late in the war, we had begun to classify riots and had a scale: words; words and blows; words, blows, and missiles. We also scaled to size: crowds less than 25, 25–99, and 100 and over. The riots in 1946 steadily went to the top of the scale. There were many episodes, but one stands out because it moved President Truman to action.

Isaac Woodard was a Black veteran who was on his way home when he stopped off in a bus station in South Carolina. For some reason, he came into the hands of police who beat him so severely he was blinded. A group of Black leaders asked to see Truman about it, and he assured them he would take action.

At this point, the long-dormant Commission—proposed but never actualized in WWII—was revived. It became the President’s Committee on Civil Rights (Truman 1946). The name of this Committee came from David K. Niles. Mr. Niles had been a protege of Harry Hopkins in the Roosevelt White House and when Truman became President, he inherited Niles and with him, myself. Niles had succeeded Daniels because late in the Roosevelt presidency Daniels was made Press Secretary, leaving the minorities post vacant (Daniels 1975:252).

Niles was the polar opposite of Daniels. He came from Boston, was of Lithuanian immigrant forbears, a veteran of Sacco-Vanzetti and many other liberal causes. He was devoted to Truman, who trusted him completely and assigned him to numerous delicate tasks. Although Niles had a very different temperament from Daniels—Niles was shy and secretive while

Daniels was gregarious and ebullient—we worked together much as Daniels and I had.

Niles put the Committee together with such help as he wanted from me; and of course together we called on numerous government agencies for help. It was important to have balance, or the Committee's report would lack credibility. If it had too many advocates with strong positions publicly taken, the report would not be unanimous and would lack force. The hope was that the Committee would address the central issue of Black-White relations in our society. The denial of access to the goods and services of our whole society had led to mutual hostility and aggression to the point that citizens were becoming tolerant of violence and would not witness against it.

The Committee's report could not be foreseen nor could the outcome be assumed, but I could hope. The Report, when it was submitted to the President and released to the public, was sensational (President's Committee on Civil Rights 1947). For the first time in a Presidential document, the genteelism of "discrimination" was replaced by open discussion of segregation and its consequences. The report scrutinized all aspects of American life from this standpoint, no holds barred. The report did not restrict its findings and recommendations to Blacks and Whites. It criticized the evacuation of Japanese-Americans from the West Coast, made recommendations for restitution, and went beyond that to the governance of the Canal Zone, Guam, American Samoa, Puerto Rico, the Virgin Islands, Hawaii, and Alaska.

"To Secure These Rights" was front page news and it was not a one-day sensation. Discussion continued in Congress for many days. One Senatorial opponent was heard to say that he had heard the same things from other Presidents but that Truman meant it.

In fact, Truman did mean it and was prepared to risk his election in 1948 on the reception it received. From the time of the Report, segregation, not integration, was on the defensive. Implementation of the Committee's recommendations was to take many years and more than one Presidential term, but the mood of the country had changed.

For me, it was reassurance that my thinking about the relationship between deprivation and violence, actual and symbolic, was correct. The path toward implementation was clear, if difficult. The Committee had shown the way; the President had accepted the Report and endorsed it. The Executive must go ahead whether Congress and the Courts could move or not.

With President Truman's approval and under Niles' direction, I renewed my old association with the agencies on Daniels' network. Each of them had steps to take, programs that could be improved, resistances that could be overcome. It was not a question of ordering or supervising, merely of asking about progress, clearing away underbrush, and getting quick review of programs and proposals.

This time the personnel of the network were different. During the New Deal many heads of Federal departments and agencies appointed Black special assistants who helped their Heads identify Black problems, constituencies, and needs. Now their successors in a dozen agencies became partners in planning and implementing the President's programs.

There were some very significant areas that were full of resistance. Segregation, therefore discrimination, was deeply entrenched in the military establishment. It was only slightly and recently improved in the Civil Service. The District of Columbia, the Presidential city, was substantially segregated. And in spite of the war-time efforts of the Fair Employment Practice Committee, segregation and discrimination were much in evidence in the industries that supplied the nation's material needs even in procurement under government contract.

These stubborn areas might benefit, I thought, from the Committee and Report approach, but they would have to be different in each instance. The President, as Commander-in-Chief,

could order an end to segregation in the Armed Forces. But the subterranean methods of job description and classification which had grown up in the traditionally segregated military services would undermine any order unless they were fully explored and brought to the light of day. Without asking permission, I began preparation of an Executive Order creating a Presidential Committee within the Defense Establishment to look into and report to the President on these matters, and put it in my files.

The Civil Service Commission, I knew, had received authority to prevent discrimination, including racial discrimination, when it was created in the 1880s. But the Commission could hold that the appointing process was not discriminatory if candidates were given equal opportunity for employment and advancement. Under the Commission's rules, each appointing officer was given three candidates from the civil service register. Such an officer thus retained the freedom of his choice. I had many letters addressed to the President which asserted that they were often among the three, but were never chosen. Perhaps a Committee could help and could at least hear complaints. Again, I prepared a draft Executive Order and put it in my files.

By this time, the impact of the Civil Rights report was such that opinion leaders were looking for some action by the President greater than merely endorsing the Report. Clark M. Clifford, President Truman's Special Counsel, recommended that a Special Message be sent to Congress. Drafting this message was the duty of other specialists, though I was invited to participate.

Civil Rights in the District of Columbia proved to be a knotty problem. The franchise, the right to choose local government, was forbidden all residents of the District of Columbia, not just one racial or ethnic group. This seemed so fundamental that it had to take priority over segregation and discrimination, so the section on the District of Columbia referred only to the need for legislation to provide Home Rule. However, I was able to encourage the formation of a private group, the "Committee on Segregation in the Nation's Capitol," which published its own report.

The Special Message was sent to Congress on February 2, 1948 (Truman 1948c). It went straight to the point, asking for legislation to implement the Civil Rights Committee's major recommendations. It called for the establishment of a permanent commission on Civil Rights, a joint Congressional Committee on Civil Rights, and a Civil Rights Section in the Department of Justice.

Most of the then existing civil rights statutes dated from just after the Civil War. Truman asked for them to be strengthened. There had never been Federal protection against lynching; the Message asked for it. The right to vote was obviously in need of protection and the President asked for numerous specific improvements, including Federal protection against discriminatory interference by public officers.

The wartime Fair Employment Practice Commission had been created by Executive Order and lacked authority. Truman asked for a statutory FEPC. Segregation and discrimination in inter-state transport were common practice and were not prohibited by Federal law. Truman asked for Federal legislation.

Citizens of the District of Columbia had enjoyed home-rule and the right of Presidential suffrage only for a brief period after the Civil War. Truman asked Congress to provide it again.

Hawaii and Alaska had both sought statehood for some time. Truman urged Congress to take action and at the same time to grant a greater measure of self-government to our island possessions: Guam, American Samoa, the Virgin Islands, and Puerto Rico.

Racial and national bars to citizenship had been both legal and customary for many years. President Truman asked Congress to remove the remaining barriers.

Lastly, he asked Congress to act promptly on legislation already before it to settle the claims arising out of the forced evacuation of 100,000 Japanese-Americans from their homes during WWII. This request was acted on promptly; most of the others became law, but not until many years had passed.

By now, the election of 1948 was approaching. Liberals and conservatives alike regarded Truman as a loser, in part on the grounds that the Civil Rights report had so alienated the South that he could not win. It seemed time to temporize, and I agreed with others that a soft plank on civil rights in the proposed Democratic platform might prevent a walkout.

That was not to be. Two delegates from the Middle West, the then Mayor of Minneapolis, Hubert Humphrey, and Representative Andrew Biemiller from Wisconsin, introduced their own strongly worded plank and it passed. The delegates from four southern states walked out and nominated their own candidate for President.

The Humphrey-Biemiller plank dealt, among other things, with "Equal Treatment and Opportunity in the Armed Forces." The President's political advisors insisted that an Executive Order be issued using those very words. Clark Clifford sent for me in Wisconsin, where I had spent the summer looking after the family cranberry marsh, and I returned to Washington where I brought my two draft orders out of the deep freeze. After very full discussion the President agreed and issued the two orders simultaneously.

The die was cast. The President had endorsed the Civil Rights Report (Truman 1947), sent up a Special Message to Congress (Truman 1948c), and issued two Executive Orders covering the areas of responsibility directly in his charge (Truman 1948a, 1948b). He was about to run against the record of the 80th Congress, and Civil Rights was a central issue (Truman 1948d).

As everyone knows, Mr. Truman went on to win that election, notwithstanding the votes that were cast against him on both the Dixiecrat side and in the Henry Wallace campaign. The way was now clear to implement the findings and recommendations of the Civil Rights and related Committees.

Numerous efforts were made to disband those committees, especially the Armed Forces Committee, but President Truman stood up against all corners, even during the Korean war when he was under heavy pressure to let up. To do so would have meant a return to the FDR philosophy of no social changes during war. That would have been inappropriate under the conditions in Korea. There, our troops, many of them Black, quickly transferred from garrison duty in Japan, were thrown into close association on the field of battle with Republic of Korea troops. Under the shifting conditions, there was no way the military could keep track of personnel as they had been doing, and de facto integration carried the day. That was the beginning of the end, and, although it took a long time, the policy was never in doubt from that time forward through all succeeding Presidencies (Lee 1966; MacGregor 1981; President's Committee on Equality of Treatment and Opportunity in the Armed Services 1950).

One area remained untouched by Committee or by Executive Action. That was the question of discrimination in private employment. Throughout the Korean War and many Presidential emergency actions, liberals were eager to see a revival of the FEPC or some similar organization. It would have been difficult to fund such an organization after the FEPC had been terminated by the Congress. It seemed to me that procurement by government agencies again offered a means of improving the situation, and I proposed a Committee on Government

Contract Compliance with power to look into the existing contractual requirements of all the procuring agencies and to look for ways of enforcement by the agencies themselves.

That one proved ticklish to fund, and we eventually settled on the format of an interdepartmental committee with a minority of public members and the Departments footing the bill. We got that one through the hurdles of review and timed it for release later in the year. As word got out, it seemed necessary to release the order earlier. Eventually the final decision was made on the beach at Key West when the President was vacationing, so that the signed copy was rather sticky with salty sand (Truman 1951; President's Committee on Government Contract Compliance 1953).

With this order the work I had laid out for myself, but always done under the direction of others, was finished. Mr. Niles died in office, and I was handed over that part of his duties I could reasonably expect to carry out. No one could replace his deep knowledge of and experience with the Liberal-Labor establishment and the independence of Israel. In other ways I could help President Truman and I did so in the final months of his Presidency, regretful for the country that he was determined not to run again, but respectful of his stated feeling that seven years was close to two terms and that the spirit of the Constitution required him to step down.

I turned in my White House pass on his last day in office. On that day I was one of the few who had been on his staff during the entire seven years of his term, and the only one who had also been on the Roosevelt staff. Although it was the end of my task, the work was far from done—it had only begun (Nash 1979, 1984).

What I had done so casually would be done in future years by three fully staffed and funded Federal agencies. Their efforts are not enough in the face of needs for employment, housing and health that far exceed those at the close of the Great Depression and the start of World War II. How feeble my system of “I call you and you call me” seems in the face of needs so great. Nevertheless, I felt then and on reflection feel again, that deprivation theory, the practice of prompt intervention, and the equalitarian human values of our profession had all justified themselves.

In September 1961, my wish was granted and I became U.S. Commissioner of Indian Affairs. I resigned involuntarily on March 15, 1966, having had a little less than five years of spirited and—on the whole—successful activity. When I left I had general support among the tribes and within the Bureau of Indian Affairs (BIA); no support from the Chairmen of the Congressional Committees exercising oversight of Indian Affairs; I had lost the support of the Secretary of the Interior; the President of the United States was indifferent. Continuing in office could only damage the programs that were just beginning to be effective. By resigning when requested I could avoid a fruitless confrontation, protect my programs, and open the way to an Indian person who was qualified, a friend from my own state. The decision was painful, but the course was clear.

The process by which all this came about and the interplay of sub-cultures are pertinent to applied anthropology. We have a legitimate, special interest in the well-being of American Indians. For more than a century they have provided us with a data base with little recognition other than an acknowledgment in prefaces and footnotes.

The U.S. Congress, which has been given exclusive jurisdiction over Indian Affairs by the Constitution, had no interest in anthropology and its application when I was Commissioner. If anything, its members who served on committees active in Indian Affairs, viewed anthropologists as persons who stood in the way of incorporating Indians into the mainstream. Those values which we anthropologists take for granted—cultural identity, cultural pluralism,

and cultural diversity—appear to most Western Senators and Congressmen as negatives to the development of their resources.

In this light the fact that I was nominated, confirmed, and managed to survive for more than four years appears as a unique event. I did not—as I fantasized in my youth on Klamath Reservation—set everything straight and do everything right. Nevertheless, any one of us can look at the Congressional attitude toward Indian Affairs today—25 years after I took office—and see a great change in attitude. For me they are symbolized by the Menominee Restoration Act. This historic reversal of the Termination of the Menominee Tribe was not only a departure from the attitude of Congress between 1950 and 1960, it was contrary to the spirit of Congress during the reform era of which I was a part.

The U.S. Congress discharges its duties in Indian Affairs with the aid of four committees; two each in the House of Representatives and the Senate. The legislative Committees concern themselves with policy and programs; the appropriations committees provide the money to fund them. The Departments of the Executive Branch, including the Department of the Interior, must deal with the Congress through these committees. The nomination of the Commissioner of Indian Affairs is sent by the President of the United States, on the recommendation of the Secretary of the Interior, to the Senate for its “advice and consent.” Before the Senate takes action the matter is referred to the Committee on Interior and Insular Affairs which holds one or more hearings to determine the fitness and competence of the nominee.

The Commissioner of Indian Affairs is one of the few Bureau chiefs whose appointment must have the approval of the Senate. This is true for many reasons. Only the Congress can legislate on behalf of Indians. Congress holds the Trust, the Secretary of the Interior is the Trustee, and the Indian Commissioner is his executive officer. But in addition the role of Commissioner goes far back in the nation’s history (Kvasnicka and Viola 1979). The title originated with the Continental Congress, which designated Commissioners to deal with the Indian tribes on the frontier—then in Upper New York, western Pennsylvania, and the Carolinas. Benjamin Franklin was one of the Commissioners and so was Benedict Arnold.

John F. Kennedy had run for the office of President on a platform which included, among other things, a commitment to have an Indian person as Indian Commissioner. Stuart Udall was his first appointment to the Cabinet, and he too was committed to this ideal; but he was further committed to remaking the programs of the BIA through consultation with the Indian tribes. To meet both of these objectives, John O. Crow, a highly respected Cherokee from Oklahoma, and a career civil servant in the BIA, was named Acting Commissioner while a Secretarial Task Force pursued the course of consultation.

My search for employment after my defeat for re-election as Lieutenant-Governor of Wisconsin led me to a group which was screening candidates for upper-level appointments in the new administration. This group, headed by President Kennedy’s brother-in-law, Sargeant Shriver, was not averse to my appointment, but was not about to move hastily. Although I had campaigned for John Kennedy, and had even gone down to defeat with him in the election in Wisconsin, I was a “too-later,” for I had supported Hubert Humphrey in the crucial Wisconsin Presidential Primary. In the eyes of Robert Kennedy, the President’s brother, this was forgivable—but not quickly. The situation was made more sticky by the fact that the Kennedy campaign staff confused me with another guitar-playing campaigner who had sung anti-Kennedy songs in West Virginia. This sort of thing I never got a chance to clear up because I could never catch up with it; it didn’t count that I never did campaign in West Virginia.

Eventually I was appointed an assistant to Assistant Secretary of the Interior John A. Carver, who put together for Secretary Udall a Task Force on Indian Affairs and made me a member of it. Other members were W. W. Keeler, Chairman, then Chairman of the Executive Committee of Phillips Petroleum Company; James E. Officer, an anthropologist from Arizona, a friend of Secretary Udall and a member of this Society; William Zimmerman, formerly Assistant Commissioner of Indian Affairs to John Collier; and Acting Commissioner Crow.

Secretary Udall took the Task Force very seriously indeed, and called us together to give us his guidance and make us aware of our historic importance (Task Force on Indian Affairs 1961a). We travelled widely through Indian country, held ten hearings at convenient locations; heard from both Indian leaders and followers and from some non-Indians; interviewed key personnel from the BIA and from other Bureaus in the Interior Department; and eventually sat down and wrote our report as a joint effort. We submitted it on July 10, 1961 (Task Force on Indian Affairs 1961b), and the Secretary of the Interior presented it to the Congressional leaders at a luncheon meeting in his private dining room. The difficulties I was about to have were foreshadowed in the general dissatisfaction with the Report. It did not deal forthrightly with Termination they said, and the burning issue of land tenure was not dealt with adequately.

The Indians, the BIA and the members of the Task Force saw things quite differently; more important, we saw them the same. We saw the Report as sober and realistic. It did not over-promise, a common failure of earlier Indian programs. The Indians recognized their own input, and the BIA, though it did not universally approve of every statement in it, saw it as something it could live with. In point of fact the Report turned out to be a highly useful and workable document for the next five years. It also provided a basis for the BIA and the Tribes to work together toward common goals in a way that had not been the case since the Collier Administration.

The Congressional dissatisfaction with the handling of "Termination" in the Report could hardly have been avoided unless we had purposely sought a confrontation. The Kennedy Administration was not going to be Terminationist; yet we were aware that Congressional sentiment still favored Termination. We could have pointed out that the House Concurrent Resolution, which stated the views of Congress on Termination as they were in 1953, was only an expression of Congressional intent, never sent to the President for his signature and therefore not a Statute, not binding on the Administration, or even on succeeding Congresses. That would have wounded the Congressional egos even more than what we did say, namely, that all the talk about Termination was alienating the Indians from the BIA and the rest of the Federal Government, and that it was time to get back into communication by not talking about Termination.

By the time the Task Force Report had been reproduced and circulated, the Indian candidates for the Commissionership had become so numerous they were cancelling each other out, since each candidate had his own Congressional sponsors. On the advice of Chairman Keeler of the Task Force, Secretary Udall decided to recommend to President Kennedy that he send my name to the Senate. Acting Commissioner Crow was passed over but loyally remained silent. Thus, I slipped into the nomination between the ranks of those who wanted it as much as I did but were not lucky enough to be in the right place at the right time.

I, however, was on the hot seat with my name on a policy document that had already irritated important members of the Committee that would investigate my qualifications. It was certain that the Task Force Report would be litigated, to the detriment of my confirmation. Wiser men, less given to risk-taking, would have yielded to someone else, accepting a subordinate post



not subject to Senate scrutiny. I was in for a battle and I got it, but I have never regretted the decision.

When the day came that I was to appear before the Senate Committee on Interior and Insular Affairs, both Senators from Wisconsin, Senator Alexander Wiley, Republican, and Senator William Proxmire, Democrat, came to support my nomination and present me to the Committee. All members of the Committee had an opportunity to interrogate me and most of them did so. Their questions and my answers are on record as a “Committee Print” (Committee on Interior and Insular Affairs 1961), but many details were handed over to the Sub-Committee on Indian Affairs. In Executive Session this Committee went into exhaustive detail on my experience, loyalty to the United States, my familiarity with Indian Affairs, and my personal views related to the Task Force Report.

My background in anthropology was not ignored, but it was not treated as important. Important topics were my views on the continued existence of the tribes; private property vs. tribal; the eventual incorporation of American Indians into the “mainstream,” and how long I had lived on “reservations.” The Committee accepted without comment a Resolution of the Wisconsin Senate endorsing me but asked no questions about my White House service. The Task Force Report and legislation on the matter of heirship lands, which had recently been sent to the Senate from the Interior Department, were thoroughly worked over.

It was a humbling experience. I had played hardball politics in Wisconsin and did not expect softball rules. I realized the reason for thorough inquiry and I welcomed the favorable outcome. But I did not feel the Committee was getting at the real qualifications for the post; rather that the members wanted me to feel the weight of their views on Termination, individualization of tribal property, and cultural assimilation.

It seemed to me that the final vote of the full Committee was interminably delayed. The strategy of the minority on the Committee was to delay the vote, hoping to compel President Kennedy to make an interim appointment, perhaps even to withdraw the nomination. To accomplish this they stayed away from the Committee sessions at a time when several members were in Europe. One of the minority members, however, inadvertently stepped across the threshold of the meeting room. The Chairman immediately declared a quorum was present, put my nomination to the vote, and successfully sent it to the floor of the Senate.

The minority still persisted. Now the strategy was for minority Senators to speak against the nomination deploring my lack of experience for the job, the small size of my business, and depicting my nomination as a reward to a political hack. The maneuver failed. Three Senators spoke, as I expected. Then Senator Wiley from Wisconsin said he had read the report of the Committee, studied the record, and listened to the debate and would vote for me. Senator Humphrey spoke last and made a detailed and lengthy exposition that addressed the real issues. Among other things, he alluded to applied anthropology and the Society for Applied Anthropology.

The Senate gave its “advice and consent” and the long wait was over (Congressional Record 1961:19204–19210). I had won confirmation, but the long struggle had soured my relations with the Senate Interior Committee and that seemed to be reflected in the companion Committee in the House. Five years later it was the Chairmen of these two Committees who successfully demanded my resignation.

Now it was time to go to work. John Crow had loyally filled in as Acting Commissioner and knew the BIA in a way that I could not without putting in the years that he had. Jim Officer had been on the Task Force, was a well qualified anthropologist and a personal friend of

Secretary Udall. With Crow as Deputy Commissioner, and Officer as Associate Commissioner, we had the combination needed to make up for lost time.

The years of Termination had taken a terrible toll in Indian Affairs. The tribes had lost confidence in the BIA: indeed in the entire Federal establishment. The BIA, I soon found, had not been demoralized. It had simply gone on doing its technical job without much direction. Except for a few diehards it was ready to get on with new programs and would accept them if it could get a clear signal. I took it as my job to line up the Bureau in vigorous implementation of the Task Force recommendations. My hope was to develop working relations between the BIA and the tribes such that the BIA could become a technical assistance agency with a minimum of paternalism.

The Task Force hearings had done a lot to restore the Indians' faith in the government as a whole. Now I felt we should build on that confidence. We organized a series of meetings on the reservations, following the principle that Indian delegations were welcome to present their views in the Nation's capital, but that open communication required that they should have an opportunity to do the same thing on their own turf. In the course of the next five years I spent much time in travel status, visiting and holding meetings on most of the large reservations and many of the smaller ones. Included in the BIA bailiwick are numerous boarding schools, a large number of day schools, and several large irrigation projects. I visited all of them I could get time for, but there were many I missed. Throughout, the emphasis was on meeting with the Indians.

All of these meetings had the same general format. I was the host and the Indians were my invited guests. If it was at all possible we ate together before the meeting, as we usually did if I were their invited guest. We made use of the BIA school cafeterias for this purpose and often continued the meeting in the same building. At other times we met by invitation in tribal headquarters or Tribal Community Centers.

In either case the format was the same. I introduced the Tribal Chairman or he introduced me, whichever was appropriate. I spoke briefly, the Tribal Chairman spoke as long as he wished and introduced his Council members, who had the same opportunity. At this point I introduced the Area Director and the local Superintendent, and invited questions from the audience.

This was the real meat of the meeting. Everyone who wished had a chance to speak once before we went around a second time. There was an order of speaking: members of the local tribes first; members of other tribes next; local non-Indians last. BIA personnel were encouraged to attend, but they were told they were there primarily as resource people who would be called on to supply information and answer questions. I protected them from harassment by stating at the outset that no one would be put on trial and that no one's job was at stake.

To make sure I was believable I said that I would stay as long as anyone had something he thought I ought to hear. This was a risky statement, but it was necessary in view of the general belief that Commissioners came out to the reservations, made long speeches, gave no one else a chance to say anything, and then rushed back to Washington for important meetings. I had a lot of late nights, but the meetings were informative, disposed of hot issues to the extent possible, and had a generally ventilating effect.

While I was making the rounds of the reservations I also had a chance to meet with the BIA staff. The BIA is a large organization and there was no way I could touch base with everyone. But I made it my business to go for a day with those who had top line responsibility, especially the Superintendents, and see how they worked with the tribes. I soon found that the "quiet" ones were the best. They were the ones who did not raise their voices, were not peremptory and were able to listen because they were not speaking all the time.

Generally speaking, those who just couldn't accept the change took themselves out. There were a few changes I had to make, and there I operated on the premise that a large organization ought to have a place for a wide variety of talents. Where it was necessary to make separations I had the satisfaction of knowing that I had seen the person at work; there were very few of these.

Another part of my job as I saw it was the exchange of information with the interested public. In Indian Affairs that means almost everyone. For five years I accepted, to the full limit of my energy, speaking invitations to lunch groups, civic groups, church and education groups all over the country. These were wonderfully stimulating occasions, almost as exciting as the meetings with the Indians because of the opportunity it gave me to pick up on the strong feelings most Americans have about Indian Affairs. I found a large sense of wrongs done by our forefathers, little awareness of current wrongs, and much interest in our programs (Nash 1961a, 1961b, 1962a, 1962b, 1963a, 1963b, 1964a, 1964b, 1964c, 1964d, 1965a, 1965b, 1966).

Most of our programs had their origins in the Collier Administration and the potentials of the Indian Reorganization Act that had never been fully realized. To receive the benefits of the IRA it was necessary for a tribe to organize itself along modern non-traditional lines. The work of adopting these organization structures—constitutions and by-laws—was carried on in the Collier years by an applied anthropology unit. In that era there had also been significant anthropological input in cooperation between the Soil Conservation Service and the Indian Bureau (TC-BIA). I considered at some length the feasibility of organizing a new applied anthropology unit and decided against it. In the Collier era the applied anthropology units (Kelly 1977) and the Personality and Government study (Thompson 1951) had met with serious funding problems. In view of the attitudes expressed by the Committees of Congress in my early months on the job, I feared the effect on our appropriations if I should attempt a renewal of the applied anthropology unit. Thus my administration of the BIA, even though it was headed by two anthropologists, did little to advance the role of anthropology in government; we kept too low a profile. This is not to say that individual anthropologists were not invited or that research was not used.

In line with my feeling about open communication, I created in the Washington office of the BIA the "Operating Committee." This Committee consisted of the dozen or so upper level administrators who were in charge of old programs and the development of new ones. The functions represented consisted of such activities as Economic Development, Education, Social Services, Administration, Tribal Affairs, Information, Legislation, and Legal Affairs. The purpose of the Committee was to exchange information and to advise the Commissioner. The Committee met every morning when I was in Washington, but not when I was in the field, or when I was called to testify before Congressional Committees. I set the agenda, although I never refused a suggestion from the others. No votes were taken. Government departments are necessarily run on clear lines of authority. Bureau chiefs are responsible for everything in their organization. All the members of the Operating Committee were called on to express their opinions whether or not the subjects under discussion lay within their areas of expertise. After thorough discussion, then or later, I made my own decision, subject always to further review from the Secretary of the Interior and his departmental officers.

This was a substantial departure from the usual staff meeting in which each specialist reports to the head and does not venture to have opinions in other fields. It was not popular with some of my associates. The morning hours it took up were among the most precious to them. They said they couldn't get at their mail, and that they found the endless discussions with their associates who were not familiar with their problems to be a waste of time.

I felt just the opposite. The opinions of those who were unfamiliar with the multiple specialties of this large human service organization, but were required to work within it, needed to be heard so they could be clarified. The only way in which we could find our common goals and develop better methods to attain them was by sharing in their creation. We were attempting to help the Indians by working with their tribal governments. They tended to operate by consensus while using the formal governmental procedures that we had imposed on them through the IRA. What better method could there be than to seek the consensus in our own group?

Toward the end of my tenure in office, as relations with the Congress worsened and tension grew within the Interior Department, I yielded to the pleas of my associates and gave up the Operating Committee. That was a mistake. From that moment my leadership of the Bureau weakened as I became less and less familiar with what was in the minds of my associates. I knew that if I ever again had an opportunity to manage a large organization I would have an Operating Committee. I said this to myself at the same time that I knew this would never happen. Commissioners of the BIA do not usually materialize in other agencies. The job tends to eat them up.

The time came all too soon when I had to leave the BIA. I did not do so willingly, but I tried to be as good a soldier as John Crow had been. I accepted the inevitable in order to preserve the momentum I had generated without hurting the Indians I had tried to help.

I was asked to leave because my presence had become an embarrassment to the Interior Department: not with the Indians, not with my associates in the BIA, and not with the general public, but with those parts of the Department that depend on Congress for appropriations to maintain National Parks, Wildlife Refuges, the Public Lands and Reclamation Projects. The chairmen of the major committees who look after the programs and provide the money for the Interior Department had never been enthusiastic about me, and now they found me unacceptable. They made it clear that their major complaint was the amount of money I spent while the number of Indians in the service population was increasing. In other words, the purpose of spending money on Indians is to reduce the number of Indians on the reservations. That adds up to Termination no matter how it is spelled. I came up the political ladder fighting Termination and I went out the same way.

One of the outcomes of the philosophy of self-determination we introduced with the end of Termination has been a vigorous, even fierce, growth of Indian tribal identity. It has its present excited state in the movement for recognition of Indian tribal sovereignty. The demand has come a long way through a series of Federal Court decisions. The Federal Courts, the best defenders of Indian rights since the early 19th Century, are now beginning to interpret the Constitution along lines that acknowledge a high degree of tribal independence within the federal-state system.

The press of the tribes for recognition of their sovereignty will bring them into confrontation with all units of our federal, state and municipal governments. As they do, the anthropologists who owe so much to the Indian people will, I hope, help find a way to a non-violent accommodation.

In the middle of March, 20 years ago, I took the final step. I still understood the culture of the Executive and spoke its language. I submitted my resignation to the President of the United States and I accompanied it by my own draft of a reply accepting my resignation. Lyndon Johnson signed it and returned it within 45 minutes.

It might seem that my experiences are irrelevant to today's generation of applied anthropologists who are having their own difficulties and are solving their problems in their own

way. Civil Rights is now an established federal program with agency heads, statutes, appropriations and all the paraphernalia of bureaucracy. There is need for change, but it is unlikely to involve the combination of presidential assistant, anthropologist and newsman that existed when Daniels, Nash and Poston were working as a team. Recently, the Commissioner of Indian Affairs post has been vacant for long periods and, in any case, has been subordinated to the specially created position "Assistant Secretary for Indian Affairs." These changes suggest that the elements of social organization that I am qualified to speak about are a part of our history, but are no longer operational.

In point of fact my experiences between 1941 and 1966 are highly relevant today. In every organization the effective work force is the face-to-face group which forms and reforms itself every day in pursuit of a goal. This is the selfsame group studied by cultural anthropologists in the small communities that are our specialty. Does it really matter that our work may not be done in the name of anthropology, or that our constituencies may not have a clear picture of what anthropology is, only of the work we do? If they did, would it better validate our role?

While I was practicing applied anthropology in government, Margaret Mead was making the name of anthropology and its usefulness known throughout the entire world. Yet, no one asked me to work because I was a member of the same profession as Margaret Mead, a friend and colleague. My employers, clients and constituents made their own definitions of the term "anthropology" and used it to suit their own purposes if they used it at all. It became my responsibility to hold to the content of anthropology, its premises, values and ethics, while I performed services related to goals and programs defined by others. It is not significant that these services were performed in the heady atmosphere of the White House, the Senate and the Interior Department. The same inter-personal and inter-group fears and rivalries exist in the smallest, least pretentious work situations. The need is to understand them well enough to prevent them from interfering with performance of the task at hand.

In work-a-day government the desire to be of service is more important than degrees. The same is true of academic titles. Also important is the willingness to accept anonymity. In the academy we are taught to publish so that we will not perish, but we expect to publish over our own names and to receive advancement when our writing is recognized. The ethos of government requires that the "Head Man" accept responsibility and receive credit for everything that is done, including reports and publications. Even the style of communication is different when anthropology is being applied. We anthropologists are acutely aware of the continuity of past and present. Hence, we tend to preface our reports with complete culture histories to the dismay of our readers in government and industry. They communicate by "memos" in which everything important is put in the first paragraph, like a news story, and the rest is stated on the same page if possible.

These folkways are hard for some people to accept, but those who do will find that they are able to influence many outcomes, including the formulation of policy; and this, surely, is what we were taught we should do as applied anthropologists. By being of service, we anthropologists gain credibility based on our performance in helping our agencies reach their goals. With credibility comes confidence in our ability to solve problems, and after that we will be called on to provide answers, and that means to receive assignments. Once this has happened the way is open to define areas for ourselves, and so to make our own assignments rather than to wait passively.

It is at this point that core training in anthropology becomes significant. Given the opportunity to reach out, the areas we define should be appropriate to our personal competence, consistent with the agency's needs, related to our profession's subject matter and not inconsistent with its code of ethics.

My thoughts go back to the day I was advised to stay away from the Indian Agency. The tribal governments are closely bound by law and custom to the federal government in all its branches: the Congress, the Courts and the Executive. There is no way we can be helpful to the tribes if we deny the intimate relation between them and the federal government. The existence of simultaneous love-hate relationships in dependency situations was well-known to Malinowski and many others among our founders. It should not be a stranger to us.

The weaknesses and failures of the Bureau of Indian Affairs in the past are well known. Forced acculturation and Termination are both a part of the BIA's history, but we should not forget that these policies were handed to the BIA by the representatives of the states in the U.S. Congress, sometimes against the wishes of the BIA. There have been many times when the BIA was all that stood between the destructive forces of the frontier and the Indians.

Except for the Termination era, the BIA has existed primarily to preserve the reservation system through a close affiliation of the tribes and their elected officials with the federal government and its agencies. With all its limitations, the reservation system has been the principal bulwark of tribalism against the efforts of the states to assert jurisdiction. "Termination" was nothing more nor less than such an assertion. Currently it is not fashionable to assert states' rights so vigorously, except in the matter of hunting and fishing, and there the battle is as fierce as ever.

We anthropologists see ourselves as the charismatic saviors of Indian culture and the tribes; and, in truth, we have done a lot. But the Indians do not all see us this way. Many of them regard our willingness to testify on either side of the claims cases (for government or for tribe) as doubletalk. We cannot blame the Indians for our failure to explain clearly the Western notion of adversary proceedings and the role of the expert witness.

The BIA and the tribes need the help of the anthropological profession. We have made it available only on our own terms; we want to be asked, and we are not asked because we have denied ourselves the essential quality of the expert. We have taken our position in advance of being asked for advice. That is the role of the advocate: the tribal representative, or the tribal lawyer. The expert who wants to influence policy must offer data before he offers advice. If his data are appropriate he might be asked for advice.

The time is coming when we will be needed. Right now the BIA is healthy in the field, but is dying in the front office. At the same time the tribes are feeling the power of multiple court decisions that strengthen the concept of tribal sovereignty. If the tribes and their lawyers prevail—and they have been very successful so far—the federal-tribal-state relationship will be altered right up to the constitutional limit.

The Constitution acknowledged the existence of tribes but did not define them; generally that has been left to the courts. Most anthropologists will applaud this process and the probable outcome. But the tensions generated are already foreshadowed in the hostilities surrounding the exercise of Treaty rights. Will the members of our profession be in far away places offering their expertise to developing countries who wish to make the same mistakes our country made with the Indians a hundred years ago? Or will we accept a humbler mediating role between the courts, the tribes and the federal government as they seek to work out long-standing differences that go back to the settlement of the West?

If I can pass on anything of value across the generation gap it is likely to be the reminder that our profession has never fully met its obligation to our American Indian friends, and that the prototype of development plans in the Third World has been our own country's program on the American Indian reservations. Perhaps we ought to do a little better there before we export development programs as freely as we have been doing recently.

### Notes

<sup>1</sup>These observations have been paraphrased from an unpublished proposal on corporate internships made to the American Anthropological Association by an Ad Hoc Committee, F. L. W. Richardson, Chairman, on June 22, 1982.

<sup>2</sup> This bit of history was described to me personally by Radcliffe-Brown.

### References Cited

- Aberle, David F.  
1982 The Peyote Religion among the Navaho (2nd ed.). Chicago: University of Chicago Press.
- Brophy, William A., and Sophie D. Aberle  
1966 The Indian: America's Unfinished Business. Report of the Commission on the Rights, Liberties, and Responsibilities of the American Indian. Norman: University of Oklahoma Press.
- Chapman, Charlotte Gower  
1971 Milocca: A Sicilian Village. Cambridge, MA and London: Schenkman.
- Cohen, Felix  
1982 Handbook of Federal Indian Law. Charlottesville, VA: Michie Bobbs-Merrill Law Publishers.
- Collier, John  
1949 On the Gleaming Way: Navajos, Eastern Pueblos, Zuni, Hopis, Apaches and Their Land; and Their Meanings to the World. Denver, CO: Sage Books.  
1963 From Every Zenith: A Memoir and Some Essays on Life and Thought. Denver, CO: Sage Books.
- Committee on Interior and Insular Affairs  
1961 Nomination for Commissioner of Indian Affairs. Hearings before the Committee of Interior and Insular Affairs, United States Senate, Eighty-seventh Congress, First Session, on the nomination of Philleo Nash to be Commissioner of Indian Affairs, August 14 and 17, 1961. Washington, D.C.: U.S. Government Printing Office.
- Congressional Record  
1961 Commissioner of Indian Affairs. September 20. Pp. 19204–19210. Washington, D.C.: U.S. Government Printing Office.
- Daniels, Jonathan  
1938 A Southerner Discovers the South. New York: MacMillan.  
1975 White House Witness. New York: Doubleday.
- Dillon, Wilton S.  
1974 Margaret Mead: President-Elect, 1974. Science 184:490–493.
- DuBois, Cora  
1939 The 1870 Ghost Dance. Anthropological Records 3:1. Berkeley: University of California Press.

- Eagles, Charles W.  
1982 Jonathan Daniels and Race Relations: The Evolution of a Southern Liberal. Knoxville: University of Tennessee Press.
- Johnson, Charles S., and Associates  
1943 To Stem This Tide: A Survey of Racial Tension Areas in the United States. Boston and Chicago: Pilgrim Press.
- Kelly, Lawrence C.  
1977 Anthropology and Anthropologists in the Indian New Deal. Paper presented at the Organization of American Historians, April 1977, Atlanta, Georgia.
- Kvasnicka, Robert N., and Herbert J. Viola  
1979 The Commissioners of Indian Affairs 1824–1977. Lincoln and London: University of Nebraska Press.
- Lasswell, Harold D.  
1930 Psychopathology and Politics. Chicago: University of Chicago Press.  
1935a Collective Autism as a Consequence of Culture Contact: Notes on Religious Training and Peyote Cult at Taos. *In Zeitschrift fur Sozialforschung*. Pp. 232–247. Paris: Librairie Felix Alcan.  
1935b World Politics and Personal Insecurity. New York and London: McGraw Hill.
- Lee, Ulysses  
1966 The Employment of Negro Troops. United States Army in World War II, Special Studies. Washington, D.C.: U.S. Government Printing Office.
- Loram, C. T., and T. F. McInwraith  
1943 The North American Indian Today. University of Toronto-Yale University Seminar Conference, Toronto, September 4–16, 1939. Toronto: University of Toronto Press.
- MacGregor, Morris J., Jr.  
1981 Integration of the Armed Forces 1940–1965. Defense Studies Series. Washington, D.C.: U.S. Government Printing Office.
- Nash, Philleo  
1937 The Place of Religious Revivalism in the Formation of the Intercultural Community on Klamath Reservation. *In Social Anthropology of North American Tribes*. Fred Eggan, ed. Pp. 377–449. Chicago: University of Chicago Press.  
1961a Address Before the 18th Annual Convention, National Congress of American Indians, Lewiston, Idaho, September 21. Washington, D.C.: U.S. Department of the Interior.  
1961b Address before Conference of Indian Bureau Superintendents, Denver, Colorado, October 16. Washington, D.C.: U.S. Department of the Interior.  
1962a Address Before Annual Meeting of the American Indian Affairs, New York, May 7. Washington, D.C.: U.S. Department of the Interior.  
1962b Address Before 19th Annual Convention, National Congress of American Indians, Cherokee, North Carolina, September 4. Washington, D.C.: U.S. Department of the Interior.  
1963a The Causes of Indian Poverty. Sunday Evening Forum, Tucson, Arizona, March 3. Washington, D.C.: U.S. Department of the Interior.  
1963b Remarks Opening a Joint Conference of Canadian and U.S. Administrators of Indian Affairs, Scottsdale, Arizona, October 7. Washington, D.C.: U.S. Department of the Interior.  
1964a The War Against Poverty—The American Indians. Abraham Lincoln Centre, Chicago, Illinois, February 12. Washington, D.C.: U.S. Department of the Interior.  
1964b Remarks Before the City Club of Cleveland, Cleveland, Ohio, April 11. Washington, D.C.: U.S. Department of the Interior.  
1964c Remarks Before the American Indian Capital Conference on Poverty, Washington, D.C., May 12. Washington, D.C.: U.S. Department of the Interior.



- 1964d Implementation of the Economic Opportunity Act of 1964. Wisconsin State AFL-CIO Conference, Green Bay, Wisconsin, October 5. Washington, D.C.: U.S. Department of the Interior.
- 1965a Statement Before the U.S. House of Representatives Subcommittee on Indian Affairs of the Committee on Interior and Insular Affairs, Committee Print, January 27.
- 1965b American Indians and the American Society. Institute on Human Relations, Fisk University, Nashville, Tennessee. Washington, D.C.: U.S. Government Printing Office.
- 1966 Dillon Lectures on Law and Government, University of South Dakota, Vermillion, South Dakota (audiotape).
- 1979 Anthropologist in the White House. *Practicing Anthropology* 1(3):3, 23, 24.
- 1984 Memorandum: President Truman and Civil Rights. Prepared for Truman Centennial Committee, Clark M. Clifford, Chairman (manuscript).
- Philp, Kenneth R.  
1977 John Collier's Crusade for Indian Reform 1920–1954. Tucson: University of Arizona Press.
- Powell, J. W.  
1881 First Annual Report of the Bureau of Ethnology to the Secretary of the Smithsonian Institution: 1879–'80. Washington, D.C.: U.S. Government Printing Office.
- President's Committee on Civil Rights  
1947 To Secure These Rights. Washington, D.C.: U.S. Government Printing Office.
- President's Committee on Equality of Treatment and Opportunity in the Armed Services  
1950 Freedom to Serve. Washington, D.C.: U.S. Government Printing Office.
- President's Committee on Government Contract Compliance  
1953 Equal Economic Opportunity. Washington, D.C.: U.S. Government Printing Office.
- Redfield, Robert, Ralph Linton, and Melville Herskovits  
1936 Memorandum For the Study of Acculturation. *American Anthropologist* 38:142–152.
- Spier, Leslie  
1930 Klamath Ethnography. University of California Publications in American Archaeology and Ethnology, Vol. 30. Berkeley: University of California Press.
- Stern, Theodore  
1965 The Klamath Tribe: A People and Their Reservation. Seattle and London: University of Washington Press.
- Task Force on Indian Affairs  
1961a Official Report of Proceedings Before the Department of the Interior. February 9, 1961. In the Matter of: Secretary's Meeting on Report of Task Force on Indian Affairs. Washington, D.C.: Alderson Reporting Company.  
1961b Report to the Secretary of the Interior. July 10, 1961. Washington, D.C.: U.S. Department of the Interior.
- Thompson, Laura  
1951 Personality and Government: Findings and Recommendations of the Indian Administration Research. Mexico, D.F.: Ediciones del Instituto Indigenista Interamericano.
- Truman, Harry S.  
1946 The President's Committee on Civil Rights Established. December 5. Executive Order No. 9808, 11 Federal Register, p. 14153.

- 1947     Remarks to Members of the President's Committee on Civil Rights, January 15. *In Public Papers of the Presidents of the United States: Harry S. Truman: Containing the Public Messages, Speeches, and Statements of the President: January 1 to December 31, 1947.* Pp. 98–99. Washington, D.C.: U.S. Government Printing Office.
- 1948a     Regulations Governing Fair Employment Practices Within the Federal Establishment. July 26. Executive Order No. 9980, 13 Federal Register, p. 4311.
- 1948b     Establishing the President's Committee on Equality of Treatment and Opportunity in the Armed Services. July 26. Executive Order No. 9981, 13 Federal Register, p. 4313.
- 1948c     Special Message to the Congress on Civil Rights, February 2, 1948. *In Public Papers of the Presidents of the United States: Harry S. Truman: Containing the Public Messages, Speeches, and Statements of the President: January 1 to December 31, 1948.* Pp. 121–126. Washington, D.C.: U.S. Government Printing Office.
- 1948d     Address in Harlem, New York, upon receiving the Franklin Roosevelt Award, October 29, 1948. *In Public Papers of the Presidents of the United States: Harry S. Truman: Containing the Public Messages, Speeches, and Statements of the President: January 1 to December 31, 1948.* Pp. 923–925. Washington, D.C.: U.S. Government Printing Office.
- 1951     Improving the Means for Obtaining Compliance with the Nondiscrimination Provisions of Federal Contracts. Executive Order No. 10308 in Equal Economic Opportunity. Pp. vii, viii. Washington, D.C.: U.S. Government Printing Office (1953).

## **Chapter 17**

### **Margaret Lantis: Culture, Personality, and Acculturation**

**Thomas Weaver**

The work of Margaret Lantis, the 1987 recipient of the Society for Applied Anthropology's Malinowski Award, was much influenced by the culture and personality studies of fellow Malinowski Award winner Alexander Leighton (1984) and Margaret Mead.<sup>1</sup> Also significant in her development as an anthropologist were undergraduate courses in psychology, anthropology, and languages taken at the University of Minnesota. Lantis used Rorschach and other psychological tests and life-history techniques in studies of Inuit childhood (1960). Her prior knowledge of rural sociology, a relatively new discipline at the time, and of agricultural extension work, both predecessors of applied anthropology, together with her breadth of experience in consulting and employment with federal and state agencies shaped her approach to applied anthropology (Lantis, personal communication, 1998).

Lantis (1906–) received a bachelor's degree from the University of Minnesota in 1930 and a doctoral degree in 1939 from the University of California at Berkeley with a dissertation entitled "Alaskan Eskimo Ceremonialism," the first study to focus on this topic. Lantis is best known for her work in Alaska, beginning in the isolated Aleutian Islands in 1933, after only one year of graduate studies. Like Philleo Nash, the 1986 recipient of the Malinowski Award (Nash 1986), Lantis spent much of her early career (from 1943 to 1963) working with public agencies. Even before her work in Alaska she had acquired experience with a social agency in Minneapolis. Lantis conducted field studies in ethnography, socialization, health, and the economy of various Inuit, Aleut, and Indian communities in Alaska, and on the health and economy of rural communities in southeastern U.S. (1952).

Lantis began federal service in the War Relocation Authority (WRA), first in the rapidly constructed relocation centers for Japanese Americans in the west, and later with the central office in Washington, D.C. In this she shared experiences with three other Malinowski Award recipients—Alexander Leighton, Edward Spicer, and Elizabeth Colson. (See Leighton 1984, Spicer 1976, and Colson 1985.) Her time with the WRA included work with farming families. Lantis' longest government employment was almost a decade in the U.S. Public Health Service. The work ranged from the village to the national level and from personal care and sanitation surveys to national health and education projects. She was also employed by or consulted for the Department of Agriculture, the Bureau of the Census, the Bureau of Indian Affairs, the Arctic Health Research Center, the Arctic Institute of North America, the Alaska Psychiatric Institute, and Encyclopaedia Britannica.

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In these positions, Lantis obtained information through research and then provided it to relevant administrators. She did not publish much; her work was only available in agency reports and contributed to anthropology's "fugitive literature," as Barton Clark and John van Willigen have labeled applied anthropologists' unpublished reports (Clark and van Willigen 1981). She wrote about her experiences in Washington, D.C., in the Society for Applied Anthropology's major journal, *Applied Anthropology*, in an article entitled "Applied Anthropology in Public Service" (1945). A later item on the same subject was published in the society's renamed journal, *Human Organization* (1961). The first article focused on responsibilities, community and other relationships, and principles of involvement in federal service, while the 1961 article described more practical matters in dealing with agencies.

Lantis was a visiting professor at several universities: Reed, California (Berkeley), Alaska (Fairbanks), Washington, Minnesota, George Washington, and McGill (Montreal). She also participated in a study of Harvard graduates for four years in connection with her culture and personality interests. Anthropology and social science were expanding in the 1960s, with applied anthropology being taught in medical schools and in other departments. Lantis received a teaching appointment at the University of Kentucky in 1965, from which she retired in 1974.

In her Malinowski Award presentation entitled "Two Important Roles in Organizations and Communities" (1987), Lantis focused on the roles of leader and follower. She selected this topic with the intention of addressing "working applied anthropologists," by then found in a variety of institutions (Lantis, personal communication, 1998). Her summary surveyed aspects of leadership, such as personality traits, skills, interactions between leaders and followers, location and situations, functions, and styles, and provided good clues to the literature up to the time of the presentation. She highlighted interdisciplinary contributions to this field and called attention to the paucity of work on this topic by anthropologists. Hers is one of the earliest references by anthropologists to the organizational culture literature, today found mainly in the work of business management experts and psychologists (Lantis 1987:190–191).

Like most recipients of the Malinowski Award, Lantis did not mention Malinowski in her presentation, nor did she refer to any of his writings or any influence he may have had on her work. As Elizabeth Colson so aptly observed (e-mail to author, 1998), the influence of Malinowski for the U.S. was not so much in his applied work, but in the general ethnographic and other writings that most anthropologists of the time were reading.

The Malinowski Award was given to Lantis in recognition of her many years applying anthropology and helping people through service with public agencies. She is also well known for her pioneering work on the Inuit. Additionally, she served the profession as president of the American Ethnological Society and of the Society for Applied Anthropology.

### Notes

<sup>1</sup>This annual award is given to a senior colleague in recognition of efforts to understand and serve the needs of the world through social science. See Thomas Weaver's "The Malinowski Award and the History of Applied Anthropology" (2002b) for a brief history of the award and an overview of the recipients and their work, and Weaver's "Malinowski as Applied Anthropologist" (2002a) for an introduction to Bronislaw Malinowski and his work.

### References Cited

Clark, Barton M., and John van Willigen

- 1981 Documentation and Data Management in Applied Anthropology. *Journal of Cultural and Educational Futures* 2(2-3):23-27.

Colson, Elizabeth

- 1985 Using Anthropology in a World on the Move. *Human Organization* 44(3):191-196. (Republished at <<http://www.sfaa.net/>>.)

Lantis, Margaret

- 1945 Applied Anthropology in Public Service. *Applied Anthropology* 4(1):20-32.  
 1952 Eskimo Herdsmen: Introduction of Reindeer Herding to the Natives of Alaska. *In Human Problems and Technological Change*. E. H. Spicer, ed. Pp. 127-148. New York: Russell Sage Foundation.  
 1960 Eskimo Childhood and Interpersonal Relationships: Nunivak Biographies and Genealogies. *Monographs of the American Ethnological Society*, Number 33. Seattle: University of Washington Press.  
 1961 Anthropologists in the Federal Government. *Human Organization* 20(1):36-41.  
 1987 Two Important Roles in Organizations and Communities. *Human Organization* 46 (3):189-199. (Republished at <<http://www.sfaa.net/>>.)

Leighton, Alexander

- 1984 Then and Now: Some Notes on the Interaction of Person and Social Environment. *Human Organization* 43(3):189-197. (Republished at <<http://www.sfaa.net/>>.)

Nash, Philleo

- 1986 Science, Politics, and Human Values: A Memoir. *Human Organization* 45(3):189-201 (Republished at <<http://www.sfaa.net/>>.)

Spicer, Edward H.

- 1978 Beyond Analysis and Explanation? Notes on the Life and Times of the Society for Applied Anthropology. *Human Organization* 35(4):335-343. (Republished at <<http://www.sfaa.net/>>.)

Weaver, Thomas

- 2002a Malinowski as Applied Anthropologist. *In The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers*. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.  
 2002b The Malinowski Award and the History of Applied Anthropology. *In The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers*. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.

## Two Important Roles in Organizations and Communities

Margaret Lantis

In the front of this society's journal, *Human Organization*, it is stated that the Society's "primary object is scientific investigation of the principles controlling the relations of human beings to one another . . . and the wide application of these principles to practical problems."

Throughout this century, general anthropologists, not only applied anthropologists, have paid attention to special persons, and to special roles in the relations of human beings. There was early attention to headship, the heads of kin groups or tribes, and the "big men" of the economy, and the shamans and ritualists. That emphasis endured a long time. In the 1940s and 1950s another set of human relations came to attention—relationships of parent and child. A whole literature on socialization appeared.

Increasingly from the 1950s into the next decade, anthropologists, especially applied anthropologists, paid attention to innovators, technical change agents, and culture brokers. Cultural change, like socialization, was not new. However, the persons occupying the roles and performing the functions were the subjects of case studies instead of our talking only about long processes and the results of change.

Then the feminist movement hit our discipline. There were and are case studies of women's roles in all kinds of situations and relationships. More recently the elderly, adolescents, and people with special medical or social problems have received attention from new specialists within anthropology. There also has been in the 1970s and 1980s a wealth of publication on the "person," the "self," whether the person was the anthropologist reacting to the host group or the recognition of self in the group (Lantis 1982). More recently, in this decade, one sees publications on patrons and clients, on "clientalism" (for an example, see Schmidt et al. 1977).

Throughout these periods a few anthropologists, combining part of sociology and part of psychology, have researched other relationships: in street gangs and criminal gangs, in ethnic enclaves, in game and play groups, and in the arts. We have always noted the role of a good storyteller. Surprisingly, a set of roles that is universal has received only sporadic attention and rarely a study fully recorded and fully analyzed—the personal relations of leader and followers! Here are two roles that we must no longer ignore. Note that I speak of a set of roles, the followers being as important as the leader, and there are more of them. It is trite but true that there are no leaders without followers.

I am not here discussing any individual current or recent political leaders. This is not a paper about politics. It is about the scientific study of a set of human relations and the principles guiding them, as stated in this journal. I will review some of what is already known about the three parts of the subject: the leaders, their actual or potential followers, including the interaction between them, and the importance of the situation in which they act.

The situation is especially in our domain of interest, but most of the literature comes from social psychology, applied psychology, and political science, including political anthropology.

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This address was previously published in *Human Organization* 46,3(1987):189–199.

Sociologists and anthropologists have contributed, but not so extensively, as we shall see. It is time for us to delve further into this subject, and we will look at some reasons why we can profitably do so in our work.

The principal relevant topic of earlier anthropologists' research was revitalization, where local leaders were well known and the course of the cultural movements that they led could be observed. Also, there always was interest in chieftains of tribes, heads of kindreds, and religious leaders (see Human Relations Area Files 1972 for numerous references). More recently, applied anthropologists have written about change agents but usually in general terms. They have not often analyzed the dynamics of the relationship and the factors affecting it. Regarding recent social science publications, I regret that I do not know enough about those in other countries to evaluate the trends and topical emphases of research although individual reports are known. We can begin by looking at research in the United States. Then, we can hope, there will be more reviews of work elsewhere.

In the 15-year period 1972 to 1986 inclusive, the *Journal of Applied Psychology* alone contained 22 articles on leaders and followers, while there were also 21 papers in various other psychology journals in the ten years from 1972 to 1982 (Santora 1983): a total of 43 papers at least. In the same 15 years through 1986, *Human Organization* had four articles on the subject. It is true that many of the social psychology and applied psychology papers were short reports of structured research situations testing a limited number of factors, whereas the anthropology articles presented field research and were more comprehensive. However, one must recognize that the psychologists also discussed theory, while anthropologists have been little concerned with a theory of leadership and followership.

Two recent annual meetings (1984 and 1986) of the American Anthropological Association, together containing 800 sessions with about 3,000 papers, showed in their abstracts only eight papers on leader-follower relations. The situation at annual meetings of the Society for Applied Anthropology has been about the same. At the 1984 meeting, for example, there was nothing specifically on leadership. One should remark that more than 15 years ago John Honigsmann organized—at an American Anthropological Association meeting—an all-day session on leadership in the American Arctic and Subarctic. That session first induced me to look at this subject.

Now that anthropologists are working in a great variety of government agencies, in education, welfare and other organizations and private businesses, in urban neighborhoods or in villages, in regions or provinces, we encounter in our work the people who “take the lead” and we see the reactions to them. Here I am not speaking about Mahatma Gandhi and Martin Luther, but about the people—often in ordinary situations—who do more than ordinary work; and they are people to whom others *as a group* listen and accept. This is a relationship beyond that of private counselor, even though personal inspiration or guidance may be given. The relationship is not only interesting to see—it can affect our own work. We often become involved. Why, then, have we paid so little attention to it? The following are some possible answers:

First, there have been so many bad leaders, really terrible ones. We usually are working with ordinary people, and we are likely to avoid the extraordinary, problematic or threatening people.

Second, leadership to some people is a “pop” concept: being a leader means being popular, like a new rock music star; and not many anthropologists are dealing with “pop” culture.

Third, the most prominent leaders, at a higher level than oneself, often are religious or political figures. We pay our respects and then keep a proper distance from them, perhaps to avoid identification with any factions.

Fourth, leadership is hard to define and to analyze. It is thought to be in the province of social psychology, not a common adjunct field of anthropologists. Yet we write about innovators and trend-setters, about social gate-keepers and cultural brokers, and sometimes about officials and administrators. We have rarely gone farther to ask: is there something common to all of them in their social functions?

A political scientist, in a recent book on leadership seen in multi-disciplinary perspective, has noted—regarding United States Americans in general—that they are ambivalent and “even opposed” to power and to strong leadership (Kellerman 1984:66). Indeed, U.S. field workers in health work or community development have been described as usually identifying with or at least wanting to work with the under-dogs, not the top dog. Yet, as Laura Nader wrote about “studying up” the socio-economic scale: studying the higher government, business, church or other officials in our own societies, we can “participant-observe” leadership among various roles. We can watch it in the daily functioning of the organization in both external and internal relations (Nader 1969:305–307).

From the political scientist comes another explanation: it is harder to see and understand leadership in settled routine situations than in a crisis (Kellerman 1984:69). We all hear about leaders in crises, but anthropologists are not journalists watching a revolution. We are most likely to observe or participate in slow change in everyday situations.

Finally, we have been indoctrinated, as have most of the modern historians, against the “great man theory,” “the hero in history” explanation of cultural and social progress. The rebuttal to Thomas Carlyle’s extreme statement of the value of the great man has been made by the *Zeitgeist* concept, by cultural determinism, social determinism, and economic determinism, with sub-theories of cultural, economic and political cycles regardless of the identity of leaders.

Historians as well as people in other social and behavioral sciences have latterly emphasized social movements, but there is no movement without people interacting and moving, as there is no culture except in people’s minds and behavior. We must not make the mistake that the general public makes when confusing the products of culture and culture itself.

As we shall see, the combination of leadership and followership is really a group activity that can sometimes become a mass movement—for good or ill.

### **Where to Research Leadership**

Let’s assume that you do want to look closely at these complementary or reciprocal roles. Where and how do you start? The answer is: wherever you are working. A small sample of work by anthropologists or studies by people in related disciplines whose reports on leadership have been published in an anthropology journal includes the following: a peasant community in Italy (Silverman 1967); labor union leaders in El Salvador (Makofsky 1978); a Black neighborhood in Minneapolis (Provinzano 1984); Democratic Party leaders in a town in New York State (Sady 1985); Afikpo groups in Africa (Ottenberg 1971); the Chinese community of Thailand (Skinner 1958); a group in the New Guinea Highlands (Hatanaka 1972); a Maya community (Thomas 1981); several Eskimo communities; and U.S. business corporation leaders (Hunt and Larson 1979). Of special interest to us is an article surveying psychology reports on rural areas in India, including studies of leadership in rural development (Sinha 1985:437–438 on leadership).



Though brief, it covers nine publications by Indian scientists on leadership. Other topics include communication and motivation in agricultural and other development projects.

The social and applied psychologists have devoted much effort to research on leadership in business and other large organizations, thus giving their attention to people in assigned positions rather than acclaimed roles—people with stated work responsibilities—or in simulations of task-oriented positions. The research purpose was to test specific factors in the relations of leader and followers. One anthropologist states that social psychology research “has tended to take the form of questionnaire and ‘created situation’ methodologies and has generally been confined to leaders in bureaucratically organized, formalistic, complex organizations . . . [although intended to formulate or test theory] commonly these studies have been oriented toward the social engineering goal of improving outcomes. Thus we see repeatedly that the question addressed is ‘How can we improve leader effectiveness?’ rather than ‘What is leadership?’” (Provinzano 1984).

We must not criticize the applied psychologists too harshly for trying to improve an important human relationship, as we applied anthropologists similarly may try to help a rural community improve its health care or education of children. The psychologists have made a few efforts to compare field and laboratory research (that is, structured situation research). One of their real contributions that we must not disregard has been a survey of all types of literature on the subject, to sort out the valid and useful from the useless or false. And they have paid attention to the followers or subordinates or whomever the leaders are working with and also paid attention to the context. It is particularly in their emphasis on the context or situation of the interaction that social psychologists and social anthropologists can come together, as well as other specialists in both disciplines.

As we all know, historians, essayists, philosophers, theologians, political scientists and psychiatrists have studied the lives of historic leaders—the famous, the heroic, the tyrants and dictators. An interesting recent book (Strozier and Offer 1985), a collection of “Psychohistorical Essays” on famous leaders and on people who have studied them, contains a chapter on Erik Erikson, the psychoanalyst, and his series of studies of Martin Luther, Gandhi, George Bernard Shaw, Gorky, Albert Einstein, and a few others. Erikson’s connection to anthropology can be seen in his analysis of leaders as well as in his other writings. About 1940 he lived in Berkeley and worked closely with A. L. Kroeber at the University of California (together they published a psychocultural analysis of the Yurok Indians of northern California). He also was acquainted with various other anthropologists.

Sociologists have their own research literature on people whom they prefer to call “influentials” instead of “leaders,” as people who have read Robert Merton and other sociologists know (Merton 1957:131–194). The concept of “influential” seems not fully congruent with “leadership,” but it is best for interested persons to look at recent articles in sociology journals to see what is being researched and written. The most famous statement by a sociologist is Max Weber’s (1947) definition and discussion of “charisma.” Social and behavioral scientists still occasionally publish a comment on it. It will be mentioned again later.

Anthropological viewpoint and experience are good for research on questions of multidisciplinary scope, and fortunately it can be done almost everywhere. Aren’t we supposed to be the take-it-all-in and get-it-all-together people?

## Definition of Leadership

Perhaps the anthropologist is in a situation where he is expected to be a leader, or perhaps he is looking for someone whom he can trust and follow. What knowledge, assumptions, expectations should and can one bring to this subject?

First, how does one recognize real leadership? One political scientist has compiled a list of eleven categories of definitions of it. For example, “as the art of inducing compliance,” that is, the exercise of power or influence to mold a group to one’s will; or “as a form of persuasion . . . management of people by persuasion and inspiration”; or “as an instrument of goal achievement” for accomplishment of group goals (Kellerman 1984:70). Those are only three of the eleven.

We can deal with this rather difficult question of definition in another way: by considering how leadership differs from other roles. “Headship” referring to the holder of an office or the head of an organization does not also indicate “leadership.” A head *may* be a leader but is not one inevitably. The head’s authority and relationship to subordinates is maintained by an organized system. The leader, in contrast, is accorded his authority by the group members who “follow because they *want* to rather than because they *must*” (Kellerman 1984:71, original italics).

A leader is not necessarily a model. The group members may not like the leader’s personality and do not want to act like him, but they think that he can direct them in accomplishing the job that they want done but have been unable to do. (Hereafter, for the sake of brevity I will use “he” instead of “he or she.”)

In reference to identification of a leader, in a 1978 conference in which heads of large businesses participated with university consultants, it was stated that tests to identify leadership were inadequate (Burack 1979:30–32). (There is evidence elsewhere that some training courses for leadership do not accomplish what they are intended to do.) This group did say that for a definition one must observe people who are accepted by others as leaders.

Is acceptance enough? Probably not. Leadership can be demonstrated only “when it can be shown that those said to be followers would otherwise have behaved differently” (Kellerman 1984:73, quoting Lewis T. Edinger in *The Comparative Analysis of Political Leadership*). In short, the leader has changed some people’s behavior, leading them to do something that they would not have done otherwise. The implication here is that basic attitudes and behaviors have been changed, and not by use of force. There is a range of power short of physical force.

What about other designations, other categories? Because leaders influence others’ attitudes and behavior, why not call them “influentials?” Bierstadt has said that influence is persuasive while power is coercive (quoted by Antoun 1979:205). The term “leader” has been used realistically for a range of powers from respected guidance or inspiration, through religious, economic, or social status threats, to ultimate use of force. Apparently somewhere in the middle of this range one should draw a line between “want to” and “have to,” although that is not easy to do. A person occupying a managerial position, for example, has some economic control over an employee—an unstated threat—but can also influence the way the employee works and can help him to understand the work.

The term for one variant of leader has had an interesting history. Originally a “demagogue” was a leader championing the common people. Now he is a person who uses popular prejudice and/or false claims and promises to gain power. Unfortunately, the demagogue is sometimes very persuasive.

Finally, what is a charismatic leader? Tucker has stated well Max Weber's original analysis of a public "charismatic movement" that explained the followers' picture of its leader: "the leader who enjoys his authority not through enacted position or traditional dignity, but owing to gifts of grace (charisma) 'by virtue of which he is set apart from other men and treated as endowed with supernatural, superhuman, or at least specifically exceptional powers or qualities' (Weber 1947:358)" (Tucker 1968:731). Weber did not say this leader is superhuman but that people, in their desperate need for an inspired person who appears able to achieve what no ordinary means have achieved, see the leader as divinely endowed. Weber emphasized people's circumstances of misery and desire for change in which they find hope in a mystical belief in the leader. Since we are dealing not with such rare relationships but with those that we are likely to encounter and because today the term is often misused, "charisma" will not be used here.

A simple conclusion regarding definition is that a leader is a person who influences in a given direction the attitudes and behavior of not one person or a few people but a whole group or many people.

### **Leaders' Personal Traits**

We arrive at something that is popularly considered the magic key: personality. Early writers assumed that leaders have special recognizable personal traits (Goldenson 1970:678). Among the eleven categories of leadership definitions already mentioned, one states that "Leadership is a combination of traits than enables a particular individual to motivate others to accomplish given tasks" (Kellerman 1984:70). However, writers on the subject have never agreed on the list of essential traits. In 20 studies that were examined by one psychologist, 79 different traits were thought to distinguish leaders from non-leaders. There was only a little consistency among the lists (Wrightsman 1977:637-638). There is no such thing as "a born leader," as is stated in popular terms.

Furthermore, because so many different personalities have been seen among leaders, one must conclude that there is no ability, no set of traits, effective in every kind of situation. Different situations require different personal qualities. Regarding what characteristics the followers demand of their leaders, Cecil Gibb, one of the best writers on the subject, has said that "the evidence suggests that every increment of intelligence [among government leaders] means wiser government, but that the crowd prefers to be ill-governed by people it can understand" (Gibb 1969:218).

According to Wrightsman, a social psychologist, a few personal traits or qualities of leaders have been found in several studies but not always together as a package: intelligence, self-confidence, initiative, persistence, dependability, sociability, and humor (Wrightsman 1977:661). However, people with practical experience say that they have known good leaders who were shy rather than sociable, some leaders who were cheerful and optimistic, others who were pessimistic. It is more fruitful to consider what people do rather than what they are in their inner selves.

### **Leadership Abilities and Skills**

For us, probably more useful than a search for personality traits is an examination of the skills that leaders show in their dealings with people. Some researchers seem to have paid much

more attention to the group work to be done and to the leader's goals than to the skills in directing or helping people to achieve them. For special situations and special roles, there have been explanations and directives for success. Peter Drucker has written about the skills of effective executives, and many people have sought the bases of great religious and political leaders' effectiveness (Drucker, quoted in Ames 1978:53-54; Strozier and Offer 1985). Some applied anthropologists, notably George Foster writing about change agents, have given guides based on experience (Foster 1969:115-116). Provinzano, in a case study of a strong ethnic leader in a crisis situation, has presented a list of demonstrated skills (Provinzano 1984).

Though phrased differently for the various role relationships, some common abilities do appear. One realizes that the need for specific abilities varies and that no person has all of the following: ability to learn and to analyze the situation in which one is working, both inside and outside one's group; to conduct two-way communication regarding any needed action; to plan action and to assess probabilities of the plan's success, taking into account local readiness for change; to make decisions and/or resolve differences in order to get decision agreement; to motivate people for initial acceptance and for continued personal support; to organize people or to maintain whatever organization exists; to manage the program or movement that is undertaken; and to know and accept the moral, economic, personal, or other limitations on the enterprise (although not often mentioned, this is an important qualification).

George Foster specifically stated the types of knowledge about the total situation that the anthropologist and the technical specialist need (Foster 1969:115-117). Ames has stressed consideration for maintaining what he calls the health of the organization, with attention to both its durability and its ability to change (Ames 1978:57). In a field study that I undertook, it was apparent that a dynamic person can arouse others' interest, even enthusiasm, for a problem that he has defined, and can lead them in protest; but if he cannot organize and manage the desired change, the protest will weaken and the group will fall apart (Lantis 1972:60). Although different writers have discussed different skills, one can summarize that a leader must know the essentials of organization and management. Inspiration is not sufficient.

There is a danger that the strong organizer and manager might manipulate and coerce his group, to their detriment or even death—as happened to the followers of Jim Jones in the tragedy at Jonestown. Nevertheless, in the usual circumstances, not only the goal but the practical means to it must be realized. The leader of a successful project or movement is likely to be a remarkable combination of inspiration and pragmatism.

The essential ability to communicate, about which various people have written, is actually a set of skills and techniques. The techniques can be subtle in showing an understanding of potential followers and one's identity with them, and in establishing one's own credentials for handling common tasks and purposes. Establishing believability, trust and authority in oneself is essential. At times in the relationship there is a need to dramatize and personalize the issues. Teaching also is likely to be needed. Surprisingly, teaching is not often mentioned in discussions of the components of leadership, but was included in the conclusions of a good survey of organizational leadership (Ames 1978:57).

The various messages can be conveyed by appeal to local values and by use of local symbols and stories. Sometimes imagery instead of logic must be used. An anthropologist, if he were to be a leader or an adviser to one, ought to use that technique well because of his familiarity with values, symbols and images. Communication is so important for political and religious leaders, and even for managerial, technician or art leaders, that there are analyses and manuals on the subject (see, for example, Paine 1981, a book of cross-cultural studies of rhetoric

in politics). Admittedly it is hard for anthropologists, often working in a language new to them, to be subtle, dramatic or humorous without being offensive or meaningless. Leaders must be good communicators, by some means. Therefore, one reason for attention to a local leader is to learn the local style of communication.

### Interaction Between Leader and Followers

Essayists and theorists seem to have enjoyed developing typologies of leaders, especially the great political ones, using their ways of dealing with other people as the criteria. There are psychobiographies of Revolutionary Ascetics, of Agitators, of Political Theorists, and Paranoid Dictators. As much or more has been written about the Managerial or Administrative type. In addition, there are sub-types. This is Michael Maccoby's (1981) list of corporate managers, in which we might include university, foundation, or other administrators whom we know—the Company Man, the Jungle Fighter, the Craftsman, and the Gamesman. He obviously referred to the way they operate (Ames 1978:52–53). Perhaps others would label applied anthropologists as craftsmen or, more likely, technicians. Surely we are something more, but what? The designations of *contrasting types* of leaders may help to place others and ourselves.

The pair of types that seems to have dominated the social psychological research for several years was the goal or task oriented versus the personnel or group-relations oriented leader. Is he more concerned with accomplishing the work and attaining the group's objectives, or is he more concerned with maintaining the harmony, the agreement of his group? Related to this pair is the one contrasting a controlling (or dominating?) and an egalitarian type.

The sociologist, Gunnar Myrdal, in his famous study of Blacks in the United States, described the alternatives of accommodation or protest; and recently in a collection of papers on the leadership of ethnic groups in the United States Myrdal's typology of social movements and their leaders has been explored (Higham 1979). (I am pleased to note that 12 years ago, Myrdal gave the Malinowski Address to this Society.) The basis of this pair of types is the interaction between the leader-and-group entity and its outside world, the dominant society, rather than simply interaction between leader and followers.

Of the latter type—internal like the task versus person orientation—there is an important pair of contrasting relationships, namely transaction and transformation, or some people would say “conversion.” A prominent writer on leadership, George MacGregor Burns (1978), defined these behaviors in the following way. In the transactional relation between followers and leader, they give each other what each wants, as seen most clearly in politics. Followers give votes, and perhaps money and electioneering work. The leader in response obtains a larger appropriation of funds for their schools or roads, or obtains job appointments. The satisfaction of need is specific, is likely to be individual—an individual payoff—but may be a group satisfaction.

In transformation, giving people what they desire is more complex, less likely to be specifically acknowledged, more related to ideas and ideals, less related to satisfaction of material needs. Whereas Burns assumes that transformation is a moral elevation, Kellerman states that it can be elevating or debasing, and we must agree. A gang leader can transform his followers into criminals, in one sense perhaps elevated by excitement and daring accomplishment but not morally elevated (Kellerman 1984:79–80). Possibly implied but not stated is an answer to the question, “What satisfaction does the leader get?”

Another set of three labels for leaders' orientation probably is more familiar to anthropologists, namely, traditional, progressive, and revolutionary. Anthropologists have

reported especially on traditionalists who wanted to retain or revive old customs, and on progressives who wanted to live in new ways. Revolutionaries generally have been avoided, except by the political anthropologists. These categories refer more to the objectives than to leaders' and followers' means of attaining those objectives. Accommodation, transformation or conversion refer to processes, and it is the process that we need to understand.

Perhaps one reason that people have thought there is a nearly universal leader type is that their society—and of course each other society—has an ideal role model (Antoun 1979:204). However, we suspect that the models vary from place to place and period to period. Antoun has written that one of the sources of the leader's effectiveness in personal relations is "the degree to which he approximates the cultural ideals of the group . . ." (Antoun 1979:206). If this is true, then since group ideals are likely to differ, the local leaders will differ.

In China "I-Ching," a behavior guide of 2,500 years ago, spoke of the "time of enthusiasm" when "there is at hand an eminent man who is in sympathy with the spirit of the people and acts in accord with it. Hence he finds universal and willing obedience. It is enthusiasm that enables us to install helpers for the completion of an undertaking without fear of secret opposition" (Wilhelm and Baynes 1967:Hexagram 16). This was a good ideal. Is it the same today? The sinologists can judge.

### **The Location and Legitimation of Leaders**

The relationship between people and their leader is consistent with the leader's social placement. Kurt Lewin and others after him have contrasted the leader who is fully within the group (in the center, in his terms) or on the periphery or outside it (Lewin 1948:194–197). (For a current emphasis on center placement, see Sacks 1987.) An example of the outside influential person is the publisher of an ethnic or language newspaper (Higham 1979:2–3). Regarding people who might be labeled peripheral, Makofsky has discussed place and identity of union leaders among fellow workers in El Salvador in the 1970s, leaders who are also at a middle level between the other union members and the top level of the union hierarchy (Makofsky 1978:58–59, 61). In another example, Strauch has described different "middlemen" in a Chinese community in Malaysia (Strauch 1981:164–166). A village or other group has different expectations of inside and outside persons, of lower, middle, and high people in the system, and the leaders' work must vary accordingly.

Middlemen have been labeled by various writers as "brokers," "mediators" or "hinges" between the local community and the larger political entity such as the province or nation. An example from Italy has been given by Silverman (1967:291–292). One must note that a middleman is likely to be both a follower and a leader, following—perhaps sponsored by—someone above him and leading those at a lower level.

Foster has stated the need for "go-betweens" working between specialized professionals like engineers and the public, a role that an anthropologist can play (Foster 1969:128). In the 1950s, Barnett wrote about what he called "intermediation," the anthropologist's value as a go-between (Barnett 1956:117–118). The go-between may or may not become a leader or may be a cultural interpreter for the specialist-leader and the group.

Another contrast in work, sometimes a very strong one, is seen in neighborhood or community members, not office holders, who emerge to fight some wrong or threat, such as environmental pollution, gang violence or ill treatment by a landlord. Increasingly we see such individuals who initially do not belong to any organization, although an organization may

develop and the new leader may become an office holder. At some point this individual sees that he must act beyond the neighborhood to get legal evidence of wrongdoing, publicity, money, or other outside help, and his role changes.

In another situation an office holder, seeing a misuse of power or of money in his organization, becomes a “whistleblower.” He must have courage, credible knowledge of the problem and outside help. In both cases this potential leader ultimately must be legitimized, that is, given power or authority (see Antoun 1979:205–206 for a discussion of definitions, and of sources and uses of power, authority, and influence).

Legitimation will be partially or wholly different, depending upon the identity and placement of the individual. Within his community, membership in an economically and socially strong family, or being at the center of a network, may be sufficient. At the periphery, the leader is accepted by his ethnic group or fellow workers probably because they like his personality and trust his ability to represent their needs and intentions. At the same time, outside power-holders can accept him for a different reason: because his group is large and unified. In short, the *basis* of his influence, authority or power is different in each relationship (regarding legitimation, see Hollander 1985:504). (Don’t professionals like ourselves, who are usually outsiders starting to work in an organization or a community, need to get legitimacy inside as well as outside it?)

There formerly was frequent supernatural legitimation, and it still occasionally occurs. This suggests a question that has general application but that is of special importance in Latin America today: the possible leadership roles of religious ritualists, spiritual leaders or people in church headship positions. Those who know the *cofradia* complex of positions and activities can say whether a *cofradia* ritualist is likely to be a leader in other relationships. Those who know the situations of church social or political activists can evaluate the sources of their legitimacy. Are they accepted because of religious authority, personal qualities, or support by factions of the national society? People in traditional religious roles and people in new roles more or less related to religious position deserve the attention of anyone interested in cultural processes.

In the modern “information society,” Cleveland has written that the administrator-leader, because of the great amount and specialized variety of information to be used and the diversity of relationships today, *must be a generalist*. “The generalists may start as scientists or Masters of Business Administration or . . . union organizers or civil servants or mobilizers of feminists or ethnic groups, or citizen advocates of a particular cause. They may be managers who . . . know how to ‘lead while being led.’” Executive leaders “pursue a wide variety of goals and purposes, and operate in many modes—in federal, state, and local bureaus, in big corporations, in small businesses, in academic settings, in non-profit agencies ranging from the EXXON Education Foundation to Alcoholics Anonymous. But each . . . is ultimately responsible . . . to people-in-general” (Cleveland 1985:5–6).

## The Followers

In various contexts, I have referred to the relationship between a leader and his following – “his people.” The followers deserve much more attention than social scientists have given them or than I can give them here. One reason for the meagerness of publication is that there seems to have been little research compared with the large speculative and research literature on leaders, who are the attention grabbers. Any group of followers of course multiplies the complexity of the relationship, for there are as many interactions as the number of involved persons. Nevertheless, sociologists and psychologists began to realize that they had a one-sided picture of the process in

which their subjects functioned, as Goldenson has written in his *Encyclopedia of Human Behavior*:

When they observed different groups in action, they found that the leader did not always initiate activities by himself, and that his effectiveness depended as much on the structure of the group as on his own personal qualities. . . . As Tannenbaum and Massarik (1957) point out, ‘The *personality of the follower* (as it manifests itself in a given situation) becomes a key variable with which the leader must deal. The needs, attitudes, values, and feelings of the follower determine the kinds of stimuli [from] the leader to which the follower will respond’ (Goldenson 1970:678–679).

A story was told about Mahatma Gandhi. One day he saw a crowd of people coming along the street. As he hurried out, he said, “I must see where they are going. I am their leader!”

A former U.S. President, Harry Truman, in an oft-quoted statement said: “A leader is a man who has the ability to get other people to do what they don’t want to do, and like it.” This statement fails to tell *how* he gets his people to do what they have not wanted to do and even to like it. Since the true leader depends principally on influence instead of power, he is to the same extent dependent on his followers’ free response. He must sense how to appeal to them. In either transaction or transformation, Truman’s leader must offer something that the people do want. In transaction, he probably offers some present satisfaction. In conversion of attitude, he is likely to offer a future satisfaction. In either case, the public response will be the decision.

Let us say that the people are angry and want violent protest. It will be very difficult to convince them of the future benefit of being accommodating today. Yet the age, past experience, ethnic or class identity and other group characteristics will determine what argument they accept: a statement of all the risks of the action, or the feasibility and probable effectiveness of an alternative plan, or the example of successful past action. As a political scientist has said, “Those to be led may be ignorant, educated, emotional, rational, or they may vary in the symbols, myths, and fantasies to which they are responsive. They may define goals or means to goals in a disparate fashion” (Ulmer 1986:2).

In an attempt to outline the psychosocial and/or economic motives for followers’ acceptance of a leader—actually the various individual bases of the interaction process—I have assembled this list:

- Identification with the leader, adopting his attitudes and behavior in response to his personal magnetism.
- Seeing the leader as one’s representative, even as oneself speaking and acting.
- Acceptance of a general moral appeal that gives an image of good, either personal or societal.
- Acceptance of the promise of a path to salvation, with avoidance of or with recovery from loss or punishment.
- Seeing in the leader the qualities that one wants to see, the response that one needs, such as gratitude, appreciation, wisdom, or strength.
- Transference from early relationships: parent and child or master/teacher and student.
- Socio-cultural (traditional) acceptance of authority.



Indebtedness, possibly with fear of the person to whom something is owed.  
 Rational choice of offered program, because of reasonable objective value, social,  
 economic, or political.  
 Self-actualization in increased responsibility given to the follower.  
 Curiosity, experimentation with the new and different.  
 Hope of financial or status reward.  
 Response to peer pressure in the follower group, i.e., the desire to belong.  
 Need for physical or economic protection from danger, from repression.  
 Desire for basic social or political change: a revolution.

Kellerman has noted that there is another type of explanation, a biobehavioral one: a general motivation of followers, namely, that human groups are programmed for dominance-deference hierarchies (Kellerman 1984:84). I leave it to other anthropologists to evaluate that theory.

In the 1970s psychologists presented the “complex man,” the worker-follower whose needs and potential vary, since each person has his own hierarchy of motives. Attitudes can differ in different situations even within an organization. The person’s motivation and the “psychological contract” that he establishes with the organization—or the leader—is the result of a complex comprising his needs and his experience in the organization or community or other social unit. The manager or leader, like the worker/follower, can show any of these motivations, being also a complex person (Wrightsman 1977:654–660).

A question still being studied by the psychologists concerns the followers’ perception of the manager/leader, that is, how they define and rate him (Wrightsman 1977:649–650, 654). Their behavior as a follower is, of course, affected by their *perception* of his ability to understand their situation and respond to their motives. A follower’s relationship to a leader can be strongly emotional and committed (Kellerman 1984:83). Other followers in the group may be merely compliant, casual or tentative. This may indicate not only a difference in emotional need and in expectations, but also in the particular way the individual sees and evaluates the leader.

A list of a leader’s motives comparable to the list for followers was not prepared as I wanted to draw special attention to the total situation and to the potential followers. I have asked, in effect, how the leader will work with them, by referring to his abilities and skills and asking what is his strongest orientation? Is his mission to change society regardless of the effect on individuals? Or to hold together his constituency and obtain recognition for it? Or to do his assigned job and thereby move himself up in the hierarchy? These are the basic questions for the followers to ask. In short, what is he really asking them to do? Then, how do they react?

If the followers are not starving or facing a gun, they can guide the leader because they have alternatives. As a group they can passively accept his interpretation of their needs and his proposal for action. Or they can acclaim his leadership and give it the strength of mass support. Or they can refuse to agree or to act. The fourth alternative is often the best response. The people listen and accept that part of the proposal that they think is best in their situation, responding with a guiding message to the leader. If he is not a fanatic or a dictator, he probably will see the wisdom of agreeing with his followers, at least in part. Thus they can give *selective reinforcement* to any individuals who make different appeals and offers to them. As one person has written, for example, “An ethnic group, if it is to survive, finds, or is given, or develops the leaders it needs” (Cross 1979:195). His example is the Americans of Irish origin, themselves various, who have produced a remarkable variety of outstanding leaders, among them rogues and saints.

There are limits to any leader's power or influence because there is almost always a limit to the behavior that the followers will *knowingly accept*. The problem is that they may not understand or may refuse to believe what is known about the leader(s). Popularly chosen persons often get themselves and their people into serious trouble when they demand complete loyalty. Most people consider loyalty a virtue, but when it conflicts with common sense and/or ethics, both sides in the relationship must abandon loyalty. Then, which side retreats first? Usually the follower group, containing various viewpoints, can make the decision, perhaps breaking into factions. Sometimes, however, people simply do not see and do not understand what is happening and what is needed in either selection of a leader or abandonment of one.

Community workers, political anthropologists, other socio-behavioral scientists, and especially people in education must help to develop a more discerning public—a public that understands the principles, the functions, the risks and rewards of the system in which they must live and work. An incipient movement for change needs shrewd, knowledgeable followers as much as it needs strong leaders. Instead of a conference or workshop to develop leaders, how about education for followers, to keep them from being dupes of a good talker? Educators often think that they are preparing leaders. If they are smart, they will principally educate critical followers.

We can help in a more immediate way. As the social psychologists state, leader and followers act together to do a job, to accomplish a communal task, achieve a stated goal. Since blind trust in a leader to accomplish this is very dangerous, everyone involved must know enough to evaluate the means, the method toward the common goal. In an institution or a community, we—if we are really involved in the situation—can learn what the people already know, understand and want regarding the common problem. Then Edmund Burke's statement regarding legislative leaders, quoted by Woodrow Wilson in an 1890 lecture on leaders, can be applied to any of us whose work relates to making decisions that affect the public: “‘to follow, not to force, the public inclination—to give a direction, a form, a technical dress, and a specific sanction to the general sense of the community, is the true end of legislation’.” (Wilson 1952:45). In a changing society, getting and communicating in two directions any needed information and understanding is not easy but is one of our valuable services.

Social psychologists have not only done the research on leadership, they have developed theories of it: Social Exchange Theory; Vertical Dyad Linkage Theory; Path-Goal Theory of Leadership; Situational Theory of Leadership; Expectancy Theory; Adaptive-Reactive Theory. There are also models: multiple linkage model of leader effectiveness and role model of leader behavior determinants. Some theories intend to explain why the leader acts in a particular way, others to explain the followers' reactions (Yukl 1981:Chapter 7). On any major subject, anthropologists seem to have a number of concepts but very few theories. The psychologists may have been more influenced by the natural and physical sciences. Aren't theories supposed to explain the relations of facts and/or the principles of action? If so, they are needed. Perhaps the psychologists are too quick to state theories and the anthropologists too slow.

## **The Situation**

The third important element in the role interaction is the situation in which it occurs. Social psychologists have stated the need to understand the effect of context on people's relationships, but people's actual situations are usually complex and greatly variable. It takes a long time to identify and to vary realistically all the factors in only one type of interactive behavior.

Even in a clearly defined and controlled organization, an industrial workplace for example, the size of the business, the stage in its development, its financial health, the encompassing economic trend, the personal dynamics within the business, and other factors affect manager-leader's and worker-followers' demands of each other. In a comparable office of a bureaucracy, the legislated regulations, status hierarchy and protocol, number of employees, any contemplated change of organization or of pay rates can strongly affect interactions within the office. In a nation, there is a totality of more leaders and followers in these so-called routine situations than in a mass movement.

In the more open and even more variable context of the community or the general public, it has been said in answer to the "hero in history" concept that "leaders emerge as the result of task, time, place, and circumstance" (Kellerman 1984:73). It is understood that "place" and "circumstance" cover a lot. Further, a leader is likely to make a difference when: (1) the environment is favorable (there can be change, movement); (2) he is strategically located in the group or in the larger society; and (3) his particular skills (and personal qualities) meet the requirements of the particular situation (Greenstein formulation of leader's effect, paraphrased by Kellerman 1984:74).

As social scientists, readers probably are thinking, "Well, of course!" Most other people, however, are more likely to emphasize individuals instead of the milieu. And don't we all tend to personalize? For other purposes than understanding people's reactions to political or other leaders, we anthropologists do study what can be labeled "the situation." The complexity of modern society in any part of the world has made difficult the research of a specific village's or other group's situation relative to larger economic and political conditions. For example, different political groups' efforts to control the technology and economic development can seriously affect the villages. Yet development specialists do look at this complexity, and may note the roles of known individuals. If it is possible to do, in one's reports an explanation of the opportunities and limitations provided specifically by all three elements—leaders, the public, and their situation—would be a big contribution to social and behavioral sciences besides helping in the local work.

After this nod to fieldwork, let us turn briefly to theory. When Fred Fiedler presented his "contingency theory" to psychologists, there was a flood of research by himself and others, and in the journals there were criticisms of the theory and rebuttals. In its simplest essential, the contingency theory states that leadership is contingent upon the situation in which it occurs. Or, substituting a few words, one can say that the leader-and-follower interaction is *dependent upon the circumstances* in which it occurs. This presents a *situational relativism* in psychology comparable to *cultural relativism* in anthropology. We should welcome it, shouldn't we? I think that we should. (Regarding importance of the situation, see Hollander 1985:495–497.)

In applied anthropology work, we learn that more than culture is involved in a group's situation or circumstance. One must consider demographics, income or other support, dominant personalities and factional conflict, and ecological changes. (Even though culture may be a factor in all of these, the evident realities are expressed in other terms.) As anthropologists in the past 20 years have paid more attention to individuals and their roles in society (Lantis 1982), in the same period the psychologists have paid more attention to groups and their situations. We can help each other.

## Conclusions

It is not possible here to cover other interesting elements of leadership, especially two: functions and styles. Possible functions that one should keep in mind are the constitutive (giving identity to the group); the catalytic (bringing together the necessary elements to initiate action, that is, activating); the avoidance of extremes (maintaining balance, combining tradition and modernity, for example); and finally, the understanding and use of the mechanisms of organization (such as setting criteria of work or assigning personnel).

There have been serious studies of style (Hollander 1985:513,515–516), always a difficult subject to depersonalize, as well as satirical characterizations of leadership style. A list of metaphors of leadership has also been published: “batting a thousand,” “the lone ranger,” “the chosen one,” “the office makes the man,” “power corrupts,” “wisdom versus expertise” (Dubin 1979). All of this attention is evidence of the importance and the common presence of leaders with whom the people-in-general must deal.

There should also be a section on studies of women leaders and others’ response to them. Such literature exists although it is not extensive (Hollander 1983:519–523).

We need to consider again the “big man” concept in anthropology: the great hunter, the shrewdest trader, the biggest landowner or owner of the most camels and the functions of each in society, including leadership. I hope that each anthropologist can combine two such concepts as needed.

Anthropologists can benefit from an understanding of specific leaders and also of leadership in general. Most of the effective leaders are action-oriented, either for or against a given action, and applied scientists are, almost by definition, action oriented.

A true leader usually is proposing a change of course, either backward or forward. His justification of the change and his way of communicating and motivating can be very instructive because the applied social/behavior scientist also is usually involved in change. At the same time, those leaders who want only the status quo cannot be disregarded. In every community, governing agency, institution and field of work, there is likely to be an active, past or rising leader who might be useful, not only for his ideas but for his influence. It is wise to understand him and his situation well enough to decide whether to court him or avoid him.

At the same time one does not neglect the actual or potential followers. Every experienced field worker knows this. Then why not deal in one’s reports specifically with these elements, these relationships and their functions? Some anthropologists are doing that, and we can be grateful.

Anthropologists often are expected to be consultants, resource persons, or research planners for the director-leader unit in an organization—possibly for a housing or other neighborhood program. The question is how much do they know about the potential followers of the program and their situation? The anthropologist’s best contributions are:

- (1) To help make the potential actors the actual group actors.
- (2) To make them more perceptive, more realistic regarding any proposed action, so that *they* ask and understand, “What is needed? What can be done? What are the costs and benefits?”
- (3) To learn from people’s attitudes toward local group leaders whether the people distinguish between substance and symbol and distinguish between scoundrels and scapegoats.
- (4) To learn the real basis of accepted leaders’ power or influence.
- (5) Where there are contending factions, to avoid alliance with any while learning the basis of their opposition.

All this is really fundamentally learning the situation and the actors in it.

In a broader, less specific situation involving a general emotional commitment, what is the nature of the people's investment in the leader and his undertaking? What will happen after the decline or loss of the leader?

It is being said now, even in the public media, that in the United States and perhaps in other Western nations individualism has gone too far, and responsibility to the community is needed. The leader figure seems to epitomize the effective individual. Yet we have seen that, except for the person who uses only military or economic power, a leader must appeal to and rely upon a good number of others. It must be a common group process. Though leaders are needed and good ones are appreciated, they should not be glorified except in rare crises. They should be understood and constantly evaluated, especially the new ones. Let us not concentrate on the individual, but let us pay particular attention to the relationship, the motivations and reactions of the people toward their leaders. This will help us understand the situation in which we may be working.

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#### References

- Ames, ArDee  
1978 Management Improvement: Some Thoughts on Large Organization Leadership. *The Bureaucrat* (Fall):50-57.
- Antoun, Richard T.  
1979 Low Key Politics. New York: State University of New York Press.
- Argyris, Chris  
1979 How Normal Methodology Makes Leadership Research Less Additive and Less Applicable. *In* *Crosscurrents in Leadership*. J. G. Hunt and L. L. Larson, eds. Carbondale: Southern Illinois Press.
- Barnett, Homer G.  
1956 Anthropology in Administration. Evanston, IL: Row, Peterson.
- Basehart, Harry W.  
1970 Mescalero Apache Band Organization and Leadership. *Southwest Journal of Anthropology* 26(1):87-106.
- Bathory, Peter D.  
1978 Leadership in America: Consensus, Corruption, and Charisma. New York: Longman Inc.
- Brian, Gerald M., and Ronald Cohen (eds.)  
1980 Hierarchy and Society. Philadelphia: Institute for the Study of Human Issues.
- Burack, Elmer R.  
1979 Leadership Findings and Applications: The Viewpoints of Power from the Real World. *In* *Crosscurrents in Leadership*. J.G. Hunt and L. L. Larson, eds. Carbondale: Southern Illinois Press.

- Burns, James MacGregor  
1978 Leadership. New York: Harper and Row.
- Carbonell, Joyce L.  
1984 Sex Roles and Leadership Revisited. *Journal of Applied Psychology* 69(1):44–49.
- Cleveland, Harlan  
1985 The Knowledge Executive: Leadership in an Information Society. New York: E. P. Dutton.
- Cornelius, Wayne A.  
1977 Leaders, Followers and Official Patrons in Urban Mexico. *In* Friends, Followers, and Factions. Steffen W. Schmidt et al., eds. Berkeley: University of California Press.
- Cross, Robert D.  
1979 The Irish. *In* Ethnic Leadership in America. John Higham, ed. Baltimore, MD: Johns Hopkins University Press.
- Diaz-Guerrero, Rogelio (ed.)  
1985 Cross-cultural and National Studies in Social Psychology. Proceedings of the 23<sup>rd</sup> International Congress of Psychology, Vol. 2. The International Union of Psychological Science, Mexico, 1984.
- Dubin, Robert  
1979 Metaphors of Leadership. *In* Crosscurrents in Leadership. J. G. Hunt and L. L. Larson, eds. Carbondale: Southern Illinois Press.
- Easton, David  
1979 A Systems Analysis of Political Life. Berkeley, CA: University of California Press.
- Fabian, Johannes  
1969 Charisma and Cultural Change: The Case of the Jamaa Movement in Katanga (Congo Republic). *Comparative Studies in Society and History* 11(2):155–173.
- Foster, George M.  
1969 Applied Anthropology. Boston: Little, Brown.
- Gibb, Cecil A.  
1966 The Principles and Traits of Leadership. *In* Small Groups: Studies in Social Interaction. A. Paul Hare, ed. New York: Alfred A. Knopf.  
1969 Leadership. *In* The Handbook of Social Psychology, 2nd ed., Vol. 4. G. Lindzey and E. Aronson, eds. Pp. 205–282. Reading, MA: Addison-Wesley.
- Goldenson, Robert M.  
1970 The Encyclopedia of Human Behavior: Psychology, Psychiatry, and Mental Health, 2 vols. Garden City, NY: Doubleday.
- Hatanaka, Suchiko  
1972 Leadership and Socio-economic Change in Sinasina, New Guinea Highlands. New Guinea Research Bulletin No. 45. Port Moresby: Australian National University.
- Heller, Trudy  
1982 Women and Men as Leaders: In Business, Educational and Social Service Organizations. New York: Praeger Publications.
- Higham, John, ed.  
1979 Ethnic Leadership in America. Baltimore: Johns Hopkins University Press.

- Hogbin, Herbert I.  
1978    *The Leaders and the Led: Social Control in Wogeo, New Guinea*. Melbourne University Press.
- Hollander, Edwin P.  
1983    *Women and Leadership*. In *Small Groups and Social Interaction*, Vol. 1. H. H. Blumberg et al., eds. London and New York: Wiley.  
1985    *Leadership and Power*. In *The Handbook of Social Psychology*, Vol. 2. Gardner Lindzey and Elliot Aronson, eds. New York: Random House.
- Human Relations Area Files  
1972    *Social Relations and Groups*, File No. 571. New Haven, CT: Yale University.
- Hunt, J.G., and L. L. Larson, eds.  
1979    *Crosscurrents in Leadership*. Carbondale: Southern Illinois Press.
- Hunt, James G., Uma Sekaran, and Chester A. Schriesheim, eds.  
1982    *Leadership: Beyond Establishment Views*. Carbondale: Southern Illinois Press.
- Izrael, Dafne, and Dove Izrael  
1985    *Sex Effects in Evaluating Leaders: A Replication Study*. *Journal of Applied Psychology* 70(3):340–346.
- Kaufman, Herbert  
1985    *Time, Chance, and Organizations: Natural Selection in a Perilous Environment*. New Jersey: Chatham House.
- Kellerman, Barbara, ed.  
1984    *Leadership: Multidisciplinary Perspectives*. Englewood Cliffs, NJ: Prentice-Hall.
- Kenny, David A., and Stephen J. Zaccaro  
1984    *An Estimate of Variance Due to Traits in Leadership*. *Journal of Applied Psychology* 68(4):678–683.
- Lantis, Margaret L.  
1972    *Factionalism and Leadership: A Case Study of Nunivak Island*. *Arctic Anthropology* 9(1):43–65.  
1982    *Evolution and Resolution: The Personal in American Anthropology*. Paper presented at the annual meeting of the American Anthropological Association (mimeo).
- Lewin, Kurt  
1948    *Resolving Social Conflicts. Selected Papers on Group Dynamics*, New York: Harper and Row.
- Lowry, Ritchie P.  
1965    *Who's Running This Town? Community Leadership and Social Change*. New York: Harper and Row.
- Loy, John W., Jr.  
1969    *Social Psychological Characteristics of Innovators*. *American Sociological Review* 34(1):73–82.
- Maccoby, Michael  
1981    *The Leader: A New Face for American Management*. New York: Simon and Schuster.
- Makofsky, Abraham  
1978    *Voluntary Association in a Climate of Repression: Union Activists in El Salvador*. *Human Organization* 37(1):57–63.

- McBeath, G. A.  
1978 Political Behavior of Ethnic Leaders. *Comparative Politics* 10:393–417.
- Merton, Robert  
1957 *Social Theory and Social Structure*. Glencoe, IL: Free Press.
- Nader, Laura  
1969 Up the Anthropologist—Perspectives Gained from Studying Up. *In Reinventing Anthropology*. Dell Hymes, ed. New York: Random House.
- Nayacakalou, R. R.  
1975 *Leadership in Fiji*. London: Oxford University Press.
- Ottenberg, Simon  
1971 *Leadership and Authority in an African Society: The Afikpo Village Group*. American Ethnological Society Monograph 52. Seattle: University of Washington Press.
- Paine, Robert, ed.  
1981 *Politically Speaking: Cross-cultural Studies of Rhetoric*. Philadelphia: Institute for the Study of Human Issues.
- Pareek, Udai, ed.  
1966 *Studies in Rural Leadership*. Delhi: Behavioural Sciences Center.
- Park, George  
1981 “Weaklings Wax Biggest with Words.” *In Politically Speaking*. Robert Paine, ed. Institute for the Study of Human Issues.
- Provinzano, James  
1984 *A Model of Leadership: A Working Paper*. Presented at the annual meeting of the American Anthropological Association (manuscript).
- Raine, David F.  
1971 The Eskimo Bosses. *The Beaver* 302(1):30–35.
- Rice, Robert M., Debra Instone, and Jerome Adams  
1984 Leader Sex, Leader Success, and Leadership Process in Two Field Studies. *Journal of Applied Psychology* 69(1):12–31.
- Sacks, Karen B.  
1987 *Caring by the Hour*. Urbana: University of Illinois Press.
- Sady, Rachel Reese  
1985 *District Leaders* (manuscript).
- Santora, Joseph C.  
1983 *Leadership: A Selected Guide to Periodicals, 1972–1982*. Public Administration Series: Bibliography Pt 190. Monticello, IL: Vance Bibliographies.
- Schmidt, Steffen W., et al., eds.  
1977 *Friends, Followers, and Factions: A Reader in Political Clientalism*. Berkeley: University of California Press.
- Shils, E.  
1958 The Concentration and Dispersion of Charisma: Their Bearing on Economic Policy in Underdeveloped Countries. *World Politics* 11:1–19.



- Shultz, George P.  
1975    Leaders and Followers in an Age of Ambiguity. New York: New York University Press.
- Silverman, Sydel F.  
1967    The Community-Nation Mediator in Traditional Central Italy. *In* Peasant Society, Reader. Jack M. Potter et al., eds. Boston: Little, Brown.
- Singer, Henry A.  
1976    Human Values and Leadership: A Ten-Year Study of Administrators in Large Organizations. *Human Organization* 35(1):83–87.
- Sinha, Durganand  
1985    Psychology in Rural Areas: The Case of a Developing Country. *In* Cross-cultural and National Studies in Social Psychology. Rogelio Diaz-Guerrero, ed. Proceedings of the 23<sup>rd</sup> International Congress of Psychology, Vol. 1. Mexico.
- Skinner, George W.  
1958    Leadership and Power in the Chinese Community of Thailand. Association for Asian Studies Monograph 3. Ithaca, NY: Cornell University Press.
- Strauch, Judith  
1981    Chinese Village Politics in the Malaysian State. Cambridge, MA: Harvard University Press.
- Strozier, Charles B., and Daniel Offer, eds.  
1985    The Leader: Psychohistorical Essays. New York: Plenum Press.
- Taifel, Henry, ed.  
1984    The Social Dimension: European Developments in Social Psychology, 2 vols. Cambridge, U.K.: Cambridge University Press.
- Thomas, I.  
1981    The Socioeconomic Determinants of Political Leadership in a Tojolabal Maya Community. *American Ethnologist* 8:127–138.
- Tucker, Robert C.  
1968    The Theory of Charismatic Leadership. *Daedalus* 97:731–756.
- Ulmer, S. Sidney  
1986    Leadership Is Result of Complex Equation. *Ampersand* (Fall):1–3. University of Kentucky.
- Wall, James  
1987    Bosses. San Diego, CA: Lexington Books.
- Weber, Max  
1947    Theory of Social and Economic Organization. Talcott Parsons, ed. and trans. New York: Oxford University Press.
- Whyte, Martin King  
1980    Bureaucracy and Antibureaucracy in China. *In* Hierarchy and Society. Gerald M. Britan and Ronald Cohen, eds. Philadelphia: Institute for the Study of Human Issues.
- Wildavsky, Aaron  
1964    Leadership in a Small Town. Totowa, NJ: Bedminster Press.

- Wilhelm, Richard, and Cary F. Baynes, translators  
1967 The I-Ching, Book of Changes (3rd ed.). Princeton, NJ: Princeton University Press.
- Wilson, Woodrow  
1952 Leaders of Men [An address, 1890]. T. H. Vail Motter, ed. Princeton, NJ: Princeton University Press.
- Wrightsman, Lawrence S.  
1977 Social Psychology (2nd ed.) Monterey, CA: Brooks/Cole.
- Yukl, Gary A.  
1981 Leadership in Organizations. Englewood Cliffs, NJ: Prentice-Hall.

## **Chapter 18**

### **Frederick L. W. Richardson: Industrial and Organizational Research**

**Thomas Weaver**

Frederick Richardson, recipient of the Society for Applied Anthropology's 1988 Malinowski Award in 1988, took research and consulting in business management to a different level.<sup>1</sup> His contributions include the use of participant observation, quantitative measures of interactional processes, and the unique assessment of the historical and ethological basis of communication skills. Richardson made numerous studies on corporations, hospitals, and leaders testing these methods and the resulting hypotheses. One of the elements missing in recent organizational culture studies is a linkage to the rich, earlier industrial studies conducted by investigators like Richardson.

Fred Richardson (1909–1988) is one of four Harvard Ph.D.s to have received the Malinowski Award by 2000. Because he was handicapped by severe dyslexia, his parents selected alternate educational routes including a school in Switzerland and travel with them to the Middle East. This stimulated his interest in geology, archaeology, and the beginnings of civilization. He spent several years touring early cities for the British Museum, the University of Pennsylvania Museum, and the Chicago Oriental Institute. After receiving a bachelor's degree in geology in 1931, he shifted to industrial and applied anthropology, in which he received a doctorate from Harvard in 1941. His years at Harvard coincided with the interdisciplinary work of Elton Mayo and W. Lloyd Warner in industrialism (Richardson 1961, 1979). His dissertation work was with an Appalachian coal mining resettlement project run by the American Friends Service Committee.

As for others of his generation, World War II provided Richardson with opportunities for employment and for the development of applied anthropology. Richardson's main job was with Middle East intelligence, but he also had time to publish on food production in third world countries. He consulted with the Department of Agriculture and in Cairo, out of which came a world congress in Washington to encourage third world food production for the Allied Armed Forces (Richardson 2002). After the war Richardson spent 33 years in organizational research, consulting, and teaching. He taught at the Labor Management Center at Yale, the Harvard School of Public Health, and the graduate business schools of the University of Pittsburgh and the University of Virginia. Seven of these years were spent as an independent consultant for such business corporations as Ford, IBM, Westinghouse, Gulf, and Raytheon. He made studies of about ten hospitals, including the Massachusetts General Hospital. At the Harvard School of

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Public Health he was the senior social scientist for five years in a pioneer study of community mental health.

Due to Richardson's ill health his Malinowski Award address was not published. A paper he sent to the Society for Applied Anthropology when he was nominated for the Malinowski Award is published here in lieu of his address. In this document, Richardson outlined the trajectory of his career and discussed strategies used in his research on industrialism and human interaction. This paper, he wrote, "forms the focus for the book I am writing and the talk next spring"—that is, the Malinowski Award address (2002).

In a paper published just before his address (1987), and in the one published here, Richardson covered the topic of leadership, as Margaret Lantis had in her Malinowski Award presentation (Lantis 1987) the previous year and as Conrad Arensberg was to do in his Malinowski Award presentation in 1991 (Weaver 2002a). Richardson addressed communication from the perspective of evolution, ethology, and physiology, all the while relating them to business and corporate needs. He began his analysis by indicating four research strategies for analyzing and transforming adversarial into collaborative relationships in business: participant observation, the quantification of interaction processes, history, and ethology. The use of participant observation was developed by W. Lloyd Warner and his students and collaborators, including Richardson and Conrad Arensberg (Richardson 1979, 1987:100–103). Quantification as a management research tool was pioneered by Eliot Chapple, Arensberg, and Richardson (Richardson 1987:103–107). The ethological approach calls on the biological and physiological training received by anthropologists (Richardson 1987:107–122).

Richardson suggested that communication skills are deeply rooted in the development of the mammalian brain, and he worked with the hypothesis that gradations in contact and degree of friendly to hostile exchange are closely related. His review of studies of bands of monkeys, apes, lions, wolves, and other species found support for the hypothesis. Richardson summarized his findings as follows:

In the evolution from amphibians to primitive long-living mammals to man, there have been an increasing frequency and duration of interaction to the point that organizational leaders and managers, for example, devote from about one-half to nine-tenths of their working day interacting with others. . . . Such interactional stimulation, and in particular pleasurable interchange, has reached the point that it has apparently become essential for human health and well-being (1987:117).

Richardson, in collaboration with fellow anthropologist Donald Kennedy, conducted a study that provided statistical validity for his hypothesis that productivity of a work group is closely dependent on contact frequency, duration, and number of pair and group contacts of one minute or longer between each subordinate and manager (Richardson 2002). This followed upon Chapple's earlier observation that it was the timing and duration of interaction, measured during stress interviews, and not the content of the verbal exchange that differentiated good from poor salespersons and managers.

Richardson was one of the founders of the Society for Applied Anthropology, along with fellow Malinowski Award winners Conrad Arensberg (Weaver 2002a), Laura Thompson (Thompson 1979), and Lauriston Sharp (Weaver 2002b), and an early president of the society.

### Notes

<sup>1</sup>This annual award is given to a senior colleague in recognition of efforts to understand and serve the needs of the world through social science. See Thomas Weaver's "The Malinowski Award and the History of Applied Anthropology" (2002d) for a brief history of the award and an overview of the recipients and their work, and Weaver's "Malinowski as Applied Anthropologist" (2002c) for an introduction to Bronislaw Malinowski and his work.

### References Cited

Lantis, Margaret

- 1987 Two Important Roles in Organizations and Communities. *Human Organization* 46(3):189–199. (Republished at <<http://www.sfaa.net/>>.)

Richardson, Frederick L.W.

- 1961 Talk, Work, and Action. Society for Applied Anthropology Monograph Series, No. 3. Ithaca: Society for Applied Anthropology.
- 1979 Social Interaction and Industrial Anthropology. *In The Uses of Anthropology*. W. Goldschmidt, ed. Pp. 79–99. Special Publication Number 11. Washington, D.C.: American Anthropological Association.
- 1987 The Elusive Nature of Cooperation and Leadership: Discovering a Primitive Process that Regulates Human Behavior. *In Applied Anthropology in America*. Second Edition, E. M. Eddy and W. L. Partridge, editors. Pp. 97–122. New York: Columbia University.
- 2002 Career Development, Strategies, Discoveries, and Turning Points. *In The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers*. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.

Thompson, Laura

- 1979 The Challenge of Applied Anthropology. *Human Organization* 38(2):114–119. (Republished at <<http://www.sfaa.net/>>.)

Weaver, Thomas

- 2002a Conrad Arensberg: Economic and Industrial Anthropologist. *In The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers*. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.
- 2002b Lauriston Sharp: Innovator of Applied Anthropology Programs. *In The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers*. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.
- 2002c Malinowski as Applied Anthropologist. *In The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers*. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.
- 2002d The Malinowski Award and the History of Applied Anthropology. *In The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers*. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.

## **Career Development, Strategies, Discoveries and Turning Points**

**F. L. W. Richardson**

### **A. MY PRE-COLLEGE OUTLOOK, INTERESTS, AND EDUCATION**

I was born in 1909 into a large and affluent family of seven siblings with unusual parents.<sup>1</sup> One was outgoing and gregarious, the other reflective and creative.

The former saw to it that all in the family would regularly associate with a wide variety of persons representing different nations, professions, incomes, and ages. The latter often questioned conventional thinking—challenging us, from time to time to keep an open mind about “way out” ideas, some of which seemed to us absurd.

At eight years old, under the influence of a remarkable old-fashioned spinster science teacher, I became absorbed in Natural History and commenced my field work career at an early age. My main interest was bird-watching which I continued thereafter into my college years but always careful to hide the same from most of my friends and age group who would have sneered.

Unrecognized at the time, I was a severe dyslexic - slow and poor at my lessons. But what now is generally considered a disability was to me a blessing in disguise because I developed a keen interest in direct, firsthand observation which I did well. Hence, at a very early age I began my field work career discovering the real world—not primarily relying on fiction or hearsay of which the social and psychological sciences have an excessive share.

When 16 years old I was unable to advance into the next grade. Considering this an opportunity rather than a misfortune, my parents sent me to school in Lausanne, Switzerland rather than repeat the same grade at home. There I enjoyed one of my best years. Amazingly enough it turned out to be one of the highest points in my life because, although only a moderately good ice hockey player at home, I was a star on the Lausanne hockey team and even was recruited as a ringer for a pseudo French-German Olympic game where I scored the only goal for the French!

This European experience for a 16–17 year old was also critical for my future career because unexpectedly one of my parents arranged for me to join them during Easter holidays on a Mediterranean cruise to the Middle East, then called the Near East. In Istanbul, then called Constantinople, and in Jerusalem and Cairo, I became entranced with the Moslem World, especially their art and architecture. This in turn led to an early interest in the beginnings of civilization in the Fertile Crescent ranging from the Persian Gulf up the Tigris and Euphrates and down through Palestine, now Israel, to Egypt and the Nile.

### **B. HARVARD UNDERGRADUATE AND EARLY GRADUATE YEARS (1927–1936)**

I concentrated in Geology where I came under the influence of one of those rare teachers who takes a personal interest in many of his students. I devoted three undergraduate summers to field work in surface or recent geology which I was good at, and as the volume of written material I had to read was not great, I did well. This experience was invaluable in that it

thoroughly familiarized me with a natural field science, in striking contrast to some pseudo-science practices common in the social and psychological disciplines.

Paralleling my geology, I maintained my interest in the beginnings of civilization. After graduation therefore I went on field trips for two years to Mesopotamia. The first year I surveyed the Sumerian city of Ur for the British and University of Pennsylvania Museums where I also developed an informal familiarity with the Marsh Arabs. The second year was at an Assyrian site run by Chicago's Oriental Institute where I also developed friendly relations with Kurds, Nestorian Christians, and Devil Worshippers or Yazidis.

By this time I had decided to become an anthropo-geologist. As a result I devoted several months in the spring of 1933 surveying the ancient irrigation systems of southern Iraq and the conformity of land and water at the head of the Persian Gulf. And in 1934, as a member of the Harvard-Irish expedition, I devoted a month's geological field work to a microlithic site in Northern Ireland which was published by the Royal Irish Academy.

As the Great Depression was well along and financing for archaeology was on the downswing, I decided that specializing in archaeology and recent geology was too risky. I therefore expanded my field to include Anthropology and Human Geography. This in turn resulted in a doctorate at Harvard and a radical shift to industrial and Applied Anthropology.

During this phase of my career, I wish to point out the most significant educational truth I learned. This wisdom was developed in the course "Philosophy of Science" by the eminent philosopher, Alfred North Whitehead and may be paraphrased as follows: There are two paths to truth—Science is one, Intuition the other, and it is a wise person who knows when to rely on which.

### C. LATE GRADUATE SCHOOL, APPLIED ANTHROPOLOGY, AND WORLD WAR II (1937–1945)

The 1930s was an exciting and intellectually broadening time to have been at Harvard where the President stressed the development of young scholars and a focus on problems rather than disciplines. The psychologist, Elton Mayo, and the anthropologist, Lloyd Warner, were collaborating on long-term projects. One was at the Harvard Business School to pioneer a new discipline, namely Human Relations in Business and other large organizations. Here also were Eliot Chapple and Conrad Arensberg, anthropologists, developing their revolutionary interactional observations and theory.

It was also a time of widespread realization that much was amiss in the Western World. Numerous intellectuals and statesmen, including some business leaders and others, were searching for a new way. I was one of this group breaking with my conventional geographical-anthropological past. I joined the researching physiologists, psychologists, and anthropologists at the Harvard Business School where, in addition, we had frequent exposure to business leaders and other natural scientists.

It was this breakaway environment that encouraged a number of us young upstart "turks" to found the Society for Applied Anthropology. It also helped launch me on my doctoral field work living in and studying an Appalachian coal mining resettlement project run by the American Friends Service Committee.

This breakaway environment continued into World War II particularly for enterprising anthropologists moving to Washington. While my main job was Middle East intelligence, I organized and published, in *Human Organization*, studies on projects successful in increasing

food production in third world countries. In the process, I worked closely with many in the Department of Agriculture including the Under Secretary. To continue the momentum after I left Washington for Lend Lease work in the Middle East, the latter arranged to have a world congress meet in Washington to further third world food production for the Allied armies.

As a Lend Lease representative stationed in Cairo, Egypt, I worked closely with both the British and Egyptians. Informally it provided an opportunity to learn, almost at first hand, much about diplomatic maneuverings and the human foibles and weaknesses of top and high-level persons.

#### D. POST-WORLD WAR II ORGANIZATION RESEARCH, CONSULTING, AND TEACHING (1946–1979 Retirement)

In post-World War II years I devoted 33 years to organizational research, consulting, and teaching based successively in four different universities. Seven of these years I became an unaffiliated independent researcher-consultant. The universities in order of sequence were the Yale Labor Management Center, the Harvard School of Public Health, and the graduate business schools of both the Universities of Pittsburgh and Virginia. In the latter two I was also a formal member of their anthropology departments.

During this period, I, together with assistants and students, devoted considerable time to over a dozen firms including Ford, IBM, Westinghouse, and Gulf and in one, Raytheon, I was closely associated for 13 years as researcher and consultant. In addition, I made reconnaissance studies of about 10 hospitals with one of which I was closely associated both as researcher and consultant for 4 years. While at the Harvard School of Public Health, I was for 5 years from 1949–1954, the senior social scientist for a pioneer community mental health project, to help further their priority aim to prevent mental illness by helping patients alter their relationships. Unfortunately this worthy and feasible aim was professed more than practiced due mainly to a strong psychoanalytic bias that then permeated psychological circles in the Boston area.

It was midway through this period, around 1960, that together with applied anthropologist, Donald Kennedy, I made the surprising and statistically valid discovery that the productivity of a work group was closely dependent on contact patterns or e.g. the frequency, duration and number of pair and group contacts one minute or longer between each subordinate and their manager. This statistical evidence had been preceded by years of increasing intuitive suspicion that it might be true. If so, high group productivity was not primarily dependent on high I.Q. or intellectual intelligence, a view strengthened by the considerable evidence that high I.Q. people often make miserable administrators, managers, or executives.

The above paralleled a revolutionary and earlier discovery by Eliot Chapple that good salespersons and managers could be clearly differentiated from poor. This was based on timing to fractions of a second their patterns of acting and reacting during stress interviews much of which was published decades ago in *Human Organization*. Chapple's method of only timing the actions and reactions of a conversing pair, with no regard to verbal meaning, was an affront to a professional faith in the all importance of human language and intelligence. Such a hearsay was therefore automatically disclaimed or ignored—a circumstance which to a lesser degree still prevails today.

It was in 1962 when recognizing these two discoveries—the importance of contact patterns and the timing of actions and reactions while conversing—that I clearly formulated the proposition that skilled leaders of productive groups did not primarily depend on their



intellectual skills. Instead, they won the willing cooperation of others by their interactional manner and timing. This further led me to the proposition that this interactional skill preceeded the development of high human intellect and is widely characteristic of all primates if not other family organized mammals.

#### E. EVOLUTION OF COOPERATIVE LIVING FROM FAMILIES TO HIERARCHIES (1963– PRESENT)

Despite generations of professionals, particularly psychologists, striving to identify the skills and charisma of superior leaders, their search continues to elude. As the strategy of nearly all such searches has relied heavily on imprecisely described verbal categories and statements by subjects, it has seemed all the more promising to me to continue the strategy described above of exploring further the critical importance for leaders of their interactional timing while conversing and e.g. the frequency and duration of their interchange, or contact pattern, over periods of time. This I have done with some success and urge many of you to pursue this lead further.

A second strategy I have tried has been to determine the importance of interactional patterns among family organized animals. This I have increasingly pursued through the literature from 1963 to about 1980. It has resulted in confirming an obvious but much neglected conclusion that frequently associating individuals, whether humans or animals, in contrast to strangers, usually cooperate closely and particularly in opposing others.

A third strategy I have pursued is to learn from brain physiologists about discoveries relating to connections between the brain and different kinds of interactional exchanges such as hostility, courting, mating, mother-love, and play. This has been professionally rewarding and I urge many to pursue this lead also.

And finally, I have explored the evolution of human cooperation as evidenced by the progressive enlarging of human societies particularly over the last 10,000 years. This has also been an immensely rewarding undertaking providing important insights into the unique predicament that technological advance and hierarchical organization place on weak and gullible human beings. It is this predicament that forms the focus for the book I am writing and the talk next spring I propose giving to the Society for Applied Anthropology.

#### Notes

<sup>1</sup>Prepared for the Society for Applied Anthropology, Late August 1987.

## **Chapter 19**

### **Lauriston Sharp:**

### **Innovator of Applied Anthropology Programs**

**Thomas Weaver**

Lauriston Sharp received the Society for Applied Anthropology's Malinowski Award in 1989, but his acceptance speech was not published and there is no known copy.<sup>1</sup> Sharp was a founding member of the Society for Applied Anthropology. Foremost in his applied work was his role in developing an applied anthropology program at Cornell University and his long-term interest in culture change. His publications "Steel Axes for Stone Age Australians" (1952) and "Continuities and Discontinuities in Southeast Asia" (1962) quickly attained the status of classics in anthropology. In the former, Sharp demonstrated how the replacement of a single material item had repercussions throughout the society, supporting the importance of a holistic model to understand introduced change. In the second article, a "must read" for specialists in Southeast Asia, Sharp criticized the concept of culture and demonstrated that it was insufficient for establishing boundaries in a region impacted by an expanding Chinese civilization since it is an artifact of Western classification systems (Freedman 1974:302). In addition to teaching, research, and publishing, Sharp's participation in various governmental boards and committees allowed him to have a voice in matters that impacted people in the U.S. and Southeast Asia and affected anthropology.

Lauriston Sharp (1907–1993) was born and raised in Wisconsin and attended the University of Wisconsin. While a senior in high school he met Clyde Kluckhohn, who was in college at the time, and with other friends they formed a discussion club. After graduating from college in 1929, Sharp accompanied Kluckhohn and J. J. Hanks on two summer reconnaissance trips to the Kaiparowitz Plateau, an experience that changed his major interest from philosophy to anthropology. He served a year as freshmen dean at the University of Wisconsin, during which time he attended lectures by Ralph Linton. He joined an expedition to the Berbers in North Africa, accompanied by Sol Tax (also a Malinowski Award winner; see Tax 1997) and John Gillin. This experience concretized his interest in anthropology.

Upon the advice of Robert Lowie, Sharp enrolled at the University of Vienna to study South Asian cultures with Robert Heine-Geldern in 1931. Linton advised him to attend Harvard where he studied with Alfred Tozzer, Earnest Hooton, Talcott Parsons, and Roland Dixon. In 1932 he accompanied Sol Tax, who was working with the Fox Indians on his doctoral dissertation. Sharp received a master's degree from Harvard for this work. Recommended by W. Lloyd Warner to A. R. Radcliffe-Brown, Sharp went on to do fieldwork in northern Australia with the Yir Yiront from 1932 to 1935 (Sharp 1952). After making brief visits to New Guinea,

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China, and Southeast Asia, he returned to Harvard where he received a Ph.D. in 1937 for a dissertation entitled “The Social Anthropology of a Totemic System in North Queensland, Australia” (Smith 1974:7–10).

World War II had a profound impact on Sharp. On his return from service in the State Department as assistant chief of the Division of Southeast Asian Affairs, he developed innovative programs at Cornell in applied anthropology that examined the impact of change and modernization on tribal and peasant societies. Morris Opler, Allan Holmberg and John Adair were invited to join the department. Supported by the Carnegie Corporation, linked projects were commenced in North India (directed by Opler), Thailand (Sharp), Peru (Holmberg), and the Navajo Reservation (Adair). Alexander Leighton (also a Malinowski Award winner; see Leighton 1984) became involved with the Navajo project and associated his work on health in Nova Scotia, Canada, with the Cornell program.

The interdisciplinary program thus established was known as the Cornell University Studies in Culture and Applied Anthropology. The projects under this program trained many applied anthropologists and social scientists of the 1950s and 1960s. The Holmberg Vicos project in Peru became one of the most famous applied endeavors in the discipline, partially because of the number of specialists in Latin American anthropology it produced, but also because of the ramifications of the changes effected there, planned and directed by the Indians themselves (Smith 1974:11–13). The Cornell program’s production in publications was extensive; the Cornell Thailand project alone consisted of over 450 items, including 50 doctoral dissertations. (See, for example, Sharp and Hanks 1978.) The Department of Anthropology at Cornell was also the headquarters for the Society for Applied Anthropology, with its journal *Human Organization* published there until 1965.

Although he had visiting professorships at various universities, including the University of Sydney, Yale University, Haverford College, the University of California at Berkeley, the University of London, and Bangkok University, the locus of Sharp’s teaching career was Cornell University. There his most important contributions were in the development of programs and the encouragement of students, especially minority students from the U.S. and abroad. In addition to the aforementioned Cornell Program for Studies in Culture and Applied Anthropology, Sharp was involved in the development of the Southeast Asia Program and the Center for International Studies. He served as chair of the combined Sociology and Anthropology Department and later of the Anthropology Department for many years. Sharp was a consultant to the Department of State, the United States Agency for International Development, and the Social Science Research Council, and he sat on various governmental boards and committees. He also served on many professional boards including that of the American Anthropological Association and the Pacific Science Board of the National Research Council, and he was president of the Association for Asian Studies.

Sharp received Guggenheim, Ford, Fulbright, and National Endowment for the Humanities fellowships and was a Distinguished Lecturer at Haverford College. He was a fellow of the National Endowment for the Humanities, a foreign fellow of the Royal Anthropological Institute and of the Australian Institute for Aboriginal Studies, and a life member of the Siam Society (Smith 1974:14–15). His students established a Lauriston Sharp Essay Prize in 1967 and a Lauriston Sharp Scholarship Fund for the study of Thailand; they also published two books in his honor (Smith 1974, Skinner and Kirsch 1975).

### Notes

<sup>1</sup>This annual award is given to a senior colleague in recognition of efforts to understand and serve the needs of the world through social science. See Thomas Weaver's "The Malinowski Award and the History of Applied Anthropology" (2002b) for a brief history of the award and an overview of the recipients and their work, and Weaver's "Malinowski as Applied Anthropologist" (2002a) for an introduction to Bronislaw Malinowski and his work.

### References Cited

Freedman, Maurice

- 1974 An Epicycle of Cathay; or, The Southward Expansion of the Sinologists. *In Social Organizations and the Applications of Anthropology: Essays in Honor of Lauriston Sharp*. R. J. Smith, ed. Pp. 302–332. Ithaca: Cornell University Press.

Leighton, Alexander H.

- 1984 Then and Now: Some Notes on the Interaction of Person and Social Environment. *Human Organization* 43(3):189–197. (Republished at <<http://www.sfaa.net/>>.)

Sharp, Lauriston

- 1952 Steel Axes for Stone Age Australians. *In Human Problems in Technological Change*. E. H. Spicer, ed. Pp. 69–90. New York: Russell Sage Foundation.
- 1962 Cultural Continuities and Discontinuities in Southeast Asia. *Journal of Asian Studies* 22(1):3–11.

Sharp, Lauriston and Lucien M. Hanks, eds.

- 1978 Bang Chan: Social History of a Rural Community in Thailand. Ithaca: Cornell University Press.

Skinner, G. William and A. Thomas Kirsch, eds.

- 1975 Change and Persistence in Thai Society: Essays in Honor of Lauriston Sharp. Ithaca: Cornell University Press.

Smith, Robert J., ed.

- 1974 Social Organizations and the Applications of Anthropology: Essays in Honor of Lauriston Sharp. Ithaca: Cornell University Press.

Tax, Sol

- 1977 Anthropology for the World of the Future: Thirteen Professions and Three Proposals. *Human Organization* 36(3):225–234. (Republished at <<http://www.sfaa.net/>>.)

Weaver, Thomas

- 2002a Malinowski as Applied Anthropologist. *In The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers*. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.
- 2002b The Malinowski Award and the History of Applied Anthropology. *In The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers*. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.

## **Chapter 20**

### **J. G. St. Clair Drake: Activist-Advocate before His Time**

**Thomas Weaver**

J. G. St. Clair Drake's acceptance speech at his receipt of the Society for Applied Anthropology's Malinowski Award in 1990 was not published, but it was videotaped.<sup>1</sup> Unfortunately, there is no known copy of that tape. Drake's works are permeated by themes of political economy, injustice, capitalism, nationalism, and race relations (Baber 1999; Bond 1988; Drake 1960, 1966, 1970, 1987). His book *Black Metropolis*, published with Horace Cayton in 1945, is a classic. The book details the lives of Blacks in Chicago, providing facts that supported Gunnar Myrdal's *An American Dilemma* (1942). (Myrdal was also a Malinowski Award winner; see Myrdal 1975.) Drake was an activist long before it was considered a legitimate role for academics. His activity in Afro-American studies and innovative courses on racism and the Black experience were closely related to his approach to applied anthropology (Baber 1999; Drake 1960, 1966).

St. Clair Drake (1911–1990) was the first American Black to receive the Malinowski Award. It was given to him in recognition of his life-long fight against racism and for his help to people of developing societies. The son of an activist minister from Barbados, he grew up in Virginia of parents born in the first generation after the liberation of slaves. He attended mostly segregated schools: Booker T. Washington High School and Hampton University. He graduated from Hampton in 1931 with a degree in biology and then attended Pendle Hill, a Quaker graduate school near Philadelphia. Here he was exposed to activism with roots in the Quaker peace movement, Gandhism, and Marxism (Baber 1999:195). After Pendle Hill he taught at a Quaker boarding high school for Blacks and helped establish summer camps for Quaker youth. Although Quakerism influenced his life, he did not join the Church because he did not believe completely in nonviolence (Bond 1988:768).

As a young man, Drake was taken under the wing of members of a Black intellectual class that had been germinating since the second decade of this century. One of these, Allison Davis, had studied in England, and he tutored Drake in social anthropology, providing him with a Malinowskian view of applied anthropology. Drake's first fieldwork in 1935 was with Allison Davis, co-author of *Deep South* (1941), a study of Natchez, Mississippi. Drake was responsible for interviewing working-class Blacks, while the co-directors of the project, Davis and Burleigh Gardner, interviewed the middle and upper classes (Bond 1988:772–773). During this period Drake became an instructor in anthropology at Dillard in New Orleans, another Black institution, and in the summer of 1936 he enrolled in two courses taught by W. Lloyd Warner at Columbia

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University. Advised by Warner and Davis, he studied anthropology at the University of Chicago from 1937 to 1942 and from 1946 to 1948. In the interim period he served in the Merchant Marines during World War II.

Professors who influenced Drake's development, besides the early contact mentioned with Warner and Davis, were Robert Redfield, Edward Shils, Samuel Kinchloe, Louis Wirth (the last three were urban sociologists), Everett Hughes (also a recipient of the Malinowski Award; see Hughes 1974), Horace Cayton, Robert Braidwood, and Fred Eggan. Drake also studied with Malinowski (Baber 1999:192), one of the few Malinowski Award recipients to have done so. Fellow Blacks among students and instructors at the University of Chicago included Horace Cayton, Oliver Cox, Lawrence Reddick, Hylan Lewis, and Allison Davis. The hiring of Davis at Chicago in 1941 made him the first Black to receive a tenure line appointment in a major university (Bond 1988:780).

Much of Drake's applied work was accomplished during graduate school in conjunction with research projects sponsored by his professors. In addition to the *Deep South* study, he worked on Cayton's project examining Negro-White relations and the structure of the Black community in Chicago. Another study was carried out for a state commission on the conditions of urban Blacks in Chicago. During the research that led to *Black Metropolis* (1945), Drake worked with the unemployed and unions. In 1945 he received a small grant with Warner's help to conduct a study of a community in Wales (Bond 1988:773).

After this came research on the African press, radio, and film and teaching in Ghana and Liberia. In Ghana Drake also conducted a series of studies on the encroachment of the government on sacred tribal lands and on nudity as a political issue. In addition to making policy suggestions to the government, he used these materials for Peace Corps teacher-training programs in Ghana from 1961 to 1964 (Baber 1999:197). At this time he organized the first West African Conference of Social Workers (Bond 1988:775).

Upon his return to the U.S., Drake became involved in the Civil Rights Movement with SNCC (the Student Nonviolent Coordinating Committee) and Martin Luther King. He also served on the Ford Foundation Committee on Minority Fellowships. He taught at Roosevelt University for twenty-three years, a period dedicated to fighting racism, and he ended his career at Stanford University.

In an interview with George Bond, Drake characterized his approach to activism and anthropological theory in the following words:

My attraction to the field had . . . a utilitarian goal: "How can anthropology aid in dissipating stereotypes about black people and in eliminating errors based upon confusion between biological and environmental factors in accounting for observed racial differences?" But having made a commitment to the discipline, concern with general problems of theory became of interest, too. The theory underlying the sociology of knowledge as developed by Karl Mannheim provided a rationale for an activist role in social issues. Not only did it express commitment to values, but praxis also had an epistemological function. Here, [came] my interest in Marxist approaches (Bond 1988:774).

Drake's most important contribution may have been to the study of the Black Diaspora and of institutional racism (Baber 1999:200). During his first course in anthropology, Drake was warned by Warner that he could expect his career to be restricted to Negro schools and to race

relations activity (Bond 1988:780). He recognized this fact in a retrospective assessment: “Had I not been black I would have been a very different kind of anthropologist. Interest in theory has always been subordinated to my preoccupation with black liberation in the United States, Africa, the Caribbean and the British Isles.”

Drake was a fellow in the Royal Anthropological Institute of Great Britain and Ireland and a senior scholar at the W. E. B. DuBois Institute at Harvard University. Other awards he received include the DuBois-Johnson-Frazier Award from the American Sociological Association, honorary doctorates from Roosevelt University and the University of Maryland, and the Association of Black Anthropologists’ Distinguished Achievement Award for Extraordinary Scholarship and Activism.

### Notes

<sup>1</sup>This annual award is given to a senior colleague in recognition of efforts to understand and serve the needs of the world through social science. See Thomas Weaver’s “The Malinowski Award and the History of Applied Anthropology” (2002b) for a brief history of the award and an overview of the recipients and their work, and Weaver’s “Malinowski as Applied Anthropologist” (2002a) for an introduction to Bronislaw Malinowski and his work.

### References Cited

- Baber, Willie L.  
1999 St. Clair Drake: Scholar and Activist. *In* African-American Pioneers in Anthropology. I. E. Harrison and F. V. Harrison, eds. Pp. 184–204. Urbana: University of Illinois Press.
- Bond, George C.  
1988 Portrait of John Gibbs St. Clair Drake: An American Anthropologist. *American Ethnologist* 15(4):762–781.
- Davis, Allison, Burleigh B. Gardner, and Mary R. Gardner  
1941 Deep South: A Social Anthropological Study of Caste and Class. Chicago: University of Chicago Press.
- Drake, J. G. St. Clair  
1960 Traditional Authority and Social Action in Former British West Africa. *Human Organization* 19(3):150–163.  
1966 Race Relations in a Time of Rapid Social Change; Report of a Survey. New York: National Federation of Settlements and Neighborhood Centers.  
1970 The Redemption of Africa and Black Religion. *Black Paper* (Atlanta, GA). Chicago: Third World Press.  
1987 Black Folk Here and There: An Essay in History and Anthropology. Volume 1. Los Angeles: Center for Afro-American Studies, University of California at Los Angeles.
- Drake, St. Clair and Horace R. Cayton  
1945 Black Metropolis: A Study of Negro Life in a Northern City. New York: Harcourt, Brace and Company.
- Hughes, Everett C.  
1974 Who Studies Whom? *Human Organization* 33(4):327–334. (Republished at <<http://www.sfaa.net/>>.)

Myrdal, Gunnar

- 1942    An American Dilemma. New York: Harper & Row.
- 1975    The Unity of the Social Sciences. *Human Organization* 34(4)327–332. (Republished at <http://www.sfaa.net/>.)

Weaver, Thomas

- 2002a    Malinowski as Applied Anthropologist. *In* *The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers*. T. Weaver, ed. <http://www.sfaa.net/>. Oklahoma City: Society for Applied Anthropology.
- 2002b    The Malinowski Award and the History of Applied Anthropology. *In* *The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers*. T. Weaver, ed. <http://www.sfaa.net/>. Oklahoma City: Society for Applied Anthropology.



## **Chapter 21**

### **Conrad Arensberg: Economic and Industrial Anthropologist**

**Thomas Weaver**

Conrad Arensberg, recipient of the Society for Applied Anthropology's Malinowski Award in 1991, specialized in economic and industrial anthropology, urban studies, development and applied anthropology, and community studies.<sup>1</sup> Many chapters of his works, particularly those on community study and industrialism, were reprinted in edited collections or as separate publications and have been cited extensively by other scholars. His book on economics, published with Karl Polanyi (1957), garnered much discussion and critique and was issued in French and Japanese in 1975 and in Spanish in 1976. Particularly important was the distinction between the uses of substantive versus formalist conceptions in discussing "primitive" versus industrial economies. Arensberg's contributions to industrial anthropology and decision-making theory are important as background for the further development of research in organizational culture, and his studies in development and agriculture were precursors to current development anthropology. In all these endeavors Arensberg emphasized the link between theory and application.

Conrad Arensberg (1910–1997) was born in Pittsburgh, Pennsylvania, and graduated from Harvard with a B.A. in 1931 and a Ph.D. in 1934. His academic career began with a junior teaching fellowship at Harvard and appointments at the Massachusetts Institute of Technology (MIT). He was chair and founder of a combined sociology and anthropology department at Brooklyn College, chair of sociology at Barnard College, and finally professor of anthropology at Columbia University. Arensberg helped Alexander Leighton and Edward Spicer (fellow Malinowski Award recipients; see Leighton 1984 and Spicer 1976) in a preliminary analysis of the Poston Japanese internment camp in connection with the War Relocation Project in 1942, and he served as a Major in U.S. Army Military Intelligence from 1943 to 1945. Later he was a research consultant in Ruhr, Germany, and research director of the Institute for Social Science for UNESCO in Cologne. As a consultant for UNESCO he conducted a survey of social science in Germany, and in 1949–1950 he convened a meeting on East European Jewish Research at Columbia. At various times he was a consultant for the U.S. Departments of State, Interior (Bureau of Indian Affairs), and Agriculture (Agricultural Economics), and the Rockefeller Foundation.

Arensberg's important ethnological and theoretical contributions include *Family and Community in Ireland* (with Solon T. Kimball, 1940), *Trade and Markets in Early Empires* (with Karl Polanyi, 1957), and *Introducing Social Change* (with Arthur Niehoff, 1964). He also

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published *Measuring Human Relations* (with Eliot Chapple, 1940). In 1952 and 1954 he edited and wrote parts of two of the American Anthropological Association's Point IV Manuals. He served on the editorial board of the Industrial Relations Research Association that published a summary volume on human relations research in 1955.

Arensberg did not publish his Malinowski Award acceptance speech in *Human Organization* because of failing health. His wife, Vivian Garrison, indicated that his address was a summary of his life work. A paper published in 1987 reflects his contributions to applied and development anthropology and may have formed the basis for his Malinowski Award presentation.

Arensberg's approach to anthropology was developed under the influence of W. Lloyd Warner and others doing then-new research in industrial relations at Harvard University. He worked with Warner in the Yankee City studies and in the Hawthorne study, and he was directed by Warner in his fieldwork in Ireland. He was joined in studying modern society by Solon Kimball, Frederick Richardson (a fellow Malinowski Award winner; see Richardson 2002), Eliot Chapple, and other students or colleagues influenced by Warner (Comitas 1997).

One result of Arensberg's association with Warner, Richardson, Elton Mayo, and others in the interdisciplinary study of industrial relations at Harvard was the innovation by anthropological researchers of many research techniques (Richardson 1979). Among these techniques were sociometric measures of human interaction and network and event analysis (Arensberg 1987:61). Arensberg and Chapple (1940) developed special recording equipment for measuring frequency and duration of contact between two persons and the tempo of nonverbal exchange.

Arensberg's most important contributions relate to industrialism. The anthropological interest in industrial work had come about in the years before and after World War II. Rapid technological advances had led to mass unemployment during the depression of the 1930s. The new technology had also led to boredom and repetitive work habits that decreased productivity. The goal of industrial anthropology was to discover the roots of workers' alienation from their jobs in factories and offices and to provide solutions. Part of the answer, Arensberg believed, lay in the creation of teams of industrial workers with common tasks. Another solution was to use informal networks as a means to improve morale (Arensberg 1987:65).

Arensberg expanded his early interests in interaction into issues dealing with development, planned change, and the transfer of technology. The result was what he called "The Reciprocal Accommodation Process." This phrase referred to the necessary interaction among planners, policy makers, change agents, and the proposed recipients of change in order to make planned change successful (1987:70–80). His work in organizations and planning agencies led to his later study of hierarchical systems, leadership, and decision-making theory (1987:80–88).

Conrad Arensberg was a Sheldon Fellow at Harvard University and presented the Lowell Lectures in Boston early in his career. He was a founding member of the Society for Applied Anthropology, editor of *Human Organization*, president of the Society for Applied Anthropology, and president of the American Anthropological Association. At the time of his retirement he was Battenwieser Professor of Human Relations at Columbia University. The Society for the Study of Work, a unit of the American Anthropological Association, named a prize in his honor and conferred the first award on him in 1991.

### Notes

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### References Cited

- Arensberg, Conrad M.  
 1952 Cross-cultural Transfer of Techniques, a manual for Point IV Workers. Washington, D.C.: American Anthropological Association and the Foreign Service Institute, U.S. Department of State.  
 1954 Cross-cultural Transfer of Techniques, a Manual for Point IV Workers. Washington, D.C.: American Anthropological Association and the Foreign Service Institute, U.S. Department of State.  
 1955 Research in Industrial Human Relations: A Critical Appraisal. New York: Harper.  
 1987 Theoretical Contributions of Industrial and Development Studies. *In* Applied Anthropology in America, Second Edition. E. M. Eddy and W. L. Partridge, eds. Pp. 59–88. New York: Columbia University.
- Arensberg, Conrad M., and Arthur H. Niehoff  
 1964 Introducing Social Change: A Manual for Americans Overseas. Chicago: Aldine.
- Arensberg, Conrad M., and Eliot D. Chapple  
 1940 Measuring Human Relations: An Introduction to the Study of the Interaction of Individuals. Genetic Psychology Monographs 22:3–147.
- Arensberg, Conrad M., Karl Polanyi, and H. W. Pearson, eds.  
 1957 Trade and Market in the Early Empires. Glencoe, Ill.: Free Press.
- Arensberg, Conrad M., and Solon T. Kimball  
 1940 Family and Community in Ireland. Cambridge: Harvard University Press.
- Comitas, Lambros  
 1997 Conrad M. Arensberg. Anthropology Newsletter 38(5):18.
- Leighton, Alexander  
 1985 Then and Now: Some Notes on the Interaction of Person and Social Environment. Human Organization 43(3):189–197. (Republished at <<http://www.sfaa.net/>>.)
- Richardson, F. L. K.  
 1979 Social Interaction and Industrial Anthropology. *In* The Uses of Anthropology. W. Goldschmidt, ed. Pp. 79–99. Special Publication Number 11. Washington, D.C.: American Anthropological Association.  
 2002 Career Development, Strategies, Discoveries, and Turning Points. *In* The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers. T. Weaver, ed. <<http://www.sffa.net/>>. Oklahoma City: Society for Applied Anthropology.
- Spicer, Edward H.

- 1976 Beyond Analysis and Explanation? Notes on the Life and Times of the Society for Applied Anthropology. *Human Organization* 34(4):335–343. (Republished at <<http://www.sfaa.net/>>.)

Weaver, Thomas

- 2002a Malinowski as Applied Anthropologist. *In* *The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers*. T. Weaver, ed. <<http://www.sffa.net/>>. Oklahoma City: Society for Applied Anthropology.
- 2002b The Malinowski Award and the History of Applied Anthropology. *In* *The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers*. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.

## **Chapter 22**

### **M. Margaret Clark:**

### **Medical Anthropologist and Advocate for the Aged**

**Thomas Weaver**

Recipient of the Society for Applied Anthropology's Malinowski Award in 1992, Margaret Clark has worked in medical anthropology, medical education, and aging and health policy.<sup>1</sup> She has been employed by the U.S. Public Health Service, the School of Public Health at the University of California, Berkeley, and the University of California at San Francisco in the Departments of Psychiatry, Epidemiology, and International Health. At the time of the award she was Professor Emerita in the Division of Medical Anthropology, Department of Epidemiology and Biostatistics, School of Medicine, University of California, San Francisco. Along with George Foster (a fellow Malinowski Award winner; see Foster 1982) and Fred Dunn, Clark was one of the founders in 1975 of the joint medical anthropology program on the San Francisco and Berkeley campuses of the University of California. She headed the joint program for many years, and under her leadership it came to serve as a model for other medical anthropology programs in the country. In 1983 Clark edited a special issue of *The Western Journal of Medicine* on culture and medicine that has had great influence in medical education, with over 100,000 copies distributed.

Margaret Clark (1925–) was educated at Southern Methodist University and received a Ph.D. in anthropology from the University of California, Berkeley in 1957. She entered anthropology after leaving medical school because of her dissatisfaction with the treatment of ethnic minorities and with physicians' almost complete focus on biomedical treatment rather than on sociocultural factors. Professors at the University of California at Berkeley hailed her dissertation research on health in a Mexican American community for its combination of quantitative and qualitative methods. It was probably the first dissertation in medical anthropology (Foster, personal communication, 1998). First published in 1959, it went through two editions and seven printings. Her 1967 book on culture and aging was the first book in anthropology on aging and charted the direction of the field.

Clark was co-director and co-founder of the Institute for Health and Aging, an organization that has had major policy impacts in Sacramento and Washington. She has been a member of three of the aging and health conferences of the Senate Select Committee on Aging. President Carter appointed her to the National Advisory Committee for Aging. All this work has had profound effects on changing tax structures and creating local programs for the aged.

In addition to medical anthropology, ethnic groups, and gerontology, Clark's interests include ethnopsychiatry, medical education, and comparative bioethics. She was co-chair and

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U.S. delegate to the Permanent Council of the Commission for the Study of Peace of the International Union of Anthropological and Ethnological Sciences. She has been a consultant to the National Academy of Sciences, the National Institute on Aging, the National Institute of Mental Health, the National Science Foundation, the Veteran's Administration, the Wenner-Gren Foundation, and a variety of agencies concerned with health and aging.

Clark's Malinowski Award presentation, entitled "Medical Anthropology and the Redefining of Human Nature" (1993), focused on the relationship between innovation in Western medical science and changing definitions of human nature. A major question confronting modern medicine is what Clark calls "the nature of personhood." This question is key to such issues as when a fetus becomes a human, the use of amniocentesis, and the right to terminate life. When do doctors stop treating a patient? How do birth control programs pass over into racism and genocide when applied to Third World countries? Other ethical problems are those related to organ transplants—issues involving cost, the commodification of body parts, and inequities in access—and body alterations such as breast implants, cosmetic plastic surgery, sexual alterations, and female circumcision. Related issues concerns control of health risks associated with voluntary behaviors such as the use of tobacco, drugs, and alcohol. Aging populations have given physicians and anthropologists a whole new set of problems such as elder abuse, living wills, and the use of technologies that prolong life.

Clark concluded her speech with an iteration of the critical role of anthropology in bioethics and with what Paula Marshall has called "the social meaning of human suffering and the cultural dimensions of healing" (1992:62, quoted in Clark 1993).

In our roles as applied anthropologists . . . we . . . continue to expose . . . oppression and deprivation. Women are being deprived of control over their own bodies and their own reproduction. The aged are being deprived of a dignified later life and a humane death. Sexual minorities are being deprived of freedom from stigma and adequate and prompt attention to their ills. The poor are being deprived of equity in medical care and protection from the noxious byproducts of a greedy industrial economy. As we apply our skills to these problems, we will help preserve what is most humane in human life (Clark 1993:239).

Margaret Clark has been honored in many ways. She was the Annual Faculty Research Lecturer at the University of California at San Francisco in 1980 and the first social scientist to be so honored. She was a fellow at the Center for Advanced Studies in Behavioral Sciences and received the 1990 Distinguished Mentorship Award from the Gerontological Society of America which also created a scholarship in her name. In recognition of her work on aging, she received the Distinguished Service Award from the Senior Centers in the San Francisco Bay Area. She has been president of the American Anthropological Association and the Society for Medical Anthropology and vice-president of the Gerontological Society.

#### Notes

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### References Cited

- Clark, Margaret
- 1959 Health in the Mexican-American Culture. Berkeley: University of California Press.
  - 1983 Cultural Context of Medical Education. *In* Cross-Cultural Medicine. M. Clark, ed. Pp. 806–810. *The Western Journal of Medicine* 39(6).
  - 1993 Medical Anthropology and the Redefining of Human Nature. *Human Organization* 52(3):233–242. (Republished at <<http://www.sfaa.net/>>.)
- Clark, Margaret, and Barbara G. Anderson
- 1967 Culture and Aging: An Anthropological Study of Older Americans. Springfield, IL: Charles C. Thomas.
- Foster, George M.
- 1982 Applied Anthropology and International Health: Retrospect and Prospect. *Human Organization* 41(3):189–197. (Republished at <<http://www.sfaa.net/>>.)
- Marshall, Paula
- 1992 Anthropology and Bioethics. *Medical Anthropology Quarterly* 6:49–73.
- Weaver, Thomas
- 2002a Malinowski as Applied Anthropologist. *In* The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.
  - 2002b The Malinowski Award and the History of Applied Anthropology. *In* The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.

## Medical Anthropology and the Redefining of Human Nature

**M. Margaret Clark**

When President Hill called to inform me of the marvelous honor the Society has given me as the 20th Malinowski lecturer, my initial reaction was the usual one: that of course I don't deserve it, particularly given the distinguished company that I now join. My second thought was that knowledge of my limitations did nothing to eliminate the requirement that I give this address.

I have been in the audience for many of these lectures, beginning with the first one by Dr. Aguirre-Beltran in 1973. The prospect of attempting to generate some new conceptual synthesis for the field, as Sol Tax did in 1977, or to summarize from personal experience the entire history of our Society as Ned Spicer did the following year, succeeded in paralyzing all my thought for several months. It is especially hard for me to follow my mentor, George Foster, who just ten years ago gave the first of the Malinowski lectures to deal with medical anthropology, basing it on his intimate knowledge as one of the founders of that branch of our field. Now I have the task of continuing the discussion of medical anthropology, today speaking about developments in western medical science and the ways in which those innovations are modifying long-held conceptions of human nature.

It came as a great shock to me last July, when I retired from the University of California, to realize that I have spent the past 35 years as a medical anthropologist, working along with my teachers, peers, and students, in the nebulous area comprising the junction between anthropology and medicine. Yet during those 35 years, I have never had more than a visiting appointment in a department of anthropology. I have worked in hospitals, in a school of public health, as a research anthropologist in the Public Health Service, and in a variety of capacities at the University of California School of Medicine in San Francisco.

In some corner of my mind, I have always felt marginal to the field of academic anthropology, perhaps because I started my graduate education as a medical student in Dallas, Texas. I had never taken a course in anthropology when I arrogantly requested admission to Berkeley as a graduate student, fleeing medical school after my Junior clerkship in internal medicine, in search of some way of understanding the human side of the strange world of the teaching hospital.

During my third year in medical school, I first encountered the work of Malinowski. I had gone to see my advisor, the chairman of the psychiatry department, to voice my unhappiness with medical school. On the medical school faculty and on the medical staff of Parkland, our teaching hospital, there was not a single African American or Hispanic physician, and precious few women of any ethnicity. The same situation was true of the student body, although there were many Jewish students there escaping the anti-semitism of eastern establishment schools. The nursing staff was all white, all female, and grossly underpaid, overworked, and patronized by the doctors.

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Yet in contrast to the professional staff, more than two-thirds of the patients we saw in that county hospital were black or Spanish-speaking. Language was only one of the many barriers we observed in almost every encounter between patients and clinicians. No heroic measures were taken to facilitate this communication, although lip service was paid to the importance of the medical history. The message the students got from their mentors was that patients either didn't know much or couldn't say much of value about their illnesses. Thus, high priority was given to clinical examination and especially to laboratory testing. In retrospect, I began to think of this type of practice as human veterinary medicine.

It is very easy, at the historical distance of almost four decades, to criticize the physicians of my youth for their lack of social and cultural sophistication. But that was a very different world; the challenges facing medicine at that time were enormous, and patient-doctor communication was far down on the list of priorities. There were widespread polio epidemics during my years as a medical student. In fact, my medical school roommate contracted the disease during one summer working on the Parkland polio wards; she finished her medical training in a wheel-chair. Infectious diseases were far from under control; most chronic diseases were untreatable; and the mentally ill were subjects of what we now think of as wildly experimental treatments. During my psychiatry rotation, patients—regardless of diagnosis in most cases—were given electroconvulsive therapy, insulin shock, or were subjected to psychosurgery. Paretics were injected with live typhoid cultures—the so-called febrile therapy. Cancers were most often found too late for successful treatment, of which there was not much other than radiation.

Anti-inflammatory drugs had not yet been developed in the 1950s, and open-heart surgery was too dangerous to be considered at all. Tuberculosis was still a major public health problem in the United States, and cases of smallpox were frequently seen among recent arrivals from Asia or Latin America.

The periods just before and following that time were years of great adventure and discovery in the medical sciences. This odyssey has been described by contemporary medical historians, and even by some anthropologists (Kaufman 1993).

I will return to this topic in a moment, but first I would like to speak about anthropology, and especially applied anthropology, as I learned of it in the mid-1940s. I wish I could say that as a student I was brilliant and farsighted enough to sense at once the need for sociocultural perspectives in the practice of clinical medicine. But that was not the case. I simply was vaguely uneasy about the kind of role modelling I was receiving in medical school, without having much of a clue as to what caused such enormous distance between patients and practitioners. I did speak to my advisor, however, about my dissatisfaction with a diagnosis of hysteria given by the attending physician for a middle-aged Hispanic woman who had recurrent bouts of false pregnancy. And I thought there must be something other than ignorance and uncooperativeness behind the repeated flights from medical custody on the part of a single African American mother of three with active tuberculosis.

It was partly this discomfort and partly a general third-year student malaise that led me to ask for a year's leave to take some graduate work in anthropology. I knew very little about anthropology, except for three books that I had read: Ralph Linton's *The Study of Man* (1936), Margaret Mead's *Sex and Temperament in Three Primitive Societies* (1935), and Bronislaw Malinowski's posthumous work, *The Dynamics of Culture Change* (1949). Thus my first introduction to the work of Malinowski was to the most applied of all his writings. My initial view of anthropology, therefore, was that it was a clearly practical science. This conviction has

failed to be eradicated even by years of exposure to a graduate faculty, among many of whom—when I came to Berkeley—the term “applied anthropology” was viewed as an oxymoron.

Perhaps encouraged by the fact that Foster joined the faculty a few years later, I was able to view the general dismissal of applied work as an eccentricity—just a slight glitch—on the part of otherwise brilliant, creative, and generous teachers. During my first year as a graduate student, I came to love anthropology and to feel blessed to be among Robert Lowie’s last class of graduate students before his retirement. I became convinced that anthropology provided exactly the world view that I had found missing in my earlier studies, and that I could find no other work as satisfying to my soul.

The topic of my talk today actually came from one of my earliest readings at Berkeley, Kroeber’s 1948 textbook, in which I found a sentence setting forth the task of anthropology as the continual redefinition of the nature of human nature. This idea struck me as very close to one of the core ideas of Malinowski’s model of culture: that culture is grounded in the human organism, in human needs and capacities, and in the individual as the carrier of culture.

Today, it is clear that human nature continues to undergo redefining. As cultures have become more complex, our species’ role in nature has changed. Humankind no longer exists in partnership with other species in a pristine and provident world; rather, we have overpopulated the globe and are rapidly destroying the environment and jeopardizing our own survival. Furthermore, the human organism is being altered medically, surgically, genetically, and cybernetically, all at an increasing velocity. As medicine and medical sciences have developed, there are new challenges to earlier definitions of those aspects of human existence that once seemed most stable—the nature of life, of birth, of longevity, of pain, of death, of consciousness, of personhood, of wholeness. Today I will speak a little about some of these changes and the tasks they pose for medical anthropology. First, though, I want to mention a concept that seems to me to be central to any such discussion, that of ambiguity.

### **Ambiguity in the Culture of Medicine**

It has been claimed by many anthropologists that the primary function of culture is to provide consistency, coherence, and meaning out of what would otherwise be a random assemblage of natural objects, living beings, and their artifacts, both tangible and intangible. The position I take in this discussion, however, is that cultural inconsistencies are inevitable, for three reasons.

First, change is universal, and new cultural items and innovations are not always consistent with previous organization of the culture. Second, the diverse institutions of a society, religion, law, medicine, kinship, polity, economics, must each function according to a system of values that cannot but differ from those supporting other institutions. These, as one writer has said, are not reducible to a single core pattern (Levine 1985). Third, as Tony Wallace pointed out (1961), the content of no culture is uniformly shared by its members. Role differentiation, specialization, competition, individual differences, and variations in learning all contribute to diversity.

The diverse character of a society makes it highly probable that those living in that society will experience constant and complex conflicts of interest, value, and choice. Life everywhere is fraught with ambiguities. In post-industrial western societies, however—and especially in America—there is a markedly low tolerance for ambiguity. As a nation we pride ourselves on being plainspoken, straightforward, with strong reliance on definitions of reality

based in a rational science and the rule of law. Ambiguity is a problem for us.

Ambiguity is most problematic for a society in cases where someone's destiny depends on the way in which he or she is classified. Every society places utmost importance on certain categories of existence. The most universal is probably the distinction between the living and the dead. But there are other nosological placements that can have nearly as great an impact on the welfare or survival of human beings. Is this man guilty or innocent of a heinous crime? Is this spontaneously aborted fetus of 20 weeks a person or the unfortunate detritus of a failed pregnancy? Is this newborn with indeterminate genital organs a male or a female? Is this madwoman formerly raving in her own personal Bedlam, but now a quietly docile though over-medicated patient, sane or insane? Is this prisoner now so seemingly contrite, safe to return to the company of those whom he formerly robbed or maimed? If he is paroled, is he no longer a criminal?

I became more acutely aware of the function of such symbolic constructions in medicine when I was teaching a medical anthropology seminar a few years ago. A graduate student in nursing asked about the use of "slow codes" as a procedure in the resuscitation of hospital patients. This term is used to notify emergency staff that a patient who is known to be terminally ill and close to death has gone into cardiac or respiratory failure, but there is no doctor's order on the chart that the patient should *not* be placed on a life support system. If such an order were given, it would be called a "do not resuscitate order." A slow code, on the other hand, is a verbally conveyed order—never put in writing—to staff to proceed slowly, to walk rather than run to the patient's bedside with emergency equipment. The purpose of such a code is to avoid making an overt decision to let such a patient die without taking heroic measures, and yet to give the patient a chance to die in peace without further interventions. Thus, medical culture has contrived an institution, an arbitrary resolution of the ambiguous situation: is this patient dying or not?

My student was angry because she felt "tricked." This was her term. Many nurses, who after all had the immediate responsibility of providing emergency resuscitation treatment, did not know the attending physician's intentions. They resented the failure of the doctors to resolve the ambiguities inherent in such cases, and wondered if the use of such a trick were not somehow unethical. Recently medical anthropologists have discussed the use of "do not resuscitate" orders in the training of residents in internal medicine as a means for avoiding the final classification of a patient as "dying" (Muller 1992, Muller and Koenig 1988).

All cultures have means of dealing with true dilemmas, since failing to do so sets the stage for unresolvable confusion and conflicts among partisans of opposing definitions of reality. A number of examples come to mind: a familiar one is trial by ordeal in cases of witchcraft, to determine guilt or innocence in the absence of evidence. I would like to discuss four of the many areas of ambiguity that present our society with a set of ethical dilemmas. These areas arise in large part through new medical discoveries and technological developments of recent decades. In most of these instances the conflicts have not yet been resolved; that is, no generally accepted institutional inventions have been contrived to provide arbitrary answers. In western societies we usually allow familiar institutions (law, medicine and religion for the most part) to manufacture resolutions, but they are still in process.

First, what is a person, a full human being, at the beginning of life, at the end of life, and with respect to the quality of life? Second, how long should people live, and what should their lives be like? How much of a society's resources should be devoted to methods for creating or prolonging life? What is death? Third, what is the connection of the person with the body? Does

cosmetic alteration or organ transplantation alter this connection? Fourth, how much risk, and what kinds of risk, should people take, or be required to take? How much freedom should be sacrificed for the sake of safety and health?

Before I begin to discuss these topics, I should make a few disclaimers. I will have to limit this presentation to only a few topics, so I will not deal with work anthropologists have done in biocultural research, mental illness, delivery of services, or alternative medical systems. Let me also say that I am speaking today using the contributions of hundreds of my colleagues in medical anthropology, whose work I have freely drawn upon in this discussion.

## **The Nature of Personhood**

Anthropologists have been studying the cultural patterning of childbirth and perinatal behavior for over two decades (Browner and Sargent 1990; Jordan 1978; Martin 1987; McClain 1975, 1982; Michaelson 1988; Newman 1970, 1972, 1975). They have clearly demonstrated that reproductive behavior is as highly patterned culturally as is any other aspect of human action.

In western society today, the crux of the abortion controversy lies in the ambiguity about the definition of a person at the beginning of life. Is it at conception, at quickening, at birth (Doerr and Prescott 1989, Morgan 1989)? There is no question that medical technology has influenced some of these beliefs. Unnumbered Americans who watch public television have seen actual video recordings of ovulation, spermatogenesis, fertilization, embryonic development, and birth, all in living color on their living room screens. This visualization technology, along with the introduction of elaborate fetal monitoring equipment, has given us a new cultural image of a fetus as a creature separate from its mother, a view that contributes to the abortion debate (Bassett 1988, Petchesky 1987).

The question of when and under what circumstances a fetus is regarded as an individual separate in some legal or moral sense from its mother led to research on court ordered caesarian sections (Irwin and Jordan 1987). The use of medical technology, they found, was crucial in the decisions that caesarians were essential, and that women could be forced to undergo them. While there are many cultural implications of these and similar cases, the aspect of them that relates to personhood is the resolution of a conflict between doctors and patients by legally defining the fetus as a separate individual; in some of the cases, "custody" of the unborn child was granted to an attorney, or it was made a "ward of the court," and the surgery was then performed over parental objection. With courts entering the scene to support this precedent, it is now possible to accuse a woman of child abuse if she smokes or consumes alcohol during a pregnancy.

The development of techniques of amniocentesis and chorionic villus sampling for identifying certain genetic markers of embryos soon after conception has made it possible to perform selective abortions. This scientific advance has created more than one ethical problem. First, should pregnancies be aborted because the resultant child may be abnormal in some sense, with Downs syndrome, with Tay-Sachs disease, with sickle cell anemia, or perhaps anencephalic (Rapp 1988)? But with this technology, children who are merely undesirable though not impaired can also be aborted, for example to eliminate births of an unwanted gender. The preference for male children in patrilineal societies has caused concern among demographers that through selective aborting of female embryos an undesirable gender ratio is being produced that will have serious social repercussions in years to come. Spallone and Lynn (1987) present a fuller discussion of these issues.

It has been suggested by some ethicists that a reasonable cutting point for assignment of a

fetus to the category of person is the age at which such a fetus could survive independent of its mother. This would be a possible resolution of the question except for the fact that we now have such efficient life support systems for premature infants that extremely low birth weight infants who could never survive on their own in the normal course of events are kept alive with ever more invasive and complex machinery. A number of anthropologists have dealt with this issue (Guillemin and Holmstrom 1986, Jennings 1990, Levin 1990).

New in vitro fertilization technologies have spawned a host of ethical dilemmas, from such surrogacy issues as in the Baby M case, conflicts between divorcing parents over ownership of frozen embryos, a grandmother's incubation of her hysterectomized daughter's fertilized ovum, and on and on. The social and emotional consequences of continued infertility is also the subject of anthropological study (Becker 1990, Becker and Machtigall 1991).

We are beginning to examine the impact of these reproductive innovations on concepts of kinship and definitions of family. In early February 1991, a conference was held at the University of Southern California on the subject of "Families by Design." Physicians, ethicists, and social scientists all contributed to this discussion. Some of the topics discussed were the implications of surrogacy for children; issues about gay and lesbian parents; multi-fetal pregnancies and their management, and the rationale for infertility services (Weston 1991, Wikler and Wikler 1991).

In contrast to issues of child survival (Justice 1991; Korbin 1981; Nations and Rebhun 1988; Scheper-Hughes 1984, 1987) is the question of overpopulation. The programs advocating wider use of birth control techniques were among the first dealing with reproduction to come to the attention of applied anthropologists (Marshall 1977, Newman 1972, Polgar 1971, Scrimshaw 1980). While some have served as advisors in a number of birth control programs, there are some serious ethical issues about the selection of cultures and communities to which such programs are directed, by and large ethnic minority communities in the United States and countries of Asia, Africa, and Latin America. The issue is whether or not such programs represent a type of racism or even genocide aimed at reducing the proportions of ethnic minorities in the general population or of non-whites in the world at large (Newman 1972).

Without question, our fragile planet cannot support an infinitely expanding human population, but the selection of types of population to be held in closest check seem to be dictated more by issue of political economy than by humane choices and equity.

## **The Life Span**

My next topic deals with the prolongation of life. Some of the conflicts about extension of the life span are a direct result of the fact that ageism is rampant in our society. We have long had a youth-centered culture, yet our population is rapidly aging. Vastly improved public health, nutrition, environmental sanitation, and medical care have made it possible for many more people to live out a full life span. Since many more people live to be old and are therefore no longer in the work force, they tend to be regarded as a surplus population, absorbing a disproportionate share of money and services.

These demographics have been well known for a half-century, and have led to the development of the relatively new subfields of geriatrics in medicine and gerontology in social and behavioral science. Anthropologists were somewhat later getting into this field than either sociologists or psychologists, for reasons that I have discussed elsewhere (Clark 1967; also see Clark and Anderson 1967; Cohen and Sokolovsky 1989; Fry 1980, 1981; Kaufman 1987; Keith

1980; Keith and Kertzer 1984; Myerhoff and Simic 1978).

The health status of the over-60 population is fraught with ambiguities. First of all, there is a confusion in the minds of many people, including physicians, between age and illness. It is true that, as a result of increases in longevity, more people than was once the case live with chronic illnesses. But most older people are not ill, and many of those who are ill have quite treatable and even reversible disorders. The confusion, however, leads to what gerontologists refer to as medical nihilism, often expressed in reluctance to treat an older patient for what is labelled as "just being old."

Medical anthropologists have turned their attention to such issues as stroke rehabilitation (Kaufman 1988), urinary incontinence (Mitteness 1987), diabetes (Urdaneta and Krehbiel 1989a, 1989b) and hypertension (Janes 1986). By and large their findings indicate that such conditions show an enormous range of stabilization, reversibility, and recovery depending upon a complex of family and other social factors.

Just as there are questions about the full humanity of the organism at the beginning of life, so there are similar issues near the end of life. Along with many other gerontologists, I have written about the loss of a sense of self among aged persons in American culture, as well as the less than full personal status that is accorded to the elderly by society at large. Given the system of values we have with a high premium on work, productivity, financial independence and personal autonomy, elders are by definition undervalued both by themselves and by others (Clark and Anderson 1967). Ethnographic studies of aging in other cultures do not show as great a contrast to American patterns as we once thought they would (Amoss and Harrell 1981, Barker 1990, Counts and Counts 1985, Fry 1981, Sokolovsky 1982).

The inherent ageism in our society is intensified when issues arise concerning the mental or physical competence of older persons. An extreme case is that of Alzheimer's Disease. As this dreadful malady begins to rob an individual of memory, of judgment and ability to reason and solve problems, and finally of ability to recognize even close kin or to be aware of the nature of surroundings, is the human being in the body lost? How much of a person remains when much of the former personality is irrevocably altered?

In much of the research that anthropologists have done on patients in nursing homes, there is no doubt that patients with psychological or serious physical disabilities are dehumanized (Kayser-Jones 1990). Serious concerns among gerontologists about mistreatment and exploitation of the aged have led to some anthropological study of the problem of what has come to be called elder abuse (Iris and Segal 1989).

There is a long-standing ethical debate over issues of right to die. The recent criminal proceedings taken against the so-called "suicide doctor" have been reported widely in the press and have accentuated public concerns over whether or not termination of life is a basic human right. Many older people are themselves very concerned that they not be kept alive by the use of extraordinary measures when their quality of life has become so poor that death is preferable. Many are also concerned about the cost to their families of such measures, and the institution of the so-called "living will" has arisen in some states to return some control over the dying process to the individual.

While the high cost of terminal care concerns many individuals and families, it is also a matter that involves society as a whole. Ethical questions around longevity relate to allocation of resources (Brown in press). It is not uncommon for terminal hospitalizations to cost close to \$100,000, and much of this outlay provides extremely ill aged people only a few extra months, weeks, or even days of life. If organ transplants are involved, the bill can sometimes reach a

million dollars.

It has become very difficult to die quickly in an American hospital. Medical science has introduced two new technologies that have created, and continue to create, dilemmas for physicians, for seriously ill people and their families, and for those charged with protecting the ethical and legal rights of patients. The first of these is the invention of machinery for the artificial prolongation of life. At one time it was possible for everyone to agree, by witness of their own senses, that a death had occurred. The signals of death were well-known: the silent heart, the stilled breath, the cold and unanswering flesh. Now there are machines that push oxygen in and pull carbon dioxide out of the lungs, that rhythmically shock the heart into a physiologically normal beat, that keep the blood warmed and flowing through the tissues of the body, and the skin pink and resilient—even if human consciousness and sensibility are forever lost to an irreversible coma.

In view of these developments, physicians began about 30 years ago to propose new definitions of legal death, based on the use of another medical invention, the electroencephalogram. This new definition of death is clearly a useful one in medicine, but much of the general public, from whose ranks comatose or dying patients and their grieving families are drawn, is generally uninformed about these discussions and their implications. This is particularly true in cultures where the seat of the self or the soul is believed to be housed not in the brain, as western biomedicine would have it, but in the heart, the breath, or in the body as a whole (Clark 1989, Lock and Honde 1990).

The concept of brain death makes it possible for medical people to keep bodies in a live vegetative state until their valuable internal organs can be removed for transplantation. Organ transplant technology has emerged as one of the most ethically ambiguous of areas in modern medicine (Caplan 1988; Marshall 1992; Plough 1981, 1986; Sharp 1991). There are questions about the objectification of the body. When medicine can do so much to the human body in order to preserve it through a full life span, attitudes toward manipulations of the body come to be acceptable to an ever larger public. The body comes to be seen as a temporary apartment, easily remodelled for the health and comfort of the occupant. The development of both organ transplantation and artificial prostheses reinforce the concept of body parts as commodities (Clark 1989).

A second ambiguity is the relation of medicine, and especially high technology medicine to the forces of a market economy. As one writer has put it, critics of western medicine vary in their assessments, some seeing it as a socially unresponsive institution captured by the political and economic forces of capitalism, while others point to the fact that physicians are caught in a constant dialogue between altruism versus the profit motive (Plough 1986). Others have demonstrated that market forces are factors in the promotion of even ineffective technologies (Koenig 1988). And all agree that there are enormous inequities by gender, race, and class in access to technologies such as transplantation and artificial organs (Fox and Swazey 1974).

### **Bodily Alterations**

Not long ago I went with a friend to do some shopping, and became acquainted with a type of store that I have rarely had occasion to enter, a shop that sells various types of augmented garments. At first I was astonished at the variety and ingenuity of the goods available for changing the bodily profile, but finally realized that these high-tech products are simply updated solutions to a persistent human concern with the size, shape, and proportion of bodily parts.

As anthropologists know, people in virtually all societies have standards of attractiveness and criteria of bodily perfection that vary widely from one society to another. Bodies are manipulated in various ways, usually by temporary modifications such as body painting or other ornamentation, but frequently through more invasive and permanent alterations. Some changes are designed to render the individual more ritually acceptable, some to bestow a mark of social affiliation, and many to enhance sexual attractiveness or beauty, as it is defined in the culture. We can think of dozens of examples: tattooing, scarification, piercing of earlobes, lips, or nostrils, or even nipples in the leather bar culture of urban America. There is tooth filing or excision, enlargement of lips or earlobes, flattening of heads, and stretching of necks. And there are those other more draconian measures such as foot binding, subincision, circumcision, or infibulation (Polhemus 1978).

In our own society, the miracles of plastic surgery have enabled those who are sufficiently affluent to modify their bodies in new and inventive ways. Of course, a great deal of the work plastic surgeons do is to restore damage done to the body by mutilating accidents, severe burns, and the disfigurements of cancer surgery or other essential therapies. But an increasing number of procedures are unrelated to health or functional status. These purely cosmetic alterations are analogous to the less invasive efforts of people of color in previous decades to straighten their hair or bleach their skin to a lighter shade. All are demonstrations of internalized oppression and self-disapproval.

In the years following the close of World War II, there was a flurry of interest in Japan in removal of the inner epicanthic fold in order to simulate a European round eye shape. Nose jobs to alter a Semitic profile have been sought by many Jews in anti-semitic societies. In the youth-oriented American culture, face lifts, breast lifts, and hair implants for baldness have become a profitable industry. Elizabeth Taylor's most recent face lift in connection with her 60th birthday and penultimate wedding made the front page of my newspaper. Since even moderate obesity has become regarded as a disfigurement in many sectors of our society, the use of liposuction and "body sculpture" has opened a vast new market for plastic surgeons. In every one of these cases, the hope of the client is to obliterate the stigmata of belonging to an oppressed group.

Because sexuality is so primary an aspect of human life, enhancing sexual attractiveness is the incentive for much body alteration. We diet, change the color of our hair, spend millions of dollars on cosmetic preparations and beautifying treatments, and now consider the option of surgical augmentation of sexual markers. Thousands of American women, most conspicuously those in the performing arts, have availed themselves of silicone breast implants, now much in the news because of debates over their health risks. American men have had pectoral implants, or have had their calf muscles enlarged with this artificial material. There have been many scandals over the use of injectable steroids for enhancement of musculature, primarily among professional athletes.

Since Christine Jorgensen returned to the United States from Denmark in the 1950s as the first recipient of a sex-change operation, there have been thousands of such surgeries performed in the world. There has been considerable outcry in the public media and discussion in medical publications about the ethics of sex-change surgery (Benthall 1974, 1986; Garfinkel 1967; Wikan 1977). In some ways, such genital alteration arouses the same kind of impassioned reaction among members of the public as does the existence of female circumcision and infibulation procedures in Sudanese and other North African cultures, no doubt because sexuality itself is such a charged topic.



The two situations differ, however, in that in the case of sex change, the recipient of the alteration is the instigator, and must be an autonomous adult in order to qualify for the surgery. Society merely countenances the act, acknowledging the individual's decision as primary. In the other case, a whole society justifies the procedures through social and religious sanctions. The recipients, however, are minor children who have no voice in the matter. Despite the differences in the two cases, they both raise a serious moral ambiguity that is not yet resolved: should cultural ideals and conventions give license to physicians or other authorities in a society to perform bodily alterations that either cause pain and loss of function or expose the subject to serious risk of maiming, deformation, or even death?

The conflict between western critics and representatives of North African cultures over clitoridectomy and infibulation is a prototype of this ambiguity (Gordon 1991, Gruenbaum 1982). The conflict has many facets, and even reaches into the ranks of medical anthropology itself, as witness a discussion in a recent issue of the *Medical Anthropology Quarterly* of the practice and its challenge to cultural relativism and the hubris of advocated intervention by outside critics of Oriental societies (Morsy 1991). As one anthropologist has written in this discussion (Sargent 1990), it is doubtful if any anthropologist, even acknowledging the pressure of social and religious values, would condone the maiming of female children, exposing them to hemorrhage, septicemia, shock, urine retention, and other serious health hazards (Lieban 1990). But the solution is not a simple one, and the consensus, even among feminist scholars, is that it is best dealt with by members of the societies in which it occurs.

To put this conflict in even broader terms, how can the ambiguity be resolved between the right to freedom of either individuals or groups and the risk to survival? This ambiguity goes beyond issues in body alterations; it is also at the root of partisan struggles in our society over other dangers: gun control; availability and use of tobacco, drugs and alcohol; and sexual freedom in the age of the AIDS epidemic. Let me turn now to discuss some of those issues.

## **Risk and Freedom**

Ever since ancient times, people have been making efforts to control the amount of danger they are exposed to in their lives. Lepers were banished from towns and cities; witches were burned; offerings were made to gods or ancestors in the hope of eliciting their protection against flood, famine, and pestilence. As notions of causality have been informed by new discoveries, people evolve new views of what constitutes a threat to survival, health, or security.

Two major factors influence these definitions of risk. One is the degree of vulnerability people feel, either because of a perceived threat in the environment, or from a sense of being unworthy of the protections that others deserve. A second factor is the nature and interpretation of information provided. Perception of risk, as social scientists have shown, is less a function of actual danger than it is of political, economic, and cultural contexts (Douglas and Wildavsky 1982, Nelkin 1989). Information concerning the nature of danger may be withheld from those most at risk, as in the case of failure to inform patients that they have a potentially fatal disease (Gordon 1991), or workers that they are being exposed to deadly industrial substances (Michaels 1988, Siskind 1988).

One of the major characteristics of postindustrial western society is the reliance on mass media of communication for information. Numerous studies have shown that most Americans get information about health risks and environmental hazards not from medical personnel or scientific

reports, but from television, newspapers, magazines, and word of mouth, more or less in that order (Nelkin 1989). People may disagree about the extent to which the news is managed by business and industry leaders intent on increasing their profit margins, but there is no question that risk perception can be either minimized or maximized by the nature of reporting (Nelkin 1989).

Much of the information we receive in the public media about safety and threat is contradictory, sporadic, incomplete, or slanted to protect vested political and economic interests. As a result, most people are unable to assess risks in any reasonable way. The resultant ambiguities are frequently indeterminate, and people come to rely on magical thinking to resolve the dilemmas. Such thinking is often characteristic of people who continue to smoke cigarettes even in the face of bountiful epidemiological evidence pointing to high risk of developing cancer or other lung diseases. It is a type of gambling that one will beat the odds, that one has special protection, or will just be one of the lucky ones. Promiscuous sexual behavior in the midst of the AIDS epidemic is another example.

In view of the reluctance of many people to agree with widely held estimates of health risk, there are sentiments that they should be protected from their own self-destructive tendencies by some legally mandated controls. Restriction of smoking from certain public areas, limitations on alcohol sales to minors or during certain hours of the day or night, and the continued criminalization of narcotics are cases in point. The many social and cultural issues in drug and alcohol use and abuse have been a major field of investigation for medical and general anthropologists during the past several decades (Ablon 1990; Agar 1973; Ames and Janes 1987; Bennett 1988; Bennett and Ames 1985; Dobkin de Rios 1984; Douglas 1987; Heath 1976, 1987; Levy and Kunitz 1974; Marshall 1979; Spradley 1970; Stein 1990; Weibel-Orlando 1984, 1987; Weidman and Page 1982).

With respect to drug use, the debates over clean needle programs illustrate the nature and depth of the ambiguities in this sphere of life. There are those whose major concern is stopping the spread of disease through needle-sharing among addicts, while others are more concerned with addiction itself. Those holding this latter view claim that distribution of hypodermic needles on the streets will simply encourage greater use of drugs and increase the numbers of addicts. Different localities have made their own arbitrary choices in this conflict, but there is not enough information available to inform logical decisions.

The ambiguity in the situation of legal prohibitions arises from a conflict between the values of safety and freedom. We can see the conflict most clearly in instances of attempts to restrict the movements and behavior of persons with communicable diseases. The early ostracism of lepers, the quarantine of plague victims during the Middle Ages, the intermittent incarceration of prostitutes for venereal disease testing, and the seclusion of persons with AIDS in present-day Cuba are all examples of attempts of societies to protect its uninfected members from contagion.

Since AIDS is becoming the foremost public health concern of this generation, many social as well as medical scientists have turned their attention to the complexities of this baffling and tragic epidemic. A number of issues bearing on the crisis have engaged anthropologists (Bateson and Goldsby 1988; Feldman 1990; Gorman 1986a, 1986b; Juengst and Koenig 1989; Marshall and Bennett 1990; Page 1990; Singer et al. 1990; Stall et al. 1990; Stall, Coates, and Hoff 1988). Some of these studies relate to the social or sexual ethnography of HIV transmission; some to the scapegoating of stigmatized populations at high risk of infection; some to fears of contracting the disease, originally health providers from patients (Koenig and Cooke 1989), and more recently, patients from health providers (Marshall, O'Keefe, and Fisher 1990;

Marshall, Thomasma, and O'Keefe 1991).

The AIDS epidemic has highlighted in a most dramatic way some of the basic ambiguities in our society—the conflicting values of freedom and safety; the choice between responsibility to society and minimizing personal danger. Solutions are nowhere in sight.

## Conclusion

Let me conclude by returning to the title of this lecture. In the course of our lifetimes, earlier conceptions of human life and its character are being challenged. The beginning of life, the emergence of a person, the nature of death, the integrity of the organism, the life span, notions of the normal body, the meaning of parenthood, the definition of family and kinship—all are undergoing shifts in meaning under the steady onslaught of medical discovery and technological development. All of these new definitions give rise to ambiguities and moral conflicts. Our society, however, is highly intolerant of ambiguity and strives to resolve dilemmas either by the creation of cultural fictions to contain the uncertainty, or the assignment of problems to arbiters, usually our adversarial courts, for adjudication based on precedence or rules. These solutions may not be universally acceptable, and factionalism and partisan conflict extend the strife.

With the advent of a multidisciplinary field of bioethics, anthropologists are finding a critical role in attending to those aspects of bioethical discourse that render intelligible and significant “the social meaning of human suffering and the cultural dimensions of healing” (Marshall 1992:62).

In our roles as applied anthropologists, we can go further. It is we who have long worked against oppression in our own society and throughout the world. Our work continues to expose those aspects of society that promote oppression and deprivation. Women are being deprived of control over their own bodies and their own reproduction. The aged are being deprived of a dignified later life and a humane death. Sexual minorities are being deprived of freedom from stigma and adequate and prompt attention to their ills. The poor are being deprived of equity in medical care and protection from the noxious byproducts of a greedy industrial economy.

As we apply our skills to these problems, we will help preserve what is most humane in human life.

## References Cited

- Ablon, Joan  
1990     Ambiguity and Difference: Families with Dwarf Children. *Social Science and Medicine* 30:879–887.
- Agar, Michael  
1973     *Ripping and Running*. New York: Academic Press.
- Ames, Genevieve, and Craig Janes  
1987     Heavy and Problem Drinking in an American Blue Collar Population: Implications for Prevention. *Social Science and Medicine* 25:949–960.
- Amoss, Pamela T., and Steven Harrell  
1981     Introduction: An Anthropological Perspective on Aging. *In Other Ways of Growing Old*. P. Amoss and S. Harrell, eds. Pp. 1–24. Stanford: Stanford University Press.

- Barker, Judith  
1990 Between Humans and Ghosts: The Decrepit Elderly on a Polynesian Island. *In* The Cultural Context of Aging: Worldwide Perspectives. Jay Sokolovsky, ed. Pp. 295–313. Boston: Bergin & Garvey.
- Bassett, Ken  
1988 The Fetal Patient. Paper presented at annual meeting of the American Anthropological Association, Phoenix, AZ.
- Bateson, Mary Catherine, and Richard Goldsby  
1988 Thinking AIDS. New York: Addison-Wesley.
- Becker, Gaylene  
1990 Healing the Infertile Family: Strengthening Your Relationship in the Search for Parenthood. New York: Bantam Books.
- Becker, Gay, and Robert D. Nachtigall  
1991 Ambiguous Responsibility in the Doctor-Patient Relationship: The Case of Infertility. *Social Science and Medicine* 32:875–885.
- Bennett, Linda  
1988 Alcohol in Context. *Drugs and Society* 2:89–131.
- Bennett, Linda, and Genevieve Ames  
1985 The American Experience with Alcohol: Contrasting Cultural Perspectives. New York: Plenum.
- Benthall, Jonathan  
1974 The Body as a Medium of Expression. London: Allen, Lane and Dutton.  
1986 Modern Surgery as a Form of Body Alteration. *Anthropology Today* 2:16–17.
- Brown, Kate  
In press Death and Access: Ethics in Cross-Cultural Health Care. *In* Making Choices. Emily Friedman, ed. Chicago: American Hospital Association Publishing Company.
- Browner, Carole, and Carolyn Sargent  
1990 Anthropology and Studies of Human Reproduction. *In* Medical Anthropology: Contemporary Theory and Method. Thomas Johnson and Carolyn Sargent, eds. Pp. 215–229. New York: Praeger.
- Caplan, Arthur  
1988 Ethical Issues in the Use of Anencephalic Infants as a Source of Organs and Tissues for Transplantation. *Transplantation Proceedings* 20:42–49.
- Clark, Margaret  
1967 Anthropology and Aging: A New Field for Culture and Personality Studies. *Gerontologist* 7:55–64.  
1989 The Body as Commodity: Ritual Dismemberment in Contemporary Biomedicine. Paper presented at the annual meeting for American Anthropological Association, Washington, DC.
- Clark, Margaret, and Barbara G. Anderson  
1967 Culture and Aging: An Anthropological Study. Springfield, IL: Charles C. Thomas.
- Cohen, Carl, and Jay Sokolovsky  
1989 Old Men of the Bowery: Strategies for Survival Among the Homeless. New York: Guilford.
- Counts, Dorothy, and David Counts, eds.  
1985 Aging and its Transformations: Moving Toward Death in Pacific Societies. Lanham, NY: University Press of America.

- Dobkin de Rios, Marlene  
1984 *Hallucinogens: Cross-Cultural Perspectives*. Albuquerque: University of New Mexico Press.
- Doerr, Edd, and James Prescott, eds.  
1989 *Abortion Rights and "Fetal Personhood."* Long Beach, CA: Centerline Press.
- Douglas, Mary, ed.  
1987 *Constructive Drinking*. Cambridge: Cambridge University Press.
- Douglas, Mary, and Aaron Wildavsky  
1982 *Risk and Culture*. Berkeley: University of California Press.
- Feldman, Douglas A., ed.  
1990 *Culture and AIDS*. New York: Praeger.
- Fox, Renee C., and Judith P. Swazey  
1974 *The Courage to Fail: A Social View of Organ Transplants and Dialysis*. Chicago: University of Chicago Press.
- Fry, Christine L.  
1980 *Aging in Culture and Society: Comparative Viewpoints and Strategies*. New York: Praeger.  
1981 *Dimensions: Aging, Culture and Health*. New York: Praeger.
- Garfinkel, Harold  
1967 *Passing and the Management of Sex Status in an Intersexed Person*. In *Studies in Ethnomethodology*. Harold Garfinkel, ed. Pp. 214–227. New York: Prentice-Hall.
- Gordon, Daniel  
1991 *Female Circumcision and Genital Operations in Egypt and the Sudan: A Dilemma for Medical Anthropology*. *Medical Anthropology Quarterly* 5:3–14.
- Gordon, Deborah R.  
1991 *Culture, Cancer and Communication in Italy*. In *Anthropologies of Medicine: A Colloquium on West European and North American Perspectives*. Beatrix Pfleiderer and Gilles Bibeau, eds. Pp. 137–156. Heidelberg: Vieweg, Bertelsmann Publishing Group International.
- Gorman, E. Michael  
1986a *The AIDS Epidemic in San Francisco: Epidemiological and Anthropological Perspectives*. In *Anthropology and Epidemiology*. Craig Janes, Ron Stall, and Sandra Gifford, eds. Pp. 157–172. Dordrecht: D. Reidel.  
1986b *Introduction: Anthropology and AIDS*. *Medical Anthropology Quarterly* 17:31–32.
- Gruenbaum, Ellen  
1982 *The Movement Against Clitoridectomy and Infibulation in Sudan: Public Health Policy and the Women's Movement*. *Medical Anthropology Newsletter* 13:4–11.
- Guillemin, Jeanne Harley, and Lynda Lytle Holmstrom  
1986 *Mixed Blessings: Intensive Care for Newborns*. New York: Oxford University Press.
- Heath, Dwight  
1976 *Anthropological Perspectives on Alcohol: An Historical Review*. In *Cross-Cultural Approaches to the Study of Alcohol: An Interdisciplinary Approach*. M. Everett, ed. Pp. 41–101. The Hague: Mouton.  
1987 *Anthropology and Alcohol Studies: Current Issues*. *Annual Review of Anthropology* 16:99–120.

- Iris, Madelaine A., and Susan Ringel Segal  
 1989 Strategies for Service Provision: The Use of Legal Intervention in a Systems Approach to Case Work. *In* *Elder Abuse: Practice and Policy*. Rachel Filenson and Stanley Ingman, eds. Pp. 104–116. New York: Human Science Press.
- Irwin, Susan, and Brigitte Jordan  
 1987 Knowledge, Practice and Power: Court-Ordered Cesarean Sections. *Medical Anthropology Quarterly* 1:319–334.
- Janes, Craig R.  
 1986 Migration and Hypertension: An Ethnography of Disease Risk in an Urban Samoan Community. *In* *Anthropology and Epidemiology*. C. Janes, ed. Pp. 175–212. Dordrecht: D. Reidel.
- Jennings, Bruce  
 1990 Ethics and Ethnography in Neonatal Intensive Care. *In* *Social Science Perspectives on Medical Ethics*. George Weisz, ed. Pp. 261–272. Philadelphia: University of Pennsylvania Press.
- Jordan, Brigitte  
 1978 *Birth in Four Cultures*. Montreal: Eden Press Women's Publications.
- Juengst, Eric T., and Barbara Koenig  
 1989 The Meaning of AIDS: Implications for Medical Science, Clinical Practice, and Public Health Policy. New York: Praeger.
- Justice, Judith  
 1991 Cultural and Political Dimensions of the Child Survival Revolution. Paper presented at the annual meeting of the American Anthropological Association, Chicago.
- Kaufman, Sharon  
 1987 *The Ageless Self*. Madison: University of Wisconsin Press.  
 1988 Toward a Phenomenology of Boundaries in Medicine: Chronic Illness Experience in the Case of Stroke. *Medical Anthropology Quarterly* 2:338–345.  
 1993 *The Healer's Tale: Transforming Medicine and Culture*. Madison: University of Wisconsin Press.
- Kayser-Jones, Jeanie  
 1990 The Use of Nasogastral Feeding Tubes in Nursing Homes: Patient, Family and Health Care Provider Perspectives. *Gerontologist* 30:469–479.
- Keith, Jennie  
 1980 "The Best is Yet to Be": Toward an Anthropology of Age. *Annual Review of Anthropology* 9:339–364.
- Keith, Jennie, and David Kertzer, eds.  
 1984 *Aging and Anthropology Theory*. Ithaca: Cornell University Press.
- Koenig, Barbara  
 1988 The Technological Imperative in Medical Practice: The Social Creation of "Routine" Treatment. *In* *Biomedicine Examined*. Margaret Lock and Deborah R. Gordon, eds. Pp. 465–496. Boston: Kluwer Academic Publishers.
- Koenig, Barbara, and Molly Cooke  
 1989 Physician Response to a New, Lethal, and Presumably Infectious Disease: Medical Residents and the AIDS Epidemic in San Francisco. *In* *The Meaning of AIDS: Implications for Medical Science, Clinical Practice and Public Health Policy*. Eric Juengst and Barbara Koenig, eds. Pp. 72–85. New York: Praeger.

- Korbin, Jill  
1981 Child Abuse and Neglect: Cross Cultural Perspectives. Berkeley: University of California Press.
- Kroeber, Alfred  
1948 Anthropology. New York: Harcourt Brace.
- Levin, Betty W.  
1990 International Perspectives on Treatment Choice in Neonatal Intensive Care. *Social Science and Medicine* 30:901–912.
- Levine, Donald N.  
1985 The Flight from Ambiguity: Essays in Social and Cultural Theory. Chicago: University of Chicago Press.
- Levy, Jerrold, and Steven Kunitz  
1974 Indian Drinking: Navajo Practices and Anglo-American Theories. New York: Wiley.
- Lieban, Richard  
1990 Medical Anthropology and the Comparative Study of Medical Ethics. *In Social Science Perspectives on Medical Ethics*. George Weisz, ed. Pp. 221–240. Philadelphia: University of Pennsylvania Press.
- Linton, Ralph  
1936 The Study of Man. New York: D. Appleton-Century.
- Lock, Margaret, and Christina Honde  
1990 Reaching Consensus About Death: Heart Transplants and Cultural Identity in Japan. *In Social Science Perspectives on Medical Ethics*. George Weisz, ed. Pp. 99–120. Philadelphia: University of Pennsylvania Press.
- Malinowski, Bronislaw  
1949 The Dynamics of Culture Change: An Inquiry into Race Relations in Africa. New Haven: Yale University Press.
- Marshall, John F.  
1977 Acceptability of Fertility Regulating Methods: Designing Technology to Fit People. *Preventive Medicine* 6:65–73.
- Marshall, Mac, ed.  
1979 Beliefs, Behaviors, and Alcoholic Beverages: A Cross-Cultural Survey. Ann Arbor: University of Michigan Press.
- Marshall, Patricia  
1992 Anthropology and Bioethics. *Medical Anthropology Quarterly* 6:49–73.
- Marshall, Patricia, and Linda Bennett  
1990 Anthropological Contributions to AIDS Research. *Medical Anthropology Quarterly* 4:3–5.
- Marshall, Patricia A., J. Paul O’Keefe, and Susan Gross Fisher  
1990 Touch and Contamination: Patients’ Fear of AIDS. *Medical Anthropology Quarterly* 4:129–144.
- Marshall, Patricia, David C. Thomasma, and J. Paul O’Keefe  
1991 Disclosing HIV Status: Ethical Issues Explored. *Journal of the American Dental Association* 122:11–15.

- Martin, Emily  
1987     The Woman in the Body. Boston: Beacon Press.
- McClain, Carol Shepherd  
1975     Ethno-obstetrics in Ajijic. *Anthropological Quarterly* 48:38–56.  
1982     Toward a Comparative Framework for the Study of Childbirth: A Review of the Literature. *In* *Anthropology of Human Birth*. Margarita Kay, ed. Pp. 25–29. Philadelphia: F. A. Davis Co.
- Mead, Margaret  
1935     Sex and Temperament in Three Primitive Societies. New York: Morrow.
- Michaels, David  
1988     Waiting for the Body Count: Corporate Decision Making and Bladder Cancer in the U.S. Dye Industry. *Medical Anthropology Quarterly* 2:215–232.
- Michaelson, Karen, ed.  
1988     Childbirth in America: Anthropological Perspectives. New York: Bergin and Garvey.
- Mitteness, Linda S.  
1987     So What Do You Expect When You're 85?: Urinary Incontinence in Late Life. *Research in the Sociology of Health Care* 6:177–219.
- Morgan, Lynn  
1989     When Does Life Begin? *In* *Abortion Rights and "Fetal Personhood."* Edd Doerr and James W. Prescott, eds. Pp. 97–114. Long Beach, CA: Centerline Press.
- Morsy, Soheir A.  
1991     Safeguarding Women's Bodies: The White Man's Burden Medicalized. *Medical Anthropology Quarterly* 5:19–23.
- Muller, Jessica  
1992     Shades of Blue: The Negotiation of Limited Codes by Medical Residents. *Social Science and Medicine* 34:885–898.
- Muller, Jessica, and Barbara Koenig  
1988     On the Boundary of Life and Death: The Definition of Dying by Medical Residents. *In* *Biomedicine Examined*. Margaret Lock and Deborah Gordon, eds. Pp. 19–57. Boston: Kluwer Academic Publishers.
- Myerhoff, Barbara, and Andrei Simic  
1978     Life's Career-Aging: Cultural Variations on Growing Old. Beverly Hills, CA: Sage Publications.
- Nations, Marilyn, and LindaAnn Rebhun  
1988     Angels with Wet Wings Won't Fly: Maternal Sentiment in Brazil and the Image of Neglect. *Culture, Medicine and Psychiatry* 12:141–200.
- Nelkin, Dorothy  
1989     Communicating Technological Risk: The Social Construction of Risk Perception. *Annual Review of Public Health* 10:93–113.
- Newman, Lucille  
1970     Cultural Factors in Family Planning. *Annals of the New York Academy of Sciences* 175:833–846.  
1972     Birth Control: An Anthropological View. Reading, MA: Addison-Wesley.  
1985     Women's Medicine. New Brunswick, NJ: Rutgers University Press.



- Page, J. Bryan  
1990 Shooting Scenarios and Risk of HIV-1 Infection. *American Behavioral Scientist* 33:478–490.
- Petchesky, Rosalind  
1987 Foetal Images: The Power of Visual Culture in the Politics of Reproduction. *In* *Reproductive Technologies: Gender, Motherhood and Medicine*. Michelle Stanworth, ed. Pp. 290–293. Minneapolis: University of Minnesota Press.
- Plough, Alonzo  
1981 Medical Technology and the Crisis of Experience: The Cost of Clinical Legitimation. *Social Science and Medicine* 15F:89–101.  
1986 *Borrowed Time: Artificial Organs and the Politics of Extending Lives*. Philadelphia: Temple University Press.
- Polgar, Steven, ed.  
1971 *Culture and Population: A Collection of Current Studies*. Cambridge, MA: Schenkman Publishing Company.
- Poihemus, Ted, ed.  
1978 *Social Aspects of the Human Body*. New York: Penguin Books.
- Rapp, Rayna  
1988 Chromosomes and Communication: The Discourse of Genetic Counseling. *Medical Anthropology Quarterly* 2:143–157.
- Sargent, Carolyn  
1990 Confronting Patriarchy: The Potential for Advocacy in Medical Anthropology. *Medical Anthropology Quarterly* 5:24–25.
- Scheper-Hughes, Nancy  
1984 Infant Mortality and Infant Care. *Social Science and Medicine* 19:535–546.  
1987 *Child Survival: Anthropological Perspectives on the Treatment and Maltreatment of Children*. Dordrecht: D. Reidel.
- Scrimshaw, Susan  
1980 Acceptability of New Contraceptive Technology. *In* *Research Frontiers in Fertility Regulation*. Gerald I. Zatuchni, Mariam H. Labook, and John J. Sciarra, eds. Pp. 72–82. New York: Harper and Row.
- Sharp, Lesley  
1991 Surgical Alterations of Bodies and Selves: Organ Transplants in Contemporary America. Paper presented at the annual meeting of the American Anthropological Association, Chicago.
- Singer, Merrill, Candida Flores, Lani Davison, Georgine Burke, Zaida Castillo, Kelley Scanlon, and Migdalia Rivera  
1990 SIDA: The Economic, Social and Cultural Context of AIDS Among Latinos. *Medical Anthropology Quarterly* 4:72–114.
- Siskind, Janet  
1988 An Axe to Grind: Class Relations and Silicosis in a 19th-Century Factory. *Medical Anthropology Quarterly* 2:199–214.
- Sokolovsky, Jay  
1982 *Growing Old in Different Cultures*. Belmont, CA: Wadsworth.

- Spallone, Patricia, and Deborah Lynn, eds.  
1987 *Made to Order: The Myth of Reproductive and Genetic Progress*. Oxford: Pergamon Press.
- Spradley, James  
1970 *You Owe Yourself a Drunk: An Ethnography of Urban Nomads*. Boston: Little, Brown.
- Stall, Ron, Thomas J. Coates, and Colleen Hoff  
1988 Behavioral Risk Reduction for HIV Infection among Gay and Bisexual Men: A Review of Results from the United States. *American Psychologist* 43:878–885.
- Stall, Ron, Suzanne Heurtin-Roberts, Leon McKusick, Colleen Hoff, and Sylvia Wanner Lang  
1990 Sexual Risk for HIV Transmission among Singles-Bar Patrons in San Francisco. *Medical Anthropology Quarterly* 4: 115–128.
- Stein, Howard  
1990 In What Systems do Alcohol/Chemical Addictions Make Sense? Clinical Ideologies and Practices as Cultural Metaphors. *Social Science and Medicine* 30:987–1000.
- Urdaneta, Maria Luisa, and Rodney Krehbiel  
1989a Cultural Heterogeneity of Mexican-Americans and Its Implications for the Treatment of Diabetes Mellitus Type II. *Medical Anthropology* 11:269–282.  
1989b Introduction, Anthropological Perspectives on Diabetes Mellitus Type II. *Medical Anthropology* 11:221–225.
- Wallace, Anthony F. C.  
1961 *Culture and Personality*. New York: Random House.
- Weibel-Orlando, Joan  
1984 Substance Abuse among American Indian Youth: A Continuing Crisis. *Journal of Drug Issues* (Spring):313–335.  
1987 Culture-Specific Treatment Modalities: Assessing Client-to- Treatment Fit in Indian Alcoholism Programs. *In Treatment and Prevention of Alcohol Problems: A Resource Manual*. W. Miles Cox, ed. Pp. 105–113. New York: Academic Press.
- Weidman, Dennis, and J. Bryan Page  
1982 Drug Use on the Street, Health on the Brink. *Urban Anthropology* 11:212–226.
- Weston, Kath  
1991 *Families We Choose*. New York: Columbia University Press.
- Wikan, Unni  
1977 Man Becomes Woman. *Man* 12:304–318.
- Wikler, D., and N. J. Wikler  
1991 Turkey-baster Babies: The Demedicalization of Artificial Insemination. *Milbank Quarterly* 69:5–40; Discussion 41–44.

## **Chapter 23**

### **Ronald Frankenberg: From Industrial Anthropology to AIDS**

**Thomas Weaver**

The research of Ronald Frankenberg, recipient of the Society for Applied Anthropology's Malinowski Award in 1995, covers a wide range of applied fields.<sup>1</sup> He pioneered in industrial anthropology and medical anthropology in Great Britain in the late 1950s. Later he studied prisons and schools and investigated domestic violence and police intervention. He has worked with the mentally handicapped and with the definition of patienthood and the sick role. Other areas of his scholarship include Marxist theory and feminist criticism, anthropology and clinical consultation, time and the social experience of sickness, alternate healing systems, cultural performance, and AIDS. His best-known books are *Village on the Border* (1957), *Communities in Britain* (1966), and *Custom and Conflict in British Society* (1982).

Ronald Frankenberg (1929–) received a master's degree in economics in 1952 and a doctorate in anthropology in 1954 from Manchester University. A. R. Radcliffe-Brown and Elizabeth Colson, a fellow Malinowski Award recipient (Colson 1985), supervised his master's thesis on the "Political System of Rwanda." His doctoral research, directed by Max Gluckman, was an anthropological study of the social impact of industrial change in a Welsh community. Frankenberg was Gluckman's first student at Manchester, and his cohort included Victor Turner, Fred Bailey and Peter Worsley.

Frankenberg taught at the Universities of Manchester, Keele, and Brunel in Great Britain, and Zambia, and he was a visiting professor at the Universities of Dar es Salaam, Delhi, California at Berkeley, and Case Western Reserve. With Max Gluckman he introduced courses on anthropology to nurses, physicians, and social workers. He insisted that students also learn sociological concepts, conduct fieldwork, and translate their experiences into practice with patients and clients. At the time of receiving the Malinowski Award, Frankenberg was teaching at Keele and Brunel the only medical anthropology programs in Great Britain.

During his academic career Frankenberg held a number of administrative posts. He was co-director of industrial research at Manchester University from 1960 to 1965 and head of the Department of Sociology and Social Work at the University of Keele from 1967 until 1984 when he retired as professor emeritus. He served as chair of the medical school planning committee, interim dean of the medical school, and head of sociology at the University of Zambia, and dean of Humanities and Social Sciences, also in Zambia, all during 1967 and 1968. His editorial experience is also extensive; he served as editor of the *African Sociological Review*, the

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Sociological Review, and the Sociological Review Monographs and on editorial boards of the International Journal of Health Sciences and the British Journal of Law and Society.

In addition to research and teaching in applied areas, Frankenberg was directly involved in several applied projects. He was an educational officer for the National Union of Mine Workers (1957–1960) and collaborated in authoring a history of mining trade unionism in South Wales (1962). He also consulted on urban planning with Shankland Cox Associates at Bolton Town Centre and in a number of other places. While in Zambia Frankenberg and his physician-wife built a laboratory and health center and conducted a health survey.

The title of Frankenberg's Malinowski Award address was "Multi-cultural Products Incorporated. Help Wanted. Cultural Anthropologists Please Apply—Terms to be Negotiated." The paper was not published in *Human Organization*, as were most Malinowski Award addresses, but was revised and published elsewhere (1995). In his address, as revised for publication, Frankenberg used his work in AIDS and his knowledge of the literature to make the point that anthropologists must uphold anthropological perspectives in their applied work. In the case of HIV/AIDS and other applied medical research, anthropologists must be careful not to be caught up in the goals and "cultures" of physicians and epidemiologists. Frankenberg explained that the differences between the cultures of biomedicine, epidemiology and anthropology "arise directly from the urgent and time-limited pragmatic concerns of biomedicine as against the more usual 'need to understand' allegedly timeless modes of anthropology" (1995:112).

Frankenberg had more to say in his address about Malinowski than had many other Malinowski Award winners, although what he said was colored by his relationship with his mentor, Max Gluckman. Gluckman (1963) had been involved in a dispute with the followers of Malinowski (1945) "about how relationships between different and perhaps opposing cultures could most usefully be analysed. The choice is between seeing the situation either as contact between discrete entities (the latter's view) or as involved in an increasingly integrated but conflictual and asymmetrically power-laden unity" (Frankenberg 1995:111). Frankenberg characterizes this argument as a valuable lesson, but also as an "Oedipal clash" on the part of both combatants—Malinowski with his mentor James Frazer, on the one hand, and with his followers Evans Pritchard, Fortes, Shapera, and Mair, on the other hand (Frankenberg 1995:116). Gluckman's disagreement with Malinowski might be better labeled peer envy or seen as a product of Gluckman's loyalty to his mentor, Radcliffe-Brown, another person with whom Malinowski disagreed. Frankenberg, in turn, was inclined to agree with Gluckman that it is most productive to see cultural contact as "interacting persons each using as a daily praxis a complex interaction of cultural realities which, while diverse in origin and sometimes separately appropriate to particular situations, are no longer discrete or separable in time or place" (Frankenberg 1995:117). This approach, he suggested could help us understand the relationship between health professionals, anthropologists, and persons with AIDS.

In his address Frankenberg focused on the contribution anthropologists can make in the fight against AIDS and other health threats. Concerning the concept of culture, he noted that anthropologists have been in a sense too successful in making this term "part of the common currency of intellectual analysis" (1995:128). When used by persons in power, such as management consultants, culture often means "any belief or principle which stands in the way of changes which their government clients wish them to take" (1995:128). The concept has also been inappropriately applied by epidemiologists and medical researchers to any category of persons who share "risk factors." This has led in HIV/AIDS research to the futile search for not

only a “culture” of injection drug users, but also a “culture” of the spouses and partners of injection drug users.

The past experiences of anthropologists in studying other cultures may not be directly relevant to AIDS epidemics in specific places today, but, Frankenberg maintains, “what may be relevant are the questions I have learned to ask and the concepts by which they are framed” (1995:111). Later, addressing the same point in terms of what anthropology would call the etic versus the emic approach, he stated, “physicians . . . elicit from the patient a history, an account within the already learned and taken-for-granted discourse of biomedicine . . .” (1995:120). Anthropologists, on the other hand, starting “with a set of answers emically posed . . . by the persons whom they observe and in whose life they seek to participate, . . . they end by devising new questions” (1995:120).

Anthropologists must set their own agendas and not follow those of biomedicine or the views of local elites. Frankenberg’s advice to anthropologists in developing an effective strategy is “to present material in such a way that it enables the people . . . to act for themselves, to choose their own agenda, rather than to enable and empower those who, however well-meaning, want to act on or for them” (1995:112). In this way he is supporting anthropology’s methodology (holism, culture, the emic approach) of letting the answers derived from the people studied suggest new questions to help find a solution for the problem at hand. By remaining anthropologists, working within our own frameworks, we avoid giving “scientific validation to socially constructed stereotypes” (1995:115). Frankenberg’s presentation should be required reading for all anthropology students, not just for those majoring in medical anthropology.

Ronald Frankenberg is a member or fellow of the Royal Anthropological Institute, the American Anthropological Association, the Association of Social Anthropologists of the Commonwealth, and the British Sociological Association, and he is an associate in the Institute of Development Studies at Sussex. He is a member of several advisory panels on AIDS to the World Health Organization and the British Overseas Development Administration and is National Chairman of an association concerned with neurofibromatosis. In 1986 Frankenberg received the first Virchow Prize for Critical Medical Anthropology from the Critical Medical Anthropology Group of the Society for Medical Anthropology for his article “Sickness as Cultural Performance” (1986). The publication of his revised Malinowski Award address received the AARG annual prize for the best publication on AIDS and Anthropology in 1995.

### Notes

<sup>1</sup>This annual award is given to a senior colleague in recognition of efforts to understand and serve the needs of the world through social science. See Thomas Weaver’s “The Malinowski Award and the History of Applied Anthropology” (2002b) for a brief history of this award and an overview of the recipients and their work, and Weaver’s “Malinowski as Applied Anthropologist” (2002a) for an introduction to Bronislaw Malinowski and his work.

### References Cited

- Colson, Elizabeth  
1985 Using Anthropology in a World on the Move. *Human Organization* 44(3):191–196. (Republished at <<http://www.sfaa.net/>>.)

## Frankenberg, Ronald

- 1957 Village on the Border. London: Cohen and West. (Reissued in 1990.)
- 1962 On Mining Trade Union History. *Bulletin of the Society for Labour History*.
- 1966 Communities in Britain. Harmondsworth: Penguin Books.
- 1986 Sickness as Cultural Performance: Drama, Trajectory and Pilgrimage; Root Metaphors and the Making Social of Disease. *International Journal of Health Services* 16(4):603–626.
- 1995 Learning from AIDS: The Future of Anthropology. *In The Future of Anthropology: Its Relevance to the Contemporary World*. Akbar S. Ahmed and Cris N. Shore, eds. Pp. 110–133. London: Athlone. (Republished at <<http://www.sfaa.net/>>.)

## Frankenberg, Ronald, ed.

- 1982 Custom and Conflict in British Society. Manchester: Manchester University Press.

## Gluckman, Max

- 1963 Order and Rebellion in Tribal Africa: Collected Essays with an Autobiographical Introduction. London: Cohen and West.

## Malinowski, Bronislaw

- 1945 The Dynamics of Culture Change: An Enquiry into Race Relations in Africa. P. Kavberry, ed. New Haven: Yale University Press.

## Weaver, Thomas

- 2002a Malinowski as Applied Anthropologist. *In The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers*. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.
- 2002b The Malinowski Award and the History of Applied Anthropology. *In The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers*. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.

## Learning from AIDS: The Future of Anthropology

Ronald Frankenberg

### The Argument Summarized

In most parts of the world, people who feel ill at ease with their bodies, after consulting friends and relatives, visit a doctor or other professional healer, seeking from a recognized expert both an explanation and advice as how best to proceed. When they have been given what they seek, they once more enter into discussion with themselves and with others who, unlike the experts perhaps, know them in health as well as in sickness. Then, in whole or in part, they may reject either explanation or cure or both. Practitioners of biomedicine call this *non-compliance*, an almost abusive term which implies at least ignorance and often also irresponsibility, irrationality and moral blameworthiness. Patients explain it in terms of medical insensitivity and lack of understanding of the problems of “real” life.

As a result of the emergence of a pattern of bodily symptoms which came to be known as AIDS/HIV, afflicting many people at the same time and therefore described as epidemic, professionally trained healers consulted professionally trained students of culture. An at least three-cornered set of interactions ensued between persons with AIDS/HIV, social scientists and doctors of different viewpoints and disciplines. (Moralists, power brokers, nation-states and international organizations were also involved. For a brilliant but concise analysis of the impact of an epidemic and its generation of diverse explanations and actions see Nichter 1987.)

As in the case of the patient-healer dyad, misunderstanding and accusations of bad faith sometimes ensued: from persons with AIDS (PWA's) and the seropositive against doctors and anthropologists; from doctors against their patients and those whom they thought they had enlisted to help them help others; and from anthropologists who considered themselves as being helpful to members and potential members of both the other categories but who thought they were misunderstood. This situation was and is complicated in many ways, not least by the fact that some persons may occupy simultaneously two or even all three of the social positions mentioned (see Bolton 1992; Farmer 1990, 1992).

This paper has a simple central theme which is that the practitioner of anthropology needs to be educated with a rather different emphasis to that usually considered necessary for the “expert” biomedical practitioner. It may be that the doctors’ “expert” knowledge of diseases, viruses and vaccines are *directly* applicable to a new situation. Possibly s/he comes with a set of either ready made *answers* or the framework for them, although this is, of course, a not uncontroversial statement. The research-grant awarding policy of the British Medical Research Council is however based on this presumed scenario. On the other hand, my “expert” knowledge of Welsh culture in the 1950s, of Zambian culture in the 1960s or of Indian culture in the 1970s

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and of British and other academic cultures during all these decades is not *directly* relevant to understanding, and certainly not to explaining, social and cultural perceptions in an epidemic in 1990s San Francisco, New York, Uganda, Haiti or Bangkok. However, what may be relevant are the *questions* I have learned to ask and the concepts by which they are framed.

To develop this argument I first briefly address one of the questions which emerges sharply for anthropologists out of the controversies mentioned: namely, who has the right to speak at all. I reject the view that only those most directly involved in a situation can usefully comment on it.

The reason why anthropologists have sought, and been sought after, to become involved at all has been because of their perceived concern with culture. However, not only have non-anthropologists very diverse views about what culture is, but there is also disagreement amongst anthropologists. I suggest this is related to the historically changing culture of anthropology itself. Anthropologists, like other social groups, tend to assume that what they do is natural: cultural activities are confined to others. More recently they have been driven by experience to diversify and elaborate the way culture is perceived. This leads me to recall one of the classical disputes in British anthropology (between Gluckman and followers of Malinowski) about how relationships between different and perhaps opposing cultures could most usefully be analysed. The choice is between seeing the situation either as contact between discrete entities (the latter's view) or as involved in an increasingly integrated but conflictual and asymmetrically power-laden unity.

I suggest that this has continuing resonance and that the triad, anthropologist-administrator-subject is mirrored by the relationship anthropologist-doctor-"patient." With this in mind I explore in more detail differences and similarities between some of the cultural patterns of professional medicine and those of anthropological approaches. Just as in the earlier argument, the fact that one actor may embody more than one cultural experience complexifies rather than invalidates the argument. There is particular poignancy in the internal self-analysis and struggle which may be involved. (See for example Monks and Frankenberg 1994 on clinically trained sufferers from multiple sclerosis and Narayan's 1993 discussion based on fieldwork in her father's native village in India.) I argue that the differences in questions posed between anthropology and medicine in general arise directly from the urgent and time-limited pragmatic concerns of biomedicine as against the more usual "need to understand" allegedly timeless modes of anthropology. Finally I present some of the experience of anthropologists in working directly in the AIDS/HIV field to illustrate not only the difficulties which they underwent in working with agenda set for them but how unexpectedly successful (from an orthodox biomedical point of view) they were, when they were able to set their own. I relate this to the political questions involved not only in the choice of an audience of the first instance, usually fellow professionals, but also more widely. The only effective strategy I suggest is to take sides and to present material in such a way that it enables the people on the sides chosen to act for themselves, to choose *their* own agenda, rather than to enable and empower those who, however well meaning, want to act on or for them.

## **Introduction: Culture in and of Anthropology**

Contrary to present fashionable belief, anthropology always did begin at home in the implicit study of its own national and disciplinary culture. Anthropology constituted and continues to constitute itself as a multicultural society. There is first a pluralism of national intellectual traditions (not, of course, innocent of differential class and gender perceptions and



misperceptions): German, Italian, Eastern European and French, for example. There is even a sense at times that the bases of Anglo-Saxon anthropology are merely treasonable translations of selectively (mis?)understood French philosophical speculation. Finally, African, Asian, Australian and bilingually Canadian matured offshoots add further complications to a complex cultural picture. Contrary to the unstated mythical assumptions of the dominance of the fieldwork model over the subject, some of its most influential figures did little or none, and most interchange of ideas amongst the denizens of anthropology is, as in most cultural congeries, intracultural.

As in the cultures of other societies, some traits and practices are adaptive for individuals and groups and others disruptive to the point of destruction. Which traits are which in this regard is graphically revealed in crises of action and interaction. Johannes Fabian, a Dutch stranger within the anglophone gates, in his *Time and the Other* (1983) gave advance notice of such a developing crisis inside the discipline. Clifford (1988) and Marcus and Fischer (1986) issued what they saw as the final warning seen reflexively and individualistically from within; and Said (1979) anticipated and reinforced it from without. The embodiment came in the gay and lesbian slogan, itself born of frustration with the anthropological response to AIDS/HIV, "These natives can speak for themselves": a slogan which can no longer be either ignored or left out of context. It is well on the way to achievement by women, at least in the industrial countries. It is now echoed by the physically disadvantaged and by the homeless. It was foreshadowed and is asserted with vigour by the marginalized inhabitants of the formerly colonized, and still increasingly indebted, periphery whose crystallized cultural past was rendered static by imprisonment in Western museums and libraries and whose economic future is similarly confined in the vaults of Western banks. They can indeed all speak, but in what circumstances and to whom? Will such voices remain muted amongst the many (including some anthropologists) who are not silent, who speak but are nevertheless not heard (Ardener 1989a, 1989b; Hardman 1973)? And what part, if any, might be played by anthropologists who see themselves as interpreters, guardians and advocates of the culture of the other?

### **The Problem of AIDS/HIV: Culture as An Applied Concept**

I shall first define by contrast with medical approaches my view of the nature of the anthropological enterprise as encompassed by and within the concept of culture. I also explore the consequences for the future of anthropology and for the concept of culture of its encounter with HIV/AIDS.

When HIV/AIDS was initially identified as a syndrome (but not under this name) in the United States, it was represented as a disease rooted in only partially disguised metaphor: a "sickness" in social relations; cultural cancer in the behaviour of the internal (homosexual, heroin-using, hookers, haemophiliacs) or external (Haitian, Hispanic, African) other. Anthropologists did not have to gatecrash this particular clinical and epidemiological party: as "experts" on culture they were invited in. The brief that they were given was, in the main, to advise on how culture needed to be understood as a necessary prerequisite to changing it, and thus preventing self-infection. They were asked to help prevent, in the words of the British Princess Royal, "mankind scoring own goals against itself." Some anthropologists suggested a search for cultural factors which might modify the speed and seriousness of progression of those already infected to the manifestations of the AIDS syndrome. Others sought cultural change or affirmation to enhance the quality of life or remove obstacles to living "Positively." These were,

at first, applauded, but later regarded as impertinently superfluous by HIV-infected persons. Epidemiologists, laboratory scientists and clinicians within the rapidly growing, medically based, AIDS industry regarded such approaches as either irrelevant or contaminated by association with alternative therapies.

This is not the first time that anthropologists have been invited to contribute to policy decisions. Nor is it the first time that they have been ascribed the role of consultants on the culture of the afflicted, but allegedly ill-informed and certainly silenced, other. In the case of AIDS/HIV, it is the association with the symbolism of “sinful” sex and untimely death which now gives the epidemic the power to precipitate intellectual crisis and self-questioning, in general, as well as in anthropology.

The potential use of anthropology in helping to order White control of Amerindians in the U.S. or to impose indirect rule in British-colonized Africa was more often unexploited or, at most, itself indirect (but see Asad 1973). In Britain it existed mainly as one strand, but a relatively minor one, in the training of administrators. The feared influence was even perhaps the other way. When the Association of Social Anthropologists of the Commonwealth was founded as a more exclusive forum than the Royal Anthropological Institute, it was primarily to protect British structural functionalism against U.S. cultural anthropology. It was also intended to exclude the colonial and ex-colonial officials who frequented the former (Max Gluckman 1950 and John A. Barnes 1993, personal communications). Nevertheless, the rhetorical principles of indirect rule did require sufficient understanding of local political culture to identify who were the leaders to whom power was to be partially transferred (and who could most effectively recruit labour and collect taxes). And if the so-called principles of “paramountcy of native interests” and of “tolerance of native custom,” if not abhorrent to universal (i.e. British Victorian) morality, were even to appear to be put into practice, anthropology might again have been useful.

Lately, in countries with migrant workers or settled ethnic minorities, anthropologists have been recruited as teachers on courses for trainee social workers and nurses. They are asked to explain through exposition of cultural practices what *they*, the “alien” other are really like; to give scientific validation to socially constructed stereotype. Even in India, China and the Russian Empire, anthropologists inherited the role of explaining the culture of “tribal and aboriginal” peoples to the national majorities. Anthropologists have not usually been encouraged in these contexts to explore the culture of political administration, social work or health provision to see what obstacles it might put in the way of persons unfamiliar with it. Although more recently still, some anthropologically trained insiders to such groups, or others with consciousness raised by the latter or by black and feminist writings, have sought to influence professionals to ask themselves how they can change to relate to a client who, while remaining an individual, has nevertheless a class, educational or ethnic formation different from their own.

All these cumulative experiences, topped by the trauma of partial failure or success in responding to AIDS, have created a crisis of confidence in the key concept of culture itself. Some have suggested that even at this late stage it should be abandoned altogether by the discipline. As an alternative, since it is easier to imagine texts than societies in thing-like terms, some suggest it could be relegated, or transferred along with themselves, to “cultural studies.” The problem is, of course, that multicultural anthropology has developed, unsurprisingly, many diverse concepts of culture since Boas and Tylor in the nineteenth and early twentieth centuries. Perhaps the most common, in practice if not in definition, is that culture is a thing (realty rather

than reality) which is possessed by a group. To the nationalist, and to some well-meaning liberals, it is always a good thing and it can only be fully understood by its owners. To the epidemiologist, the specialist in agricultural development, the “rational” medical administrator or business consultant, it is always a bad thing; a synonym for mindless conservatism standing in the way of progress and the prevention of disease. A second view sees culture as a framework of choices available to persons at a particular place and time. This has the possible implication of each society having as many cultures as there are social groups (but how are the boundaries of the groups to be defined?) and that within each social group there are as many cultures as there are persons. A third approach, seeking to overcome the problems of the second, is to see culture as an evanescent process which is continually negotiated between persons and within and between groups. Once more a problem of boundaries and continuities in space and time is evoked. There are familiar, if partial, solutions to these like Victor Turner’s (Turner and Turner 1978) view of the dissolution of continuity in the liminal periods of *communitas* at the centers of recurrent crises. These he contrasts to the repetitive reproductions of relatively crisis-free societal flow. Yet another set of solutions is provided by Douglas’s cultural theory (1973) where situational change changes the meanings of hierarchical classifications and group memberships and loosens the structure of accepted temporal and spatial norms. More recent approaches to cultural analysis move either outward from the specific society to its embeddedness in historical period and geographical space (Burke 1987)—its conjuncture; or inward to its embodiment in the incarnate selves of its subjects (Schweder and Levine 1984; Schweder 1991). Paradoxically these two directions merge in those anthropologists like Bloch and Bourdieu, Taussig and Toren who, sharing a heritage of phenomenology derived from Hegel, Marx and Benjamin or from Merleau-Ponty, see culture as the living process of the embodiment and re-present-ation of past experience.

### **The Answers Are Questions: Voices Echoing from the Past**

In this section I argue that the enduring *answers* produced by anthropology are more usefully seen as ways of formulating *questions*. There are two sets of reasons why this may be so. The main one can be illustrated by looking at the difference of approach of anthropology and clinical medicine.

Before doing this it is useful to look briefly at a dispute between Gluckman and Malinowski’s followers about “Culture Contact in Africa” (Gluckman 1963; Malinowski 1945) which serves as an object lesson in similar problems. This was an Oedipal clash since Gluckman’s analysis of Malinowski’s inconsistencies was based on the argument that the latter wanted at once to refute and destroy the divine kingship of his mentor and intellectual father, James Frazer, and to reject his own intellectual offspring: Evans-Pritchard, Fortes, Schapera and Mair. The substantive argument was, however, about discrete perceptions of three separate cultural realities existing side by side, which Malinowski formulates in terms of three basic columns in a table:

White influence, interests and intentions	Processes of culture contact and change	Surviving forms of tradition
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Table 5.1

Gluckman used Malinowski's notes to show how these might be applied to African warfare:

European conquest and political control	The new political system as affected by loss of military sovereignty of the African tribe or monarchy and resultant changes in African organization	African Resistance and political submission in tribal memory and reaction
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Table 5.2

Malinowski's difficulty was to keep the cultural realities separate as entities and yet still to demonstrate and to analyse their interactions. He tried to achieve this by positing their interaction through institutions. There cannot be conflict within cultural realities because by definition they are functional systems. There cannot be conflict between them because they are separate. Gluckman suggested that this led Malinowski to a theory of social change and applied anthropology very close to the advocacy of separate development, a consensual view in anglophone colonial Africa long before the Afrikaner concept of apartheid, its extreme version, was specifically formulated and adopted as official South African policy (see Gluckman 1963:217–220).

Gluckman and others found their solution to this impasse by seeing Africans, whites and administrators as interacting persons each using in daily praxis a complex interaction of cultural realities which, while diverse in origin and sometimes separately appropriate to particular situations, are no longer discrete or separable in time or place. Gluckman's (1958 [1940]) *Analysis of a Social Situation in Modern Zululand* is a case study of how this might be done. The sets of examples relating to AIDS below illustrate the continuing reality of contrasting approaches.

The aims of clinical medicine are to diagnose the causes of the disturbance of bodily function by assimilating it, if possible, to similar episodes with the same combination of symptoms described as a specific disease. The latter can already be well known or, as in the case of HIV/AIDS, "invented" from a new combination of partially pre-existing symptoms. The disease is described or invented specifically in order to advance understanding of these causes and their effects so as to hasten and to facilitate the disappearance from the affected person of the disease entity. Practitioners may well say "we think, that is to say we know, that the cause of this is usually . . .": a statement intended to be interpreted by the patient as "the causes are, the cure is." The implication is that if the patient complies with advice or submits to procedure s/he will more or less rapidly become an ex-patient. This pragmatic approach necessitated, if not Occam's Razor, at least his scalpel. What has to be sought is the simplest explanation that will effectively facilitate cure. From this viewpoint, medicine may or may not require to see the patient whole, or indeed, except in the most literal sense, to see the patient at all.

Quantitative probabilities may be enough: the qualitative niceties of social and cultural background may be both redundant and causes of unnecessarily postponing action. The clinical interviewer is not usually interested in, for example, who else is in the room, the mood or feelings of the interviewee or whether s/he would give the same answers or history in a different situation. The patient's notes, the clinical report, the published paper in the *British Medical Journal* or *New England Journal* are expected to be decisive and definitive—the best available current knowledge refuting earlier statements and conclusions. The range of facts taken into account has been for practical and utilitarian purposes limited. Reality is there, but selected reality—the effectiveness of drugs, the lessons of experimental procedures—not “all human life.” The clinician and the clinical scientist think in numbers, perforce, because that has been culturally defined for them as the acceptable mode of action-legitimation. As Kleinman (1988:22–23) has argued for psychiatric epidemiology, they are caught in the contradiction of the traditional clinical recognition of the idiosyncratic individual and the epidemiological disregard of the statistically atypical. For the same reasons they are constrained to seek the single linear cause, isolable within the parameters of “natural” pathology, real episodic disease situated in the (somatic, i.e. segmented) body as a lesion (Armstrong 1987). Multicausality and pluralism of explanation are not necessarily unrecognized but they do have to be ignored in the interests of the pragmatic political economy of cure. Physicians do not merely listen to, but elicit from, the patient a history, an account, within the already learned and taken-for-granted discourse of biomedicine and which provides answers to questions set at the outset of their enquiries. Anthropologists, by contrast, veer in the opposite direction. Starting with a set of answers emically posed to them by the persons whom they observe and in whose life they seek to participate (in so far as they are able to transcend their own subjective biases and the concerns imposed by the discipline itself), they end by devising new questions. Academic interpretive anthropology, at least, is aimed at the understanding of social process which may or may not turn out to influence the operation of simple causes on the linear outcome of events. They cannot *a priori* assume any action to be irrelevant, and have therefore to involve themselves in their subjects' experience in a way not only not required of the clinician but also often forbidden as either inethical or leading to loss of objectivity. Anthropologists, then, may count and calculate differentiating and differential attributes but, in contrast to clinicians, may never need to tabulate quantifiable answers to questions formally posed to their subjects. If successful they may contribute to knowledges of process through time rather than to a set of instantaneous actions. The outcome of their work is not necessarily expected to be definitive in its individual own right. It must be consistent with earlier work in the field but it may be at once contradictory, enhancing, and transcending, without having, like clinical conclusions, totally to replace its predecessors. Nor can there be, as there is (and has to be) in medicine, a simple and recognized hierarchy of decision-makers in which registrars, for example, can only hold the trump card in the absence of consultants. To the looser orthodoxies of anthropologists, pluralisms are unthreatening not least because, imperialist although they may have appeared and still appear to be, they do not directly inscribe their conclusions upon the bodies of their informants.

Biomedicine	Social Anthropology
<i>In general outlook:</i>	
Pragmatic—aims towards action	Analytic—aims towards understanding
Complexity if essential for action	Complexity always necessary for holistic view
Disease as limited episodes	Illness as flow events
Linear causation by major cause	Overdetermination by multiple factors
Socio-cultural factors secondary	Socio-cultural factors primary
<i>In investigations:</i>	
Formal questions posed	Questions are formal, informal or emerge in process
Focus on the case	Focus on relational context
<i>Publications required to be:</i>	
Definitive and refutational	Consistent but enhancing
<i>Aims at:</i>	
Limited reflection of reality	Totality
<i>Investigator:</i>	
Distanced	Involved
Quantitative analysis required	Quantitative analysis if necessary
Atypical cases disregarded	Atypical cases crucial
<i>Centres on:</i>	
<i>Disease as first focus</i>	<i>Illness as first focus</i>
taken for granted, real, body-situated, lesions	Unbounded in time and space socially constructed through situated praxis and then <i>Sickness</i> socially, economically and politically determined_ <i>Disease</i> as social construct
Elicits accounts within its own framework	Tries to be accepting of subjects' discursive frameworks
Ends by providing answers to questions questions posed at outset	Ends by devising questions to answers from outset
<b>Uncreative but right!</b>	<b>Creative but wrong?</b>

Table 5.3

The discursive practices of clinical science and medical anthropology can thus be seen as a refraction of similar difference between the past practicalities of political action in colonial territories and the teasing out of tribal law beloved of political anthropology in the 1940s and 1950s. When some anthropologists, like Evans-Pritchard (1949) encountering Italian oppression in Cyrenaica or Gluckman and the Rhodes-Livingstone Fellows vigorously opposing Central African Federation (Epstein 1992), sought to

radicalize themselves, they sought to find ways at worst of advocacy for, at best of co-operation with, their erstwhile subjects of study.

A new liberal applied anthropology of sickness in general and AIDS in particular had to seek similar bridging concepts; but with whom? It thought to find it in a symbiotic relationship with an intercalary discipline like epidemiology which shares some of its values with anthropology and others with more orthodox clinical medicine. If not love at first sight, there was at least a tentative mutual attraction. Epidemiology (see Frankenberg 1993), in the United Kingdom and most of Europe, unlike in the U.S., operates from within clinical medicine. It is however a greater challenge to it than, for example, homoeopathy or acupuncture. The last two, in whatever other ways they pose a radical challenge to orthodoxy, remain focused on the individual, unlike epidemiology. Social epidemiology is, as its name implies, just that. It focuses on social rather than individual characteristics, it seeks causes of disorder that remain in the social environment and that are either not or not yet internal to the isolated body. Furthermore it is based on a supposedly (although not always convincingly) morally neutral concept of risk (see Douglas and Wildavsky 1983; Douglas 1986) which is discovered by identifying a category of people most likely to be affected by a symptom or syndrome. These are characterized as a risk “group” (as it will emerge, a dangerous misnomer) and the relevant attributes or behaviors of its members are in turn identified as risk factors. Epidemiology’s myth of origin (shared incidentally with medical geography) is The Broad Street Pump. This tells how folk hero John Snow (cited and discussed in Morris 1976:206–212) “linked his prestige as an influential London surgeon to the analytic power of descriptive statistics” (Morris 1976:207) and in the Soho cholera outbreak of 1854 (Donaldson and Donaldson 1983:115–116) mapped the distribution of deaths by household and found that the frequency fell off with the distance between household and the eponymous pump. He then sought out what such households had in common and discovered as he expected that they drew their water supplies from it. He clinched the argument by discovering that the distant households visited people in nearby dwellings and collected their water from there and by a controlled trial using the products of two different water companies. He caused the pump to be closed down and cholera in that neighbourhood ceased. Like his successors, having established the external cause he needed but failed, despite conciliatory language, to satisfy his clinical and commercial critics of the method of transmission into the body. Snow was amongst those who devised standard epidemiological practice as based on *socio-cultural investigation, external to the body* followed ideally by the theorization of a problem, collection and classification of cases, identification of a category or categories of persons at risk, identification of risk factors, *transitional*—demonstration of method of transmission into body, *internal/bionatural*—demonstration of pathological processes.

In the case of AIDS/HIV, epidemiology went quickly through its normal cultural course, little changed since Snow’s day. It named five categories as being at risk, the coyly humoral concept of exchange of body fluids (to cover blood transfusion, sexual intercourse and intravenous injection) was invented as a description of the common transmissible risk factor, and the aetiological processes of virus infection and immunological damage were eventually described. At this point, however, they decided that they had a problem of group culture and therefore called in the experts on culture, the anthropologists. The combination of flattery and a vaguely defined, but apparently shared, boundary concept (Star 1988) is a powerful instrument of intellectual seduction. There was a mutual recognition of possible alliance on the principle that those (respectively within medicine on the one hand and social science on the other) who are not against are for us. This over-rode the more cautious view that uncritically to accept others’

definition of the situation is to concur with it. Thus developed an unfortunate complex set of paradoxes that good ethnographies might have disastrous sequelae and bad ones less disastrous ones. Still more galling, it eventually emerged that excellent ethnography with beneficial political outcomes at one place and time might be disastrously misused in different circumstances and at different periods occurring a few months later or a few miles away.

One example of this can be given in hypothetical terms, and illustrates not only this point but the pitfalls of liberal multiculturalism, a truly repressive tolerance of repression itself. British and American governmental and non-governmental agencies and international agencies, including the World Health Organization, pride themselves on filling needs identified mainly by the societies whom they set out to help. In the past, ironically, anthropologists often assumed a false homogeneity of class and gender. They reported hegemonic views without questioning consensus (Frankenberg 1979). Similarly in work on AIDS/HIV, as in liberal multiculturalism, it is often the views of an elite that are counted as local views (Frankenberg 1992; Patton 1990). Thus it has not been uncommon in the current crisis for elites in aid-receiving countries themselves to define the source of HIV infection as being other (just as happened in earlier world epidemics). A favourite source of blame, because they are there, usually female and politically weak, and can be represented as morally so, are foreign prostitutes. A metropolitan government can be persuaded to send anthropologists to study them, and since their access is confined to them, to confirm (without of course technically verifying) the convenient hypothesis. The better and more ingenious the ethnography, the more self-fulfilling the prophetic research. A variation on this is reported by Day (1988, 1993 and personal communication; Day et al. 1988) nearer (my!) home. She found that, at one point of time and in one specific place, prostitute women in London were unlikely because of their use of condoms to give or receive HIV from johns (clients). Her findings at this point countered the stereotype and helped to avert attribution of blame, but were later generalized by an argument for cutting preventive, welfare and clinical services to prostitute women whenever and wherever. A cultural approach then is always contextual and involves conflict and interaction of cultures including those of readers and interpreters.

There are, however, also pitfalls surrounding contextualization. Glick Schiller (1992) is a scholar who contextualizes with care and rigour but even her findings may be muted, as she points out, by being refracted through the spectacles of biomedicine not as science but as agent of U.S. hegemonic ideology. In her article, which she was only able to publish after she was no longer employed on the project, and thus no longer subject to medical censorship, she writes that when bioethicists invited her and her anthropological colleagues to report on their findings, they knew, as good diagnosticians always do, what questions they wanted answered and how to ignore answers which they saw as irrelevant. They demanded to be told about communitarian values in ethnic communities and, when told about the effects of gender and power differentials and economic disadvantage within and between communities and individuals, failed to hear it. Their warm thanks were from what they had learned about what they thought of as being cultural factors.

Culture was also seen as synecdoche, part for the whole, in the other experiences she reports. Her account of black family life was interpreted in terms of cultural matricentrality rather than in terms of the economic necessity which both creates and, at least in micro terms, is created and reinforced by it. Her third example concerns the expectation that she should create a uniform cultural whole, a unified and manageable other, out of disparate, but in the broad sense language-sharing, parts: Hispanicity. The general approach to the problems of different Hispanic



groups in the epidemic, e.g. those of Puerto Rican, Haitian, Cuban, Mexican and other Central and South American origin has been to lump them together. Similarly the vast continent and diverse peoples and economies of Africa are often grouped together in the discourse of “African AIDS.”

Kane (Kane and Mason 1992) had an even more difficult task. The semantic and clinical confusion created by the “concept” of risk group not only led to the assumption that there was a discoverable entity, the culture of intravenous drug-users, but that there would also be a culture of non-drug-injecting, non-HIV-positive spouses or sex partners of drug-injecting, HIV-infected persons. A brief experience of the fact that such people in the city might be bankers or bench-sleepers or of any other occupation or of none; Spanish, English, Chinese, Vietnamese, Russian, French, German, or other language, speakers; of various skin colour, of nearly any age; Hindu, Buddhist, Jewish, Christian or Muslim or any other religion, convinced Kane of the futility of the general task and her employers of her incompetence as an anthropologist, and they eventually parted company (but see Kane 1991:1049).

### **The Culture of Locality: Resurgent but Transcended**

Faced with a crisis, some anthropologists sought to cooperate, directly or indirectly, rather than merging, with epidemiologists by doing what they always had done. They studied a locality, a culture situated in a bounded place at a particular time. Most of these, encouraged by the new-found clinical interest of medical colleagues and health promoters, returned to the study of sexual cultures and behaviours virtually abandoned since Malinowski. They were now respectably and legitimately able to do what they had always wanted to do and could never afford for lack of funding and fear of stigmatization and future unemployment. These activities have produced a lively and well-crafted literature, although their contribution either to the welfare of persons with AIDS or to anthropological theory has recently been severely and perhaps justly questioned (Bolton 1992, 1993; Herdt 1987, 1992; Parker 1991, 1992). Furthermore, sometimes, the sharpness and relevance of their analyses have been weakened because they have dealt with the activities of male homosexuals where power relations based on age and wealth are not always as obvious as are those in heterosexual relationships based on gender.

I turn to those who have focused, but with new critical awareness, on problems traditional to the established theoretical concerns of the discipline. One such study is by Douglas and Calvez (1990) based on Calvez’s fieldwork in Brittany (1989). In this they develop Douglas’s (Douglas and Wildavsky 1983; Douglas 1986) concern with personal risk-taking as a culturally rather than a psychologically determined attribute. A fixed, but abstract (see Bellaby 1990) view of the nature of culture, as expressed in her well-known axes of grid and group, is related to changing cultural realities in an hypothetical “city.” This is then used to interpret and to predict possible attitudes, beliefs and actions of different social groups as their interacting cultural positions change in response to their respective perceptions of the epidemic. Douglas’s ideas about the body, risk and the boundaries of both body as person and body social are beneficially related to each other to generate hypotheses about reactions hopefully generalizable to other “cities” in similar situations. For example, the core of the city closes its ranks and reaffirms its boundaries, both territorial and corporeal. It strengthens its reliance on its police for the former and its orthodox doctors for the latter. Once disapproved but tolerated deviants, whether eccentric citizens or alternative practitioners, are required to close ranks or are excluded from

participation. “Strangers within the gates” who cannot comply, like homosexuals, outsiders or drug-users, are constrained to form and then to stay within their own boundaries. From the point of view of the latter groups and especially homosexual men “rejection meets rejection” and in Douglas and Calvez’s view, in conservative and hierarchical Brittany, in contrast to solidary and egalitarian gay San Francisco, this leads some within the community to religious or other fatalism and thence to risk-taking in defiance of death. This attitude and behaviour reinforces and creates a dialectic of despair for the oppressed and infected, and callous discriminatory indifference by the respectable core. “The conscience of the central community is not essentially compassionate to all the citizenry” (Douglas and Calvez 1990:463). The alternative but essentially similar reactions of “Eat drink and be merry, for tomorrow you may die” and “Prepare with fasting and prayer to meet your doom” have frequently been reported in epidemics. Douglas seeks to analyse them in terms of a predictive theory of culture rather than as merely haphazard personal choices.

Barnett and Blaikie (1992) have returned to a view of culture and anthropology associated with the publications of the Rhodes Livingstone Institute, in which agriculture was seen to be earthily and realistically essential as was symbolic culture represented in art, sexual and mortuary practices. They studied what they called upstream vulnerability to infection and downstream vulnerability to socio-economic disadvantage in an area of Uganda where people were most likely to be infected with HIV by heterosexual intercourse and supported themselves through cultivation of their own food. This led them not only to a recognition that the impact of the epidemic varied amongst the indirectly involved as well as in afflicted and affected families and over time, but also on different aspects of culture, including sexual behaviour. They demonstrate the local and general relevance of anthropological study not just to prevention, but also to coping and living with the disease. They are also conscious of its relevance to the ways in which government and world organizations formulate and execute policy (cf. Robertson 1984). As a result of the last, their voices have been muted, and they have been excluded and marginalized at conferences and in published collections. Their critique of the mainstream of the epidemiological-anthropological alliance makes them seem by definition irrelevant to policy in the real world. Significantly their work is now funded by the Food and Agricultural Organization rather than by health institutions, for whom they have fallen into the residual category of “intersectoral.”

### **The Anthropologist as Shaman**

A very different attempt to see culture as process and as experience through time is the work of Paul Farmer in Haiti (1990, 1992). Barnett and Blaikie are in the tradition of relatively pragmatic (even Fabian) British political *economy* and economic anthropology. While they regret anthropology’s failure to find ways to collaborate either with outside disciplines like mathematical epidemiology or between warring schools within the discipline, they do use theory but at some times more explicitly than at others (1992:8). By contrast, Farmer’s theoretical analysis uses an historically based, *political* economic approach to modify the concepts of a phenomenologically informed, clinically derived, American theoretical medical anthropology. In his own words (cf. Marcus and Fischer 1986) he “attempts to constitute an interpretive anthropology of affliction based on complementary ethnographic, historical, epidemiologic, and political-economic analyses” (1992:13). Even without the impact of AIDS he would have probably have reacted to his experience as engaged clinician to, and anthropological chronicler

of, the Haitian village of Do Kay in the same way. However, the epidemic dramatically demonstrates both the power and the necessity of such an approach (as similar kinds of suffering have in analogous ways for Scheper-Hughes in north-east Brazil, 1992, and for Taussig, 1987, in Colombia). What Farmer and similar writers do is to bring to processual life concepts like sematic network, explanatory model and therapy management group. Paradoxically, he does this by focusing locally, not merely on the village or even the family or group of intimates, but also right down to the body praxis of a single person. However, he situates their cultural experience in a way that is territorially and temporally unbounded. This is “anthropology as shamanism” in Taussig’s (1987) analysis of that phenomenon. The songs of Taussig’s Shaman focus the minds of her patients on the simultaneous co-presence of the Putumayo historical past in all its nonlinear representations from the seventeenth century to the present day and in its territorial spread from Spain to the Andes (even linked to his reader’s minds through Casement with Congo and Ireland). So Farmer shows that to understand the cultural praxis surrounding the life and death of Dieudonné Gracia (the village’s third person to develop AIDS (1992:95–109)) we have to recognize culture as a set of processes limited neither in time nor in place. There were significant events related to it which lasted seconds and others which have endured over centuries. Still other events have every temporal duration in between—minutes, days, months and years. As far as place is concerned, events at the UN in New York the day before, or politics there a century ago, and events in seventeenth-century slave-trading Africa, the contemporary Center for Disease Control in Atlanta, Georgia, the National Institute for Health, Bethesda, and WHO, Geneva, Switzerland, may—through mechanisms of politics, economics, or racist values, or competition between the cultures of clinical and public health medicine—be exercising, even determining, influences on life and death in Do Kay. Nor is it necessarily the closest elements in time or space that had the greatest effect.

While the main audience for both these last two studies may still be anthropological colleagues and the policymakers and professionalized curers they hope to influence, they appeal in my view, to different rationalities. The first seeks to influence through the liberal logic of the administrator; it poses creative possible questions but it also seeks to suggest pragmatic and policy answers. The second operates through the grounded praxis of mutual participation; Taussig (1987:460–442) calls it “dialogic.” It identifies and understands questions in the minds of the subjects but leaves the possibility of answers for cooperative solution in various relevant places (including Washington and Geneva but centrally focused on Haiti and Do Kay). Future and current practical and intellectual problems and answers to them are formulated in and by ever-changing circumstances and continuing conversation. In Fabian’s term, it asserts the necessity not only of empathy but of continued co-evality (sharing and participating in daily life in experienced shared time), of real participation. Brooke Schoepf’s action research with women in Zaire (1992 and reference list therein) and the best of the sexual behaviour studies in Brazil (Parker 1991, 1992), Glasgow (McKeganey and Barnard 1992) and elsewhere share this quality. The time and resource limitations on most anthropologists, and the lack of theoretical imagination of a few, means that such achievement remains mere aspiration for the majority.

### **Conclusion: Advance with Culture or Retreat from It?**

In one sense cultural anthropology has been too successful. “Culture” in its roughly anthropological meaning, as opposed to that of Goering or the *Times Literary Supplement* or the *New York Reviews of Books*, has become part of the common currency of intellectual analysis.

Perhaps, however, caviare has become cod's roe in its transfer to the general. Thus Leo Chavez (personal communication, San Antonio, 1993) as a student of his own Spanish-speaking culture finds that others wrongly appreciate his work as being about the supposed lack of scientific knowledge, the enthusiasm of his subjects for folklore and quaint beliefs like fatalism and God's will. In a similar way I have found that management consultants in the British National Health Service use culture to mean any belief or principle which stands in the way of changes which their government clients wish them to make. These "irrational" habits they see as shared by all nurses, some doctors and most patients and as being embedded in the way things are done which they want to change.

The effect of work on AIDS seems to have had a schismogenetic (Bateson 1980 [1958]) effect on anthropologists' use of their core concept of culture. On the one hand, to please those who have appealed to them for help, they have intensified, without even noticing it, their tendency to reify culture, to see it as a thing to be possessed. People who want to act *on* others (and especially those who want to help others to act on others), however noble their intentions, behave in this way. People who want to act *with* others, on the other hand, have perforce to emphasize the importance of recognizing individually and categorically determined differences in the continuous process of culture creation, use and change.

The culture(s) of anthropology itself imprison(s) the anthropologist in a language which may frustrate if recognition is not given to the creative tension within multicultural cultural patterns. This is as true of the triangle health professional-anthropologist-person with AIDS as it was in the classic triangle in the Gluckman-Malinowski dispute: administrator-anthropologist-African subject. Epidemiologists share the social experience of academic milieux with anthropologists; they may therefore have a similar gaze. The subjects of anthropological study and the objects of would-be behaviour changers may have had similar experience. The anthropologist's appeal and the appeal to the anthropologist may be based on at first misperceived shared assumptions which unthinking practice has made real. Two such are that events are instantaneous or independent of time, and mind/body dualism. The avoidance of these may lie in the development (in Csordas's (1990) words) of "embodiment as a paradigm for anthropology" with its recognition that the anthropologist like her/his subject embodies diverse cultural experiences and that the analysis of experience involves both internal and external dialogics (see Haraway 1988 and 1989:6-8; Lata Mani 1990).

However, embodiment in this sense is not just the somatic content, often adequate to biomedical practice, for example the description of sexual acts independent of meaning experienced or social context in a broad sense. Nor is it the corporeal reality of mere biological conceptions of the body as ecologically situated subsystem. It has to be seen as body incarnate, lived and ongoing cultural practice of the situated person in space and time: remembered, actual and anticipated. The anthropologist's role is to walk and talk with, not just about, such persons. Taking sides is not a new experience for the field anthropologist or for the applied anthropologist. Narayan (1993:679) recalls Maquet discussing the issue in relation to the decolonization of Africa as long ago as 1964, and it is, of course, near the surface of the Gluckman critique of Malinowski originally published in 1947. Serious consideration of which side to be on has become less rare but, in the complexities and rapid changes of situations like the AIDS/HIV epidemic, has also become more difficult. Many voices are perhaps the right replacement for forked tongues. The anthropologist has thus to provide clear and forceful argument and overtly

revealed viewpoints rather than seek to mimic the increasingly uncertain certainties and prescriptions of medical colleagues.

### References Cited

- Ardener, Edwin  
 1989a Belief and the Problem of Women. *In* *The Voice of Prophecy and Other Essays*. Malcolm Chapman, ed. Oxford: Basil Blackwell. (First published in 1972.)  
 1989b The 'Problem' Revisited. *In* *The Voice of Prophecy and Other Essays*. Malcolm Chapman, ed. Oxford: Basil Blackwell. (First published in 1975.)
- Armstrong, David  
 1987 Theoretical Tensions in Biopsychosocial Medicine. *Social Science and Medicine* 25(11):1213–1218.
- Asad, Talal, ed.  
 1973 *Anthropology and the Colonial Encounter*. London: Ithaca.
- Barnett, Tony and Piers Blaikie  
 1992 *AIDS in Africa: Its Present and Future Impact*. London: Belhaven.
- Bateson, Gregory  
 1980 *Naven*. London: Wildwood House. (First published in 1958.)
- Bellaby, Paul  
 1990 To Risk or Not to Risk? Uses and Limitations of Mary Douglas on Risk-Acceptability for Understanding Health and Safety at Work and Road Accidents. *Sociological Review* 38(3):465–483.
- Bolton, Ralph  
 1992 Mapping Terra Incognita: Sex Research for AIDS Prevention—An Urgent Agenda for the 1990s. *In* *The Time of AIDS: Social Analysis, Theory and Method*. G. Herdt and S. Lindenbaum, eds. London: Sage.  
 1993 Rethinking Anthropology: The Study of AIDS. Paper presented at conference on AIDS and Anthropology, Free University of Amsterdam, 25 July.
- Burke, Peter  
 1987 *The Historical Anthropology of Early Modern Italy*. Cambridge: Cambridge University Press.
- Calvez, Marcel  
 1989 *Composer avec un Danger: Approche des réponses sociales à l'infection au VIH et au SIDA*. Rennes: IRTS de Bretagne.
- Clammer, David, ed.  
 1979 *The New Economic Anthropology*. London: Macmillan.
- Clifford, James  
 1988 *The Predicament of Culture: Twentieth Century Ethnography, Literature and Art*. Cambridge, Mass.: Harvard University Press.
- Clifford, James, and George Marcus, eds.  
 1986 *Writing Culture*. Berkeley: University of California Press.

- Csordas, Thomas J.  
1990 Embodiment as a Paradigm for Anthropology. *Ethos* 18:5–47.
- Day, Sophie  
1988 Prostitute Women and AIDS: Anthropology. *AIDS* 2:421–428.
- Day, Sophie, H. Ward, and J. R. W. Harris  
1988 Prostitute Women and Public Health. *British Medical Journal* 297:1585.
- Donaldson, R. J., and L. J. Donaldson  
1983 Essential Community Medicine. Lancaster, UK: MTP
- Douglas, Mary  
1973 Natural Symbols: Explorations in Cosmology, 2<sup>nd</sup> edition. London: Barrie and Jenkins.  
1986 Risk Acceptability According to the Social Sciences. London: Routledge & Kegan Paul.
- Douglas, Mary and Marcel Calvez  
1990 The Self as Risk Taker: A Cultural Theory of Contagion in Relation to AIDS. *Sociological Review* 38(3):445–464.
- Douglas, Mary, and Aaron Wildavsky  
1983 Risk and Culture. Berkeley: University of California Press.
- Epstein, A. L.  
1992 Scenes from African Urban Life: Collected Copperbelt Essays. Edinburgh: Edinburgh University Press.
- Evans-Pritchard, E. E.  
1949 The Sanusi of Cyrenaica. Oxford: Clarendon.
- Fabian, Johannes  
1983 Time and the Other: How Anthropology Makes its Object. New York: Columbia University Press.
- Farmer, Paul  
1990 Sending Sickness: Sorcery, Politics and Changing Concepts of AIDS in Rural Haiti. *Medical Anthropology Quarterly* (n.s.) 4(1):6–27.  
1992 AIDS and Accusation: Haiti and the Geography of Blame. Berkeley: University of California Press.
- Frankenberg, Ronald  
1979 Economic Anthropology or Political Economy: The Barotse Social Formation. In *The New Economic Anthropology*. D. Clammer, ed. London: Macmillan.  
1988 AIDS and Anthropology. *Anthropology Today* 4(2):13–15.  
1992 What Identity's at Risk? AIDS, Anthropology and Culture. In *Anthropology in Action*. London: British Association for Social Anthropology in Policy and Practice.  
1993 Risk: Anthropological and Epidemiological Narratives of Prevention. In *Knowledge, Power and Practice in Medicine and Everyday Life*. S. Lindenbaum and M. Lock, eds. Berkeley: University of California Press.
- Gluckman, Max  
1958 Analysis of a Social Situation in Modern Zululand. (First published in 1940.) Rhodes Livingstone Paper 28. Manchester: Manchester University Press.  
1963 Order and Rebellion in Tribal Africa: Collected Essays with an Autobiographical Introduction. London: Cohen & West.

- Haraway, Donna  
 1988     Situated Knowledges: The Science Question in Feminism and Privilege of Partial Perspective. *Feminist Studies* 14:575–599.  
 1989     Primate Visions. London: Routledge.  
 1991     Simans, Cyborgs, Women: The Reinvention of Nature. London: Free Association.
- Hardman, Charlotte  
 1973     Can There Be an Anthropology of Children? *Journal of the Anthropology Society of Oxford* 4:85–89.
- Herd, Gilbert  
 1987     AIDS and Anthropology. *Anthropology Today* 3:1–3.  
 1992     Introduction. *In The Time of AIDS: Social Analysis, Theory, and Method.* G. Herdt and S. Lindenbaum, eds. London: Sage.
- Herd, Gilbert, and Shirley Lindenbaum, eds  
 1992     The Time of AIDS: Social Analysis, Theory and Method. London: Sage
- Kane, Stephanie H  
 1991     HIV, Heroin and Heterosexual Relations. *Social Science and Medicine* 32(9):1037–1050
- Kane, Stephanie, and Theresa Mason  
 1992     “IV Drug Users” and “Sex Partners”: The Limits of Ethnographic Categories and the Epidemiology of Risk. *In The Time of AIDS: Social Analysis, Theory, and Method.* G. Herdt and S. Lindenbaum, eds. London: Sage.
- Kleinman, Arthur  
 1998     Rethinking Psychiatry: From Cultural Category to Personal Experience. New York: Free Press.
- Lindenbaum, Shirley, and Margaret Lock, eds.  
 1993     Knowledge, Power and Practice in Medicine and Everyday Life. Berkeley: University of California Press.
- McKeganey, Neil, and Marina Barnard  
 1992     AIDS, Drugs, and Sexual Risk: Lives in the Balance. Buckingham: Open University Press.
- Malinowski, Bronislaw  
 1945     The Dynamics of Culture Change: An Enquiry into Race Relations in Africa. P. Kaberry, ed, New Haven: Yale University Press.
- Mani, Lata  
 1990     Multiple Mediations: Feminist Scholarship in the Age of Multinational Reception. *Feminist Review* 35:24–41.
- Maquet, Jacques  
 1964     Objectivity in Anthropology. *Current Anthropology* 5:47–55.
- Marcus, George, and Michael Fischer, Michael  
 1986     Anthropology as Cultural Critique: An Experimental Moment in the Human Services. Chicago: University of Chicago Press.
- Monks, Judith, and Ronald Frankenberg  
 1994     Being Ill and Being Me: Self, Body and Time in MS Narratives. *In The Anthropology of Disability.* B. Ingstad and S. Reynolds White, eds. Berkeley: University of California Press.

- Morris, R. J.  
1976 Cholera 1832. London: Croom Helm.
- Narayan, Kirin  
1993 How Native is a "Native" Anthropologist? *American Anthropologist* 95(3):671–686.
- Nichter, Mark  
1987 Kyasanur Forest Disease: An Ethnography of a Disease of Development. *Medical Anthropology Quarterly* (n.s.) 1(4):406–423.
- Parker, Richard  
1991 Bodies, Pleasures, and Patterns: Sexual Culture in Contemporary Brazil. Boston: Beacon.  
1992 Sexual Diversity, Cultural Analysis and AIDS Education in Brazil. *In* *The Time of AIDS: Social Analysis, Theory and Method*. G. Herdt and S. Lindenbaum, eds. London: Sage.
- Patton, Cindy  
1990 *Inventing AIDS*. New York and London: Routledge.
- Robertsson, A. F.  
1984 *People and the State: An Anthropology of Planned Development*. Cambridge: Cambridge University Press.
- Said, Edward  
1979 *Orientalism*. New York: Vintage.
- Scheper-Hughes, Nancy  
1992 *Death Without Weeping: The Violence of Everyday Life in Brazil*. Berkeley: University of California Press.
- Schiller, Nancy Glick  
1992 What's Wrong with This Picture? The Hegemonic Construction of Vulture in AIDS Research in the United States. *Medical Anthropology Quarterly* (n.s.) 6(3):237–254.
- Schoepf, Brooke Grundfest  
1992 Women at Risk: Case Studies from Zaire. *In* *The Time of AIDS: Social Analysis, Theory, and Method*. G. Herdt and S. Lindenbaum, eds. London: Sage.
- Schweder, Richard A.  
1991 *Thinking Through Cultures: Expeditions in Cultural Psychology*. Cambridge, Mass: Harvard University Press.
- Schweder, Richard A. and R. A. Levine, eds.  
1984 *Culture Theory: Essays on Mind, Self and Emotion*. Cambridge and New York: Cambridge University Press.
- Star, Susan Leigh  
1988 The Structure of Ill-Structured Solutions: Boundary Objects and Heterogeneous Distributed Problem Solving. *In* *Distributed Artificial Intelligence*, 2. L. Gasser and M. Huhns. Ed. Menlo Park: Morgan Kauffman.
- Taussig, Michael  
1987 *Shamanism, Colonialism and the Wild Man: A Study in Terro and Healing*. London and Chicago: Chicago University Press.



Treichler, Paula

1992 AIDS, HIV and the Cultural Construction of Reality. *In* The Time of AIDS: Social Analysis, Theory, and Method. G. Herdt and S. Lindenbaum, eds. London: Sage.

Turner, Victor and Edith Turner

1978 Image and Pilgrimage in Christian Culture. Oxford: Basil Blackwell.

## **Chapter 24**

### **Claudio Esteva Fabregat: A Four-Fields Anthropologist in Spain**

**Thomas Weaver**

Claudio Esteva Fabregat, recipient of the Society for Applied Anthropology's Malinowski Award in 1994, has been one of the most important anthropologists in Spain and Europe for the past four decades, and he has compiled an impressive record of service and publication.<sup>1</sup> While studying in Mexico, Esteva Fabregat was inculcated with the four-fields approach to anthropology—a legacy of Franz Boas through Manual Gamio, who had studied with Boas and graduated from Columbia University as Mexico's first Ph.D. in anthropology (Nahmad Sitton and Weaver 1991). Upon his return to Barcelona, Esteva Fabregat established the first program in cultural and applied anthropology in Spain with a four-fields approach. Esteva Fabregat influenced university, provincial, and federal policy regarding the creation of museums, departments, and research centers and made significant contributions to the institution of applied programs and projects in Spain. His students, in turn, have developed departments of anthropology and research centers throughout the country.

Claudio Esteva Fabregat (1918–) was born in Marseille, France, but was returned shortly after birth to his native land, Catalonia, Spain. As a result of his opposition to the Franco government, he was exiled to France, from which he immigrated in 1939 to Mexico where he spent the next seventeen years. He studied anthropology at the Escuela Nacional de Antropología e Historia (ENAH) and received psychoanalytic training from Eric Fromm, with whom he also conducted fieldwork. In 1955 he received a master's (cum laude) from ENAH and became a professor of anthropology there. He returned to Spain in 1956, with residence restricted to Madrid, where he received a master's "*con premio extraordinario*" in 1957 and a doctorate in 1958 from the Universidad Complutense. He remained in Madrid as a professor of anthropology and became director of the National Museum of Ethnography in 1965. Esteva Fabregat founded and directed the first School of Anthropology in Spain in 1966.

In 1968 Esteva Fabregat was allowed to return from Madrid to Barcelona. He held the first academic appointment in ethnology at the University of Barcelona and was made the first director of the Center for Peninsular Ethnology. Later he became Spain's first full professor in cultural anthropology and founder of the first department of cultural anthropology. He also founded and was first editor (from 1971 to 1986) of the international journal *Ethnica*. In the U.S. he lectured at New York University and as a visiting Fulbright professor at the University of Arizona. At the time of the Malinowski Award presentation, Esteva Fabregat was Professor Emeritus of Anthropology at the University of Barcelona, Spain.

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Thomas Weaver (Ph.D., University of California–Berkeley, 1965) is Professor Emeritus of Anthropology at the University of Arizona.

Esteva Fabregat has been compared to Franz Boas in his productivity, intellectual entrepreneurship, and influence in Spanish anthropology. Among the most important of his books are those on industrial anthropology (1984), culture and personality (1993), *mestizaje* (cross-breeding or mixed breeding, an important topic of research in Latin American colonialism) (1988), and ethnicity and biculturalism (1982). Esteva Fabregat has conducted fieldwork on special social problems in Mexico, Africa, South America, Spain, and New Mexico. His interests in industrial anthropology began in Mexico in the textile industry in 1946 and 1947, where he also conducted work on female-headed households in a Nahuatl-speaking community. In Barcelona he studied problems of migration, including the effects of ethnicity on second-generation migrants, urban adaptation, interethnic relations, bilingualism and biculturalism, and acculturation.

After his return to Spain, Esteva Fabregat was sent by the government to Spanish Guinea to work with a Bantu-speaking group called the Fang on educational problems; there he originated a program to provide permanent housing for shifting cultivators. He conducted a study in Zamora, Spain, with a focus on *minifundism* (land fragmentation) and nonproductive subsistence pastoralism. Later he established a field school in Alto Aragon, Spain, to study changes resulting from the introduction of a money and market economy and the outmigration of women. In Ecuador and Peru, he studied agrarian reform in a Quechua Indian community. He conducted other fieldwork in Guatemala and on Hispanos in New Mexico.

In his acceptance speech, entitled "Being an Anthropologist: A Reflection" (1996), Esteva Fabregat considered how the respective ethnic histories of Mexico and the U.S. and access to native populations within their borders influenced the development of a distinctive anthropology. In Mexico, where assimilation was the goal of a national indigenous anthropology, anthropology was tied to government structures and anthropologists served as administrators. (See also biographical sketches of Aguirre Beltrán [Weaver 2002a] and Comas [Weaver 2002b].) Esteva Fabregat viewed applied anthropology as a political science because of its need to consider distributions of power. Adaptive change almost always involves conflict among groups and individuals, and therefore the study of conflict is important in understanding the process and results of practicing anthropology (Esteva Fabregat 1996:120–121).

Esteva Fabregat's position from the beginning has been that application is the only activity that provides validity and sense to anthropology. Academic anthropology cannot be justified without application in the same way that academic medicine cannot be justified without the practice of medicine. He sees an important place for anthropology in providing guidance and policy recommendations for the betterment of humankind in the context of the continuous transformation of modern society.

Claudio Esteva Fabregat has been the recipient of many awards, among which are *Oficial de la Orden al Mérito por Servicios Distinguidos* received from Peru, the Order of Distinguished Civilian Service conferred by the King of Spain, and the Order of the Statue of Liberty and the *Giuria Premio Internazionale "Giuseppe Pitre,"* both received from Italy. He is also a fellow of The World Academy of New Zealand and was elected to the Academy of Culture and Applied Arts in Italy. In October 1998, the IV Coloquio Paul Kirchoff was dedicated in his honor in Mexico City.

### Notes

<sup>1</sup>This annual award is given to a senior colleague in recognition of efforts to understand and serve the needs of the world through social science. See Thomas Weaver's "The Malinowski Award and the History of Applied Anthropology" (2002d) for a brief history of this award and an overview of the recipients and their work, and Weaver's "Malinowski as Applied Anthropologist" (2002c) for an introduction to Bronislaw Malinowski and his work.

### References Cited

Esteva Fabregat, Claudio

- 1982 Estado, Etnicidad y Biculturalismo. Barcelona: Ediciones Península
- 1984 Antropología Industrial. Barcelona: Anthropos, Editorial del Hombre.
- 1988 El Mestizaje en Iberoamérica. Madrid: Editorial Alhambra, S.A
- 1993 Cultura, Sociedad y Personalidad. Edición Segunda. Barcelona: Anthropos.
- 1996 Being an Anthropologist: A Reflection. *Human Organization* 55(1):117–124. (Republished at <<http://www.sfaa.net/>>.)

Weaver, Thomas

- 2002a Gonzalo Aguirre Beltrán: Applied Anthropology and Indigenous Policy. *In* The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.
- 2002b Juan Comas: Indigenous Policy and the Fight Against Racism. *In* The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.
- 2002c Malinowski as Applied Anthropologist. *In* The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.
- 2002d The Malinowski Award and the History of Applied Anthropology. *In* The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.

## **Being an Anthropologist: A Reflection**

**Claudio Esteva Fabregat**

In receiving this great honor—the “Malinowski Award for 1994” it seems appropriate to reflect on this occasion about certain issues and problems related to my personal experience in applied anthropology. I must note that being here in Mexico, a country for which I hold deep affection and with which I maintain permanent intellectual interests (derived from my training at the National School of Anthropology and History [ENAH]), carries a special responsibility. I am reminded that I continue to be a product of Mexican anthropology and, as such, am very conscious that my professional career preserves many elements of this origin. At the same time, I identify myself with the tasks and objectives of this great professional organization—the Society for Applied Anthropology—of which I am a member of long-standing. Finally, I recognize that the anthropologies of the U.S. and Mexico, taken together, constitute a most comprehensive collection of empirical and theoretical contributions to cultural research and to the knowledge of human societies.

Being once again in Mexico, now in the context of the SfAA annual meeting, it is obvious to me that I have been especially fortunate in my anthropological career. I think that, forty years after having been trained at the ENAH, then having taught there, and remembering also my magnificent professors (who taught me how to think as an anthropologist and also became close friends), it is possible to link these memories to a discourse about my own life within anthropology up to this present moment here in Cancun (Esteva Fabregat 1969, 1982, 1984, 1990, 1991).

Mexico is very important in my conception of anthropology. If, now and again, in this reflection on my personal experiences I mention this land of my affection, I do so thinking in terms of these formative ideas and their applications. Thus, Mexico is very important in this discussion of anthropological themes. I am convinced that anthropology has specific historical periods of development. Therefore, I want to discuss here my personal experiences in terms of a discourse about changes that take place in oneself. It is important to know how someone becomes an anthropologist and what were the circumstances that led that person to anthropology. Thus, this will be a strictly personal reflection.

If different experiential cultures play a role in the formation of the anthropologist as an individual, then surely living for a long period in another culture must be significant in a person's adaptations. This has been my case in Mexico and thus provides a basis for referring to Mexican anthropology.

At this point, I want to emphasize that an initial “I/We” of Catalan-Spanish origins has often been replaced by an “I without the We,” an “I” in transition, one that was beginning to produce an articulation with a Mexican “I.” If the experience of living in two cultures is to be recommended, it is even more so when one thus acquires the ways of thinking of both as well as

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the conviction that one is culturally interchangeable, at least in the sense that one no longer feels a stranger in either culture.

From this perspective, if it is true that many of us came into anthropology in order to understand the “Other” and the history of their relations and transformations in time and space, then it also is certain that these peoples end up being perceived by us as cultures in terms of our own personal cultural origins and adaptations. Of course, this cultural interaction is not in itself sufficient to deny the importance of descriptive objectivity, but it is a reason for studying culture in such a way that we consider the ontological and semantic qualities of the cultures in which we have coexisted.

I think, therefore, that personal histories need to be studied and explained, precisely because they determine how anthropological knowledge has developed in the past as well as in the present. In fact, it is not difficult to understand that, when anthropology is developing as a particular “-ism,” as in the case of *indigenismo* (“Indianism”), that this can become a political enterprise. Certainly, when native culture and its social order is constructed according to official Indianist Institute policies, this brings about ideas of cultural transformation—even in applied anthropology itself. Accordingly, we come to recognize that applied anthropology, besides having value as knowledge *per se*, also has special value that goes beyond being an observer of culture or even a doctor of culture. Thus, in developing a theory for the design of cultural facts and human behavior, applied anthropologists tend to stimulate the political importance of anthropology, especially because our work involves the diagnosis of social reality and a prognosis about its transformation. In a sense, this involves contributing to the social health of culture. This always has a political dimension and, accordingly, is also part of understanding human problems as problems of conflict involving practicing anthropologists.

### **A Science of Cultural Transformation**

In fact, beginning with my first memories about my anthropological studies at the ENAH and extending to my current academic experiences, I realize that these are deeply tied to my personal history. In this sense, I recall that, when I entered the ENAH, I already felt myself to be a bit of an anthropologist on account of my background. This was pointed out to me by Dr. Gonzalo Aguirre Beltrán, himself a recipient of the Malinowski Award. He observed that anthropology involved a vocation similar to that of politics, insofar as it involved solving ethnic and cultural issues such as were faced by Mexico’s National Indianist Institute (INI) through its Coordinating Centers (INI 1962). This seems especially true for anthropologists, including Aguirre Beltrán (1952, 1976) and others (e.g., Aguirre Beltrán and Pozas 1953; Caso 1962; Various 1954), here in Mexico and in other countries, who have dedicated their brilliant professional and academic careers to the defense and respect of the historical identity of native peoples in the Americas in the context of the beautiful cultural diversity of the human species.

For my part, I believe that this involves a reflection on the motives that lead someone into anthropology and to become a professional within the academy. My decision to become an anthropologist was the result of having been a young Spanish (and Catalan) political refugee residing in Mexico who thought, in that era, that anthropology was a field which ultimately had political applications.

In the announcements of courses at the ENAH, I observed the glories of Mexico’s prehispanic world. I also realized that the ENAH was a governmental institution which had a particularly nationalist and Mexican approach to the teaching of anthropology. In that period

(1947) my social experience in Mexico was a mixture of living with Mexicans and of militant solidarity with the political ambitions of the Catalan-Spanish refugees.

During my student days at the ENAH I found that its anthropology had a distinctive Mexicanist style and commitment. Those of us who were studying it felt that anthropology was a form of political science based on scientific theories about humankind, culture, and evolution in a spatial-temporal framework. We knew that we would not only study Mexico's indigenous peoples but would also be involved with governmental agencies such as the INI and the INAH.

And we were certain that studying indigenous groups had an applied purpose, since the knowledge obtained from these cultures would serve to improve their lifeways. We were studying, in effect, a practical science concerned with the cultural transformation of native groups (see Esteva 1982 for further information on the ENAH). At that time, many of the anthropologists graduating from the ENAH were professionals involved in archaeological projects whose results formed part of a profound patriotism. This especially involved three areas: the metropolitan Federal District, site of the Nahuas and Mexicas who defined the central zone of the nation; Oaxaca, site of the Mixtecs and Zapotecs; and the Maya zone. In archaeology, especially, the notion of diffusion of cultural elements from the Old World to the New World caused a certain disgust among Mexican archaeologists. The idea of formal similarities between the two continents showed a historical relation leading to a distinctive American civilization. Still, the most common viewpoint among the archaeologists was that of independent evolution which insisted that all New World developments starting with the formative processes of agriculture were native.

In ethnology and in linguistics similar perspectives held sway. Governmental budgets were concentrated on indigenous zones and we anthropologists (including myself and other Spaniards in the program) became committed to indigenous affairs. Even in physical anthropology similar orientations occurred, such as in the case of the discovery of "Tepexpan man," whose antiquity became important as a mark of the long lineage of Mexican national character. In any case, physical anthropology was an applied science and (aside from exceptions such as studies of Mexican military cadets) was concentrated on indigenous groups with the idea of improving their health and living conditions.

This is a period that may be designated as one of "patriotic anthropology" in which anthropology was oriented to the indigenous domain of Mexican society. This emerged, moreover, from the perception of Mexican anthropology as a form of political practice whose origins were in the ideals of the 1910 Revolution in which "the indigenous" should be recovered as a critical element in the shared national character. From Lázaro Cárdenas to Miguel Alemán, Mexican anthropology was distinguished by its responsiveness to the Revolution, and the ENAH was the institution most emblematic of this feeling.

The ENAH was, at the same time, a place filled with young refugees from all over Latin America and Europe. We shared these revolutionary ideals to the extent that we converted the ENAH into an ongoing symposium about theoretical issues that went far beyond a "neutral" stance usually attributed to the sciences. In fact, the ENAH was more open and had a broader spectrum of ideas than any other school precisely because of the diversity of its students and faculty as well as ample opportunities to be in contact with diverse indigenous, mestizo, and creole groups who formed the rich cultural fabric of Mexico.

As a consequence, it was not necessary to travel abroad to fulfill the ritual obligation that anthropologists should study "other" cultures in order to become professionals. It was clear to us that we underwent a significant transformation—from the anthropology of "I" to the

anthropology of “the Other”—as we practiced applied anthropology with indigenous populations.

We became aware that studying indigenous groups was a political task, and that we had a duty to carry it out in a way that brought together its scientific features in the practice of culturally transforming the native world. Thus, the question was not “Why do we study the native groups?” but instead “How shall we use the knowledge obtained in these studies?” The argument was clear: the indigenous groups needed anthropology (of all four fields). It was obvious to us that knowledge of native customs would contribute to providing more objective and more efficient governmental assistance to these populations. Similarly, the natives would be able to receive more effective medical attention to meet their health care needs; they would be taught new productive technologies to improve their standards of living; they would have new means of communication; they would get potable water; they would have more appropriate housing built for them; and, finally, they would receive schools for the children in their communities. All of these transformations would benefit indigenous communities and would bring them up to the level of the rest of Mexican national culture, at the same time that the native peoples would be aware of the benefits of progress and would make better use of the knowledge brought about through anthropological work.

It is clear that knowing how to use knowledge was as important as knowing how to obtain and organize data on a scientific basis. But, in all cases, the most abundant knowledge corresponded to the historical and to the contemporary periods of native life. The first was critical to understand the present and to affirm the cultural grandeur of the past. The second, in many cases, served to point out the cultural decline (usually attributed to the Spanish colonial system) of the natives and to suggest that they should advance in their material and social progress so as to reach the level of the rest of Mexican society, which was linked to Mexico City and its distribution networks to distinct provincial cultures.

The specific character of applied anthropology, understood as a practice of cultural transformation, thus became a political science. In Mexico, more than in other nations, such a transformational praxis consisted of a systematic method of cultural substitution carried out by the INI in a scientific program of cultural change. It was a form of integrated transformation of native communities whose theory of application had been supported by key figures such as Manuel Gamio (1975), Julio de la Fuente (1977), and Gonzalo Aguirre Beltrán (1952, 1976). The dispersed native communities would be dealt with by the development of a network of regional coordinating centers that would open these communities to material progress, markets, and communication in the Spanish language. The idea was to unite the native world with the surrounding mestizo world as a first step in linking it to the metropolis.

The Mexicanist orientation of Mexican anthropology consisted in concentrating its efforts on the so-called native problem through a system of cultural substitution. This would be the strategy that would bring cultural changes to native communities so that they would become more “Mexican.” Thus, the anthropology of the ENAH was not only “national” in its acronym but also because of the applications of anthropology as a method of understanding native peoples and for transforming them in terms of material progress and in terms of their “Mexican” qualities through the agency of the INI.

This was all quite proper in terms of our understanding of native problems at that time. It should only be criticized for the inappropriate material goods and educational items that reached these communities. Thus, as time passed, our initially favorable opinions were modified, in the sense that the notion of cultural substitution also involved alienating experiences, especially



when the transformation weakened or destroyed the natives' consciousness of their own identity. Now, we recognize that it was the practice of the native peoples to resist assimilation and that many of their leaders told us that they did not want to be assimilated because it would destroy their identity. Being assimilated meant opening their lands to outsiders and thus began the displacement of small-scale native power to outside forces.

We knew that an important problem for the native peoples was that of the process of returning to self-rule and, perhaps more important in cultural terms, was that of continuity in native self-identity. Some showed that they preferred to be labeled as natives regardless of what that might mean from an outsider's perspective.

Although these problems appeared in the context of the development of our ideas of applied anthropology, this positive fact remained: the goal of improving the material and social conditions of the native groups was in itself a progressive approach worth accomplishing. What became debatable, on the other hand, was that this progress took place to the detriment of ethnic identity and the reduction in the natives' capacity for self-government and political self-expression, especially over their own resources and lands.

Whatever the results of these programs of cultural substitution, I have no doubt that Mexican applied anthropology was based on principles of political science: it manipulated social reality by means of its knowledge and its ability to predict adaptive processes that would follow upon programs of cultural change. Applied anthropology was, therefore, a comparative political and cultural science.

### **A Heuristic of Transformation: Conflict**

Following the above reasoning, I now turn to consider other experiences. In this regard, I find myself in accord with the British anthropologist Jack Goody who, referring to the circumstances that brought him into social anthropology, took as his starting point the importance of his World War II combat experiences, especially those as a prisoner of war. These experiences convinced him that anthropology should deal with themes of social conflict more than with other issues. Goody observed that his interests moved from Marxist explanations of human societies as consequences of the modes of production to logical-positivist ideas and those of theories of communication. He was concerned with the study of "modes of destruction" precisely because the idea of conflict was so important in his own life. Thus, in his case, personal experiences influenced his style of social anthropology as a "political" enterprise more than did any approach based on "pure" science (Goody 1991:1–5).

In my own case, the problems of liberty, oppression, and conflict; of concentration camps, migration, and refugees; and even of the liberation of oppressed peoples were also significant in my youth before I entered the ENAH as a student. For me, Mexico was an exemplary mosaic of cultural and ethnic diversity; it was a living place where different simultaneous histories were joined. And the most important fact was the oppression of the natives. We were led to give special emphasis to the causes of nativism within the development of cultural self-maintenance. As with Goody, whose experience in Africa provided a reference point for constructing anthropological theory, I found my reference point in the historical encounter between the natives and the Spaniards—and the Mexicans who came afterwards. For me, also, anthropology was part of the framework of knowledge that should be incorporated into political decisions.

Awareness, at the conscious level, that norms serve to impede social expression of destructive impulses generally tends to limit the analysis of transgressions of social control by those anthropologists who have converted ethnography into a description of the cultural status quo. Moreover, the analysis of abnormality can only be the object of study when it refers to the study of criminal groups, urban groups specializing in violence, or groups separated from social norms of such importance that they merit ethnographic treatment. This occurs to the extent that such groups have rules of organization and behavior. In this sense, only the organization and the rules of behavioral systems can confer ethnographic status on anthropological analysis.

It is often thought that if culture is a conscious construction or a logical system in its superorganic dimension (Kroeber 1968; Mucchielli 1983), undoubtedly, then, its function is to reduce transgression and to plan on how to deal with its punishment. Juridical and legal systems, whether customary or formal which act as repressive systems and are, therefore, both technical and normative modes to dissuade or punish the disruptive social expression of basic instincts. For example, we act on conscious signals, such as stopping at a red light before crossing an intersection. Sometimes, we might be upset with the authorities when they seem too arbitrary. A poor person might want to have the wealth of a rich person on the grounds that his social merits are superior to the latter. In sum, one might transgress norms that seem to be unjust; in this sense, our fundamental impulses—whether involving the destruction of a hated object or the violation of a norm—are often present either in the group or in individuals.

In fact, in recognizing that anthropology is a political science, we can link it to a theory of conflict between forces competing for power, the specific disposition of identity, the control of available resources, and the position of relative status that permit some persons or some countries to dominate others. Such conflict is always proof of a relation between a capacity for controversial action dependent on the asymmetry of unequal power. What is important about conflict is the degree to which persons or peoples express it openly or limit themselves in expressing it subconsciously. In either case, conflict is part of the dynamics of cultural life. And, thus, applied anthropology is inherent in cultural transformation. Understanding this dynamic means understanding the dialectic of transformative cultural systems as their changes actually take place.

So, what stands out most significantly is the principle that any behavior, whether of a person or of a group, that relates to other persons, not only is governed by cultural rules established in terms of status and role but also by the unequal relations that are always accompanied by political actions among relatives. This occurs especially because the connections between unequals are accompanied by different power relations among these same persons. Such differences cause conflicts which appear to be at the root of the political science of cultural transformation. In this sense, a theory of conflict should be involved in the dynamic and profound models of applied anthropology.

This is certainly true with regard to universal relationships of minimal status-role. Conflict situations exist in the socialization of children by their parents and in children's resistance to the multiple opportunities of dominance that accompany the discipline designed to teach them to behave appropriately. Moreover, relations between the sexes offer conflict situations, especially that of biological development relative to the impulse of men to dominate women. All such asymmetry is a cause of conflict; everything that is constrictive foments conflict among participants in a complementary system.

Of course, these conflict-laden manifestations are easily translated to other spheres of status and inequality and always imply conflicts related to the control of social resources. In this

way, the more complex the structure of social resources, the greater the probability that their control is the cause of asymmetrical status. Thus, given the universal need to control social resources and the verticality of society, it is certain that the control of some persons by others is the first social experience they have after birth—and that this relationship is political because it involves authority. This is also a form of conflict in which each resistance is a form of opposition to the exercise of complete domination over an inferior.

It is clear that with the independence of any unit capable of being studied anthropologically (e.g., kinship, social character, symbolic knowledge and conduct), it is also true that notions of authority are built into the primordial experiences of human beings. As we have emphasized, these experiences are sources of conflict and thus constitute political acts that may be considered in different degrees according to the circumstances. This is still the case for applied anthropology, since the principal criteria are change and acculturation and even in these processes exist turbulence and readaptations. It is also true that a system of local control always appears to deal with the problem of conflict in the local population.

With such a perspective, we come to confirm the idea that, if anthropology wants to understand systems of cultural transformation, it should also understand that cultural changes involve diverse conflicts: between persons, between persons and modes of action, between action groups with different ethnic identities, between governments and their subjects, and between disposable resources and the social access to these resources (cf. Foster 1973). Thus, human progress seems tied to the notion of conflict between modes of power and resistance to the exercise of such power. Of course, the historical pattern of acculturation is, as with each human being, seen in a controlled conflict with social systems of authority. Therefore, in bringing political science into anthropology, we are formulating an indispensable basis for the study of change programs. Moreover, the conflict that arises in this way as well as the political authorities involved in the transformation represent political acts similar to those practiced by legislators when they decide through their laws to impose new obligations and behaviors upon the people.

In these theories of conflict we always notice the existence of a structure of confrontation between normative and reproductive variables, on the one hand, and new or transformational variables, on the other. This involves the individual with a permanent revolution of the subconscious, the practical consciousness of the ego, and the ideal style of the superego. Between the individual and society it becomes a conflict between the ideals of the social group and those of personal impulses. In fact, there is a social resistance to the cultural reality, but always framed within the idea that at least part of it is necessary in order to live with others. The big problem occurs when one confronts reality. For this is when one is destroyed. Logically, although it is obvious that culture regulates the behavior of individuals and groups, it is also clear that human history tends to ignore individual conflicts and gives greater importance to collective values rather than those of individuals. In this sense, individual rationality is in essence a cultural or group rationality. But always it is the case that whatever differs from cultural logic becomes a force for conflict precisely because it constitutes a different order of reality.

Given the social importance of conflict, the approach of applied anthropology is to explain the role of selective reactions that occur in adaptive and change processes. Thus, the theory of conflict needs to be defined in terms of a theory of change and adaptation. And by historical extension, this theory becomes a theory of cultural selection. In this evolutionary form, we see that applied anthropology is as much a science concerned with adaptation as it is a dynamic integrated science concerned with the explanation of local conflict. Applied anthropology is thus an open science that not only examines problems of the protagonists of the

social process of culture but also defines the conflictive character of adaptations, of phenomena, and of social-cultural relationships.

On the other hand, this anthropology of conflict, anomalies, and adaptive confusions that follow from the process of integrative acculturation introduces transformations of another order; for instance, those of a psychological nature, which lead us to consider the existence of syndromes and alienation processes, at least for individuals if not for groups. In this sense, one thinks of social problems, of cultural disassociations, of mental problems of individuals who, because of their age or because of identification of authority, are freed from new identities and confront situations beyond their control and thus become profoundly dependent.

Evidently, these are just a few of the many examples that we might offer in observing that we anthropologists take on a predictive diagnostic role when we apply our knowledge to the explanation of problematic behaviors of individuals and groups or when we determine the direction that events follow on account of cultural pressures favoring certain conduct. It is also true that some of our anthropological work can be expanded to include the idea that conflict exists in all cultures and that in all of them exist forms for its resolution.

In general, solutions usually follow conflict. In this sense, the existence and application of laws to punish transgressions represent the basic means of conflict resolution in more or less stable cultural systems. Furthermore, the same continuity of laws, as well as its constant presence and application, allow us to understand that while these exist, conflict also exists.

So, there is no doubt that an anthropological theory of conflict must refer to the idea of asymmetrical social forces involving relationships and dependencies (as well the unequal distribution of cultural resources and the capacity for subordinating others) that belongs to those who control such resources. This situation often occurs within each culture, but can also be extended to the higher order of global culture, as in the increasing westernization of the human world which has seen western values about conflict accompanying the globalization of the West throughout the world.

By now, I am convinced of the need to give a holistic vision precedence over a segmentary vision. We are aware that, if the effects of cultural transformation are total from the beginning of the adaptive process, whether slow or rapid according to the circumstances, the knowledge of causes or of correlations arising from synchronic and functionalist experience leads us to an explanation of the process of transformation that goes beyond such experience. And we are aware that the start of a total transformation begins not at the systemic level but in one of the parts of the system and subsequently affects the rest. It is this last point which interests us. The important issue is to begin with the knowledge of the value of the transformation of an innovation, for example, the automation of a craft production system or of a financial market in a subsistence economy.

Perhaps it would be useful to give an example to show the importance of value changes in economic strategies, such as those of neoliberalism in the contemporary era and its impact on social lives of the citizens of a nation or even at the global level. One such value change is occurring with the transformation of industrial capitalism into financial capitalism.

One of the most important effects of this transformation has been the mobility of capital and investments as well as the mobility of the labor force. This flexibility in capital operations has created a causal chain that begins with competitive mobility of capital and follows with the mobility of employment. This results in labor strikes, unstable employment, permanent reductions in the work force in certain areas with resultant changes in workers' residences, and ups and downs in supply and demand for jobs. The uncertainties related to work, the constant

recycling and the adaptations involved in the production systems create a great deal of anxiety. This influences the stability of individual personality structure, with consequent feelings of illness and anomie, of insecurity and other pathologies in which individuals feel a loss in their abilities to make positive social responses to changing situations. As a result, lack of cooperation becomes more common as a cause of social breakdown and the need for increased psychological assistance as well as more economic aid.

Pursuing a first-order cause, such as that of financial capitalism, permits us to see that such a transformation is so powerful that it can influence an entire behavioral system as well as the adaptive organization of society's members at not only the national but an international level. Thus, we emphasize the existence of holism. It is the very social reality of culture in which "light" approaches—even in ethnography—provide only partial understanding of reality. It is obvious also that the demand for applied anthropologists in the marketplace is not for "heavy" anthropologists but for "light" (i.e., atheoretical) anthropologists. After all, the ideology of neoliberal productive systems and the political groups identified with its methods also involves a practical ideology, an ideology of precarious meanings that is most certainly "light."

### **Multiple Identities in Anthropology**

Now I should like to reflect further on my statements about the structure of the debate between the anthropologies of the U.S. and Mexico. I should like to take a certain philosophical license in the belief that this will stimulate our perception of sometimes obscure realities. Although this may seem beyond the purpose of this presentation, I think that it is important to justify this point—that is, to consider what we might call the "dialectical rationale" of oneself and "the Other" in the anthropologies of the U.S. and Mexico.

This becomes more necessary when we try to emphasize the origin of the differences and the national developments of each of these anthropologies, especially when we recognize that the anthropologists of empirical anthropologists have much to do with the cultural and ethnic structure of their own populations—their national "interior." I think, for instance, that the multicultural and multiethnic interior of the U.S. is critical to the national conscience of this nation and that, in turn, is more profoundly anthropological than is the case for Great Britain, Germany, or other northern European nations. And I think, in addition, that multiculturalism and multiethnicity in the development of the national conscience of the U.S. is a decisive factor in the origin and growth of its anthropology.

The presence of Native American populations, with their linguistic and adaptive diversity, as well as the varied origins of the Europeans who have populated the U.S., and the diversity of the African American, Asian, and Hispanic groups together constitute a human mosaic that contributes to the formation of a richer anthropological conscience and creates a more diverse group of professional anthropologists than would be the case in European countries.

Other considerations also come to mind. For instance, when an American anthropologist studies a Native American group in his own country, he is not studying himself but rather is a "foreigner" in his own land. This is true because the so-called "American way of life" is not equivalent to the indigenous culture; being an American is not the same as being a native Navajo, a native Lakota, or a native Kwakiutl. In most instances, American anthropologists are only "native" with respect to their own ethnic and cultural identity and origins, even though they may be living in the same political territory within the U.S.

Many times I have pondered that countries are multicultural and multiethnic for two main reasons: first, that their present populations have diverse racial and ethnocultural origins; second, that the country contains territorial “pockets” of specific ethnic groups. Both of these circumstances are true for the U.S., especially for the “reservations” on which Native Americans are settled, but also for the urban neighborhoods of African Americans, or the “Chinatowns” in many cities, or the religious minorities organized into specific communities.

Thus, fieldwork in any of these communities done by American anthropologists constitutes “foreign” fieldwork. In this sense, anthropologists in the U.S. can use the cross-cultural comparative approach and can feel themselves to be foreigners (especially in ethnic terms) in their own country. Just as there is a certain cultural strangeness for foreigners in reservations or in African American and Hispanic communities, so too with anthropologists doing fieldwork in such places. The need to use native informants and to carry out participant observation in certain prescribed ways, as well as the special linguistic circumstances of the fieldwork, shows how distinctive is the experience of anthropological research in the U.S.

What is the difference between fieldwork carried out in the U.S. and abroad by an American anthropologist? I believe that the greatest difference involves widespread cultural homogeneity and the great influence of the mass communications media in the U.S., especially in terms of expectations about social and material goods, orienting goals, and the common use of English itself. On the other hand, the situation is different when American anthropologists travel to another country to study Latin American, African, or Asian cultures. This is true even when American anthropologists do fieldwork in populations from which they themselves are descended. Being a native of the U.S., and having been socialized in its national culture, is an important condition that makes one a foreigner in such situations. Moreover, this forces American anthropologists to enter with empathy into the spirit of “the Other” in their fieldwork within their own nation and when they travel to other countries.

I also believe that the ethnic and cultural diversity of their own nation makes anthropologists foreigners in their identity, though less so in their perception of reality. It seems clear that an inventory of such a national anthropology will reflect more accurately the inherent ethnocultural diversity than is true of more homogeneous countries. This would especially be true in terms of the spreading of the national cultural pattern so that fewer localized ethnic territories exist within the nation state. Consider the case of the “internal nations” in European states which reproduce themselves through their languages and cultures within their own historical territories.

The context for anthropology in the U.S. is structurally complex, especially within the academic setting. We can understand its situation by looking to its history. The diversity of the formation of American anthropology reflects its European roots blended with the Native American focus in its ethnography and fieldwork. Subsequently, the same pressure of the antecedents of diversity and their specific identities, in combination with the global political expansion of the U.S., has allowed American anthropology to become much richer in its empirical inventory. Moreover, the personal realizations of many American anthropologists represent, in my view, a deep catharsis of “migrants unaware.” There is no doubt that inside American anthropology is a global diversity which reflects the extensive representation of anthropologists from all over the world in its academic institutions.

We can understand, therefore, that American anthropology has a global character because, aside from being interested in the entire world of humanity, and especially in the nations which maintain special historical and geopolitical connections to the U.S., its

anthropologists are themselves representatives of this ethnic and cultural diversity in their own origins. Perhaps, in this case, the greatest strengths of American anthropology are the diversity of its applications and the multiplicity of its academic offerings. At the same time, the diversity of its academic offerings is a cause of difficulty in itself. American anthropology tends not to produce “grand” theory, but rather those of a “middle level” which, paradoxically, have less universalism than empiricism. Nevertheless, the sheer weight of information accumulated by American anthropologists is definitive and is reflected in the volume and frequency of their publications.

This comment may be considered a form of praise, but it may also be seen as a criticism in terms of the specific responsibilities of American anthropology within the global academy of anthropology. Given the diversity and empirical richness of American anthropology, it needs to strive for a greater fusion with the anthropologies of other nations.

In fact, when one reads works of anthropology written by scholars from the U.S., one tends to be surprised by the lack of references to works done by anthropologists from other countries where English is not the main language, even though such works reflect solid thinking and demanding and rigorous field techniques. It may be true that non-English-speaking anthropologists usually read English—and this seems almost to be a colonial submission—it becomes an advantage for these other anthropologists because they can add the data accumulated by American anthropologists to their own intellectual heritage. In this sense, one observes the different national directions of contemporary anthropology. While the growth of anthropology in the non-English-speaking world is considerable, it is being ignored in many ways, especially in citations. If this trend continues, then American anthropology may suffer from a form of ethnocentrism that will threaten its commitment to intellectual universalism.

Doing American anthropology is a complex conceptual and empirical scheme which depends, in turn, on an awareness of its internal diversity. For this reason, it is not the same being an anthropologist in the U.S. and in Mexico, as it is being one in Europe. It is also the case that U.S. anthropologists with European backgrounds tend to reproduce anthropologies similar to those produced by Europeans interested in studying cultures in specific colonial settings involving “the Other.”

This may be part of a European colonial tradition which we find identified with the colonial origin of an implicit evolutionist way of thinking, especially among those who look at “inferior others” as the object of their anthropological research. In some sense, in anthropology “the Other” is often part of the tradition of what is different from the position of a colonial subject. That is why, in many cases, we are concerned with situating our studies in the context of the expanding power of colonialism in the global setting. In this perspective, this expansion is more a reflection of a country’s political power than the result of the independent development of its anthropology.

In this sense, even though the roots of American anthropology involve Europe, it is also true that the first objects of its studies were Native American cultures. As in Europe, anthropologists in America studied “the primitive Other” and tried to verify empirically its cultural features as well as its biological traits. This was an extension of evolutionist and historicist ideas predominant in the nineteenth century.

The later involvement of anthropologists of other ethnic backgrounds in American anthropology has not modified substantially its epistemological framework. But the perception of “the Other” in terms of one’s own situation does bring forth new interests and awareness at the heart of American anthropology. Perhaps the most surprising transformation is that of the

transfer of empirical academic knowledge into an empirical awareness of “the Other.” This represents applied anthropology understood as an insertion of oneself in the transformation of “the Other.” And, at this point, perhaps we may connect this perception with the other anthropology, that of Mexico.

For those, like myself, who have been trained at the ENAH, and reflect on its rich history, we can also observe certain oddities. One of these involves the fact that “the Other,” the indigenous groups, are present in the interior of the same Mexican anthropologists who carry out fieldwork among them. In Mexico, “the indigenous Other” is not a stranger. The indigenous cultural heritage is part of Mexican cultural history and the so-called Spanish colonial past is no longer “colonial” for the current generations of Mexico. These are a product of osmosis and regional combinations of the indigenous and colonial pasts: both the ontological and cultural “ego.” In fact, when one examines the composition of the contemporary indigenous ethnocultural mosaic, and its almost sixty different Indian ethnic groups, one realizes that its historical development is also colonial. Thus, the Spanish colonial background is a form of culture that is diluted in the construction of Mexican individuals and contemporary Mexican national identity.

This is important to recognize because it marks distinctively the orientation of Mexican anthropology. I believe that this approach to understanding the diversity of indigenous ethnic and cultural groups represents the study of Mexican identity itself, especially in its historical and regional variations. Unlike U.S. anthropology, Mexican anthropology has its roots in Mexico and is concerned with Mexico, in the sense that the Mexican is also “the Other” expressed in the indigenous populations.

Thus, contrary to the situation for European anthropologists and their descendants in the U.S. dedicated to the study of “the Other” geographically and culturally beyond themselves, Mexican anthropologists stand out—because studying indigenous groups they are really studying themselves. This is so not only because they are barely cultural foreigners inside these communities, but also because they obtain an historical awareness of what indigenous communities are in their contemporary circumstances: that is, samples of regional underdevelopment. They are cultural signs of how urban Mexicans were in colonial rural days. In fact, precisely because Mexican anthropology is so preoccupied with the study of its native populations, it is not like the anthropology of the U.S., whose anthropologists lack such indigenous roots. Mexican anthropology is more historically oriented than is U.S. anthropology, and this includes the emphasis of Mexican anthropologists on the prehispanic and colonial identities which are, in turn, juxtapositions of the modern Mexican ego.

On the other hand, if one observes the other aspects of Mexican anthropology beyond the indigenous question, one sees immediately a concern for marginality of the groups studied by anthropologists. This concept of marginality is tied to the deep and dynamic connection of individuals socially defined as indigenous who are displaced in the interior of the urban system. In fact, the indigenous can be taken as a conscious and unconscious symbol of marginality within Mexican anthropology. For this reason, even those who practice anthropology in Mexico from a Western or European perspective find it hard to escape the influence of “the indigenous” on the primordial ego.

Once we understand these differences between the anthropologies of the U.S. and Mexico, which are linked to the different ethnic populations in the two nations as well as to ways in which the native groups are reproduced in the history of each national culture, we can observe that, in the case of Mexico, the native peoples are present in the cultural and genetic mixture of the nation in regional blends, in the faces of the people, and in their awareness of their identity.



In effect, for Mexican anthropologists, “the Other” is found in themselves, whereas in the U.S. only a few anthropologists contain “the Other” in themselves. In Mexico, the development of national culture, both prehispanic and Hispanic, can be seen empirically in native peoples as well as in modern populations. The history of “the Other” is, thus, a mirror of the history of oneself.

Seen in this light, the two anthropologies—the one of the U.S., the other of Mexico — both have advantages over the anthropologies of other nations. In my opinion, the most important advantage is the fact of their own cultural diversity, including their ethnic pockets and marginal enclaves. Thus, if we as anthropologists think that anthropology is the comparative study of evolutionary and adaptive diversity of the human species in time and space, then it is without doubt the case that such diversity is present as complex contemporary mixtures in both countries. Unlike European countries who gave birth to anthropology and which construct their theories from examples drawn from beyond their own borders, based on studies of ethnic groups and cultures subjected to European colonial power, the American nations—in this instance the U.S. and Mexico—constructed their anthropologies from their own internal ethnic diversity and from their own historical experience with European colonialism.

It is clear to me that anthropology has been constructed from the study of these diversities and that, while in Europe diversity was beyond its borders, in the Americas diversity was encountered in its own domain. Thus, the historical strength of the anthropologies of the U.S. and of Mexico reflects the expansive force of their own cultural diversities. In some form, then, the anthropological study of these nations has been similar to studying “the Other” within themselves.

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#### References Cited

- Aguirre Beltrán, Gonzalo  
 1962 Integración regional. *In* Los Centros Coordinadores Indigenistas. Pp. 25–49. México:INI.  
 1976 Obra polémica. Mexico: SEP/INAH.
- Aguirre Beltrán, Gonzalo, and Ricardo Pozas Arciniega  
 1953 La Política indigenista en México. Métodos y resultados. México: Instituto Nacional Indigenista.
- Caso, Alfonso  
 1962 Los ideales de la acción indigenista. *In* Los Centros Coordinadores Indigenistas. Pp. 7–13. México: INI.
- Esteva Fabregat, Claudio  
 1969 Sobre el método y los problemas de la Antropología estructural. *Convivium* 30:5–54. Barcelona.  
 1982 Trayectoria de una vocación y de una Antropología. *Autobiografía Intelectual*. Barcelona, ANTHROPOS, Boletín de Información y Documentación 10:4–18.  
 1984 Antropología Industrial. Barcelona: Anthropos, Editorial del Hombre.

- 1990 Indígenas, memorias étnicas y sociedades indígenas. Perspectivas comparadas. *In* Indianismo e Indigenismo en América. J. Alcina, ed. Pp. 102–131. Madrid: Alianza Editorial.
- 1991 Los indios de México en la sociedad nacional mexicana. Cuadernos Hispanoamericanos 7–8:129–154.
- Foster, George  
1973 Traditional Societies and Technological Change. New York: Harper and Row.
- Fuente, Julio de la  
1977 Educación, Antropología y desarrollo de la Comunidad. Mexico: Instituto Nacional Indigenista. Serie Antropología Social, No. 2.
- Gamio, Manuel  
1975 Antología. Estudio preliminar, selección y notas por Juan Comas. México: UNAM, Biblioteca del Estudiante Universitario.
- Goody, Jack  
1991 Towards a Room with a View: A Personal Account of Contributions to Local Knowledge, Theory, and Research in Fieldwork and Comparative Studies. *Annual Review of Anthropology* 20:1–23.
- Instituto Nacional Indigenista  
1962 Los Centros Coordinadores Indigenistas. México: INI.
- Kroeber, Alfred L.  
1968 The Nature of Culture. Chicago: The University of Chicago Press.
- Mucchielli, Alex  
1983 L'analyse phénoménologique et structurale en sciences humaines. Paris: Press Universitaires de France.
- Various  
1954 Métodos y resultados de la política indigenista en México. Memorias del INI. Vol 5. México: Ediciones del Instituto Nacional Indigenista.

## **Chapter 25**

### **Michael M. Cernea:**

### **Policy and Development in the World Bank**

**Thomas Weaver**

Michael Cernea, recipient of the Society for Applied Anthropology's Malinowski Award in 1995, has been employed for many years by the World Bank, beginning in 1974 as the bank's first in-house social scientist.<sup>1</sup> His contributions include advocating within the bank for the hiring social scientists and defining the bank's policies on resettlement (1988), forestry and water resource management, rangeland use, and environmental issues. His publications include twenty-four books and monographs and numerous articles. His edited book, *Putting People First* (1991), was hailed as one of the best assessments of development issues, and it has been translated into five languages.

Michael Cernea (1933–) received his Ph.D. in 1962 from the University of Bucharest in Romania. He began his career as a researcher with the Romanian Academy of Sciences and as Professor and Chair of Sociology at the University of Bucharest. At the time of the award he held the position of Senior Advisor for Social Policy at the World Bank—then the most influential social science development policy position in the bank.

Until the 1980s few anthropologists or sociologists worked as project officers or administrators in the World Bank. There were no professional tracks for social scientists at the bank comparable to those for engineers and economists. Cernea's career trajectory provided a model for a social science career path at the bank; he moved from staff sociologist/anthropologist to principal social scientist, and from senior sociologist to policy advisor. In addition, Cernea's advocacy for the use of social scientists in World Bank development projects was effective (1987, 1991). The bank created a career line for social scientists in 1986 and formed the Social Science Staffing Group, with Cernea as chair. The group was given the task of defining standards for disciplinary credentials and evaluation criteria. Throughout his career at the bank, Cernea has assisted in integrating the social sciences into the policy process by showcasing social science talents in fieldwork and consulting. Today there are over 140 sociologists and anthropologists in the World Bank, not including several hundred temporary consultants (Cernea 1995:341).

Cernea's Malinowski Award acceptance address, entitled "Social Organization and Development Anthropology" (1995), emphasized several important issues relevant to development anthropology. Among these were promotion of a theoretical applied anthropology, improvement of university training, and encouragement of social scientists to engage in "intellectual combat" with economists to create a better approach to development. As an insider,

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Thomas Weaver (Ph.D., University of California–Berkeley, 1965) is Professor Emeritus of Anthropology at the University of Arizona.

Cernea cast the empirical work of anthropologists on involuntary settlement into a workable policy adopted by the World Bank, and later by the Asian- and the Inter-American Development Banks, all Organization for Economic Cooperation and Development (OECD) members, and several developing countries. Further, he demonstrated how anthropological premises contain a theoretical model for development application. He emphasized the need to understand social organization at the broadest plane, a holism that encompasses policy makers and development experts at local, national, and international levels. This focus on social organization forces development analysis to become actor-oriented and makes anthropologists valuable as social architects, not just project evaluators. Cernea emphasized the importance of social organization as an active agent of change and the necessity for identifying related “operational hypothesis, institutional policies, and project-level planning prescriptions” (Cernea 1995:346-347).

In his acceptance speech Cernea suggested that the dichotomy commonly postulated between theoretical and applied social science must be eliminated. He agreed with Malinowski that “science begins with application” (Malinowski 1961:5, quoted in Cernea 1995:347). There should be no difference between academic and applied fields because they share the same data and methods.

First, the research objects of applied anthropology generally have no less intrinsic potential to generate theory than the research objects of academic anthropology. Development programs are complex social processes no less theory-worthy than kinship systems, or reciprocal gift-giving, or funerary rituals. Second, the methods of data generation are not necessarily different . . . The overall research designs are different, but neither holds the monopoly on method. Individual researchers can choose either to distill from their data a course for future action or to pursue theoretical proposition building (Cernea 1995:348).

Unfortunately, social scientists have not generally received training in policy making and practical issues sufficient to participate in development programs. In order to make valid recommendations anthropological work must possess three characteristics: “it must be based on a coherent analytical framework, must be predictive, and it must be prescriptive as well” (Serageldin 1994:vi, quoted in Cernea 1995:344). In addition, more social science is needed in the technical and economic departments at universities. This is where the biases are created that privilege technical and economic factors over social and cultural ones in many development programs. Finally, social scientists must fight the prejudices found in “econocentric,” “technocentric,” and “commodocentric” models that prevail in most development agencies.

Development is not about commodities. It is not even about new technologies. It is about people, their institutions, their knowledge, their forms of social organization. That is why I think that non-economic social scientists must be present and work hand in hand with economists and technical experts in the core teams that formulate development paradigms, policies, and the content of specific programs (Cernea 1995:344–345).

Michael Cernea has been honored with a *Doctor Honoris Causa* by the University of Cluj, Romania. An active member or fellow and officer in many national and international social science organizations, he was elected to the Romanian Academy of Sciences and to the position

of Vice President of the European Society for Rural Sociology. He received the Solon T. Kimball Award for Public and Applied Anthropology of the American Anthropological Association, and he has been a fellow-in-residence at three prominent institutes for advanced studies.

### Notes

<sup>1</sup>This annual award is given to a senior colleague in recognition of efforts to understand and serve the needs of the world through social science. See Thomas Weaver's "The Malinowski Award and the History of Applied Anthropology" (2002b) for a brief history of this award and an overview of the recipients and their work, and Weaver's "Malinowski as Applied Anthropologist" (2002a) for an introduction to Bronislaw Malinowski and his work.

### References Cited

- Cernea, Michael
- 1987 Entrance Points for Sociological Knowledge in Planned Rural Development. *In* Research in Rural Sociology and Development. H. Schwartzweller, ed. Greenwich, Conn.: JAI Press.
  - 1988 Involuntary Resettlement in Development Projects. Annex 1, World Bank Technical Paper No. 80. Washington, D.C.: The World Bank.
  - 1991 Sociologists in a Development Agency: Experiences from the World Bank. *In* The Socio-Cultural Dimension in Development: The Contribution of Sociologists and Social Anthropologists to the Work of Development Agencies. M. Schonhuth, ed. Eschborn, Germany: GTZ.
  - 1995 Social Organization and Development Anthropology. *Human Organization* 54(3):340–352. (Republished at <<http://www.sfaa.net/>>.)
- Cernea, Michael, ed.
- 1991 Putting People First: Sociological Variables in Rural Development. Second Edition. First edition published in 1985. New York: Oxford University Press.
- Serageldin, Ismail
- 1994 Foreword. *In* Sociology, Anthropology and Development: An Annotated Bibliography of World Bank Publications 1975–1993. Michael Cernea with April Adams. Washington, D.C.: The International Bank for Reconstruction and Development/The World Bank.
- Weaver, Thomas
- 2002a Malinowski as Applied Anthropologist. *In* The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.
  - 2002b The Malinowski Award and the History of Applied Anthropology. *In* The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.

## Social Organization and Development Anthropology

Michael M. Cernea

In order to be of use, research must be inspired by courage and purpose. It must be briefed by that constructive statesmanship and wise foresight which establishes the relevant issues and have the courage to apply the necessary remedies. Unfortunately, there is still a strong but erroneous opinion in some circles that practical anthropology is fundamentally different from theoretical or academic anthropology. The truth is that science begins with application. What is application in science and when does “theory” become practical? When it first allows us a definite grip on empirical reality.

Bronislaw Malinowski, *The Dynamics of Culture Change*

It is a great honor to receive the Bronislaw Malinowski Award from this scholarly community of development social scientists, and I am deeply grateful for this recognition. Being associated through this Award with the name and legacy of Malinowski, and with the line of distinguished scholars who have preceded me as its recipients, is a moving and stimulating experience.

### A World on the Move

When the Society for Applied Anthropology was founded in 1941, few would have anticipated either the current expansion of applied social science or the recognition that issues of social and cultural development would receive.

Consider for a minute the gigantic wave of systemic sociopolitical changes that during the last decades have restructured the world and transformed beyond recognition the societies we once studied—their polity, economy, culture, people, and national agendas. Anthropology itself has always been rooted in reflections about community. Yet that has changed and must further change, in part because the notion of community itself began to erode, and in part because of the dynamic of economic growth, decolonization, and the ascent of scores of nations to statehood and self-construction. We live now in a newly integrated-and-fractured world system. During this decade, the very structures of our contemporary world were changed by the collapse of the former Soviet Union and Eastern European regimes, a triple collapse of political, economic, and multinational state models. In turn, ethnicity and religions revive and reshape the planet’s social map. To remain relevant in development, social scientists must learn to think differently of development itself.

Development has powerfully modified the individual’s everyday life. Since 1960, average life expectancy in the developing counties has increased by about 20 years, a change of

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still incalculable consequences. Adult literacy has risen from about 40% to over 65% and average per capita incomes in the poor countries have doubled and in some nations have tripled or quadrupled. A child born today in the developing world is only half as likely to die before the age of five as a child born just a generation ago. The same child is twice as likely to learn to read and can expect a standard of living two or three times higher.

Yet this is not the full picture.

The world still has about 1.3 billion people living in absolute poverty, defined as earnings of less than one dollar a day. More than two billion people still lack access to electricity and are forced to use sticks and dung for their energy needs. Roughly 1.7 billion lack sewage systems, and one billion lack access to clean piped water, resulting in the unnecessary death of some three million infants and children every year from diseases linked to dirty water. The inequality gap continues to widen: during the past 130 years, incomes in the countries with the richest 20% of the world's population grew nearly three times faster than in the countries with the poorest 20%. There are more refugees and displaced persons in today's world than at any time before, even in the aftermath of the World War II. One last, ominous fact: 140 million of the world's adults are unemployed and cannot feed their families.

These, and other, daunting problems prescribe a formidable agenda for curing social ills—an agenda that our 20<sup>th</sup> century will hand over to the next.

From a social development viewpoint, however, we can say that the 21<sup>st</sup> century has already started. It started in this very month, March 1995.

The first event of the new century took place only three weeks ago, when the governments of the world's countries met in Copenhagen in the first ever World Summit for Social Development. This summit set its sights explicitly beyond *economic* growth alone—toward social goals. As I was privileged to be present, I attempted to grasp what might be the Summit's likely consequences for the discourse of social sciences in a world that inscribes *social* development on its banners. The summit produced a social charter—a social contract for the world at large—and this is unprecedented. All heads of states and governments signed off on a Program of Action consisting of nine major commitments, each embodying a set of goals and actions regarding: poverty eradication; full employment; fighting social disintegration; human rights; women and equality; enabling legal and institutional frameworks; and other major social objectives. Global social development was spelled out in explicit goals, norms, and tasks. True, there were also unconcluded agreements, contradictions, rhetoric, and confusion. Yet the historic significance of this event is inescapable. If there is any link between social research and thinking, on the one hand, and actual social development, on the other, the implications for our duties as engaged social scientists are equally inescapable. This is an auspicious beginning for the 21<sup>st</sup> century. I see in it also a powerful call to our profession.

### **Anthropology at the World Bank**

The citation for all Malinowski Award recipients states that it is offered in recognition of "*efforts to understand and serve the needs of the world through social science.*" This is a tall order indeed: that my work is deemed by this Society to have done so is for me a morally and professionally rewarding judgment. Therefore, I feel it incumbent upon me, as it has been upon my scholarly predecessors, to briefly account for at least part of my intellectual biography, my work, and the ideas that inform it. For some of the structural difficulties and sleep-robbing questions that we have confronted in introducing anthropological knowledge within an economic

fortress are not atypical: they are or will be faced by many other anthropologists, struggling to accomplish the same kind of progress in diverse bureaucratic settings.

The World Bank came rather late, and hesitantly, to the perception that it needed “some” anthropological skills in-house, as part of its regular staff. The World Health Organization (WHO), for instance, hired its first anthropologist in 1950, twenty-four long years before the World Bank. George Foster, in his Malinowski address, highlighted the “perplexity” of both sides in that early encounter between anthropology and an international agency (1982:191).<sup>1</sup> I can add, and many others can certify, that such perplexity is present within domestic and government organizations as well. US Agency for International Development (AID) employed anthropologists long before the World Bank created its first staff position to put anthropology on the Bank’s skeptical intellectual map.<sup>2</sup> I am pleased to say that since that lone anthropological “slot” was allocated in 1974, the Bank’s in-house corps of non-economical social scientists—sociologists, anthropologists, political scientists—has grown steadily in both number and institutional weight. This has been a major leap. The group assembled during these twenty years is today the world’s largest group of this kind working in one place—about 50–60 social scientists who actually practice development anthropology and sociology. In addition, hundreds of social scientists from developing and developed countries are employed each year as short-term consultants, largely due to the demand for social analysis legitimized by the core in-house group.

Beyond the change in numbers, there has also been change in substance. Regarding cultural substance, the World Bank’s original manner of treating its client countries was sarcastically described in a 1952 review of three Bank country studies—for Turkey, Guatemala, and Cuba. The reviewer, C. P. Kindleberger (he was not an anthropologist, by the way) characterized the Bank studies and experts’ fieldwork in this way:

Essentially, these [three studies] are essays in comparative statistics. The [field] missions bring to the under-developed country a notion of what a developed country is like. They observe the underdeveloped country. They subtract the latter from the former. The difference is a program (1952:391).

Continuing, the reviewer commented on the Bank work teams of that time:

Most of the members of the missions came from developed countries with highly articulated institutions for achieving social, economic, and political ends. Ethnocentricity leads inevitably to the conclusion that the way to achieve the comparable levels of capital formation, productivity, and consumption is to duplicate these institutions (1952:392).

This image of the Bank is from a time long past. But, it provides a benchmark against which we can measure the subsequent changes in the institution’s practices.

When I started work at the Bank in August 1974, I had no way of knowing that it would lead me, twenty-one years later, to this rostrum: in fact, I did not know then much simpler things, such as what I would have to do the very next day after my appointment. That “next day” proved to be a field trip to Tanzania to help untangle the agricultural difficulties of a country that had just undergone villagization and “ujamaazation,” a forcible grouping of peasants in state imposed village-cooperatives.



Kigoma, the western region in which we worked on the magnificent shores of Lake Tanganyika, stood out among Tanzania's regions for having carried out ujamaazation in a particularly harsh manner. However, the grand lines of this statist approach to agricultural collectivization, and its dire consequences, were known to me from my previous studies on collectivization in the 1960s in Romania. Comparing what I knew with what I saw about patterns of village organization and change greatly facilitated my understanding. Fieldwork was what I was comfortable with--and what gave me an edge over the other colleagues on the Bank's Kigoma team. This was my first development project test. It opened for me a window into what was needed, from an anthropological perspective, and how it could fit into the patterns of Bank work. It also made clear to me that I would have to pay my way in the Bank in the coin of knowledge of recognizable organizational utility.

I set to work with determination and, I confess, fear. To give you an idea of how it felt then, bringing anthropology's message to a rather agnostic and skeptical professional group, I should recall a story from the Vatican. After Vatican II, Pope Paul decided to do something about spreading the faith in Eastern Bloc countries. He then created a new office in the Vatican, called the "*Secretariat for Non-Believers*." He appointed Cardinal Franz König of Austria (who years later told this story to the *Washington Post*), as Secretary to the Non-Believers. The poor Cardinal did not know what he was supposed to do. He went to the Pope and asked: "What shall I do?" The Pope, reportedly, shrugged and said "I don't know." Then he added in Latin "*Usus Docebit*": with God's help, "the use will teach you."

And so it was also with me, as I assumed the role of "Secretary to the Non-believers": the use taught me. That I faced a huge challenge and a tough personal test was intimidatingly obvious. Becoming the first incumbent of a new role within the Bank's organization, I stepped into an undefined and ambiguous situation. There was no formal status, no structure of expectations, not even a stereotype to live up to. My unit was itself new, a special experimental division just created by Robert McNamara to pilot the new policy for poverty alleviation that he had launched in Nairobi the previous year.<sup>3</sup> From the outset, I was cast in the role of an "ambassador of the discipline." I was told, in no uncertain terms, that my work was to demonstrate to the institution whether the discipline I represented had a legitimate and compatible place in the Bank.

What the Bank only dimly realized at that time was that I had another very strong incentive to succeed—a personal, yet frightening incentive—a win or perish option: namely, my children were kept back as "hostages" by the then government of Romania, and were not permitted to join me in my resettlement to the U.S. The only way to successfully "extract" them was to succeed at my work and hold on to my job, and thereby legitimize my request for their release. It took fifteen long months. I did hold on to my job and I did get them here. I am happy to mention this tonight, as my children are in this room with us now, their spouses and small children, too! Anthropology is about real people, so I thought this well worth mentioning.

My earlier pre-Bank training obviously had little to do with typical World Bank issues or conceptual vocabulary. I can confess now how suspended-in-the-air I felt then hearing the lingo of "credit disbursement curves," "shadow prices," or "economic rate of return calculation." But I was coming from a solid sociological and anthropological tradition of village studies, developed over decades and brought to maturity between the two World Wars by Romania's foremost sociologists and anthropologists, Dimitrie Gusti, and his principal follower, Henri H. Stahl. Like Malinowski, Dimitrie Gusti also had studied in Leipzig, with the same professors as Malinowski--the psychologist Wilhelm Wundt and the economist Karl Bücher—and at roughly

the same time as Malinowski. Later, Dimitrie Gusti created, conceptually and organizationally, what came to be recognized as the “Romanian rural monographic school” (Gusti 1935, 1941), a thoroughly holistic, anthropological manner of studying village culture, customs, beliefs, natural context, economic activities, and political and social organization. Beyond mere ethnography, Gusti and Stahl (1934, 1939) also advocated an action orientation, and directed social research towards activities for improving the life of peasants under study—not bad guidance for my own later work! Many of Gusti’s huge research teams comprised 40, 50, or over 60 members, organized to fill with empirical data the eight-tiered framework of his village monographic method. After the war, when sociology and the anthropology were ideologically banned in the country, surreptitiously studying Gusti’s and Stahl’s pre-war rural monographs was, for me and other young researchers, a way of learning about empirical investigation.

When the first opportunity appeared in the 1960s, I conducted my own village monographs, restudying communities investigated 35 years before by Gusti’s researchers to assess intervening change (Cernea, Kepes, Larionescu et al. 1970). This is how I gradually gained my dual identity as a sociologist and anthropologist, fieldworker and academic. In that period, rife with a distorting dominant ideology, the top-down prescription for research was to ascribe the image of how it was supposed to be but wasn’t. Genuine fieldwork was ostracized, as it implied a threat to the establishment: the threat of deflating the ideological balloon with empirical evidence.

From those years, what I personally cherish most is the contribution I was able to make in the 1960s, with my research team, to resuming and relegitimizing empirical fieldwork in Romania after an interruption of two decades. What I learned then about the iconoclastic power of facts for toppling falsehoods and inviting action served me then, and serves me now in my current work. That empirical research was also what led, first, to my 1967 work in France, at the *Centre D’Etudes Sociologiques*, and then, most importantly, to the unforgettable year I spent in 1970–71 at the Center for Advanced Studies in the Behavioral Sciences at Stanford. That intellectually intense year profoundly restructured my thinking and rejuvenated my conviction in the power of social research—power to explain and power to guide action. In short, it changed my life, and I cannot miss this opportunity, a full quarter-century later, to again express my gratitude to the Center.

With this brief “pre-historical” account completed, I return to the issues of applying sociology and anthropology to development programs. Anthropological work has developed inside the Bank, helping to move the institution from its initial pristine ethnocentricity to an expanding hospitality toward social variables and the “contemporary variations among existing cultures” (Mead 1976.) Since our social scientists have broken some new ground not only for the World Bank, but I submit, for our profession, as well, I will try to derive some lessons of broader validity from our group’s experiences and propose them for your reflection.

Several premises underscore my comments.

The first is that the kind of development I will refer to is *induced* development. This is development purposively pursued, accelerated, and programmed, often guided by policy based on a mix of knowledge and assumptions, and therefore distinct from *spontaneous* development. Thus, while spontaneous development is only observed and passively described in anthropology, *induced* development is one which anthropologists can influence.

The second premise is that anthropologists possess a body of professional knowledge about social organization and cultural systems sorely needed for inducing development effectively, with larger gains and fewer pains.

The third premise is that the key ontological and methodological principles for using social knowledge are common to applied anthropologists working both in international programs (frequently named “development anthropologists”) and in domestic programs. So, much of what I am saying about the former applies also to the latter. The cross-cultural nature of some development programs has spawned much misguided writing about the anthropologist’s role as “intercultural broker.” This is a concept that was developed in dignity (Wolf 1956, Wiedman 1976) but ended up frequently trivialized by practices that marginalized the utility of anthropologists, miscast as mere guides or translators of the local vernacular to their team co-members.<sup>4</sup> In both domestic and international programs, anthropologists can and must be more than “inter-cultural brokers.”

My fourth premise is that development anthropology and development sociology have essential commonalities that prevail over their differences.<sup>5</sup> Therefore, it is beneficial to both professional communities *to bridge their traditional disciplinary divide* and mutually empower their bodies of knowledge and methods, as our Bank group of sociologists and anthropologists has done harmoniously. In what follows, I will refer to both anthropology and sociology and will often use one or the other term interchangeably.

### Content in Project Analysis

The main work bench for anthropological endeavor in the World Bank is the development project. Projects come in all forms, sites, and sectors: from health care systems in Asia to urban infrastructure in Latin America, from irrigation in the Maghreb to reforestation in Pakistan, from education in Africa to reducing environmental pollution in Thailand, to combating AIDS in Uganda to structural adjustment reforms, and to projects for building hydropower dams, curing cataract blindness, or improving family planning and nutrition. More than 1,800 Bank-assisted projects are proceeding today, with Bank financing of \$150 billion and total investment costs of some \$500 billion.

Despite this enormous diversity, some common features exist. Every “project” is a social process, not just a commercial investment, and brings into play an array of different *social actors*. Yet, for a long time, the conventional approach was to treat projects as only economic or technical interventions. How to craft projects as units of purposive and organized change intervention was not, and still is not, for the most part, a science taught in the Academy. We had to invent and learn, in parallel with similar efforts of other colleagues elsewhere.

Noteworthy in this learning process are several shifts and trends over the years. The key shifts we accomplished are: (1) moving away from ghettoizing social scientists in tail-end project evaluations of limited consequence by placing them up front in project design and decision-making; and (2) moving from working on projects only to crafting policies as well. We have forcefully asserted that at issue is not the task entrusted to an individual sociologist but the overall input made by the body of knowledge encapsulated by a discipline. An individual social expert can usefully perform a segmented role, such as an ex post evaluation, but the non-economic social sciences should not be pigeonholed into one segment of the project cycle and excluded from others; nor should they be dispatched to work exclusively on projects, while being ostracized from policy formulation. We have developed a matrix of “entrance points for social knowledge,” tailored along all the key stages of the entire project timetable and including policy work as well (Cernea 1979, 1985). The main lesson of our entire experience is that the key contribution of anthropologists is not to be only data collectors or make static “assessments,” but

to actively *design* the content of induced change and chart the social action path toward accomplishing it.

Does this work make a difference to the countries where we are working and to their people? Affirmative evidence is accumulating. Quantified proof was provided, for instance, by an independent secondary analysis carried out by Conrad Kottak (1985) on a set of 57 Bank-financed projects. Kottak hypothesized that if the projects' sociocultural fit at appraisal is higher, these projects will be associated on average with a higher rate of return at completion. Conversely, initial socio-cultural misfit will be associated on average with lower rates of return.<sup>6</sup> The overall findings showed that enhanced sociocultural fit was associated with economic payoff: the average rate of return at audit time was 18.3% for projects found socioculturally compatible, while for projects that were incompatible socioculturally the returns were less than half that at only 8.6%. These findings are averages, and not every single project matched the overall trend. Better proof may be forthcoming about more recent projects.

However, it is also fair and responsible to say that social analysts have not been mistake-free. Some made erroneous judgments or validated misguided projects. Others have misassessed and mispredicted the behavior of the populations involved. The tools for our analyses, and the methods for translating social knowledge into prescriptions for action, are only developing. Judgments often need to be made with far from perfect social data, and error has not graciously bypassed us. Yet what is novel, despite such errors, is that new variables are taken into account—variables about social and cultural organization. These variables are factored in precisely because social specialists have started to “inhabit” the project-crafting process at its core not just its periphery.

Two observations are in order: the first is about the nature of knowledge needed in applied work, the second about the institutional rules of using it.

Applied and development anthropologists need two categories of knowledge: “knowledge for understanding” and “knowledge for action” (Scott and Shore 1979), to explain and to prescribe. My experience confirms that knowledge for action is indeed a distinct body of knowledge, but one that taken alone can be utterly pedestrian and deceptive. Knowledge for action is valid only if it is incremental to, and relies on, knowledge for understanding, because otherwise, precious as it may be, it is rarely self-sustaining in the long haul. These two distinct categories of knowledge, both indispensable, result from different cognitive itineraries, and only segments of these itineraries pass through university halls. It is part and parcel of applied anthropologists' jobs not just to apply knowledge, but *to create and recreate both types of knowledge* in each of their assignments. This makes the applied job no less demanding than teaching or academic research.

The second observation, also from my Bank home ground, is that inserting social knowledge in projects cannot occur on a significant scale just by simple accretion of in-house individual anthropologists. Knowledge organizations also have formal bureaucratic rules. To create systemic room for new social knowledge, we had to militate for changing these rules, in other words—for institutional change to mandate the use for this kind of knowledge. Modifying rules is an arduous effort in itself. In anthropology we call this “change in the organization's culture.” Although advances have been made, there still is a way to go for mainstreaming and generalizing social analysis in the World Bank.

Important as formal rules are, the actors behind some of the new rules—those who caused rule-changes to happen—are even more important. May I take therefore a minute to give special credit to my colleagues, the anthropologists and sociologists of the World Bank, without whom

the progress I am talking about would not have happened. I want to call your attention to the theoretical-cum-applied work of Gordon Appleby, Michael Bamberger, Doug Barnes, Lynn Bennett, the late David Burcher, Maria Clark, Michael Cohen, Cynthia Cook, Gloria Davis, Sandy Davis, Ashraf Ghani, Dan Gross, Scott Guggenheim, Steve Heyneman, Maritta Koch-Weser, Ayse Kudat, Marlaine Lockheed, Alice Morton, Shem Migot-Adholla, Raymond Noronha, William Partridge, Ellen Schaengold, and many others. The lives of uncounted people across meridians have been significantly improved due to their committed and creative work, which exceeds by far the published record.<sup>7</sup>

Social analysis for development investment decisions is under exacting demands at the Bank, being expected to meet in the words of a Bank Manager, “three explicit characteristics: it must be based on a coherent analytical framework, must be predictive, and it must be prescriptive as well” (Serageldin 1994:vi). My colleagues—responding to these challenges through social theorization or fieldwork, through analytical studies, designing tangible project components or even through earmarking budgetary provisions for social components in many programs—have stimulated and contributed to a more sophisticated treatment by the Bank and many governments of development tasks. They have helped to produce better solutions to human problems.

There is a more general lesson in this: as year after year more social specialists have joined the Bank’s staff, we have gained critical mass in-house. This has enhanced our impact, creating room for professional self-organization, networking, more refined strategies, and informal and formal alliances in intellectual battles (see Kardam 1993). The absence of a “critical mass” in many organizations also explains why the handful of social specialists are hampered and confined in their influence.

External factors have also converged in influencing this in-house institutional process. First, the outside applied community has supported our work inside the Bank in multiple ways. Eminent scholars and development anthropologists—may I highlight especially among them Thayer Scudder, Theodore Downing, Norman Uphoff. Michael Horowitz, Robert Chambers, Conrad Kottak—have contributed so regularly throughout the years that they virtually are part of our in-house community. Second, and equally important, external criticism by NGOs and public interest groups has increasingly emphasized social issues in recent years. Significantly, the criticism from the environmental community (a lobby infinitely more vocal than the social science community) now concentrates not only on physical issues but on sociocultural ones as well. Without taking into account the convergence of these (and other) factors, we could not understand what I described as a major progress of theoretical applied anthropology and sociology in the World Bank-assisted activities.

### **Fighting Biased Development Models**

The kind of knowledge brought into the Bank by development anthropologists did not land in a vacuum of knowledge. It landed on territory long colonized by economic or technical thinking, both with entrenched tenure. I know that this is the case in many other institutions. In large-scale organizations, different bodies of knowledge compete for jurisdiction over tasks and over policy formulation. The interlocking of theories and practice in inducing development creates “battlefields of knowledge” (Long and Long 1992). Therefore, how to carry out intellectual clashes with opposed conceptual paradigms is a pragmatic, tactical question that many of us must face. In our case, intellectual combat has been part of the history of anthropological work in the Bank, and it continues to be so—a creative struggle of ideas, interpretations, and models.

Several different approaches to inducing development display a similar and profoundly damaging conceptual bias. They underestimate the sociocultural structures in the development process. This distortion is often and painfully visible in the design of development projects. Most widespread among these biased perspectives are what I term the *econocentric* model of projects, the *technocentric* model, and the *commodocentric* model. (Comparable biases appear in other institutional contexts.)

By the *econocentric model* I have in mind approaches that one-sidedly focus on influencing the economic and financial variables, regarding them as the only ones that matter. Their presumption is that if you can “get the prices right,” everything else will fall into place. This widespread econo-mythical belief remains a mutilated representation of reality. It simply wishes away the noneconomic variables from theory, but does not remove them from reality. But we have seen that when the social determinants of development are left out by econocentric mindsets, projects display an unrepressed and not at all funny propensity—they fail.

By the *technocentric model*, I have in mind the approach that caters to the technological variables of development more or less “in vitro,” disembodied and disembodied from their contextual social fabric. “Technology transfer” was once described as the ultimate development paradigm. Although this rage has been muted, there is still considerable disregard of the necessary proportionality between developing new physical infrastructure and creating the social scaffolding for it simultaneously. Technocentric models underdesign and underfinance the social scaffolding. My point is that it is not enough to “get the technology right” for the missing social infrastructures to spring up automatically overnight, by God’s “Fiat lux.” Overcoming technocentrism requires careful social engineering for institution building, to induce and nurture the cultural arrangements in which the physical infrastructure is necessarily enveloped.

By the *commodocentric model* I have in mind scores of programs that focus on the commodity, the “thing,” more than on the social actors that produce it. They focus on coffee production but less on coffee-growers, on “livestock development” but not enough on herders, on water conveyance but not on water users. “Putting people first” is not a familiar idea in these approaches.

Development anthropology and sociology must militantly reject such fallacious models or exaggerations and provide integrated, convincing, and actionable alternatives. Development is not about commodities. It is not even about new technologies or information highways. It is about people, their institutions, their knowledge, their forms of social organization. This is why I think that non-economic social scientists must be present and work hand-in-hand with economists and technical experts in the core teams that formulate development paradigms, policies, and the content of specific programs (Cernea 1990).

My personal conviction is that shying away from engaging in intellectual battles about the paradigms of development results not in more “friendly acceptance” of applied anthropological or sociological work, but in less. By now, you have heard my answer about strategy in conceptual clashes on the battlefields of knowledge. We must assert our conceptual differences, because they make a difference. We must take firm positions without posturing, must be earnest without an offensively earnest tone, and must be opinionated while being free of fixed opinions. For applied social scientists, quibbling only for improving practical fixes is never enough. Winning requires intellectual wrestling and theoretical engagement.<sup>8</sup>

## Where Do Biases Originate?

A question is inescapable at this point: Where do these distorting conceptual models originate? What should be done, and where should we do it, to correct or prevent them?

My answer is a brief story. Not long ago, I was invited to give a seminar for the social science faculty of an Ivy League university. During the discussions, some highly respected academic anthropologists expressed their well-worn hopelessness and skepticism about the legitimacy and effectiveness of development anthropology or applied sociology, and in general about development dominated by biased models.

My response was, in turn, a question: “Where do you see the roots, I asked, of these biases? Why do they persist and reproduce themselves?”

There was silence, or circuitous explanations.

I gave my own answer to these questions. Yes, there is a definite place where these models originate. “My Bank colleagues with econocentric or technocentric biases, I said, came from this place, from among your own best and brightest graduates. From your university, or from other universities of similar excellence. They are the former *magna cum laude* students in finance, economics, or technical specialties, who spent eight to ten years here next to your anthropology or sociology department doors, but never entered, and were not touched intellectually by your scholarship.” Indeed, I explained to my academic colleagues. “I work at the receiving end of your university’s line of products. Many of the former students of your university bring to the Bank, or to governments and the private sector, biased, one-sided conceptual models. The models I am fighting reflect nothing else, unfortunately, than the training received in your university’s economics department, training that inculcated models that ignore social variables.” Can we correct afterwards what the university has not done at the right time? As the seminar’s chairman volunteered, there was “blood on the floor” after that seminar, but it was a discussion useful for all of us.

This is a huge issue. I submit that the way social sciences are taught in most universities in the U.S., and across the developing world, goes sadly against, rather than in support of, the role social science knowledge must exercise in modern societies. Trends and practices are at work, by commission or omission, that undermine the proactive role of non-economic social sciences.

In my view, there are two major strategic errors in academia in this respect. The first is the minuscule dose, or the “zero dose,” of social science taught to the vast majority of students majoring in non-social fields. Quite often, what they are told about social sciences does not greatly help either when they are taught generalities rather than the parts of anthropology or sociology that are directly relevant to their own specialization and future work. The second strategic error is the scarce or often non-existent curriculum emphasis on the teaching of social science for practice, as opposed to teaching for general understanding or *weltanschauung*. The results of these anachronistic attitudes, to return to my Vatican metaphor, are that the armies of “non-believers” expand with every new cohort of undergraduates while the “Secretaries to the Non-believers”—you, I, my colleagues—face a harder uphill battle.

The first battlefields of knowledge for the minds of tomorrow’s developers and policymakers are in our universities—and this is where the battle should not be lost. I leave it to my academic colleagues to draw the sober conclusions about the major restructurings indispensable, indeed imperative in the teaching of social sciences, for they prepare the terrain—fertile or infertile—for society’s practical use of social science.

## Conceptual Models for Development Anthropology

My next question at this point is: if we propose to put biased models of development interventions on trial, how do we make the prosecution's case for anthropology?<sup>9</sup> How do we argue anthropology's "can do" claim to relevance?

The constructive argument is far more crucial than the critique, for several reasons, not least because the way we legitimize to others or to ourselves the need for social analysis in development interventions creates a structure of expectations that becomes compelling. Eventually, this turns out to be the way we end up practicing social analysis. In other words, if we argue just the pragmatic, short-term operational benefits' side of using anthropology, we will end up playing a mundane fix-it-here and fix-it-there role. If, however, we convincingly construct the argument for a theoretical applied anthropology, we lay claim to having voices over the substance of development paradigm and policies.

The rigid dichotomy between applied and theoretical anthropology is a simplistic representation that must be rejected. The "practice" of anthropology can generate value-added for society only if it is practiced *as theoretical applied anthropology*. What has to be "applied" through applied anthropology is our theoretically generalized knowledge about societies and cultures. What else would anthropologists have to apply? They use the research methods. What they apply is the storehouse of knowledge (van Willingen 1993, Angrosino 1976). Anthropologists bring to their work the knowledge about what is general in individual local societies ("cross-cultural commonalities") and proceed to uncover what is unique in that individual society. I cannot imagine applied anthropology without this "theoretical understanding" lodged and carried along in the mental back-pack of practicing anthropologists.

Upon scrutinizing much of the literature, I can see several types of arguments—models of rationalizing, or ways of "making the case"—for development anthropology. I'll refer to three of them. (Another, the "cultural brokerage" model, was mentioned earlier.) Not all the models in circulation are correct or equally powerful. I submit that some of them would result only in a peripheral and diminished, even if real, role for anthropologists. Our discipline can do better than that.

The widest spread but weakest model is what I'd call the add-on model (or argument) for anthropology's or sociology's case. This is the time-honored route of vaguely claiming that there are "some" cultural-social implications to all environmental issues (or to health, or to that whatever else many be the issue of the moment) and, therefore, one needs anthropology, too, in addition . . . and on, and on. You know the litany.

This way of making our case inherently begs for a marginal role, a glorified place at the periphery, a stereotypical add-on: "me too." Such an add-on is not even our sacred holism, because if holism is pleaded as an additive list of traits it becomes un-holy, a messy eclectic mix. Consequently, the "add-on" model is neither compelling nor apt to change opposed mindsets. In our vernacular, one could also call it the "hodgepodge model," because it sees reality syncretically as an amalgam of aspects, without grasping structures, priorities, and causalities within the belly of the social beast.

The core point, as I will stress further, is that the social-cultural variables are not just another "aspect," a minor side of a mainly technical issue. These variables are essential to the structure of most major problems we encounter.

Another model is *behavioral*, so named because it focuses on the need for individuals to understand and amend their detrimental behaviors *vis-à-vis* the environment. This model is not



invalid, because education and attitudes are significant for shaping individuals' behavior. It is merely insufficient. Indeed, it gives little weight to group structures and vested interest. It also places the anthropological endeavor in the province of environmental education—a relevant but not central position either. The logic of this argument pushes anthropologists toward a psychological and educational approach directed primarily to the individual's misconstrued perceptions and attitudes, but leaves out the structural societal dimensions.

An alternative model—in my view, the strongest way of arguing the case for, and actually practicing, applied anthropological and sociological analysis—is to focus on the patterns of *social organization* within which social actors act. Predicating the value-added contribution of anthropological analysis primarily on revealing the models of social organization that underpin social processes and link their social actors will best position applied research on the strongest theoretical ground. This “locks” the laser of applied inquiry onto structural issues, giving it centrality and maximizing its contribution. This is the natural position that anthropological/sociological analysis should occupy, not because social scientists subjectively so desire it, but because of two indisputable facts. First is the centrality of social actors in development; second is the knowledge about patterns of social organization and their actors' motivations. This is the very core of the anthropological and sociological enterprise, the comparative advantage and special competence of our disciplines.

### **The Focus on Social Organization in Applied Research**

To some, suggesting social organization as the underpinning conceptual matrix in applied anthropology may appear, at first sight, as impractical or remote. We all know that applied anthropologists are expected to be “pragmatic,” “operational,” quick on their problem-solving feet. Yet, in my own fieldwork, taking social organization as the starting point for conceptualizing, thinking through, and analyzing specific practical problems in very diverse cultural contexts turned out every time to provide precisely the unexpected and original frame of reference absent in my economists or technical colleagues' perspectives. This was true in my work on pastoralists in Senegal, on reforestation constraints in Azad Kashmir, and on irrigation and water-user societies in Thailand, Mexico and India. Social organization provides a contest and a launching pad for analysis, points out to linkages and dependencies, reveals encoded knowledge and meanings,<sup>10</sup> and helps identify all possible social actors, local and distant, with a stake in the problem under analysis.

Furthermore, “bringing social organization in” does not send applied analysts always and necessarily to the *macro*-societal level. It gives applied researchers, working at whatever social level, the theoretical impetus to identify patterns of social organization in large social bodies, in remote rural communities, in inner city quarters, in service processes and sub-systems, or even in small “street corner societies.” This is true also regardless of whether the problem at hand concerns environmental pollution, health services, crime in the neighborhood, resettlement of displaced people, or irrigation water supply systems.

The analyst should not be surprised—indeed, is rewarded—when such conceptualization redefines both the problem at hand and the conventional solutions. Robert Merton pointed out (1973:94) that “perhaps the most striking role of conceptualization in applied social research is its transformation of practical problems by introducing concepts which refer to various overlooked . . . [and which may] lead to a statement of the problem that is dramatically opposed to that of the policymaker” or of whoever else is the user of applied research.

Environmental management is a domain that compellingly illustrates, first, the centrality of anthropological analysis, and second, the analytical superiority of the “social organization of actors” model over the “add-on model” or the “behavioral” model. Anthropological knowledge—from Malinowski to Radcliffe-Brown, from Raymond Firth to Fredrik Barth—is traditionally grounded in the study of the forms and patterns of social organization within which societies use the natural resources on which they depend. This storehouse of knowledge and research methods is a major thesaurus for framing environment policies and resource management programs in both developing and developed countries.

Yet the centrality of social organization issues to environmental problems and programs, however familiar it is to us, is *not* a self-evident truth. This is sadly proven by the abundance of one-sided technological eco-speak, or one-sided economythical “solutions,” and by the dearth of in-depth social understanding of these issues. Indeed, the intellectual debate about resource domains is overwhelmed by the enormous diversity of the technical issues intrinsic to each resource. The overall picture becomes fragmented into technical resource-specific approaches, while the common social underpinnings of all these domains remains clouded, less visible.

The intellectual argument that I regard as the main entry point for social scientists into the environmental debate is that an improved and sustainable use of natural resources depends decisively on improving the patterns of social organization for their management by the users themselves. Who are these users? Primarily, the world’s enormous mass of small farmers. My basic proposition is that *effective environmental policy must promote and rest on appropriate social organization*. Neither technology unembedded in social organizational structures nor free-market fundamentalism unable to control externalities can alone tackle runaway resource abuses.

Anthropologists as social architects must help build practical models for collective action in resource management. And we have to recognize that we must also revisit some of our own models, lovingly advocated in the anthropological literature but ineffective—for instance, the rather romantic model of community-based tree-plot planting. Communities are generally heterogeneous social entities and, thus, are seldom able to be the social agents of collective (unified) social actions. A case in point is the costly failure of most “village woodlot” and “community woodlot” schemes financed through hundreds of millions of wasted dollars. Although long praised uncritically by many social scientists, they have failed—and failed for social design reasons. Inadequate social models have misled many investment strategies into financing approaches which, on social grounds alone, could not—and did not—succeed, thus wasting both goodwill and money (Cernea 1992).

The focus on social organization compels development analysis to be actor- oriented. This is germane to both the explanatory and prescriptive functions of applied research. We did not claim that people were totally out of sight in conventional approaches. But we showed that the characteristics of a given social organization were stripped of the flesh and blood of real life in what I termed econocentric or commodocentric models. We demonstrated that key social actors of development were dealt with as an afterthought, mostly as passive, nonparticipating recipients.

Our argument was, and is, this:

Putting people first in projects is not a goodwill appeal or a mere ethical advocacy . . . It is a theoretically grounded request to policy makers, planners, and technical experts to explicitly recognize the centrality of what is the primary factor in development processes . . . It calls for changing the approach to planning. The

requirement to admit *the centrality of people in projects* is tantamount to asking *for reversal of the conventional approach to project making . . . The model adopted in projects that do not put people first clashes with the model intrinsic to the real social process of development*, at the core of which are—simply—its actors (Cernea 1985/1991:7–8).

Relying on theoretical and empirical knowledge about models of social organization provides development anthropologists with far-reaching inventories of analytical tools and social techniques. It is important not only to define social organization theoretically but also to “deconstruct” social organization into its building blocks, such as: the social actors at the local level; the social contract governing relations (including conflicts) between users and stakeholders (local and distant); prevailing symbolic and cultural systems; rules of entitlements, e.g. usufruct, ownership or custodianship rules; authority systems, and enforcement mechanisms; an infinite range of producers’ organizations (from family based units to large corporate enterprises); macro-social factors that undermine or solidify local social organization; etc. In turn, this facilitates creative social engineering work. For instance, the social expert must be able to figure out which available building blocks can make up more adequate social arrangements and culturally-sound action strategies in given circumstances.

As social architects, anthropologists are called to define the needs for associational infrastructure, social capital, grassroots organization or high order institutions, and help design them. In her Malinowski lecture, Elizabeth Colson pointed out that our Society for Applied Anthropology was created to promote the use of “skills of social engineering” (1985:192), and Raymond Firth, in his Malinowski address, emphasized the complexity of “analyzing the strength of relations in human engineering” (1981:196). Social engineering skills are indispensable for designing better social arrangements, improving institutions, enabling legal frameworks, and constructing adequate incentive systems. What for policymakers and development managers may seem “elusive” sociological elements can be translated, with help from the social scientist, into policy prescriptions and pragmatic action-oriented strategies.

When they work as social architects, anthropologists regularly face economic variables. What they have to propose bears directly upon the economy. Yet applied anthropologists often skate rather lightly over the economic determinants of social organization and their implications. We have very much to learn (not just criticize) from our colleagues the economists about economic analysis and measurement methods. When anthropologists bypass economic variables—and I have witnessed many such instances—the resulting recommendations are embarrassingly naïve or directly erroneous. Conversely, when they consider the relevant economic dimensions, the results are powerful.

Take, for instance, the path-breaking research and policy prescriptions developed by the Institute for Development Anthropology (IDA) in their Senegal River Basin studies. That study, as Michael Horowitz wrote, “was persuasive in large part because it provided hard field data on economic decision-making at the level of the rural production unit” (1994:11), and considered variables absent in earlier economic analyses, such as yields per unit capital, per unit labor, and per unit land.

We must remind ourselves of Gunnar Myrdal’s words, when he received the Malinowski Award, about how he lost his “inhibitions about transgressing the boundaries of separate social sciences,” to delve into anthropology and sociology. He invited anthropologists to reverse the journey. “In dealing with a problem,” Myrdal said, “it could never be a legitimate excuse that

certain facts or casual relations between facts lay outside one's own field of knowledge" (1975:327).

Living daily inside an economic tribal culture, I can confirm that anthropology as practice can—indeed must—be enriched and strengthened by learning more from economic concepts and by internalizing quantifying methodologies. This is not a ritualistic tribute to powerful neighbors; economic knowledge is intrinsically indispensable for understanding social organization patterns anywhere. Anthropologists cannot relegate the study of economic variables to the subdiscipline called “economic anthropology.” The understanding and manipulation of economic variables through applied social engineering is essential for *all* development anthropologists who take the concept of social organization as their basic guide.

### **Can Theory be Derived from Applied Research?**

Applied Anthropology is often deprecated by unfriendly voices and accused of being intrinsically incapable of benefiting theory. Long ago, Malinowski firmly advocated a “practical anthropology” (1929) concerned with answering the issues of the day. He rejected the “erroneous opinion . . . that practical anthropology is fundamentally different from theoretical or academic anthropology. The truth is that science begins with application. What is application in science, and when does ‘theory’ become practical? When it first allows us a definite grip on empirical reality” (Malinowski 1961:5).

Applied anthropology facilitates such a “grip” on empirical reality. Even more, it is able to help change social reality.

The view that applied research is atheoretical—either does not use theory or does not lead to theory—disempowers the discipline of anthropology. For some in the academic community, this opinion justifies disengagement and less concern with the public issues of the day. But this view has also induced some resigned defensiveness among a segment of the practicing anthropological community.

This charge is not only misplaced, it is epistemologically unwarranted. First, the research objects of applied anthropology generally have no less intrinsic potential to generate theory than the research objects of academic anthropology. Development programs are complex social processes no less theory-worthy than kinship systems, or reciprocal gift-giving, or funerary rituals. Second, the methods of data generation are not necessarily different—many are similar—in applied and academic research. The overall research designs are different, but neither holds the monopoly on method. Individual researchers can choose either to distill from their data a course for future action or to pursue theoretical proposition building. Both are valid endeavors.

Sol Tax was correct when, in his Malinowski lecture, he expressed the conviction that:

We deal not with a distinction between pure and applied anthropology but rather an amalgam or continuum of the two, a differing mixture of models in all of us who are anthropologists . . . At different times one of us can be doing much that is theoretical and general: at other times much more than is particular and applied, and at still other times engage in activities that are inextricably intertwined (Tax 1977:277).

Whatever our personal inclinations, the general state of our art—which ultimately is more than the sum of each individual’s work—does reveal difficulties of growth and unresolved problems. Although work in applying anthropology has expanded significantly during the last decade, progress appears mostly as a vertical accumulation of primary case accounts—with too little horizontal cross-synthesis of comparable cases. Fragmentation results also from the sheer mass of what is published. Methodologically, the overestimation of “rapid appraisal methods” has resulted in all-too-ready excuses for sloppy assessments, for weakening longitudinal research, and for neglecting the collection of long time-series data. Comparative research is little practiced. It seems that most practitioners are so driven by their case-focused pursuits that little time is left for the essential task of looking back and around—for comparison, thinking, synthesis, and generalization. These weaknesses should be of concern to all of us.

I submit, however, that the task of generalizing empirical data resulting from applied research is not the charge only of those who define themselves as applied anthropologists. It is equally a task of those working in academic and theoretical anthropology and sociology. A vast volume of factual material is laid out in countless applied reports and studies and is readily available to those interested in extracting theory from empirical findings. No tribal taboo forbids access to these empirical treasures to non-applied academic anthropologists. An outstanding example of what can be done for theory with data from applied research and case studies was given by Goodenough (1963). Spicer termed Goodenough’s approach to using applied findings for theory building as an “exciting discovery in anthropology” (1976:134).<sup>11</sup> Yet it is rather sad to realize how little this promising breakthrough has been replicated. I urge all colleagues in anthropology—academic as well as applied—to join in the effort of distilling theoretical propositions and methodological lessons from good applied research findings and experiences. “All human behavior,” noted George Foster in his Malinowski lecture, “is grist for our mill, and all good research data—whatever the context in which they are gathered—have theoretical potential” (Foster 1982:194).

To sum up: first, data from good applied research do have theoretical potential; second, exploiting that potential is a collective (professional community) task, rather than a segregated subgroup task; third, a broad spectrum of “theoretical products” can be extracted from applied work—concepts, propositions, methodologies for purposive action, hypotheses, models, etc.

### **The “Third Leg” of the Dichotomy: Policy Development**

The part of development anthropology that perhaps best demonstrates the infertility of a dichotomy between applied and theoretical anthropology is the work in policy formulation. Such work can be neither claimed nor performed by an atheoretical applied anthropology. To combine and convert knowledge and field findings into predictive and prescriptive policy propositions is intrinsically a theorizing operation.

Tom Weaver has written a passionate argument in support of anthropology’s potential as a policy science, showing how it engages “the very basis of this field, its goals, its subject matter, research techniques, theory, methodology, its very future” (Weaver 1985:203). In this vein, our experiences in practicing anthropology at the World Bank have taught us a crucial lesson: however effective our anthropological inputs have been in various individual development projects, the most important successes, those with the farthest reaching impact, have been in policy formulation.

Several categories of World Bank policies have incorporated substantive anthropological/sociological contributions, yet such contributions are little known. My point is to show not just what we have done, but what can be done along policy lines.

**CROSS-SECTORAL SOCIAL POLICIES.** Among the essential policies of this kind, written virtually in full by social scientists, are the World Bank's policy on involuntary population resettlement entailed by development programs (World Bank 1980, 1986, 1991c, 1994; see also Cernea 1988, 1993; Guggenheim 1992; Partridge 1989), the policy on indigenous peoples affected by development projects (World Bank 1982, 1990; Davis and Wali, 1993), and the policy regarding non-governmental organizations in Bank-supported activities (World Bank 1989; Cernea 1989).

**SECTORAL POLICIES.** Non-economic social scientists have made very substantial contributions in the formulations of several of the Bank's major sector development policy statements in cooperation with technical specialties, such as the urban growth policy (World Bank 1991a), the policy on investments in primary education (World Bank 1990); the forestry and reforestation policy (World Bank 1991b; Cernea 1992a), and the water resources policy (World Bank 1993).

**SOCIOECONOMIC AND ENVIRONMENTAL POLICIES.** The poverty alleviation policy, the environmental policy guidelines, and others have benefited significantly from similar inputs. Vast efforts are being invested now in codifying participatory approaches and preparing policy guidelines for other social policy domains.

For all these domains, the multiplier effect from investing the knowledge and efforts of social scientists in policy formulation is enormous. Consider the case of involuntary population resettlement. I have written too much on resettlement lately to repeat this here, so I call your attention to one fact only. At least ten million people each year are subjected to forced displacement by the construction of dams and urban infrastructure. And resettlement policies are, perhaps, the most telling case of breaking the isolation and disinterest in which much good anthropology used to be held. As you know, a valuable body of knowledge was generated in the 1960s and 1970s on the disasters of forced displacement, yet to no avail. That research was gathering dust on library shelves, largely ignored by planners and policy makers. In the late 1970s, we took that knowledge as a theoretical basis for writing a policy on resettlement for the Bank. But rather than repeat descriptions of development's disasters, we proposed policy and operational solutions to solve them. The Bank then adopted the policy in 1980.

What happened next? This is most significant; over these last fifteen years, a "cascade" of policy advances occurred in this area, all with the participation of social scientists.

The Bank's resettlement policy itself was improved in four subsequent rounds (1986, 1988, 1991, 1994).

Promoting the resettlement policy beyond the Bank itself, we helped to draft a policy statement on resettlement (essentially identical with the Bank's policy) for all 25 OECD countries, to be applied by the bilateral donor agencies of OECD countries in their aid programs. Formal adoption of this policy by OECD ministers took place in 1991 (see OECD 1992).

The multilateral development Banks for Asia, Africa, and Latin America have adopted or are now vetting their resettlement policy.

Some countries (Brazil, China, Colombia, and others) have developed domestic social policies and legal frameworks on resettlement, borrowing more than a page from the policy written by social scientists for the World Bank.

Further, and unexpectedly, some anthropologists in the United States have proposed that the US Government adopt the World Bank's resettlement policy for resolving the Navajo-Hopi dispute (Brugge 1993)—a long shot, though, we all agree!

And at the social summit in Copenhagen in March 1995, as a result of an explicit initiative of Bank social scientists supported by other anthropologists (particularly Downing) and backed by several governments (Uganda, Canada, Switzerland) and by NGOs, involuntary resettlement issues have been incorporated in the Summit's Program of Action (see Cernea 1995).

So this is an indisputable fact: we are today a long, long way from the point when anthropological studies on resettlement were languishing forgotten on library shelves and no resettlement policy whatsoever existed, neither in the Bank nor anywhere else in the developing world.

For our "state-of-the-art housekeeping," we should also note that these policies are not the whole story. During the past fifteen years the body of social science knowledge on resettlement itself has been greatly expanded, enriched, tested, corrected and recreated due primarily to operational applied research. A series of seminal papers on the anthropology of displacement and resettlement have resulted from on-the-ground association with resettlement operations, and have stimulated creative contributions in legal thinking and other related fields (Bartolome 1993; Cook 1993; Shihata 1993; Davis and Garrison 1988; Downing 1995; Guggenheim 1990, 1993; Partridge 1989; Partridge and Painter 1989; Cernea 1988, 1990, 1993, 1995; Scudder 1991; McMillan, Painter and Scudder 1992; Mathur 1994). This knowledge accumulation is bound to yield further progress in the years ahead.

Most important is that the social science contribution has resulted in major changes in the practice of involuntary resettlement throughout the world—changes in resettlers' entitlements, in planning, in financing, even in turning around insensitive bureaucracies. The overarching meaning of all these changes is that the lives and fates of many people worldwide are improved through better protection and added opportunities. We know that resettlement remains painful, and much does not yet happen along ideal policy lines. But, in improving the development process, we are now farther ahead than any development anthropologist would have dared to dream just a decade ago.

For me personally, this kind of progress is the most gratifying reward. I feel privileged, indeed, to have had the chance to be part of this process, to be a development applied social scientist, and am excited to practice this vocation. My message tonight is that our profession is consequential. It makes a difference. I would want this message to reach the students who engage in the study of anthropology and sociology, and who consider dedicating their life and work to applying this knowledge. It is not an easy profession, but it is a generous and useful one.

On this very point, it is appropriate to conclude. In its great wisdom, the Talmud teaches that "one who saves a single life is as one who saves an entire world." From this view we can derive courage and motivation for each single one of our projects, big or small, be it a major policy or a

small inner city health project. Each of you has probably experienced being the local “Secretary to the Non-believers” in one place or another. But we are gaining converts. We “win” when we make lasting professional contributions that benefit many people. Let us broaden our knowledge, refine our tools, and embolden our moral commitment to do this beautiful work.

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### Notes

<sup>1</sup>Commenting on the difficulties of the first anthropologist who in 1950 joined the World Health Organization in Geneva, George Foster wrote: “she did not plan to recommend specific courses of action. (Her) assignment was plagued by the problem that has affected many subsequent anthropologists: neither she nor the hiring organization, really knew what she should do. Shortly after joining WHO she left for India and Southeast Asia where, in the regional office, her reception was unenthusiastic” (1982:191). And Foster quoted further the personal description given him by that colleague: “I was left to make my own plans and schedules and I was more than a little perplexed as to what was expected of me . . . Much the same perplexity about my role obtained when I returned to Geneva”(Foster: 1982:191).

<sup>2</sup>Two years prior to my joining the Bank, two consultants were commissioned to carry out an in-house study to assess whether or not anthropology could contribute—and if yes how—to Bank activities. After six months, a long—and in my view good—report was submitted; nonetheless, that report did not convince anybody. Neither of the two consultant authors was retained in the institution.

<sup>3</sup>The special Division for “experimental” poverty projects was headed by Leif Christoffersen, dispatched to this new position from his previous assignment as Assistant to the President of the World Bank, Robert McNamara, and was located in the Bank’s Central Agriculture and Rural Development, directed by Montague Yudelman.

<sup>4</sup>In a broad sense, what anthropologists typically do is try to understand and explain culture. In this context, “cultural brokerage” is non-controversial as a concept and role. What is objectionable—and I witnessed numerous instances of this practice—is the limitation of anthropologists’ roles to the minor aspects of language intermediation or other mechanics of “development tourism,” to use Robert Chambers’ expression, while their competence on essential issues of social organization, stratification, ethnicity, and local institutions is not treated as indispensable to the job at hand.

<sup>5</sup>I note, as a testimony to the intimate relationship between these two disciplines, that Malinowski himself did not hesitate to term his analyses of the Trobriands “sociological,” not just “anthropological” (Malinowski 1922).

<sup>6</sup>Kottak’s secondary analysis was “blind,” in that the coding of sociocultural and socioeconomic variables (including variables of social organization, stratification, ethnicity, gender-based divisions of labor, and others) was completed without knowledge of the project’s economic performance only after the social coding had been done were the rates of return introduced in the analysis.

<sup>7</sup>The written record of their published work is described in our annotated bibliographic volume; *Sociology, Anthropology and Development, An Annotated Bibliography of World Bank Publications 1975–1993*, ESD Studies and Monograph series No. 3.

<sup>8</sup>Of enormous impact in this not-always-smooth in-house theoretical engagement has been our long and tenacious program of sociological seminars and training courses, delivered by Bank sociologists and anthropologists to the rest of our colleagues—hundreds and hundreds of such seminars over the years (see also Kardam 1993).



Many outside social scientists have joined us in this intellectual reconstruction process. There is a lesson in this respect as well—about shaping and carrying out strategies of gradual cultural and institutional change over time.

<sup>9</sup>This is a deliberate reversal of Polly Hill's title to her 1986 book "Development Economics on Trial: The Anthropological Case for a Prosecution." Unfortunately, despite many valid observations about statistics, surveys, and so on, this book, in my view, did not fulfill the promise of its provocative title. Nor did it construct the *positive* case for anthropology.

<sup>10</sup>What accounts for the forcefulness and path-breaking quality of Malinowski's selective analysis of one of the Trobrianders' activities—economic exchange? The answer is the contest: social and cultural. As Malinowski wrote, in this monograph the reader "will clearly see that, though its main theme is economic—for it deals with commercial enterprise, exchange and trade—constant reference has to be made to social organization, the power of magic, to mythology and folklore . . ."

<sup>11</sup>Praising Goodenough's *Cooperation in Change* for its success in developing "pure theory" by interpreting descriptive case studies of administrative action, Edward Spicer wrote: "In a sense [Goodenough's book] represented a swing back full circle from the excitement of anthropologists' discovery that they could analyze cases as ethnographic data in a framework useful to administrators, to excitement over discovering that the processes of administration could be brought into the context of pure theory in anthropology with resulting illumination of concept and new generalization" (1976:134).

### References Cited

- Angrosino, Michael V., ed.  
1976 Do Applied Anthropologists Apply Anthropology? SAS Proceedings No. 10. Athens: University of Georgia Press.
- Barth, Fredrik  
1966 Models of Social Organization. Royal Anthropological Institute of Great Britain and Ireland. Occasional Paper No. 23. Glasgow.  
1993 Balinese Worlds. Chicago and London: University of Chicago Press.
- Bartolome, Leopoldo J.  
1993 The Yacyreta Experience with Urban resettlement: Some Lessons and Insights. *In* Anthropological Approaches to Involuntary Resettlement: Policy, Practice, and theory. M. M. Cernea and S. E. Guggenheim, eds. Boulder, Co.: Westview Press.
- Brugge, David M.  
1993 The Relocation of Navajos from the Hopi partitioned lands in Relation to the World Bank Standards for involuntary Resettlement. *In* Papers from the Third, Fourth and Sixth Navajo Studies conferences. June-el Piper, ed. Window Rock, Arizona: Navajo Nation Historic Preservation Department.
- Butcher, David M.  
1990 A Review of the Treatment of Environmental Aspects of Bank Energy Projects. World Bank, PRE Working Paper, March.
- Cerneia, Michael M.  
1979 Entry Points for Sociological Knowledge in the Project Cycle, seminar paper, manuscript.  
1982 Indigenous Anthropologists and Development-Oriented Anthropology. *In* Indigenous Anthropology in Non-Western Countries. H. Fahim, ed. Pp. 121–137. Durham, North Carolina: Carolina Academic Press.  
1988 Involuntary Resettlement in Development Projects: Policy Guidelines in World Bank-Financed Projects. Washington, D.C.: The World Bank.  
1989 Nongovernmental Organizations and Local Development. Washington, D.C.: The World Bank.  
1990a Social Science Knowledge for Development Projects. World Bank Discussion Paper No. 114: Washington, D.C.

- 1990b Poverty Risks from Population Displacement in Water Resources Development. Harvard University/HIID, DDP No. 355, Cambridge, Mass.
- 1992a Re-Tooling in Applied Social Investigation for Development Planning. Some Methodological Issues. *In* Rapid Assessment Procedures (RAP): Qualitative Methodologies for Planning and Evaluation of Health Related Programs. N. Scrimshaw and G. R. Gleason, eds. Boston: INFDS.
- 1992b A Sociological Framework: Policy, Environment, and the Social Actors for Tree Planting. *In* Managing the World's Forests: Looking for Balance Between Conservation and Development. N. Sharma, ed. Pp. 301–355. Iowa: Kendall/Hunt Publishing Co.
- 1993 Anthropological and Sociological Research for Policy Development on Population Resettlement. *In* Anthropological Approaches to Involuntary Resettlement: Policy, Practice and Theory. M. M. Cernea and S. E. Guggenheim, eds. Boulder, Co: Westview Press.
- 1995 Understanding and Preventing Impoverishment from Development-Induced Displacement. Reflections on The State of Knowledge. Keynote Address to the International Conference on Development-Induced Displacement and Impoverishment. University of Oxford, England.
- Cernea, Michael M., ed.  
1985/1991 Putting People First. Sociological Variables in Development. Oxford University Press.
- Cernea, Michael M., and Scott E. Guggenheim  
1985 Is Anthropology Superfluous in Farming Systems Research? Farming Systems Research. Kansas State University Research Series Vol 4, No.9.
- Cernea, Mihail, Gh. Kepes, M. Larionescu, et al.  
1970 Doua Sate: Structuri Sociale is progress Technic. Edit. Pol.. Bucuresti
- Colson, Elizabeth  
1985 Using Anthropology in a World on the Move. Human Organization 44(3):191–196. (Republished at <http://www.sfaa.net/>.)
- Cook, Cynthia  
1993 Involuntary Resettlement in Africa. World Bank Technical Paper No. 227. Washington D.C.
- Davis, Shelton, and Alaki Wali  
1993 Indigenous Territories and Tropical Forest Management in Latin America. World Bank Environment Department Policy Research Working Paper. Environmental Assessment and Programs, Washington, D.C.
- Davis, Gloria, and Helen Garrison  
1988 Indonesia: The Transmigration Program in Perspective. A World Bank Country Study. Washington, D.C.
- Downing, Theodore E.  
1994 Mitigating Social Impoverishment when People are Involuntarily Displaced. Paper presented at the Oxford Conference on Development-Induced Displacement and Impoverishment.
- Firth, Raymond  
1969 Essays on Social Organization and Values. London School of Economics. Monographs of Social Anthropology, No. 28.  
1981 Engagement and Detachment Reflections on Applying Social Anthropology to Social Affairs. Human Organization 40(3):193–201. (Republished at <http://www.sfaa.net/>.)
- Foster, George M.  
1982 Applied Anthropology and International Health: Retrospect and Prospect. Human Organization 41(3):189–197. (Republished at <http://www.sfaa.net/>.)

Goodenough, Ward

- 1963 Cooperation in Change: An Anthropological Approach to Community Development. New York: Russell Sage Foundation.

Guggenheim, Scott E.

- 1990 Resettlement in Colombia: The Case of El Guavio. *Practicing Anthropology* 12 (3):14–20.  
 1991 Development and the Dynamics of Displacement. *In* Workshop on Rehabilitation of Displaced Persons. A. P. Fernandez, ed. Myrada: Bangalore.  
 1993 Peasants, Planners, and Participation: Resettlement in Mexico. *In* Anthropological Approaches to Involuntary Resettlement: Policy, Practice, and Theory. M. M. Cernea and S. E. Guggenheim, eds. Boulder, Co. Westview Press.

Gusti, Démètre

- 1935 La momographic et l'action monographique en Romaine. *In* Etudes de Sociologie et d'ethnologie juridique sous la dir. de René Maunier: Paris.  
 1941 La Science de la Réalité Sociale. Introduction à un Système de Sociologie, d'Éthique et de Politique, Bibliothèque de Philosophie Contemporaine. Alcan, Paris: Presses Universitaires de France.

Horowitz, Michael M.

- 1994 Development for Anthropology in the Mid-1990s. *Developmental Anthropology Network* 12:1/2

Kardam, Nuket

- 1993 Development Approaches and the Role of Policy Advocacy: The Case of the World Bank. *World Development* 21 (11): 1773–1786.

Kindelberger, Charles, P

- 1952 Review of "The Economy of Turkey," "The Economic Development of Guatemala," Report on Cuba." *Review of Economics and Statistics* 34:391–392.

Kottak, Conrad Phillip

- 1985 When People Don't Come First: Some Sociological Lessons from Completed Projects. *In* Putting People First: Sociological Variables in Development. M. M. Cernea, ed. Oxford University Press.

Long, Norman, and Ann Long, eds.

- 1992 Battlefields of Knowledge: The Interlocking of Theory and Practice in Social Research and Development. London and New York: Routledge.

Malinowski, Bronislaw

- 1922 Argonauts of the Western Pacific. Reprinted in 1984 by Waveland Press Inc., Prospect Heights, Illinois.  
 1929 Practical Anthropology. *Africa* 2:22–38.  
 1961 The Dynamics of Culture Change, New Haven: Yale University Press. Reprinted in 1976 by Greenwood Press Inc., Westport, Connecticut.

McMillian, Della E., Thomas Painter, and Thayer Scudder.

- 1992 Settlement and Development in the River Blindness Control Zone. World Bank Technical Paper No. 192. Washington, D.C.

Mathur, Hari Mohan, ed.

- 1995 Development, Displacement and Resettlement. Focus on Asian Experiences. Delhi: Vikas Publishing House, Ltd.

- Mead, Margaret  
1976 Applied Anthropology: The State of the Art. *In Perspectives on Anthropology 1976*. A. F.C. Wallace and assoc, eds. A special publication of the American Anthropological Association No. 10, Washington.
- Merton, Robert K.  
1973 Technical and Moral Dimensions of Policy Research. (Originally published in 1949). *In The Sociology of Science*. R. K. Merton, ed. Pp. 70–80. Chicago and London: The University of Chicago Press.
- Myrdal, Gunnar  
1975 The Unity of the Social Sciences. *Human Organization* 34(4):327–332. (Republished at <http://www.sfaa.net/>.)
- OECD (Organization for Economic Corporation and Development)  
1992 Development Assistance Committee, Guidelines for Aid Agencies on Involuntary Displacement and Resettlement in Development Projects. Paris: OECD
- Partridge, William L.  
1989 Involuntary Resettlement Development Projects. *Journal of Refugee Studies* 2(3):373–384.
- Partridge, William L., and M. Painter  
1989 Lowland Settlement in San Julian, Bolivia—Project Success and Regional Underdevelopment. *In The Human Ecology of Tropical Land Settlement in Latin America*. D. Schumann and W. L. Partridge, eds. Boulder, Co.: Westview Press.
- Scott, Robert A., and A. R. Shore  
1979 Why Sociology Does Not Apply: Sociology in Public Policy. New York and Oxford: Elsevier.
- Scudder, Thayer  
1991 A Sociological Framework for the Analysis of New Land Settlements. *In Putting People First: Sociological Variables in Rural Development*. M. M. Cernea, ed. New York and London: Oxford University Press.
- Serageldin, Ismail  
1993 Foreword. *In Sociology, Anthropology and Development. An Annotated Bibliography of World Bank Publications 1975–1993*. Michael M. Cernea with assistance of April Adams. ESD Studies and Monograph Series No. 3.
- Shihata, Ibrahim F. I.  
1993 Legal Aspects of Involuntary Population Resettlement. *In Anthropological Approaches to Involuntary Resettlement: Policy, Practice, and Theory*. M. M. Cernea and S. E. Guggenheim, eds. Boulder, Co.: Westview Press.
- Smelser, Neil J., and Richard Swedberg, eds.  
1933 The Handbook of Economic Sociology. Princeton University Press, Princeton, NJ, and Russell Sage Foundations, New York.
- Spicer, Edward H.  
1976 Early Applications of Anthropology in North America. *Perspectives on Anthropology 1976*. A. F. C. Wallace and Assoc. eds. A special publication of the American Anthropological Association, Washington, D.C.

- Stahl, Henri H., ed.  
 1934 Tehnica Monografiei Sociologice, Institutul Social Roman. Biblioteca de Sociologie, Etica si Politica, condusa de D. Husti, Bucuresti.  
 1939 Nerej, un village d'une région archaïque. Bucharest; Institut de sciences sociales.
- Tax, Sol  
 1977 Anthropology for the World of the Future; Thirteen Professions and Three Proposals. *Human Organization* 36(3):225–234. (Republished at <http://www.sfaa.net/>.)
- van Willigen, John  
 1993 *Applied Anthropology: An Introduction*. Revised edition. Westport, Connecticut/London: Bergin & Garvey.
- Weaver, Thomas  
 1985 Anthropology as a Policy Science: Part I. A Critique. *Human Organization* 44(2):97–105.  
 1985 Anthropology as a Policy Science: Part II. Development and Training. *Human Organization* 44(3):197–205.
- Wiedman, Hazel H.  
 1973 In Praise of the Double Inherent in Anthropological Applications. *In* Do Applied Anthropologists Apply Anthropology? M. V. Angrosino, ed. Athens: University of Georgia Press.
- Wolf, Eric R.  
 1956 Aspects of Group Relations in a Complex Society: Mexico. *American Anthropologists* 58:1065–1078.
- World Bank  
 1980 Operational Manual Statement No. 2.33. Social Issues Associated with Involuntary Resettlement in Bank Financed Projects, February.  
 1982 Operational Manual Statement No. 2.34. Tribal People in Bank Financed Development Projects.  
 1986 Operational Manual Statement No. 10.08. Operations Policy Issues in the Treatment of Involuntary Resettlement.  
 1989 Operational Directive 14.70. Involving Nongovernmental Organizations in Bank-Supported Activities. Washington, D.C.  
 1990 Primary Education: A World Bank Policy Paper. Washington, D.C.  
 1991a Urban Policy and Economic Development. An Agenda for the 1990s. A World Bank Policy Paper. Washington, D.C.  
 1991b The Forest Sector. A World Bank Policy Paper. Washington, D.C.  
 1991c Operational Directives 4.30. Involuntary Resettlement. Washington, D.C.  
 1992 World Development Report. Development and the Environment. Washington, DC.  
 1993 Poverty Reduction Handbook. Washington, D.C.  
 1994 Resettlement and Development. The Bankwide Review of Projects Involving Involuntary Resettlement 1986–1993. Washington, D.C.

## **Chapter 26**

### **Bea Medicine:**

### **Culture Broker among Indians and Anthropologists**

**Thomas Weaver**

Bea Medicine, a Lakota Sioux, was the first American Indian to receive the Society for Applied Anthropology's Malinowski Award, conferred on her in 1996.<sup>1</sup> Her research and teaching interests have included contemporary religious movements, pan-Indianism, Dakota ethnohistory, social change, urbanization, bilingualism, reservation culture, alcohol and drug abuse, and women's studies. She has helped many Indian students stay in school and gain meaningful employment, and she has explained anthropological theories and findings to curious, but sensitive, Indian students. She has read grant proposals, acted as a go-between for tribes and government agencies, and served as representative for American Indian and women's interests in many national and international meetings. In 1993–94 she was research director for a project on women's perspectives for the Royal Commission of Aboriginal Peoples. She was a member of the American Anthropological Association's first attempt to look at its part in dealing with "native" anthropologists through the Committee on Minorities and Anthropology (Weaver and Jones et al. 1973).

Bea Medicine (1924–) earned bachelor's and master's degrees from South Dakota and Michigan State, respectively, and her Ph.D. was awarded by the University of Wisconsin, Madison, in 1983. Her first professor in anthropology, Solon T. Kimball, taught her the value of applying anthropology, and Charles Hughes and Melford Spiro encouraged her interest in mental health issues. Florence Hawley Ellis was a perceptive guide in Pueblo Indian studies. Her field research sites have been in Canada and the U.S.

Medicine began her teaching career in schools sponsored by the U.S. Indian Service: Haskell Indian Institute in Lawrence, Kansas; the United Pueblos Agency in Albuquerque; and Flandreau, South Dakota. She also taught at California State University at Northridge, Dartmouth College, Stanford University, and the Universities of Calgary, British Columbia, Ontario, Saskatchewan, Montana, Washington, and South Dakota. She has been active, as an anthropologist, in encouraging the creation and staffing of ethnic studies programs and urban Indian centers in Canada and the U.S. (Medicine 1971). Her 1983 book, *The Hidden Half: Indian Women of the Northern Plains*, co-edited with Paula Albers, was selected as a Choice Significant Academic Book in 1983.

Medicine's Malinowski Award address, entitled "American Indians and Anthropologists: Issues of History, Empowerment, and Application" (1998a), explores the often ambivalent relationship between anthropologists and American Indians. This is a long-standing concern for

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Thomas Weaver (Ph.D., University of California–Berkeley, 1965) is Professor Emeritus of Anthropology at the University of Arizona.

Medicine, as reflected in her previous publications, “Learning to be an Anthropologist and Remaining ‘Native’” (1987) and “The Anthropologist and Ethnic Studies Programs” (1971). Her discussion of how it feels to be a “native” *and* an anthropologist is enlightening for anthropologists and American Indians alike. Much of her professional and personal effort has been directed toward accommodating demands from these two groups. In the process she has tried to make clear the native point of view to the nonnative anthropologist and the role of the anthropologist to the native. Having to explain the “Bering Straits migration theory” to Indians who hold to the validity of their origin stories is the type of situation frequently faced by Medicine. Most recently Medicine produced a film in Russia entitled “Seeking the Spirit—Plains Indians.” She filmed a powwow and returned the film before its release for a review by the Lakota people (Medicine 1998 and personal communication, 1998). Among the many suggestions she made in her address is the need for applied anthropologists to do more participatory research by involving native peoples as co-researchers (1998:256).

Medicine’s activity as expert witness created ambivalence among non-Indian groups, some of whom do not accept her in that role because she is an Indian. Her role as Indian-woman-anthropologist-teacher has also created quandaries. Indian groups have expectations of her as a mother, wife, and kin group member that often conflict with her activities as anthropologist-teacher. As an Indian among anthropologists she is often requested to act as informant. She has been privy to information on how some Indians have misled anthropologists, and how they refer to ethnographies as “Indian joke books.” Indians also relate how they feel about being mislabeled or misrepresented in published works, or about having sacred religious ceremonies exposed. Medicine’s work among Pueblo Indians demonstrates that her role as an Indian carries great personal and ethical responsibilities; she respected their carefully guarded religion and culture and did not publish her studies.

Bea Medicine sums up the motivation for her work in the following terms: “I went into anthropology to try to make living more fulfilling for Indians and . . . to apply anthropology to Indians and ‘others’ in meaningful ways” (1987:296). She reported in her 1996 address that there were then eighty-six “native” anthropologists with advanced degrees. Undoubtedly, this is in great measure due to people like herself and Edward Dozier who served as role models for the “native as anthropologist.”

In addition to the Malinowski Award, Medicine has been honored in many other ways, including being named Outstanding Woman of Color in 1983 and Outstanding Alumna by South Dakota State in the years 1956, 1977, and 1993. She received an honorary doctorate from Northern Michigan in 1979. In 1989 she was named Martin Luther King, Jr. and Rosa Parks Outstanding Minority Visiting Professor at both Wayne State University and the University of Michigan. In 1991 she received the Distinguished Service Award from the American Anthropological Association. At the time of the Malinowski Award she was the Stanley Knowles Distinguished Research Professor, Brandon University, Manitoba. Indian nations have recognized her service, too—as Outstanding Lakota Woman in 1983 and as Sacred Pipe Woman at the sundance at Standing Rock Reservation in 1977. The Makah Tribe honored Medicine for contributions to Indian education in a potlatch ceremony in 1972. As these honors indicate, Bea Medicine has been an important cultural broker among Indians, anthropologists, and various segments of North American society.

### Notes

<sup>1</sup>This annual award is given to a senior colleague in recognition of efforts to understand and serve the needs of the world through social science. See Thomas Weaver's "The Malinowski Award and the History of Applied Anthropology" (2002b) for a brief history of this award and an overview of the recipients and their work, and Weaver's "Malinowski as Applied Anthropologist" (2002a) for an introduction to Bronislaw Malinowski and his work.

### References Cited

Medicine, Beatrice

- 1971 The Anthropologist and American Indian Studies. *Indian Historian* 4(1):14–45, 63.
- 1987 Learning to be an Anthropologist and Remaining "Native." *In Applied Anthropology in America. Second Edition*, E. M. Eddy and W. L. Partridge, eds. Pp. 282–296. New York: Columbia University.
- 1998 American Indians and Anthropologists: Issues of History, Empowerment, and Application. *Human Organization* 57(3):253-257. (Republished at <<http://www.sfaa.net/>>.)

Medicine, Bea, and Patricia Albers, eds.

- 1983 *Hidden Half: Indian Women of the Northern Plains*. Washington, D.C.: American Universities Press.

Weaver, Thomas

- 2002a Malinowski as Applied Anthropologist. *In The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers*. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.
- 2001b The Malinowski Award and the History of Applied Anthropology. *In The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers*. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.

Weaver, Thomas and Delmos Jones (with J. L. Gibbs, F. L. K. Hsu, D. Lewis, and B. Medicine)

- 1972 *The Minority Experience in Anthropology: Report of the Committee on Minorities and Anthropology*. Washington, D.C.: American Anthropological Association.



## American Indians and Anthropologists: Issues of History, Empowerment, and Application

Bea Medicine

*Mitaku oyasin, chante wasteya, nape chiyusa pe.* This Lakota greeting is heard at the beginning of each speech in any community gathering on the Standing Rock reservation. I translate: “My kin, with a good heart, I shake your hand.” This salutation contains anthropological concepts: kinship, holism of a circle which is basic to Lakota ethos, and rapport and in some cases, friendship with all. This utterance has sustained me both in my life as a Lakota woman and as an anthropologist in the oftentimes hostile world of graduate school and departments of anthropology.

I thank you for honoring me with the Malinowski Award. But, I, too, have read his Journals! This has been troublesome for me. However, at my age, the thought of what may happen to my field notes and correspondence poses problems that we all must consider at some time.

I have written about my experiences in “Learning to be an Anthropologist and Remaining Native” (1978). This was published before the age of reflexivity and the “othering” of indigenous peoples. Writing in such a personal manner was an effort to honor my first professor of Anthropology, Solon T. Kimball, who taught me the value of applying anthropological analyses to everyday life. This has been a directive for me.

I have chosen to discuss the interface of American Indians and anthropologists—not in the disparaging manner of writers as Vine Deloria in his book, *Custer Died For Your Sins* (1969) and the more recent book, *Red Earth, White Lies* (1996). To assess the thrust of anthropologists into the lives of American Indians and Alaska Natives seems necessary at this time. After all, as applied anthropologists, and as change agents, this is what we do. I prefer not to use “Native American” for it is too fraught with inclusivity. In the world of government agencies for funding purposes, this term includes Native Hawaiians and Pacific Islanders. I use the labeling accepted by the advocacy group, the National Congress of American Indians, which is American Indian and Alaska Native. Most natives prefer a linguistically appropriate name for themselves, as Lakota for Sioux, or Anishinabe for Chippewa, and so on. Further, in university settings, students often checked the “Native American” category, indicating “I was born here, so I am Native American.” Moreover, now that affirmative action is so contentious, perhaps fewer faculty will claim indigenous North American heritage. Many have done so to meet quota requests. The use of perduring terms—such as Lakota—may be disarming to anthropologists, and others, but it is indicative of enhanced efforts at self-determination for indigenous nations. Moreover, the idea of nationhood is basic to anti-Indian “red neck” groups in “Indian Country.” This current backlash and issues of repatriation and intellectual property rights may be an added burden to research. Surely for “anthropological adoptee” which complicates our kinship structures, adoption is not so lauded by “anthros” these days. Labeling is still a significant

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feature in contemporary native communities, and the cause of much ire among Indians. This is evidenced by the names of sports teams—which is not a legal issue.

Perhaps, it may be of interest to note that, at last count, there were eighty-six “native” anthropologists with advanced degrees in the discipline. Dr. Jo Allyn Archambault, a Lakota anthropologist at the Smithsonian Institution, and also from Standing Rock, continues the work we began in the 1973, little-read publication, *The Minority Experience in Anthropology*. Such applied anthropologists as Delmos Jones, Tom Weaver, Jim Gibbs, and others began this venture of assessing minority input into the discipline. Unfortunately, the project Committee of Anthropologists in Primarily Minority Institutions (CAPMI), which placed retired professors in primarily minority institutions begun in the AAA while “Skip” Rappaport was president, has not continued. Subsequent presidents showed no interest in this venture. Certainly, tribally controlled colleges would benefit from being introduced to cross-cultural experience which might circumvent the tribal ethnocentrism which seems to be increasing, along with the homophobia as HIV-AIDS rates rise in native communities.

The disenchantment with anthropology as a discipline and anthropologist as “official meddler” is still a part of the fabric of research in reservation and urban communities. This disdain may increase as issues of repatriation and intellectual property rights escalate—for example, Kennewick Man.

The history of applied anthropology in “native” U.S. and “aboriginal” Canadian contexts should be updated. The need to examine our “imperialistic” navels seems propitious. Perhaps, the residue of previous applied programs have not been as efficacious as we thought. I feel, however, that we have learned a great deal by early experiments. As the theme of this conference (1996) is globalization, I call attention to a video we, a Lithuanian colleague and I, made in Russia. We call it “Seeking the Spirit—‘Plains Indians’ in Russia.” This Slavik version of a Pow Wow features tipis, feathers, and Plains Indian music and dance. This phenomenon is widespread in Europe and indicates a tenaciousness of *the* Indian image, and determined cultural appropriation.

As I come from a culture in which the spoken word and oral history are the mainstays of cultural continuity, perhaps, my written speech might be more concretized with anthropological jargon. This “Anthro Speak” has been a challenge to me in interpreting research results to native peoples—in North America, Australia, and New Zealand. There, aboriginal peoples look to North American natives in an interaction which begs for research in a globalization based upon common colonialist histories.

As some of you know, I dropped my membership in this Society, for I did not find much useful published data on “Us.” I do not, however, recommend dropping your membership to get an award! I am encouraged by what seems a new commitment to American Indian and Alaskan Native and Canadian aboriginal issues. This seems a conscious effort of such scholars as Tony Paredes, Murray Wax, Don Stull, Sally Robinson, and others, that a new Committee on Indian Concerns has been formed in this Association. I am sorry to have missed the formative meetings. I was stuck in a snow bank west of Mission, South Dakota, on my way here. Interacting in a “home/community” culture and in anthropology may underlay constant segmentalization of dual lives which may be the lot of “Anthros of Color.” It has been a survival strategy for me. In my time, doctoral degrees were seen as alienating from our societies.

But referring to the historic record—printed, that is—we find that Vine Deloria’s (1960) diatribe against the “tribe” of Anthropologists caused some to no longer refer to us as “my” Indians. Some no longer flaunted their “adoption” by “their Indian family . . .” However, some

of us “native” anthropologists were put in a triple bind—other than being Native and an anthropologist—the third being the diverse cultures of “Indian Country” and the question “who speaks for the Indians?” A new posture of defending our discipline and livelihood seemed a “White burden” to me. It was not too comforting when Vine Deloria, Sr. said, “When Vine wrote that book, he was not talking about you or Aunt Ella. He meant other anthropologists.” He was referring to his sister, Ella Deloria, a Boasian linguist informant/collaborator who produced the elegant *Dakota Grammar* (1943). Her novel, *Water Lily*, is now used as ethnographic text in Native Literature and Women’s Studies courses. She, in 1969, did not want me to read it. “It’s not an ethnography,” she said, “It is only a novel.” Appraisals of indigenous women are revealing. Kamal Viswesaran writes,

Many works, including Zora Neal Hurston’s *Tell My Horse* (1938), which explores cross-cultural experiences of womanhood in the United States and Caribbean, or Ella Deloria’s *Water Lily* (1985), a novel about coming into womanhood in the Dakota Sioux society, reveal the use of a form of gender standpoint to explore the impact of race upon the authors and those they write about, but disturb the coherence of identification between author, subject and reader by generating multiple positionings, in their texts(1997:604).

This is the type of anthropological analysis many indigenous women distrust, especially after their death. Ella Deloria was writing in the “ethnographic present.” She wrote ethnography!

I mention Ella Deloria for she came from a generation of “informants,” now soothingly called “consultants,” who added much fine material in the early years of collecting data from the “vanishing North American Indians.” Now, many anthropologists may say, “Who reads ethnography?” or decline offers of books by retired anthropologists. I was surprised by *No* response when I offered my books to Standing Rock College, now called Sitting Bull College.

Some books, such as Joseph Casagrande’s *In the Company of Man: Twenty Portraits By Anthropologists* (1970), produced sketches of natives in the genre of “my favorite informant.” These sketches are revealing of the interface of both—and would be interesting contrastive data with the more reflexive works today. This book includes an interesting contribution by Nancy Lurie. Her work with the Winnebago provides, in my view, a good model of anthropological involvement with native groups.

Kimball and Watson’s edited book, *Crossing Cultural Boundaries* (1972), is also significant in the similar history of anthropology approach. The frankness of these works possibly heralds the current rage of reflexive texts and the baring of anthropological souls. It is unfortunate that more contemporary native anthropologists as Edward P. Dozier, the respected Santa Clara Pueblo anthropologist, has not left his impressions on the discipline.

Dozier, in true understanding of Lakota culture, acted as a protecting “cousin” in my graduate work in New Mexico. Kimball worked in the Collier administration or “New Deal” for Indians. Both were well aware of the work of D’Arcy McNickle in the Bureau of Indian Affairs (BIA), and in the Society for Applied Anthropology, plus his establishing of the National Congress of American Indians as the leading tribal advocacy group which persists to the present day. It is native intellectuals such as McNickle and Dozier who initially supported Indian concerns in this association. It seems to me that this tradition of concern in the association has not persisted in the applied field by indigenous anthropologists. A stock taking of this direction might be revealing.

Some anthropology programs have included native participants. In 1973, the annual “tribal rites” of the American Anthropological Association featured a symposium on “Anthropology and the American Indian” which subsequently was published in 1974 by the now defunct American Indian Historian Press. It included such persons as Al Ortiz, Roger Buffalohead, Ken Martin, me, and the adversarial Vine Deloria, Jr. The house was packed. This symposium might be viewed as a nod in the direction of Ethnic Studies which grew out of the 1960s protests and in which many American Indian Programs were housed. The most memorable event, however, was the “take-over” of the AAA Executive Suite for an evening party.

From that period, the contributions of “people of color”—the then trendy term used to include all “minorities”—has not been a strong feature in a sociology of knowledge in approaching our discipline, especially in the applied field. For example, there is an American Indian/Alaska Native Professors Association which meets yearly. There are over 1000 aspiring-for-tenure, tenured, and retired members. The membership includes faculty from the 19 tribally controlled colleges in the U.S. and Canada, and encompasses all disciplines. This association meets the needs of the diverse cultural base of the members and presents a more egalitarian and supportive atmosphere than do their individual academic associations.

Many native professionals castigate anthropologists, but use the products of their research in classes. But the image of Vine Deloria’s portrayal still is relevant to many. Because the 1980 U.S. Census contained a “self-ascribed” category for native identification, many persons—Black and White—claimed Indian ancestry. Of course, all had a “Cherokee Princess” for a grandmother, as said Vine Deloria in 1969! This latter group, called “Wannabes” (among other terms!) played havoc with affirmative action policies and admissions standards in institutions of higher education, especially in California. They seemed to be an incipient “Elite” group.

Often the latter were products of an urban experience or products of such universities as Harvard or Stanford which were seen as more prestigious than the state universities which many of us attended. Subsequent research has indicated that the growing native professionals are rising from the ranks of bureaucrats—from those Indians employed in the Bureau of Indian Affairs, the Indian Health Service, and other government agencies. More recently, children of this new professional class are crowding the elite prep schools in the east. Other “elites” are newly emerged Indians. Self-ascription in the next census will also be significant. Many “emergent Indians” may be seen as “amateur Indians.” Self-serving enters here. Indian identification almost guarantees the funding of proposals—usually written by white persons, in an “equity” apportionment.

Some native peoples indicate that one must almost become an “amateur anthropologist” to maintain cultural integrity of indigenous life-styles and knowledge about their past history and events. This need is based upon colonialist teachings and education away from their cultures. In the present revitalization of belief systems and traditional rituals, many “traditionalists” have consulted early Bureau of Ethnology (BAE) reports. With such groups as the ailing American Indian Movement (AIM), the rhetoric against anthropologists has been chronic and rabid. Their venom especially centers upon archaeological excavations, burial remains, and sacred object removal. Repatriation offices have been established in many reservation bureaucracies, and as long as funding is available, supply much needed jobs. This area, plus gaming, needs investigation. Generally, I think that there is little knowledge of what an applied anthropologist does. The people seem to expect advocacy from any anthropologist.

Therefore, when attempting to confront disaffection with our work, one must be fully cognizant of where this discontent originates. One must consider the whole—and wide range of native social groupings. As the federal recognition section of the BIA indicates, there are more

native groups clamoring for federal recognition (AIPRCR 1977). This section of BIA has also been a source of employment for anthropologists as well as historians who do research and testify for these forgotten groups and enduring enclaves of native peoples. This direction is often seen as detrimental to reservation (or treaty) Indians for it is believed that a potential abuse may ensue—that is, “more Indians, less funds.”

This new endeavor is reminiscent of the Indian Claims Commission of the 1950s, which pitted anthropologists against each other in court cases. As always, federal Indian policy decisions have historically provided work for anthropologists and historians, yet it seems clear that such labor is not seen as applied by many of these researchers. The new enterprise of federal recognition poses new perspectives on “Who is an Indian?” Many of these emerging groups are often racially mixed. Some have clearly African American characteristics and minimize cultural ways, such as language, expressive elements of culture-rituals, ceremonies, music and dance. Others exhibit Euro-American characteristics and also lack cultural manifestation of “Indian-ness.” These social indices often lead to a growing tribal ethnocentrism in many reservations. In tandem with a growing exclusion of “Urban Indians” seen on many reservations, this is a factor in the distrust of multiculturalism in some areas. Serving as “consultants” to such a wide range of native groups has implications of what our function as applied anthropologists might be. This will color our perceptions of “advocacy” versus livelihood in an era of academic entrenchment and the realistic training of our students.

Of the million native population in the United States, 63 percent are urbanized or off-reservation (in “border towns”) contiguous to reservations. This fact is important for applied research. Given that attitudes of reservation residents are often non-sympathetic to urban groups, the needs of urban kin are overlooked, and urban adjustment often includes homelessness and resentment when city residents return home. After the preponderance of the “culture of poverty” and urban migrant studies in the 1960s, this aspect of contemporary native life is now neglected. But, it seems that studies of physical disabilities, HIV/AIDS, and diabetes research are evident, just as alcoholism studies predominated in the 1960s. The effects of alcoholism are seen in Fetal Alcohol Syndrome/Fetal Alcohol Effects research.

Often, it is implicit that our research “empowers” people. To me, empowering people—especially “people of color”—means teaching and researching issues of race, class, gender, and power relations in ways that can be understood and utilized by “target populations.” Moreover, as applied anthropologists, we should do more participatory research, and not use native peoples as “consultants” but as co-directors of research projects. Thus, they can learn research techniques and initiate and implement their own “needs assessments” and application strategies to improve the quality of life in their own communities.

The generations-old complaint by native peoples still rings true. “We never see the books or papers you write about us.” Or, “Are you writing a book about us to make a lot of money?” I wish the latter were true of all of us, Red, White, or Black. My meager royalties go to the Ella Deloria Scholarship for Indian Women in South Dakota.

Native expectation is that the finished product will be, at least, shown to them. This expectation is the source of the greatest disaffection in native groups. The question is—How many people on the reservations or in the urban enclaves read these reports? How are these reports incorporated into improving the quality of life on the local level where the manifestations of the ever-invoked litany of native dysfunctions form the basis of daily living? But many anthropologists have not had the opportunity to know or attempt to know if their reports have been read or utilized by native peoples.

I note, however, that more book reviews are being written by native peoples and appearing more frequently in the literature. This may be the result of such publications as *The American Indian Culture and Research Journal*, published at UCLA, or the Indian Studies Journal, *wicazo sa*, published at the University of Minnesota. Both are edited by Indian persons. Some of us have insisted that books be reviewed by those groups who have been the target of investigations. I quote from one review of Marla K. Powers' book, *Ogala Women: Myth Ritual and Reality* (1986). The Lakota writer, Debra White Plume, ends an astute review with this statement:

The author courageously attempts to get the buffalo, but all she has gotten is the fly on the buffalo's back (Mack and Plume 1988:78).

Perhaps we, as anthropologists, should prepare ourselves for more critical evaluations of our work, and think of ways of integrating criticism into our future work.

The impact of ethnographic and anthropological writings upon contemporary revitalization of native belief systems begs for investigation, as does contemporary socialization, clarification of "Indian values," and cultural transmission in reservation and urban communities. The results of medical anthropological and applied programs in alcohol prevention are needed to assess the utilization of native curing modalities in alcohol treatment programs based upon native traditions, although this is occurring in many places. In some reservations, residents know the treaties upon which their sovereignty is based. This interest is heightened by the "backlash" occurring in the non-native communities. Anti-Indian tracts and associations are forming in many places. Many of these present the worse racist attitudes of the people surrounding the reservations.

Environmental impact statements and needs assessments in preparation for grants writing are happening. However, studies of communities in these circumstances are not forthcoming. Many Indian people feel that the comprehension of these aspects of contemporary life is basic to their survival as distinct cultural groups. I have noted that the more "traditional" groups (whatever the term traditional means in today's Indian world) tend to be more tolerant, helpful, and intellectually interested in our tasks and results.

Some of my best insights into what we might label "ethnohistories" or ethno-methodologies have originated from "hunches" my kin and friends offered me. In my community, upon our return visits, we were expected to give summaries of our activities in research or teaching. This is a tradition of longstanding. My mother and her cousins were expected to make reports. One of my aunts was to do this. She apparently stood for ten minutes in silence. Then, she said, "*henala epikte*" ("That is all I'm going to say"). She obviously was translating English into Lakota, silently. Amid peals of laughter, the "herald" said, "*Lela pila oyampe*" ("You have honored us").

Obtaining indigenous critique from my elderly kin is still important to me. I read my writings to them. These ideas, perhaps, could be more fully acknowledged in our writings.

Recently, reflexivity (and I do not mean post-modern nonsense) has occurred in some sections of our discipline. Perhaps feminist perspectives have also had some influence. The National Association for the Practice of Anthropology and the journal *Practicing Anthropology* indicate more usable treatises for the "sub-altern" population. The SfAA, I note, has a group to create an "institutional history." I sincerely hope that "Herstory" and the impact of indigenous peoples as research constituencies are included in such a compilation.

In closing, I quote from Irving Hallowell, an anthropologist whom I greatly admired in my graduate training:

Finally, it seems to me that among these more recent influences, the impact of the Indian on modern anthropology should not be omitted. The social sciences as they have developed in the United States during the past half-century have attained an unusual prominence in American culture. Among these anthropology in its modern form was just getting underway about the time the frontier closed. It was in the 1890s that Franz Boas began to teach at Columbia University and to train students in fieldwork. Boas was a specialist in studies of the American Indian and a majority of his early students followed in his footsteps. Indeed, practically all the chief authorities on North American Indian ethnology, archaeology, and linguistics have been American. A historical accident? Of course, but that is the point. It is only recently among the younger generation that more attention is being devoted to people in the South Seas, Africa, and Asia. But it was the study of the Indians, and the problems that emerged from the investigation of the Indian as a subject, that gave American anthropology a distinctive coloring as compared with British, French, and German anthropology (1957:254–255).

I have tried, in my academic career, to respect the insights of my anthropological elders. Thus, I am hopeful that the charming and fascinating accounts of our own applied anthropologists—via the President's Advisory Council—may produce a lively and useful history of the Society for Applied Anthropology.

#### References Cited

- American Indian Policy Review Commission Reports  
1977 Volumes 1 to 6. Washington: U.S. Government Printing Office.
- Boas, Franz and Ella Deloria  
1941 Dakota Grammar. Washington, D.C.: Memoirs of the National Academy of Sciences. Volume XXIII.
- Casagrande, Joseph B.  
1960 In the Company of Man: Twenty Portraits by Anthropologists. New York: Harper and Row.
- Deloria, Ella  
1988 Water Lilly. Lincoln: University of Nebraska Press.
- Deloria, Vine, Jr.  
1969 Custer Died for Your Sins. New York: MacMillan.  
1995 Red Earth, White Lies. New York: Scribner.
- Hallowell, A. Irving  
1957 The Backwash of the Frontier: The Impact of the Indian on American Culture. *In* The Frontier in Perspective. W. Wyman and C. Kroeber, eds. Pp. 229–258. Madison, WI: University of Wisconsin Press.

- Hill, Ruth Beebe  
1977 Hanta Yo. New York: MacMillan.
- Kimball, Solon and James V. Watson  
1972 Crossing Cultural Barriers. San Francisco: Chandler.
- Mack, Dorothy and Debra White Plume.  
1988 Review of Maria N. Powers, Oglala Women: Myth, Ritual, and Reality. *American Indian Quarterly* 12(1):76–78.
- Medicine, Beatrice  
1978 Learning to be an Anthropologist and Remaining Native. *In Applied Anthropology in America: Past Contributions and Future Directions*. E. Eddy and W. Partridge, eds. Pp. 182–196. New York: Columbia University Press.
- Powers, Maria N.  
1986 Oglala Women: Myth, Ritual, and Reality. Chicago: University of Chicago Press.
- Visweswaran, Kamela  
1997 History of Feminist Ethnography. *Annual Review of Anthropology* 26:591–621.



## **Chapter 27**

### **Ward Goodenough: Academic and Applied Anthropologist**

**Thomas Weaver**

Ward Goodenough, recipient of the Society for Applied Anthropology's Malinowski Award in 1997, influenced the development of applied and academic anthropology over a long career.<sup>1</sup> His most important studies were in kinship (1955, for example), componential analysis (1956), and Pacific ethnography. Other important work concerned religion, marriage and residence, property and land tenure, ecological change, and political organization. Goodenough consulted and wrote reports on emergency planning for the National Research Council, on arms control and environmental health, on agency structure and public health programs, and on public policy and multiculturalism. He has also served with anthropologists and representatives of other disciplines on a committee of the Ocean Studies Board of the National Research Council charged by Congress to appraise fishing quota shares in the U.S. and other countries.

Goodenough attributes his interest in applied anthropology to his early experience in the Pacific: "I first became concerned about applied anthropology when I was doing fieldwork in Truk in 1947 and thinking about what kind of ethnography was needed in order to provide administrators with . . . knowledge of how local systems worked. . . . Having that in mind upgraded the quality of my ethnography" (personal communication, February 11, 1998). At the time he was part of a research team directed by George Peter Murdock, sponsored by the Pacific Science Board of the National Research Council, and financed by the Office of Naval Research. Goodenough's study of Truk has become classics (Goodenough 1951).

Ward Goodenough (1919–) received a bachelor's degree from Cornell University in 1940 and a doctorate from Yale in 1949. Lauriston Sharp, recipient of the 1989 Malinowski Award (Weaver 2002a), had just come to Cornell, fresh from completing dissertation research. Later, Sharp launched projects that would make Cornell the premier university in applied anthropology for several decades, but he was the only anthropologist at Cornell the year Goodenough graduated. At Yale Goodenough studied with Ralph Linton and was a research assistant for George Peter Murdock's cross-cultural survey. There he met Alan Holmberg who had been hired by Yale University Press to edit W. Lloyd Warner's first book in the Yankee City series. At Yale he also studied with Bronislaw Malinowski, one of the few Malinowski Award recipients to have done so. His work with Malinowski was limited, however, to the seminar he took in 1940–41 as a first-year graduate student. Goodenough's studies were interrupted by military service, during which Malinowski died (Goodenough 1998).

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Thomas Weaver (Ph.D., University of California–Berkeley, 1965) is Professor Emeritus of Anthropology at the University of Arizona.

Goodenough's most important publication for practicing anthropology is *Cooperation in Change: An Anthropological Approach to Community Development* (1963), written as part of a Cornell project on training change agents. Along with its companion volumes, case study collections on development and technological change edited by Edward Spicer (1952) and on international health edited by Benjamin Paul (1955), *Cooperation in Change* became an early guide for applied anthropologists.

Goodenough dedicated *Cooperation in Change* to Linton and Malinowski because "it was their work that I had found most helpful in thinking about the processes of cultural and social change." Regarding Malinowski's influence, he states:

When I was looking for anything in anthropology that would help forecast the course of the chain effect of change, I found his model of the institution, if adapted to activities, was the best thing available. . . . Malinowski's . . . functionalism, which saw customs as developing in response to what he called 'basic human needs' . . . provided much of the theoretical underpinning of my book (personal communication, February 11, 1998).

Goodenough's book covered both theory and methods in development work—at that time called modernization. He focused on inducing change from an anthropological perspective to minimize ill affects on third world communities. Training for the effective change agent requires, he argued, in addition to a good liberal arts education, the study of foreign languages and cultures and learning about medicine, law, agriculture, engineering, or whatever field is the focus of development (1963:29). Preparation for fieldwork also requires the development of technical skills, belief in the mission, cultural empathy, knowledge of politics, and organizational talent. Learning the client community's language, the use of interpreters, nonverbal communication, and problems with culture shock and emotional isolation are topics addressed. While some of the terminology in this field has changed, the book continues to be useful to anthropologists today.

Goodenough's Malinowski Award acceptance speech, "Communicating 10,000 Years into the Future" (1999), described a project in which he had recently been involved: designing a marking system that would protect buried nuclear wastes from disturbance for ten thousand years. The project was necessarily interdisciplinary, calling for the cooperation of anthropology, archaeology, linguistics, astronomy, human ethology, psychology, architecture, and materials science. The technical expertise of engineers would protect the materials from natural or human interventions. Social scientists had to predict the type of society that might emerge during the time period concerned and what languages and symbols would best provide a warning sufficient to save curious persons from harm. That this project was necessary is a strange commentary on just where our society has evolved technologically and an inkling of what future practicing anthropologists may face.

Ward Goodenough is a fellow of the American Philosophical Society, the American Academy of Arts and Sciences, and the National Academy of Sciences, to which he was elected at the relatively young age of 52 years, as well as a Guggenheim fellow. He served as editor of the *American Anthropologist* and was a recipient of the American Anthropological Association's Distinguished Service Award. He was also president of the Society for Applied Anthropology. At the time of the Malinowski Award presentation, he was Professor Emeritus at the University of Pennsylvania.

### Notes

<sup>1</sup>This annual award is given to a senior colleague in recognition of efforts to understand and serve the needs of the world through social science. See Thomas Weaver's "The Malinowski Award and the History of Applied Anthropology" (2002c) for a brief history of this award and an overview of the recipients and their work, and Weaver's "Malinowski as Applied Anthropologist" (2002b) for an introduction to Bronislaw Malinowski and his work.

### References Cited

Goodenough, Ward H.

- 1951 Property, Kin, and Community on Truk. New Haven: Yale University Publications in Anthropology, No. 46.
- 1955 A Problem in Malayo-Polynesian Social Organization. *American Anthropologist* 57:71–83.
- 1956 Componential Analysis and the Study of Meaning. *Language* 32:195–216.
- 1963 Cooperation in Change: An Anthropological Approach to Community Development. New York: Russell Sage Foundation.
- 1999 Communicating 10,000 Years into the Future. *Human Organization* 58(3):221–225. (Republished at <<http://www.sfaa.net/>>.)

Paul, Benjamin, ed.

- 1955 Health, Culture, and Community: Case Studies of Public Reactions to Health Programs. New York: Russell Sage Foundation.

Spicer, Edward H., ed.

- 1952 Human Problems in Technological Change: A Casebook. New York: Russell Sage Foundation.

Weaver, Thomas

- 2002a Lauriston Sharp: Innovator of Applied Anthropology Programs. *In* The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.
- 2002b Malinowski as Applied Anthropologist. *In* The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.
- 2002c The Malinowski Award and the History of Applied Anthropology. *In* The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.

## Communicating 10,000 Years into the Future

Ward H. Goodenough

Imagine getting a telephone call and being asked if you would be willing to serve on a panel of consultants to advise the Department of Energy on how to mark its Waste Isolation Pilot Plant (for disposing of radioactive waste) so as to “prevent inadvertent intrusion for the next 10,000 years.” I got such a call in the spring of 1990. I was intrigued by the challenge and agreed to serve.

The Waste Isolation Pilot Plant (henceforth WIPP) is a research and development facility near Carlsbad, New Mexico, designed to store out of harm’s way for at least 10,000 years the transuranic radioactive wastes generated by the U.S. government’s nuclear weapons program.<sup>1</sup> Transuranic, or relatively “low-level,” wastes have been contaminated by isotopes with an atomic number greater than 92 and a half-life greater than 20 years. In 10,000 years the level of radioactivity will have declined to the point where it is equivalent to levels of radioactivity that exist naturally, as in uranium ore deposits.

The WIPP site consists of 16 square miles, within which is a square, fenced-off area that is about 1.5 miles on a side. Within this fenced area is a waste-handling building and office buildings. The panels and rooms where waste is to be stored have been cut out of strata of rock salt and occupy an area 2,064 by 2,545 feet, at a depth of 2,157 feet directly under the fenced area above. There are no potable aquifers above or below the storage area. Pockets of natural gas near and underlying the site are in the process of being tapped and will be exhausted in the near future. There are no other natural resources of value near the site. The region is arid (about 12 inches of rain a year), and supports cattle grazing but not dryland farming. It is estimated that over time the canisters containing the waste materials will be crushed by the overburden so that the long-term containment of radioactive materials will be in a matrix of rocksalt. The rocksalt is not readily permeable, so, given the anticipated continuing low level of rainfall, the amount of leakage into the region adjacent to the storage area should be small, even over 10,000 years. Issues that have so far delayed bringing waste materials to the site do not appear to be technical but are political, based in part on ideology and in part on misinformed fears.

The EPA standard set in 1985 (Code of Federal Regulations 40) required several things relevant to marking the site:

1. The site must be “designated by the most permanent markers, records, and other passive institutional controls practicable to indicate the dangers and their location” (40 CFR 191.14c).
2. Performance assessment of the disposal facility must be based on assessment of probabilities. The consequences of a given scenario must not only be calculated but the likelihood of that scenario must be assessed (40 CFR 191.13).

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<sup>1</sup>This address was previously published in *Human Organization* 58,3(1999):221–225.

3. The time period to be covered is 10,000 years (CFR 191.13a).
4. Active institutional controls are considered effective for no more than 100 years after closure of the site (40 CFR 191.14a). Thereafter only passive controls, such as the site's marking, will be in effect.

These specifications require developing scenarios of the different kinds of things that might happen in the future in regard to the site, estimating the probability of their occurrence, determining their relevance to the need for marking, and, if relevant, contriving a way to address them in marking the site. Furthermore, the specifications require devising a marking system that has the highest possible probability of enduring for 10,000 years and that has the highest possible probability of communicating successfully over the same period of time. To put the time frame in perspective, think back to what humans were doing 10,000 years ago and of all the things that have happened since. Think of how much languages have changed in half that time and of the challenge such change poses for communication over 10,000 years.

The panel of consultants engaged to advise on marking the WIPP site was divided into two working teams, each representing a similar range of expertise. Each team worked independently of the other to provide a set of recommendations. The team on which I served had a materials scientist, an architect, an astronomer, a linguist, an archaeologist, and myself (an anthropologist). The teams were provided with a set of assessments already worked up by an earlier "Futures Panel" in regard to different possible modes of intrusion into the site and the likelihood of their occurrence.

Our charge was to recommend how to mark the site. We were to provide a physical description of the markers, including size, location, shape, and materials. We were to formulate the messages to appear on the markers and to indicate how those messages were to be conveyed. For each major mode of possible intrusion identified by the Futures Panel we were to estimate the likelihood that each marker will survive to the extent that it is recognizable and its message is recognizable and accessible. We were also to estimate the likelihood that a potential intruder will recognize and correctly interpret the message, given that the marker has survived.

Our team agreed on the following working assumptions (Ast et al. 1992:F-17):

1. Climate will vary between that of a desert and that of good grassland. At best, water will be scarce with little change in its availability in the next 10,000 years.
2. The region will be sparsely inhabited, even under optimum conditions, most likely by keepers of livestock.
3. A tradition directly descended from one or another of the modern technologically, scientifically, and scholarly developed societies will continue in at least some regions of the world over the next 10,000 years.
4. Whatever languages are spoken over the next 10,000 years, they will be lineal descendants of one or more languages spoken now, most probably descendants of languages that are most widely spoken and written now.
5. Since literacy has not ceased to exist from the time it was first developed about 6,000 years

ago, it will not cease to exist over the next 10,000 years; nor will the scholarly and scientific traditions that are based on literacy cease to exist, provided there are no global catastrophic events on such a scale as to wipe out civilization everywhere.

We considered three main scenarios for the future (Ast et al. 1992:F-17–18):

1. Human existence has been reduced to what can be supported by a metal-using technology like that of early medieval Europe. The probability of an intrusion into the WIPP site is relatively low. There is little need for a marking system. One that is awesome and scary, like the one we came up recommending, might invite use as a place of assembly for religious purposes, but it would not invite intrusion 2,000 feet down, given the low level of technology.
2. Human existence has continued with regional ups and downs at the present level of technological sophistication, at least, if not a higher one. The WIPP site is marginal for human habitation because of cycles of climatic change between desert and grassland. People who encounter the site are likely to be relatively unsophisticated, being herders or resource prospectors. If the site is marked in a massive and awesome way, word is likely to reach officials, scholars, and scientists. Its massive scale will then draw scholars and scientists to study it, decipher the messages inscribed there, and thus acquaint them with the dangerous nature of what is deposited there.
3. Human existence goes through a period of global catastrophe and is reduced to a state bordering on illiteracy and stone-age technology and then redevelops new technological sophistication, new literacy, and new science. The probability that people will then be able to decipher and read the messages inscribed there will be low, unless the inscriptions themselves provide a key to their interpretation. By having the same messages arranged in a way that shows them to be parallel statements in different languages and scripts, the site design can provide an equivalent of a Rosetta Stone and increase the probability of successful decipherment.

In our deliberations, we addressed the latter two scenarios. We decided on a “systems approach” that involved several components within a given marking design, a number of items within each component, and cross-referencing to link components with different levels of information and marking. We felt this should be done in a way that built in considerable redundancy of components so portions of the marking system could be lost over time without compromising the overall design. We thought of Stonehenge, where one-third of the stones are missing, but those that remain have enabled reconstruction of the entire design.

The WIPP site will not be the only site where radioactive waste is deposited. Whether or not another one is established in the United States, similar sites will be established and marked in other countries. We took the position that the international community of nations should adopt common features in the marking of all such sites. The resulting redundancy will serve to make it easier in the distant future to identify such sites for what they are, even if they have been partially destroyed. We recommended that each site do the following:

1. Display its basic warning message in at least Chinese, Russian, English, Spanish, French, Arabic (the languages of the United Nations), and the local language in common use, if it is not one of the above.

2. Prominently display the international radiation symbol flanked by faces showing expressions of horror and distress. The radiation symbol may not be understood, but facial expressions, being the same cross-culturally, are likely to be recognizable for what they are during the next 10,000 years.
3. Display in a protected chamber a world map of all disposal sites, together with a standard diagram that geometrically allows them to be located to a standard of accuracy of at least 5 kilometers. In the same chamber have inscribed on stone slabs extended parallel messages describing more fully the contents of the disposal site.
4. Delineate each disposal area with earthen berms at least 10 meters in height or in some similar way that is large-scale, long-lasting, and that in its constitution of materials and their arrangement makes the site uninviting for day-to-day human purposes and communicates, independently of language or graphics, the underlying message that the place is dangerous and to be shunned.

As this last item indicates, our problem was one of communication. Since existing cultures and languages will change enormously over the next 10,000 years, we had to seek ways to communicate that transcend specific cultures and languages and that rest on behavioral and psychological features common to the human species—features that have been relatively unchanged for a long time in the past and that can be expected to remain relatively unchanged for many millennia to come. Although we would have to use written languages to communicate the specifics of the site, the site itself would have to communicate at a more basic, panhuman level. It would have to be at once both immensely awesome and immensely uninviting, even though inevitably intriguing.

To this end, we proposed that the site be marked by a series of large, zigzag berms that would be roughly at right angles to the designated area itself so as to allow for access to it between them. They would be irregular in shape and layout, conveying by their massiveness an impression of importance and at the same time by their irregularity a lack of positive value, such as carefully laid out symmetrical patterns are likely to suggest. The berms would be constructed to a height of about 30 feet from the hardpan caliche that characterizes the area. Somewhere in the area enclosed by the berms would be a mound with an accessible entrance to a chamber in which the lengthy explanatory inscriptions would be housed. These inscriptions would be on slabs of granite, each slab covering an identical slab behind it, so that damage to an outer slab would reveal the same information on another one behind it. At the corners of the site, the berms would be more massive and higher. They would contain hidden chambers that duplicate the information in the accessible mound in the region demarcated by the berms. As the latter mound deteriorated or was vandalized over time, erosion would gradually reveal the existence of one or more of the hidden chambers.

As suggested by our recommendation that the berms be constructed of the local caliche or hardpan, our aim was to use materials that would be least likely to have value as construction materials for anyone in the future. That such construction would be relatively inexpensive was, in itself, a minor consideration. For stone blocks and slabs to bear inscriptions we recommended granite. It is durable and readily obtainable in many places. The site would be unlikely to be quarried for it except, possibly, for local use, where the need for such use could be projected as

slight. To discourage such possible use, we recommended that the granite blocks be cut in irregular shapes that would make them unsuitable for construction purposes without great effort to modify them.

We recommended that such blocks with short inscriptions describing the presence and nature of the danger be erected at the gaps between the berms at points of entry into the enclosed area. To shield them from erosion, there should be protective walls of durable concrete, as specified by the materials scientist on our panel.

We suggested a number of possible ways to treat the area enclosed by the berms, the area, that is, immediately above the actual disposal site. We came up with several ideas for how to do this. Three we suggested were:

1. A number of massive spikes that would appear to break up from under the ground, and would be made of oddly fitted granite or, better, of reinforced concrete, the latter being extraordinarily difficult to dismantle.
2. A rubble landscape consisting of a raised outer rim of stone and caliche enclosing an area crisscrossed with inner ridges of similar material enclosing spaces filled with sand. The aim would be to create an inhospitable area that would be difficult to walk on or bring machinery into.
3. A heavy, flat pavement, black in color. It would create an image of nothingness—a “black hole” as one of our team called it. It would also attract solar heat and make the area close to being intolerably hot, at least much of the time, in the desert region where the WIPP site is located. (This was the alternative our panel favored, but we felt it important to suggest a range of options in our report.)

As to the actual messages, we proposed three levels in addition to whatever message the structure of the site conveyed:

1. The first level was very brief: Between two faces expressing emotions of horror and fear would be the statement: *DANGER. POISONOUS RADIOACTIVE WASTE BURIED HERE. DO NOT DIG OR DRILL HERE BEFORE 12,000 A.D.*
2. The second level, also flanked by faces showing horror and fear, would bear a longer message consisting of four short paragraphs. It would say that dangerous radioactive waste is buried at a given distance down and that the site was chosen to put the waste far away from people. Although the rock and water in the area might look harmless, radioactive poison is invisible and can destroy or damage people, animals, and plants. Therefore do not drill, dig, or do anything with the rocks or water in the area. Do not destroy the markers, which have been designed to last 10,000 years. If the marker is difficult to read, add new markers in longer-lasting materials and copy the message onto them in whatever language the future observer speaks. For information, the observer was advised to go to a building (structure) inside the marked area. The message would conclude stating what the site is named (WIPP) and when it was closed.
3. The third level of message, housed in the building in the enclosed area and also in the concealed chambers in the corner berms, would contain much longer messages, also flanked by



faces showing horror and fear. We offered sample shorter and longer ones. The longer one would contain, in addition to the extensive text, a detailed perspective view of the repository site, a detailed representation of how the shaft leading down to the disposal chambers was sealed, and a detailed geologic cross-section of the site. It should also contain information regarding the location of other such sites around the world, using astronomical coordinates. Such information can be added in the future, as needed, after the shaft has been sealed.

We recommended further that information be provided in the long message as to where detailed reports regarding the disposal site (and other such sites) will have been archived. We also called attention in our report to the possibility of creating markers that would generate noise or even tones when the wind hit them. Since the only moving component in such structures would be air, they could be counted on to endure for a long time. We considered other self-energized marker systems but rejected them as lacking durability. We also advised that small markers, made from fired ceramics and other durable materials, be buried in the sandy or caliche areas of the site to a depth below ploughing level. These would contain the faces and the brief first-level message. Anyone trying to make use of the site's surface area would thus be confronted with further warnings.

Since the shafts leading to the subterranean chambers would be the most likely pathways for radionuclides to migrate toward the surface by natural processes, we recommended that these shafts be marked by filling the top 50 feet with alternating layers of sand (10 feet) and gravel. The sand layers should contain fired clay, glass, and titanium markers containing pictorial and verbal information showing that this is a backfilled and sealed access shaft. We suggested that the messages on these markers indicate that these shafts are the most likely area of up-migration, if any, of radioactive material. We left the specific wording to be decided on after the sealing methods and possible migration rates have been established.

We also considered the problem of deterring drilling into the site. Surface warnings may be ignored by the curious, by prospectors, or even, let us face it, by archaeologists who feel they have to uncover and describe what is in this mysterious site. Such people might undertake to drill for a sample core of what is down there. Knowing they are drilling down into layers of rock salt, their drills will presumably be selected for that purpose. Furthermore, they might decide to slant drill from outside the demarcated area to avoid any immediate subsurface obstacles. To meet this possibility, we recommended that regions immediately above the deposit chambers and extending somewhat beyond them contain a layer of very hard material that would break drill bits, communicating thereby the seriousness of warnings of danger.

Assuming that future generations of scientists will be able to recognize a diagram of the constellations near the celestial north pole, we recommended that more extended messages be accompanied by a diagram showing the position of the celestial pole near the North Star at the time of establishing the WIPP site and showing the precession of the pole as it circles around these constellations over the succeeding 10,000 years to the point where the buried waste will have become harmless. The use of faces with changing expressions would serve to communicate the reduction in toxicity over time.

I could continue with more detail, but I think I have already given enough to communicate the kind of thinking we found ourselves engaged in while trying to meet our charge. Whether what we proposed will work or not is, of course, open to question. Since the WIPP site was not to be closed for another 30 years, we expected that similar panels will be convened in the future to go through the same kind of exercise. Redundancy in a matter of this

kind is clearly desirable. We suggested that our recommendations for marking the site and others in the future be tested on samples of respondents and that the various materials proposed for use be subjected to weathering and stress tests during the ensuing years before the WIPP site is scheduled to be officially closed and then marked for posterity.

There are no theoretical points to be drawn from what I have been describing. From the standpoint of practice, however, what I found myself involved in was not simply a matter of applied anthropology. Applications of anthropology, archaeology, linguistics, astronomy, human ethology, psychology, architecture, and materials science were all combined in our thinking about the problem. Applied science projects often turn out, of necessity, to be interdisciplinary. One of the contributions applied anthropologists can make is to call attention to the range of disciplines that may be relevant for the projects in which they find themselves engaged.

In closing, I would like to comment on what this may all add up to. When WIPP was established, everyone had to assume that the only way to deal with nuclear waste from bomb manufacture and the use of nuclear power was to find a safe way of storing it while it gradually lost its radioactivity. The WIPP site was established to deal specifically with the transuranic waste materials generated by nuclear weapons production. It was not intended for the more highly radioactive materials with shorter half-lives generated by nuclear power plants. When we were taken on a tour of the WIPP site, we were shown facilities that had been constructed with the capacity for handling the latter materials, if the need should arise. Given the political infighting surrounding the proposed disposal site for such wastes in Nevada, I will venture to predict that the WIPP site will end up being the place where all kinds of nuclear waste are ultimately sent for disposal. It appears ready to handle them.

The major problem in such disposal, of course, is transporting the waste materials from the places where they are temporarily stored, trucking them over long distances. The technology for doing so safely has, I understand, been fairly well developed, but the political problem of “not through our town” or “not through our state” remains a formidable one.

It is possible, however, that new technology is about to transform the problem of disposal. I recently heard a news report on television that new chemical processes have been developed for neutralizing radioactive wastes, converting them into harmless materials. The equipment needed for doing this can be brought to the sites where such waste materials are now temporarily stockpiled so they can be neutralized there without the need of transporting them. An American firm, I read, has already been engaged to use this equipment at waste sites in Russia. It seems likely, therefore, that the WIPP site may never be needed, at least for radioactive wastes, though it may well be a useful place for the storage of other kinds of hazardous wastes or even for the disposal of chemically contaminated muck dredged from our rivers. In any event, it is quite possible that the exercise in how to mark the WIPP site to deter inadvertent intrusion for the next 10,000 years may well have been entirely unnecessary. We who were involved in it, however, had an interesting learning experience.

#### Notes

<sup>1</sup>In much of what follows I draw heavily on the report of Ast, Brill, Goodenough, Kaplan, Newmeyer, and Sullivan (1992).

### References Cited

- Ast, Dieter G., Michael Brill, Ward Goodenough, Maureen Kaplan, Frederick Newmeyer, and Woodruff Sullivan  
 1992 Marking the Waste Isolation Pilot Plant for 10,000 Years. A report prepared for the U.S. Department of Energy. Included as Appendix F. *In* Expert Judgment on Markers to Deter Inadvertent Human Intrusion into the Waste Isolation Pilot Plant. Kathleen M. Trauth, Stephen C. Hora, and Robert V. Guzowski. Pp. F1–F153. Sandia Report: SAND92-1382 \* UC—721. (Available from National Technical Information Services, U.S. Department of Commerce, 5285 Port Royal, Springfield, VA 22161.)
- Code of Federal Regulations  
 1985 Title 40, Part 191: Environmental Standards for the Management and Disposal of Spent Nuclear Fuel, High Level and Transuranic Radioactive Wastes. Federal Register 38066–38099, No. 182, September 19.

## **Chapter 28**

### **Robert A. and Beverly H. Hackenberg: Research and Policy in Anthropology**

**Thomas Weaver**

Robert A. and Beverly H. Hackenberg were nominated by their students for the Society for Applied Anthropology's Malinowski Award, which they received in 1998.<sup>1</sup> This is indicative of their extraordinary success in producing a generation (or two) of graduate students who have gone on to continue their work in various applied fields. The Hackenbergs have lectured, trained, supported, monitored, and directed their graduate students, placed them professionally, and continued mentoring and serving as examples. In addition to training others, their major contributions have an interwoven theme of cultural ecology, health, demography, and applied theory (Hackenberg 1985). Issues in medical anthropology, especially in cardiovascular and chronic diseases, have characterized their work in various regions of the world. Their identification of policy as a key variable in applied projects is extremely important, and they have been actively involved in policy advising.

While working at the University of Arizona, the Hackenbergs began research themes that would permeate future projects in Southeast Asia and Central America (Hackenberg and Murphy 1990). The first was a 1958 study of the Tohono O'odham using demographic and computer techniques to create genealogies to trace health and illness (Hackenberg 1961). Similar studies were conducted on the Gila River Pima and Navajo. This was at a time when computers were barely heard of in anthropology (remember punch cards!). The genealogical database was used to trace population growth, migration, out-marriage, and resettlement, all in the context of detecting and preventing chronic diseases (Hackenberg 1973; Hackenberg, ed. 1972). The National Institutes of Health (NIH) continued to use the Hackenbergs' model and data for many years in their laboratory in Tucson.

Beverly H. Hackenberg (1928–) received a master's degree in sociology and demography from the University of Arizona and a certificate in epidemiology from the Centers for Disease Control. She began work in anthropology after World War II in the Philippines where she was co-director of the U.S. Information Service. She and Robert Hackenberg have worked together on all projects beginning in 1953 when she was field director at the University of Arizona's Bureau of Ethnic Research (now Bureau of Applied Research in Anthropology). Later, she became a research associate at the Population Program of the Institute of Behavioral Science at the University of Colorado, Boulder. She also continued as co-director of their projects for the Davao Research and Planning Foundation in the Philippines. Most recently she has been field

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director at the University of Arizona Health Science Center for various health projects designed to determine genetic risk factors in the transmission of epilepsy (Jones and Stull 1998).

Robert A. Hackenberg (1928–) was educated at the University of Minnesota, where he received a bachelor's degree in psychology and a master's in sociology, and at Cornell University, where he received a doctorate in applied anthropology. This was during the height of Cornell's applied anthropology programs developed by Lauriston Sharp, another Malinowski Award winner (Weaver 2002a). Hackenberg's 1961 dissertation was entitled *Indian Administration and Social Change at Gila River Reservation, Arizona*. Robert Hackenberg's first professional position was at the University of Arizona's Bureau of Ethnic Research (now Bureau of Applied Anthropology), where he began work in 1954. From there he went to the University of Colorado's Institute of Behavioral Science, where he directed programs in medical anthropology and population. He has served as consultant for numerous projects for agencies such as the World Bank and USAID and for many tribal and local governments.

Together Robert and Beverly Hackenberg have received many grants amounting to several million dollars. One of the characteristics of applied anthropology that particularly applies to the Hackenbergs is that the anthropologist's scholarly contribution is largely to what John van Willigen calls "fugitive literature" (1986:17)—reports hidden in the files of development agencies. The Hackenbergs' resumes included 30 unpublished reports by 1993 alone.

In their Malinowski Award address, entitled "You CAN Do Something! Forming Policy from Applied Projects, Then and Now" (1999), the Hackenbergs maintained that the applied projects represented in the literature of anthropology are sufficient to form a theory of practicing anthropology and to formulate policies that can be used at all levels of society—local community, national, or international. They briefly review some of their most important projects beginning with Beverly Hackenberg's community oriented primary care project in Mindanao, Philippines, which was later replicated on the O'odham reservation in Arizona. There they focused on Diabetes Mellitus and the use of community health workers in a community-wide, tribally supported monitoring project. Other projects that illustrate their policy and theory scheme include developing community health practitioners and upgrading squatter settlements in Davao City, the Philippines (Hackenberg and Hackenberg 1987). Their efforts resulted in the founding of the Davao Medical School to serve the southern third of the country. The medical school features the Institute of Primary Care that uses housewives to provide paramedic care.

The main thrust of the Hackenberg's Malinowski Award address was that approaches used in their health and development projects in Davao City and among the O'odham cannot be extended to the increasingly common denucleated marginal population clusters that characterize international borderlands. Their work in the U.S.-Mexico border city of Nogales, Sonora, led them to conclude that conditions for upgrading squatter settlements do not exist there. Population turnover is rapid, households are made up of secondary relatives and townmates (rather than nuclear families), and community institutions are absent. In addition, funding agents such as the World Bank have changed their granting and lending priorities to other targets and agents such as non-governmental organizations, making funds scarce for this type of endeavor. Some of the issues present on the border include the impact of labor migration in the U.S., explosive urban growth in Mexico, GATT and NAFTA, and the maquila industry that have placed Nogales on the "postnational" map of globalization. The result of these combined factors is an increase in poverty and a decrease in access to housing and infrastructural support. What is needed to ameliorate these problems, the Hackenbergs suggested, is to employ policy instruments rather

than projects. By this they mean that applied anthropologists must become more directly involved in producing and implementing policy measures by taking advocacy roles.

Robert and Beverly Hackenberg have continued their applied work. They received a USAID contract in 1999 to consult with the Paraguayan Ministry of Health on the design of a community-oriented primary care program aimed at providing prenatal and postnatal services to women in rural municipalities. Robert Hackenberg continues to direct the medical anthropology program at the University of Colorado, Boulder, where he also has affiliate faculty appointments in the Health and Behavioral Scientist Doctoral Program and in the Spero Manson Native American Mental Health Research Center. He has an adjunct research scientist appointment in the Bureau of Applied Research in Anthropology at the University of Arizona, and is Associate Editor with responsibility for special issues for *Human Organization*.

Robert and Beverly Hackenberg have been honored with appointments to the East-West Center and the Philippine Population Institute. Robert Hackenberg is past chair of the National Institutes of Health Study Section on Social Science and Population Research and former member of the National Research Council Assembly of Behavioral and Social Sciences. The High Plains Society for Applied Anthropology conferred the Omer Stewart Award on Robert Hackenberg in 1997.

### Notes

<sup>1</sup>This annual award is given to a senior colleague in recognition of efforts to understand and serve the needs of the world through social science. See Thomas Weaver's "The Malinowski Award and the History of Applied Anthropology" (2002c) for a brief history of this award and an overview of the recipients and their work, and Weaver's "Malinowski as Applied Anthropologist" (2002b) for an introduction to Bronislaw Malinowski and his work.

### References Cited

- Jones, Delmos J., and Donald D. Stull  
1998 Hackenbergs Receive Malinowski Award. *Society for Applied Anthropology Newsletter* 8(3):1.
- Hackenberg, Robert A.  
1961 Papago Population Study: Research Methods and Preliminary Results. Tucson: Bureau of Ethnic Research, University of Arizona.  
1973 Genealogical Method in Social Anthropology. *In Handbook of Social and Cultural Anthropology*. J. J. Honigmann, ed. Pp. 289–325. Chicago: Rand-McNally.  
1985 Bringing Theory Back In: Steps Toward a Policy Science of Applied Anthropology. *American Behavioral Scientist* 29:205–228.
- Hackenberg, Robert A., ed.  
1972 Modernization Research on the Papago Indians. Special Issue. *Human Organization* 31(2).
- Hackenberg, Robert A., and Beverly H. Hackenberg  
1986 Saving the City: University Research, Political Action and the Squatter Problem in Davao City. *In Collaborative Research and Social Change*. D. D. Stull and J. J. Schensul, eds. Pp. 179–210. Boulder: Westview Press.  
1999 You CAN Do Something! Forming Policy from Applied Projects, Then and Now. *Human Organization* 58(1):1–15. (Republished at <<http://www.sfaa.net/>>.)

Hackenberg, Robert A., and Arthur Murphy

- 1990 Central Place Theory and Housing Programs in Panama's Secondary Cities. *Cities and Society* 4:144–155.

van Willigen, John

- 1986 *Applied Anthropology: An Introduction*. South Hadley, Mass.: Bergin & Garvey Publishers, Inc.

Weaver, Thomas

- 2002a Lauristan Sharp: Innovator of Applied Anthropology Programs. *In The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers*. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.
- 2002b Malinowski as Applied Anthropologist. *In The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers*. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.
- 2002c The Malinowski Award and the History of Applied Anthropology. *In The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers*. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.

## **You CAN Do Something!** **Forming Policy from Applied Projects, Then and Now**

**Robert A. Hackenberg**  
**Beverly H. Hackenberg**

The native still needs help. The anthropologist who is unable to perceive this, unable to register the tragic errors committed at times with the best of intentions, at times under the stress of dire necessity, remains an antiquarian covered with academic dust in a fool's paradise . . . But research in order to be of use must be inspired by courage and purpose . . . Shall we therefore mix politics with science? . . . Decidedly "yes."

Bronislaw Malinowski, *Dynamics of Culture Change*, 1945

### **What Is To Be Done?<sup>1</sup>**

Our title responds to the ringing declaration, "You Can't Do Nothing," with which Paul Bohannan entitled his 1980 presidential address to the American Anthropological Association. He argued that social/cultural anthropology had yet to rise from the ruins of its colonial ancestry to effectively confront a world in which advancing globalization is combined, illogically, with increasing ethnic strife:

If the decline of the nation-state continues, and if the state is replaced by a set of international checks and balances . . . then we get on with solving the world's problems. But if the ethnocentric urge continues . . . then international conflict will get worse, and the ecological balance of the planet will suffer untold damage. We can't do nothing! (Bohannan 1980:513).

With these words and others he invited us all, and particularly applied anthropologists, to accept the challenge.<sup>2</sup>

But what shall we do?

Malinowski, speaking across the decades, advises us to pursue science and engage in politics. By mixing these activities we become proponents of positions on policy issues. This is certainly what Bohannan had in mind, and according to Firth, the 1981 award recipient, so did Malinowski:

Malinowski held that ethnographers who have studied culture contact and assessed its potential and dangers have the right and duty to formulate their conclusions in a way in which they can be seriously considered by those who

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<sup>1</sup>This address was previously published in *Human Organization* 58,1(1999):1–15.



frame policy and carry it out. They also have the duty to speak as the natives' advocate (Firth 1981:195).

So the vision of applied anthropologists as necessary contributors to the policy process has both illustrious antecedents—Malinowski, Bohannan, and Firth—and time depth extending across half a century.

To borrow another metaphor from Bohannan's 1980 address, over those five decades applied anthropology has traversed an epigenetic landscape—one that changes and moves because of the activities taking place within it. Kottak (1997) describes the transformation as a replacement of the ecosystem, Rappaport's (1971) model of a delimited population residing in bounded space, with "the new ecological anthropology." On this contemporary landscape "everything must be on a larger scale." He names the following outsiders as new key players:

1. External organizations (e.g., NGOs, corporate intruders).
2. External levels (e.g., political, administrative, regulatory).
3. External agents (e.g., migrants, refugees, insurgents, tourists).

And he adds, conclusively, *everything must be much more applied*. Anthropologists, specifically, must devise socially sensitive and culturally appropriate strategies for change (see Kottak and Colson 1994; Marcus 1995; Vayda 1983).

The experience of the authors in carrying out the admonition to "do something" extends across the same span of time. It has engaged us in a range of interventions in locales as unlike as the Sonoran desert of Arizona and the tropical rain forests of Mindanao, Philippines. We have performed research and designed solutions for O'odham (Papago) villagers and Ilocano homesteaders in the Digos-Padada Valley. But also for squatter barrios in Davao and Panama City—both communities of a half million. Today we seek to prescribe for human problems in sharply contrasting locales—the new binational center of Nogales, Sonora, and the globalized U.S.-Mexican borderland zone of which it is a single part.

As we review our lengthy track record, to which we will make only brief and very selected references here, we recognize that we have lived through the transformation conceptualized by Kottak "on the ground," and responded to it as best we could. In retrospect, it appears that applied anthropology, as it reaches for maturity, has been enmeshed in a continuous upward shift in scale across four levels of sociocultural integration.<sup>3</sup> Our text addresses each of these in sequence and then elaborates with examples from our experience.

#### *Level 1: The Little Community.*

In the much less complex social order and political economy of four decades ago, we could deal with groups integrated at the village or barrio level. In that environment the applied anthropologist, the "professional stranger," could function as an independent change agent. Spicer's (1957) famous casebook, *Human Problems in Technological Change*, provides examples.

Our illustration will be drawn from Beverly's Community Oriented Primary Care (COPC) project in Mindanao which was later replicated on the Tohono O'odham Reservation in

Southern Arizona. It exemplifies “bringing an applied project back home.” It operates on the principle that basic needs are generic, and procedures for meeting them may be transferable. We will make the case that applied interventions at this level, relying more on social reorganization than expensive technology and infrastructure, have become universalized as the province of the Nongovernment Organization (NGO) and its cognate counterpart, the GRO or Grass-Roots Organization (Fisher 1996).

*Level 2: The Developing City and Its Outliers.*

As we move forward in time, expanding populations and limited resources determine that once-rural groups become engaged with the urban community and its immediate hinterland. Foster (1987) documents this process for Tzintzuntzan across four decades. Moving with them to the cities, the applied anthropologist becomes a provider of technical assistance to a development agency with the resources required to operate on this expanded scale. Both urban community and region, in this discussion, exist as organized residence units in bounded space. Mangin’s (1970) *Peasants in the City* provides illustrative material.

Our example will be drawn from Robert’s work with the World Bank’s adventure in slum improvement and resettlement in Davao City. Squatter upgrading was a major enterprise 25 years ago when Third World urbanization was a major concern of our discipline. The site-and-services concept aimed at stimulating dormant slum/squatter neighborhoods with modest infusions of repayable capital. Recipient households were to mobilize to create a balanced unit of settlement with residences, employment, commerce, and services (Laquian 1983). The World Bank and U.S. Agency for International Development funded a number of projects. Major multilateral lenders in the “poverty alleviation” years in sites as dispersed as Davao and Dakar assumed that demonstration projects and multiplier effects would achieve comprehensive upgrades (Yeh and Laquian 1979; Yeung 1983).

*Level 3: The Denucleated Marginal Cluster: An Anti-Community?*

Over the last 20 years, population has concentrated at sites along the U.S.-Mexican border (and analogous borderlands elsewhere) where commerce is stimulated by significant cost advantages (in materials, shipping, labor, or processing) achieved by crossing a political barrier. Actual sites for clusters are chosen for access to transportation avenues and nodes in communications networks.

Rapid growth in the occupied area is unmatched by extension of services. Linear extension along major thoroughfares replaces concentric zones of socioeconomic similarity. The geographic center is not a focus of administrative and commercial density. Especially in the newer districts which comprise the plurality of *colonias*, households are made up of townmates and secondary relatives (rather than nuclear families) with rapid turnover in membership and frequent return to locations of origin or movement to other employment sites.

It is unclear (but unlikely) at this point whether these households will reproduce a second generation of descendants remaining within this population cluster. The apparent absence of a balanced set of interdependent institutions merits designation as an anticomunity.

Applied interventions in this setting shift from piecemeal projects introduced on the ground to formulation and choice among policy options for providing major infrastructure, e.g., water and sewer systems, electrification, and fire prevention, requiring political action and public (or private) finance. (See Castells 1983; Hannerz 1990.)

Our example will be drawn from recent attempts to involve NAFTA-created agencies in improvement of living conditions for the rapidly expanding industrial labor force of Nogales, Sonora. Our expectation was that the Davao site-and-services model would serve as another opportunity to bring an applied anthropology solution back home. The discovery that Nogales is a categorically different type of place—possessing most of the attributes of the denucleated marginal cluster—forced us to realize conditions for squatter upgrading do not exist there.

This discovery, in turn, leads us to propose some speculations for a postindustrial, and postnational version of applied anthropology.

*Level 4: The Postnational Map: The New Chreod?*

The global marketplace (Hackenberg 1995a) has created the new cartography of flexible links between elements of production of single products dispersed across and even between continents. Disaggregated components are linked by information nets in a model of articulation that annihilates space and compresses time into microseconds. Castells (1996:26) reminds us that the power vested in these components arises from the capacity of “. . . capital flows, labor markets, markets, production, management, information and technology to operate simultaneously at the world level.”

Because these elements were once (a mere fifty years ago) confined to the central business district of a single city, Garcia Canclini (1995:750) labels the present dispersed configuration as “the city without a map.” The global marketplace is being formalized through a network of international agreements (NAFTA, GATT, European Union). All are intended to facilitate trade. Disregard of national boundaries advances, and references to our era as postnational become more frequent.

The population units dependent upon this new cartography require a new demography of disaggregated segments likewise connected by information channels. The transnational character of this process is fostered by either overt or covert arrangements across boundaries facilitating either legal or illegal movement of the work force (see Hannerz 1994).

When epigenesis has so changed the landscape that its occupants can no longer pursue former lifeways, says Bohannon (1980:51), biologists tell us that a new chreod has been created and new solutions must be engaged. And so it seems to be.

The new chreod to be addressed here is an emergent landscape, as yet unnamed. It is that strip of the boundary between the United States and Mexico referred to as the Borderlands. It has a physical description: 2,000 miles wide from east to west and 124 miles (200 kilometers) from north to south, evenly divided between the U.S. and Mexico. Though it parallels the Rio Grande

and is traversed by the Colorado River, it is arid and features a scorching hot summer season. It is inhospitable to agriculture for most of its length and offers few other resources:

In the last forty years, the border region has become a center for North American economic activity . . . . Now dotted by twelve communities where residents, economies, and cultures are divided by an international boundary, these “sister cities” are home to more than 11 million people and over 1,700 national and international companies . . . .

While the population on both sides of the border continues to grow, most of the growth has occurred in Mexico. Between 1970 and 1988, the population in Mexican border states grew at an average annual rate of 36%. At current growth rates, the population will double again by the year 2020 (Spaulding and Audley 1997:4,8).

This set of settlements is neither state, society, nor culture. While its production units, called *maquilas*, are situated in Mexico, regional economists classify them as “fragmented export industries” (FEIs). Their primary output consists of components (electronic, automotive, chemical) for assembly and marketing in First World countries. Since the privatization of the *ejidos* in 1992, the border settlements have become both refuge areas and labor markets.

Though underserved, they are not unregulated. Over the past half century a number of binational and multinational commissions have sought to direct their activities and control their productivity. Since 1993, NAFTA has doubled their number and added development agencies intended to provide investment capital which will reduce industrial pollution, provide essential infrastructure, and reduce health hazards. These are the inevitable consequences of the explosive growth of a poverty-level, \$5/day labor force.

The U.S.-Mexican Borderlands could serve as a type site for the description and analysis of the global marketplace. It consists of processes rather than products. Only 2 percent of the materials it converts to components originate in Mexico, and 98 percent of its output leaves the country to be assembled, promoted, and marketed under First World labels and brand names. In the Borderlands, commodification of both human and material aspects of production is complete. This corner of the world, as we perceive it, offers the greatest challenge to applied anthropology as we stand on the threshold of the next century. It now appears at the margin as a peripheral process. But it is becoming the central process of capitalist-inspired change (Harvey 1989). We will examine it from the local perspective at Level 3 (Nogales) and from the global perspective at Level 4.

Let us anticipate our most important conclusions. Effective intervention in this milieu, which some would designate as “glocalized,” requires that we employ policy instruments rather than projects. A major reorganization of our “fifth subdiscipline” (Baba 1994) will be required. It remains to recount those parts of our experience acquired at each level and examine their relevance to applied anthropology today.

*Level 1. The Little Community.* A village or barrio health program established by the applied anthropologist, suitable for installation in contrasting environments: Mindanao and Southern Arizona.

## Beverly Hackenberg

It was no more than 25 years ago that leading health professionals began to advise that medical services should substitute the maintenance of health for the treatment of disease as their primary objective. The inventory of inexpensive preventive interventions could be provided by lower salaried family practitioners and physician assistants using minimum technical support.

Furthermore, health education, prenatal and postnatal care, immunization, family planning service, and risk factor monitoring could be provided cheaply and effectively by outreach to the community rather than by the intake of single individuals who presented with “complaints.” What I am describing is the origin and rapid spread of the Community Oriented Primary Care Movement (COPC).<sup>4</sup>

Since its impact depends upon a range of family contacts achieved by members of the community trained to serve as “physician extenders,” COPC relies upon social organization. It has been interlocked with anthropological principles from the start. I became an agent for COPC when the first of a series of NIH grants sent us to the Philippines.

### *The Katiwala Program*

In 1968, Davao City was the second largest urban place in the Philippines, though most of its 250,000 people had arrived during the preceding 20 years. It was located at the center of a vast frontier area recently opened for homesteading, which attracted a huge migrant stream of former tenant households seeking to settle on their own land. The region was also rich in timber concessions and logging operations. More of a huge encampment than a city, Davao was deficient in almost all urban services.

Health care was a primary concern. Because of a mutual interest in the philosophy of Paulo Freire (1970) and the belief that the Chinese barefoot doctors might offer a model for service provided by paramedics, I was invited to join with local members of the Philippine Medical Association in a program to deliver affordable preventive and curative services to the slum and squatter barrios of Davao.

Together we developed a plan which sent me into the target areas to hold community meetings and organize volunteer households into a crude HMO with dues of 50 centavos a month which entitled members to a weekly clinic visit and purchase of medicines at cost. These groups, from 30 to 50 households, would elect several middle-aged housewives to receive primary health care training every Saturday for six months. With province-wide participation by Catholic mission churches, American Maryknoll, Irish Redemptorists, and French Canadian priests, and a scatter of Protestant missionaries, we organized small HMOs throughout the city and adjacent rural areas.

We graduated our health workers, who called themselves *Katiwala* (Guardians of Health), with uniforms, stethoscopes, thermometers, blood pressure cuffs, and first-aid kits, then deployed them to their home communities with the ability to detect and treat minor conditions and to recognize and refer serious symptoms to a physician. They recorded health histories for all household members, provided pre- and postnatal care, gave family planning advice, provided well-baby clinics, did case-finding and follow-up care for chronic diseases, taught sanitation and nutrition, and worked on immunization programs.

Our mutual efforts culminated in 1979 with the founding of Davao Medical School—the only one in the southern third of the country—featuring the Institute of Primary Health Care devoted to meeting the needs of indigent urban and rural households through the use of

housewives from the community who were trained as paramedics, working with medical students within the institute.

We did not reflect on it at the time, but the Katiwala program incorporated key elements of participatory development. It also contained the elements of what became known as COPC. The health program, in all phases from training to service delivery, was implemented by a local group consisting of physicians, business enterprises, anthropologists, religious and political leaders, neighborhood women, and the client households. In postmodern parlance, they would be recognized as stakeholders.

The Katiwala women, the key personnel, were selected by their peers, set the priorities for the content of their instruction, elected officers, and recruited village household participants. Once again in today's argot, they enjoyed empowerment and ownership. Our success was measured by the number of outlying communities in which doctors recruited women from their communities to come to Davao and enter the Institute of Primary Health Care for training.

And there were policy implications. The data sets collected by caregivers in the communities confirmed that these paramedics, or physician extenders, could act as the necessary interface between upper-class medical personnel and the peasantry and contribute to health maintenance cheaply and effectively. Our program was site-visited by the World Health Organization, which then provided support to the Philippine Ministry of Health to create strategically located training centers for paramedics. These were then deployed throughout the 13 regions of the Philippines.

### ***O'odham Community Health Workers***

The vast Sonoran Desert region extending west from Tucson has been home to the Tohono O'odham Indians for several thousand years. In this forbidding land they developed a rich subsistence pattern combining native plants and wild game. After the Spanish arrived, they added cultivated crops and livestock. But under the combined impact of population growth, adverse climate changes, decline of native industries and alcohol, they are no longer self-sufficient.

Ironically, the O'odham are now numbered among the "rich casino Indians" because of their reputed receipts of a million dollars per week from gaming enterprises. Yet, in 1990, 9,500 of the 17,000 enrolled tribal members were reservation residents, and 66 percent of these had incomes below the poverty line. Per capita income was less than \$3,000, and unemployment and underemployment were estimated at 46 percent of the labor force.

Despite survival in their arid wasteland over several millennia, the tribe has developed a fatal and progressive chronic disease which, beginning in the 1950s, rapidly advanced to epidemic proportions and, according to Josiah Moore, the Tribal Chairman at the beginning of the present decade, now threatens them with extinction. The disease is Type II (non-insulin dependent) Diabetes Mellitus (NIDDM). At the time of our intervention it afflicted more than half of the adult population over the age of 45 (see Justice 1994).

We arrived on the scene at the invitation of the Tohono O'odham Health Department (TOHD). The tribal health committee, made up of tribal council members and several O'odham nurses, saw the need to provide both health promotion and disease prevention (HP/DP) and asked us to design a program incorporating COPC principles. Their primary concern was diabetes control.

The risk of contracting Type II NIDDM is genetically transmitted from parents already afflicted. It is progressive once present and terminates after several decades in ultimate kidney

failure and death. Early onset (ages 15–19) among the O’odham is associated with gross obesity frequently encountered among adolescents. This in turn can be attributed to poor diet and, in this vast wasteland of unemployment, to lack of physical activity.

Prevention requires early intervention and continuous reinforcement. Proper health habits must be formed among preadolescents if possible. Both parents and children must be involved in a plan that 1) emphasizes understanding the disease and its risk factors; 2) teaches proper nutrition and weight control; 3) promotes regular exercise; and 4) develops positive self-image to reinforce and motivate compliance.

The essential personnel of the TOHD were 36 misnamed community health representatives (CHRs). They were based in TOHD’s office complex adjacent to the Indian Health Hospital in the agency town of Sells. Their village background and basic training resembled that of the Katiwala. But their assignment was transportation rather than health care. They worked eight to twelve hours per day as drivers bringing patients from the 70 villages to the Indian Health Hospital in the agency town of Sells.

To implement the Davao model in the desert, and provide continuous community-based diabetes prevention, we needed to enlist the support of the people and the district councils who represented them. TOHD administrators, in a tribal-wide series of meetings, convinced the districts that the existing centralized pattern of health department operations must be inverted.

We proposed to reassign half the CHRs (18) to six dispersed field clinics at which each could provide full-time preventive services and primary care. The remaining 18 would continue to transport patients from field sites to the hospital, but the presence of the renamed community health workers (CHWs) in the villages greatly reduced the number.

As in Davao, the decentralized CHW teams were to become responsible for the health status of households in the communities to which they were assigned, and this meant diabetes prevention. But we needed to provide them with the tools to perform this task. These included 1) identity of household members at high risk for contracting diabetes; 2) knowledge of procedures for impeding or obstructing its progress; and 3) professional backup to assist with health education, screening, and referral.

The first essential was obtained from our earlier pre-Philippine success in constructing a tribal-wide, computerized genealogical data bank, now linked with an IHS-maintained diabetes case register (Hackenberg 1967, 1970). It disclosed the family connections of all close relatives of diabetics who were, by definition, “high risk.” To educate CHWs and provide professional support, we obtained substantial grants from the Robert Wood Johnson and Kellogg Foundations (Kukulka et al. 1994) which funded employment of 1) a part-time physician; 2) a full-time nurse-practitioner; 3) a nutritionist; and 4) a physical fitness specialist.

Diabetic patients in the communities could now be monitored by the field teams. Diabetic parents could be enlisted to join their children-at-risk in participating in a multiphasic prevention program, delivered at the village level by the CHWs. The teams, in turn, were backed by our professional staff which could provide the elements described at the beginning of this section: screening and risk-factor monitoring, diabetes education, nutritional information, food preparation classes, and supervised weight-loss programs. To complete the design, a comprehensive prevention plan was introduced into the reservation schools for the benefit of junior-high and high-school children.

The O’odham prevention program was a tribal-wide undertaking with an uncommon degree of consensus. Unlike the Katiwala project, it included a substantial political dimension represented by tribal and district council approvals. Its success depended upon an alliance of

stakeholders which extended across social status, cultural and linguistic barriers, and specialized participation in health care at widely differing levels.

Priority setting and authorization to engage in this life-support plan flowed from the bottom to the top of the structure. It personified all that is essential in empowerment. Policy changes concerning the nature of care and the means to obtain it were incorporated at every step of the way.

Applied anthropology of the type illustrated by these examples not only continues but expands. The retreat by agencies and governments from a direct attack on poverty has promoted their replacement by privately sponsored sources. Fisher (1966:58) estimates that there are more than 200,000 NGOs whose operations are networked by such well-known global organizations as CARE, Catholic Relief Services, and OXFAM. Many are supported by Women in Development (WID) budgets and provide primary health care, at points as far apart as Java, Tanzania, and the Navajo Reservation. In a variety of culturally determined modes, they implement the basic model of COPC.

*Level 2. The Developing City.* A model squatter upgrading program installed in a third world regional city in the 1970s, presumed to be transferable to comparative environments.

### **Robert Hackenberg**

Beyond the year 2000, it may be progressively less possible to bring applied anthropology back home, or to bring our tried and tested solutions anywhere else for that matter. The state of the world has advanced beyond the reach of our conventional tool kit. This possibility became clear to me when I confronted a human disaster—the squatter barrio explosion engulfing hundreds of thousands of recent arrivals to the US-Mexico Borderlands community, Nogales, Sonora—and sought to transfer one of our most successful applied solutions to a similar problem: the Davao City Slum Improvement and Resettlement (SIR) Program which dealt effectively with a large-scale squatter problem in the decade following 1975.

Next to health, housing represents the most urgent need in developing countries. Urban migration has relocated millions in what have become “cities of peasants” (Roberts 1978). As with medical care the conventional Western solution, the high-rise multiunit housing project, has been priced out of reach of all but the most affluent nonwestern nations, e.g., Singapore and Hong Kong.

In its place, and analogous in many ways to COPC, people-oriented and social organization-based low-cost solutions have been invented and tested. The intent has been to convert neighborhoods which were disease-ridden fire hazards, and scenes of gang violence, to stable and healthy self-governing and self-maintaining neighborhoods. We introduced this transformation, embodied in the *site-and-services model*, to the southern Philippines (Hackenberg and Hackenberg 1987). Under World Bank auspices, it is rapidly accepted in cities across Asia, Africa, and Latin America (Laquian 1975).

### ***Piapi District, Davao City, Philippines***

Larger third world cities are frequently seaports. As centers of commerce they attract labor migrants whose numbers far exceed both housing stocks and urban services. A frequent



solution is to occupy tidal flats and sometimes submerged foreshore areas. In Davao City the seacoast opposite the central business district featured a city park and a large tidal flat known as Piapi which, during the 1960s, became the most densely populated squatter settlement in the city. Squatters acquired the land, as everywhere, by invasion and seizure.

In Davao, physical conflict with landlords and city government was averted by the improbable intersection of three factors. The first was research. The Hackenbergs, with grants to Davao Research and Planning Foundation (DRPF), a partnership formed with local developers and industrialists, constructed a citywide database measuring trends in household employment, income, and place of residence. It confirmed that in-town locations adjacent to the central business district were essential if squatters were to supplement meager and intermittent wages with commerce in the informal sector (Castells and Portes 1989).

The second was the appearance of a multilateral lender who shared our insights and proposed to invest in implementing them. After Robert McNamara's Nairobi speech in 1970, the World Bank sought direct investments in poverty alleviation. The action plan was for an improved and expanded version of the site-and-services model. Still aimed at squatter communities, it offered loan funds to purchase and provide titled homesites located on improved lots with minimum services (drainage, footpaths, standpipes for water delivery to blocks of homesites, electric circuits, and safe waste disposal).

Loans included bundles of permanent housing materials and technical assistance needed to complete construction. Labor was provided by residents. Loans were issued to groups of households, and payment by individual borrowers was guaranteed by the elected officers of each group. The bank plan was inspired by John Turner's (1967) observations in Lima which concluded that squatters who became landowners and received basic services would invest in the continuous improvement of both sites and structures using both personal and community resources.

Third, and finally, substantial political support for the World Bank loan and for the plan proposed by DRPF came from the National Housing Authority (NHA) in Manila after joint presentations were made to General Gaudencio Tobias, the Secretary of NHA and a favorite of Imelda Marcos, who may have been the most powerful force in the country. In Davao, NHA established a Slum Improvement and Resettlement (SIR) unit directed by David DeGroot.

From start to finish, 18 hectares were upgraded and 2,500 households with 18,000 former squatters were improved on sites they had previously occupied. The cost of \$3 million—\$1,200 per household averaging 7.2 persons—was considered a bargain by all parties concerned. Administrative costs, reduced by resident participation in the collection of payments, were charged against the project.

With this experience the Bank authorized a second phase intended to develop an additional 1,900 homesites serving a population of 12,000. When completed, "housing solutions" were available to 25 percent of the critical squatter barrio population of the city. Cost recovery from Phase I was expected to pay for Phase II. A subsequent "roll over" would have continued the process until redevelopment was complete.

The Davao SIR program strikes a number of contemporary themes. It was a product of cooperation between low-income residence groups, anthropologists providing technical assistance, a major financial institution, local influential citizens (sponsors of DRPF), and politicians in power. All major stakeholders took part in the process. Residents were empowered to form associations, elect officers, design and allocate sites, and choose among improvements and optional costs.

At both national and local levels, it represented a new policy direction, substituting investment and improvement for remote relocation and abandonment. A major commitment of \$60 million in World Bank funding was approved for a Regional Cities Development Program to transfer “the Davao model” to six other secondary cities in the Philippines.

*Level 3. The Denucleated Cluster.* In the wake of the establishment of the global market, the connectedness of place-with-people appears to be dissolving. Nogales, Sonora, and other sites in border country, are becoming “zones of occupancy.” Something for which, as yet, aside from science fiction and surrealist literature, we have no name.

### ***Arizona–Sonora Borderlands***<sup>5</sup>

Can the Davao SIR experience be replicated? It seems very unlikely. The modernization-based single project has been overwhelmed by an enveloping, all-consuming universal process: the advance of the global marketplace (Hackenberg 1995a). Where the applied project passes into history as normal science the shift to a new paradigm fills the space it vacated. Its major features are the disintegration of *locality* and the entailed displacement of the *resident*.

As the forces shaping events become increasingly complex and multidimensional, our capacity to control and direct them is reduced in inverse proportion. My concern here is with the northern city of Nogales, Sonora, where squatter barrios similar to those in Davao have expanded to accommodate the influx of several hundred thousand labor migrants.

The movement began quietly and inconspicuously in 1965 when Mexico initiated the Border Industrialization Program, providing tax breaks and incentives to foreign manufacturers seeking to take advantage of low-cost labor and exemption from environmental protections and employee benefits. The primary responses came from American manufacturers, and the result was the *maquiladora* program. The Borderland plants, whose primary locations are in Tijuana, Mexicali, Ciudad Juarez, and Matamoros, numbered 1,607 units employing 530,712 workers in 1997. The *maquilas* are now Mexico’s third largest foreign exchange earner, behind petroleum and tourism.

Since inception, the labor migration to the north has overwhelmed the string of small border towns. Nogales, Sonora, 65 miles south of Tucson, was typical of these, with a 1950 population of 25,000. Over the next several decades it was selected as a site for the establishment of 85 “twin plants” with 28,208 employees (1997). Electrical assembly, auto parts, and medical supplies are the principal product lines (Hackenberg 1997b).

Related transportation, supply, and maintenance activities contributed to expansion. Explosive growth brought population past the 200,000 mark in 1990 and supports a projection of 400,000 by the year 2000. A majority of labor migrants live in squatter settlements (*colonias*) on vacant land seized by associations of newly arrived workers.

More important than the ecodeographic facts is the surrealistic nature of the Nogales industrial labor force/squatter household population itself. Two recent book-length studies of Nogales cover the maquila labor force (Kopinak 1996) and the domestic residential conditions in which it exists (Ingram, Laney, and Gillilan 1995). They offer observations which combine to form a pattern:

1. Between 1986 and 1989, twenty new residential districts appeared in Nogales (Kopinak 1996:109). Each *colonia* was formed by squatters and represented a *maquila*-related settlement. The process of seizure and invasion of Colonia Emiliano Zapata, which grew in

one year from 150 invaders to 2,000 residents, was the subject of a New York Times feature article in July 1990 (Tolan 1990).

2. Residences consist of flimsy structures assembled from scrap materials. They receive no urban services. Neither water nor waste disposal are provided. Fire hazards are ubiquitous from makeshift cooking arrangements and infectious diseases, produced by garbage and human waste, are universal. Workers are young (median age 23.6 years) and unmarried. The modal employee is a single woman with children. They earn \$3.50 to \$5.00 per day.
3. Because of adverse compensation and working conditions, turnover rates in the Nogales factories are over 100 percent per year. At some plants turnover approaches 400 percent. Recruitment by posted advertisement and radio announcements of job openings is continuous. Women use intervals between jobs for visits to home villages and for child-minding. There are no labor unions in Nogales.
4. The majority of *maquiladora* employees form flexible residence arrangements with relatives and unrelated persons, often from the same origin community, to secure access to housing and mutual subsistence support. We might designate these units as *transient households*. They average 4.5 members in size.
5. The residential labor force of the *maquiladora* workers in Nogales is more of an imagined than actual community (Anderson 1983). Its ephemeral nature is underscored by reports that 81 percent of employees originated within Sonora or adjacent states of Nayarit and Sinaloa, but not in Nogales itself. Their pattern of movement is more circular than sedentary. Their residence in Nogales is expedient. It is not expected to be permanent.

On January 1, 1994, the North American Free Trade Agreement (NAFTA) removed tariff barriers to commerce and legal constraints to the free movement of production sites between Canada, Mexico, and the United States. One response was the rapid expansion of maquila establishments along the border. NAFTA recognized the inadequacy of urban infrastructure and services to cope with the resulting human flood by creating instruments to supplement and expand community resources.

Financing is the first requirement and, in the tradition of previous multilateral lenders, a North American Development Bank (NADBANK) was established. Its implementation arm, the Border Environment Cooperation Commission (BECC), is authorized to receive proposals from communities located in a radius of 100 kilometers on either side of the border. BECC's assistance is restricted to 1) development of potable water supplies; 2) removal of industrial wastes; and 3) promotion of environmental health. There is no provision for shelter or human services.

With funding from the North American Development Bank, the BECC is undertaking a \$39 million expansion of the Nogales municipal water system, the first since 1945. To our regret we discovered that the plan for this system will improve delivery to those middle- and upper-class districts already served but will bypass squatter communities. Privately owned tank trucks will still deliver water to the hillside colonias, where it is stored in 55-gallon chemical drums discarded by the maquilas (Ingram, Laney and Gillilan 1995:76–80). These drums are not inspected and considered unsafe for their present use.

At first glance, similarities of structure and process with Davao City encourage the speculation that the squatter colonias of Nogales could serve as another opportunity to “bring an applied anthropology solution back home.” And also at first glance, in the post-NAFTA environment, resources to plan and implement a site-and-services solution appear to be abundant.

The North American Development Bank could serve as the funding agency to implement environmental improvement in Nogales. Technical studies could be performed by contracts between Border Environment Cooperation Commission and the Bureau of Applied Research in Anthropology, which holds a negotiated technical assistance agreement. The Environmental Protection Agency’s Border XXI Program and the trilateral (U.S., Canada, Mexico) Commission on Economic Cooperation are possible additional resources.

Unfortunately, while loans are provided for proposal preparation, no community-level research funds are available.<sup>6</sup> The Davao City surveys cannot be repeated in Nogales. The hard quantitative data and accurate maps required for site-and-services intervention could not be obtained from the “rapid appraisal” methods now favored by prospective funding agencies (see Finan 1996; Perez 1997).

And at the investment level, there are no more 100 percent lenders like the World Bank of 20 years ago. NADBANK limits its maximum “exposure” to 50 percent of the estimated cost and negotiates with private investors for the balance—often as much as 80 percent. As a consequence loans are made at market-level interest rates rather than concessional rates established by World Bank and USAID decades ago. This places major emphasis on cost recovery from user fees and special taxes. This, in turn, forecasts that squatter settlements will be unlikely beneficiaries from NADBANK lending. The \$39-million Nogales water-system improvement loan referenced above excluded them entirely in favor of existing ratepayers.

The new colonias of Nogales today are characterized neither by *locality* nor by *residence*. Other studies of Hispanic labor migrants designate them as sojourners rather than settlers (Massey 1987; Hackenberg 1995b). The permanent population serving as a residential core for a site-and-services project is not to be found among these recent arrivals who represent an ethnoscape (Appadurai 1991) rather than a community.

And now, to flash forward across two decades from the point at which we ended our description of the transformation of Davao City’s squatter class. Nogales is shaped by global market forces and the southern Philippines is not likely to escape them. Well, it didn’t. Instead of a straight-line extrapolation, what took place at our field station looks more like a random walk.

The first element to disappear should have been the most dependable. The World Bank shifted from project support and poverty alleviation to structural adjustment lending and revising national income accounts. The Regional Cities Development Program was an early casualty. The fate of the \$60 million committed is unknown. Remaining phases of the Davao SIR project, aimed at total squatter redevelopment, were to be financed from cost recovery. But fate, once again, intervened.

In 1986, the conjugal dictatorship of Ferdinand and Imelda Marcos was terminated abruptly by the Yellow Revolution—a military coup with vast popular support which installed Corazon Aquino as president and reinstated democratic processes of decision making. Among the decisions made was the abolition of the National Housing Authority and termination of its projects.

The huge foreign debt resulting from the Marcos years put the new government under IMF controls with strict requirements to reduce “unnecessary” government spending. It follows, of course, that human services including squatter upgrading were declared unnecessary.

And what of the 4,500 households benefiting from the completed phases of the Davao SIR program? The site and its occupants have continued to improve—providing a permanent physical asset for the community and health and life-style gains for its residents. But the present beneficiaries are frequently *not* the recipients of the original improved homesites.

Throughout Southeast Asia, an entrepreneurial attitude far older than the global marketplace, and probably attributable to Chinese influence, prevails. An improved residence is treated as a capital good and not a consumer item. In contemporary terms of household economics, its exchange value is greater than its use value (Castells 1983). Your most reliable and continuous source of income may reside in the opportunity to improve it, expand it, subdivide it, and *rent* it.

Many of the original proprietors of site-and-services units have converted them to apartments to obtain multiple rental income. To enjoy their earnings they have pioneered new squatter barrios by repeating the processes of property seizure and auto-construction. Population densities within the first-improved settlements have doubled (at least) since the SIR program was completed. And present occupants are unknown to us. Once again, notions of bounded locations and permanent residents are erased by the pace of change.

Site-and-services models are primarily social organization based solutions to a basic need: shelter. In this they are typologically similar to Community Oriented Primary Care. Like the COPC process of health promotion, site-and-services has proven sufficiently sturdy and viable to accommodate a range of “add-ons” that meet related needs of poverty neighborhoods: microenterprise promotion, employment generation, child care and preschooling, adult education, and the like. Most of these income generators provide new opportunities for women. Logically, these activities are frequently subsumed under women-in-development programs.

The site-and-services protocol for shelter construction, again like COPC for health promotion, has received global acceptance:

The most far-reaching in both numbers of people affected and global impact have been the shelter assistance programs launched by the World Bank since 1975. By 1982, 90 projects involved in sites-and-services and community upgrading were in different stages of implementation in 50 Third World nations, many of which were in Asia . . . Since 1975, the World Bank has provided US \$1.3 billion for shelter programs that are estimated to have benefited more than 10 million people (Yeung 1983:15–16).

The high tide of financial support for programs of this type was experienced in the decade following 1975. They no longer appear on the agendas of multilateral lenders. The long-term outcomes for original beneficiaries and improved areas, and possible broader demonstration effects on adjacent residential sites, have never been investigated.

*Level 4. The Postnational Map.* Applied anthropology must seek to locate itself on the insubstantial terrain which we will occupy in Century XXI. The fabric on which the map is drawn is sure to contain topological properties. Harvey (1989) places space-time compression among the most important of them. Conventional core-periphery models of modernization have been eliminated (Castells 1983) and in their place we have the less substantial terrain defined by decentering, deterritorialization, diaspora, delocalization, and transnationalism (Kearney 1995). This is the ephemeral—even elusive—landscape we will document with illustrations from the U.S.-Mexico Borderlands.

## Robert and Beverly Hackenberg

On April 17, 1998, in Havana, Fidel Castro delivered a speech to a regional women's conference in which he proclaimed that "capitalism cannot last one hundred years," but acknowledged that "... if someone were to shout, 'Down with globalization,' it would be like shouting 'Down with the law of gravity'." Wish fulfillment for Fidel will probably remain elusive. The linkage between capitalism and globalization daily acquires both time depth and geographical extension.

Following Castells (1996) and Giddens (1990), we accept the proposition that the 19th century interdependence of capitalism and industry has been replaced by the 21st century interdependence of capitalism and information. Since both elements, capital and information, are subject to instantaneous deployment and transmission across the world, the postnational map exists as a concept rather than a dimensional reality.

This is the lesson of the Borderlands, which has become more of a locale than an environment. It is a scene within which, and across which, a number of events with profound human impact transpire. To document them we must include corporate decisions to invest, made in board rooms in Seoul, Tokyo, and Detroit. But also individual family decisions to relocate made around cooking fires on hillside farms in Mexico and Guatemala.

They illustrate a common theme: the Borderlands provides a stage on which both macroscale and microscale scenarios are enacted simultaneously—originating at remote points and in many different nations. The Borderlands is a "point of articulation"—perhaps a point of collision would describe it more accurately—at which the intersection of unidentified and unplanned determinants produces a kaleidoscope of unpredicted consequences.

A few examples, identified only by category and subject, must suffice for our purposes here.

1. *Allocation and control of scarce resources in the Borderlands.* Forest reserves incur stresses from both indigenous and multinational consumers sufficient to diminish sustainability (Weaver 1996; Yetman 1993). The Gulf of California fisheries are similarly depleted under competitive conflict from the same contenders (McGuire and Valdez-Guardia 1997). Water rights disputes, always a flashpoint in the Borderlands, have been intensified by collision of expanding settlements and commercial, recreational and tribal interests (Brown and Ingram 1987).
2. *Control of the consequences of development.* A multitude of toxic wastes, expelled by manufacturers without number, flow untreated into natural water courses along the entire length of the Borderlands. The Mexicali horror story, which may be a limiting case, was recently featured in the New York Times (April 18, 1996). Its millions of gallons of pesticides, toxic chemicals and raw sewage find their way into California's Salton Sea. Pollution with human wastes by residents occurs along the entire length of the Rio Grande—22 million gallons of sewage per day from Ciudad Juarez alone. Hepatitis is endemic in some areas and birth defects occur at three times the U.S. rate (Tucson Citizen, July 23, 1990, and September 9, 1993).

3. *Control and regulation of burgeoning trade.* Ford Motor Company assembles 700 cars per day at its most completely automated production center (192 computerized robots) in Hermosillo, Sonora. Two truck trains per day move the output north to the U.S. through Nogales. This prototype has been followed by others (Arizona Daily Star, March 15, 1992). Nogales is also the transit point for two-thirds of the winter vegetables produced on Mexico's west coast for sale in the U.S. Between October 1996 and August 1997, 210,454 Mexican trucks rolled across Mariposa Crossing. No effective regulation or safety inspection has as yet been devised (Tucson Weekly, October 10, 1997).

Imported tomatoes alone represent a \$350 million per year industry. This traffic is fiercely contested by Florida growers who still have two-thirds of the business but are rapidly losing market share. Another stream of truck traffic across the Borderlands brings chiles and other staples of the Mexican diet through Tijuana into the Los Angeles area. The importance of this commerce to both large and small producers, and to the labor force in the mammoth transport industry, has been described by anthropologists (Alvarez and Collier 1994; Alvarez 1998; Stanford 1994).

4. *Control of traffic in people.* Several thousand illegal workers per day were carried across the Rio Grande from Juarez to seek work in El Paso in 1986 for 25 cents per trip (Moyers 1986). Since NAFTA, El Paso employers have moved to Juarez and the traffic in workers has taken a U-turn (Denver Post, October 15, 1998). Despite this local reversal, the overall movement of illegals shows an upward geometric progression. In the Border Patrol's Tucson Sector, apprehensions increased from 272,490 in FY 1997 to 387,395 in FY 1998 (Arizona Daily Star, October 2, 1998).
5. *Control of the informal economy.* Throughout the 20th century there has been an indigenous bootlegging trade in Mexican liquor among the U.S. tribes. Since WWII, a more lucrative business involves procurement of prescription drugs in Mexico for consumption or resale to seniors within the U.S. A recent addition has been illegal traffic in exotic birds and rare animals. Since the 1960s these petty traders have been eclipsed by organized crime syndicates trading in stolen vehicles and narcotics. At the Nogales and Douglas border stations, 80 tons of marijuana were seized in FY 1997 and 90 tons in FY 1998. A record six ton single seizure of cocaine occurred in Tucson in 1996 (Arizona Daily Star, December 5, 1996).

More than half the U.S.-bound cocaine smuggled out of South America passes through Tijuana. The other major transit route is through Ciudad Juarez. Both routes are controlled by organized crime families with highly placed government connections (Arizona Daily Star, October 6, 1996, and December 5, 1996). In recent weeks gang wars over control of U.S. outlets have led to multiple murders in Sonora, Chihuahua, and Baja California Norte. In Phoenix, Arizona, it is estimated that 40 percent of the annual homicides are related to Mexican narcotics traffic. Drug trade in Mexico is estimated to be a \$20 billion per year enterprise, making it larger than the petroleum industry (Arizona Republic, November 10, 1996). And it is linked inseparably to the Borderlands. This year the U.S. government will spend \$3.8 billion to patrol the border.

6. *Concern for endangered species.* The Borderlands has become a battleground of a less lethal variety. This fragile environment, preserved since antiquity, serves as habitat for a multiplicity of species encountered nowhere else. Developers and commercial interests of all sorts find their paths impeded, not by the semimythical Monkey Wrench Gang, but by a blizzard of suits and injunctions filed under the Endangered Species Act. Currently, the protection of the flight paths and nesting areas of several thousands of migratory bird species across southern Arizona and northern Sonora has become a concern of the Council on Economic Cooperation, a Toronto-based creature of NAFTA.

These brief vignettes have been selected from our Borderlands case files to illustrate the perceived shift in levels of integration to the domain of Kottak's "new ecological anthropology" within which they reside. And if we intend to pursue our self-appointed mission of intervention and amelioration, we must find our own place in this confusing chreod.

We must act as applied anthropologists to improve the human condition of that 40 percent (at least) of the members of third world societies who are adversely affected by the polarizing income distributions resulting from capitalist globalization. At the same time, we must recognize that strategies of "flexible accumulation" (Harvey 1989; Hackenberg 1995a, 1995b) are at work at home as well as abroad and deskilling and the shift toward secondary and tertiary labor markets recognize neither national nor cultural boundaries.

### **So What Is To Be Done?**

Let the namesake of these awards answer the question. And his response, which we quoted in our epigraph, is timely and substantial. We must pursue science and engage in politics. It is fitting that we should accept this challenge since both of these goals appear in the SfAA mission statement, recently reprised by John Young, our current president:

The Society actively promotes interdisciplinary scientific investigation of those principles guiding human relations, and encourages application of such principles with the goal of solving human problems (SfAA Newsletter, Vol. 8, No. 2, May, 1998).

### ***Scientific Agenda***

The science we must pursue is indicated by the limited state of our knowledge concerning locales such as Nogales—we called it a "denucleated marginal cluster" because no conventional community description seems to fit—and the Borderlands, clearly a segment torn from the postnational map. Both are creatures of the "global ecumene" (Hannerz 1992). And both are known by logical constructs and conceptual imagery (Anderson 1983; Harvey 1989; Giddens 1990; Hannerz 1994; Garcia Canclini 1995; Castells 1996) rather than by empirical description of sites corresponding to the constructs or images.

We must extend our comparative generalizing methods to describe and analyze post-national elements and processes. By reducing globalization to a set of case studies in contrasting locales, we may be the first to produce middle-range theories (Merton 1968) of postnationalism.<sup>7</sup> While these would be significant contributions, and could validate our scientific status, their primary purpose would be to select and justify proposals for intervention.

A creative strategy will be required to reach this goal in an era of diminished funding, but



a cost-effective solution is at hand. The 1960s and 1970s were rich in their yield of meticulous community studies. The published results are on our shelves and the supporting fieldnotes are in our files. During the intervening decades, each of those study sites has been caught up in the globalization process.

Restudies of a selected set of those locations would provide an empirical basis for a comparative generalizing approach to drawing the postnational map, and also for theory building. Costs would be reduced and efficiency increased if SfAA would appoint a committee to design a research protocol identifying key variables to guide restudies and assure their comparability. The use of GIS technology and rapid appraisal methods should be considered by the committee (Hackenberg 1997a).

### ***Political Engagement***

*Political Authority.* The politics in which we must engage are policy making and policy promotion. Bohannan (1980:520), in our title piece, proposed that “. . . applied anthropology will provide leadership toward the policy sciences.” In a more recent presidential address, Peacock (1997:13) urges us to take the lead in shaping policy. The process is not unknown to us (Weaver 1985, 1996; Chambers 1989; Baba 1994). The research agenda outlined above, if implemented, should give us both the information and the assurance to act upon it.

We have achieved success and gained recognition as advocates on behalf of indigenous or minority peoples. We need to redirect the same passion and preparation involved, for example, in securing the fishing rights of the Columbia River tribes to broader policy issues at the highest, often multinational, levels of decision making. The descriptions of Nogales and the Borderlands should convince us that there is no lack of appropriate health, human services, and environmental issues on which to formulate policy.

In Norman Corwin's (1945:71) famous phrase, “. . . this is not so wild a dream as those who profit by postponing it pretend.” If it ever were a dream, be assured that it is no longer. Both the American Anthropological Association and the Society for Applied Anthropology have authorized standing committees to develop a policy agenda. Significant contributions have come from the AAA's Commission on Human Rights (ISC) (Nagengast and Turner 1997) and the SfAA's Public Policy and International Standards Committees. The ISC's involvement in the review of World Bank social and environmental policies and the International Finance Corporation's Pangu River hydroelectric project (Chile) are particularly encouraging (Winthrop 1998).<sup>8</sup>

*Moral Authority.* But applied anthropologists must also search for empowerment if our voices on policy issues are to be heard (Rappaport 1993). The attempt to leverage a policy issue in the direction of a favorable outcome rests primarily on our capacity to involve and manipulate universal human values, and focus them on specific issues. The Pangu River hydro project could serve as an example. Mounting engagement with issues of environmental equity and justice (Austin 1998; Zedeno, Austin and Stoffle 1997) chart a parallel course.

Two decades ago Virginia Hine (1977) proposed that we delocalize applied anthropology by forming dispersed networks of practitioners with shared interests who maintain contact via electronic media. She argued that the focused impact of “segmented, polycephalous idea-based networks,” or SPINs can be a potent force, both moral and intellectual:

Perhaps the most significant aspect of the segmentary mode of organization is the role of the ideological bond . . . the power of a unifying idea adds a qualitatively

different element to the equation. The power lies in a deep commitment to a very few basic tenets shared by all (Hine 1977:20).

In his statement to the plenary session on *Human Rights, Power and Difference* at the 1998 SfAA meeting, Rodolfo Stavenhagen cited the contributions of both U.S. and Mexican anthropologists to positions on human and community rights set forth by the Zapatistas of Chiapas in their dealings with the Mexican government. Let this serve as a prototype for 1) an international affiliation of practitioners and 2) an opportunity to exercise moral authority. Recent initial success of attempts to create a global dispersed network (or SPIN) of applied anthropologists is especially encouraging (Hill and Baba 1997; Baba 1998).<sup>9</sup>

Applied anthropology stands at a watershed. We may look back with pride upon a half century of accomplishments achieved through small-scale projects based on creative social organization. The COPC and SIR models remain applicable in the diminishing number of venues where the concepts of delimited *location* and continuous *residence* may still be relevant. But Paul Bohannan, our guide across this epigenetic landscape, cautioned:

I realize that the cleavage between rich and poor, the haves and have-nots is still with us and is growing more stark. Across this rift, there is much room for traditional anthropological fieldwork and traditional helping roles. But I am reminded that Arnold Schoenberg once said that there is a lot of beautiful music still to be written in the key of C major. Be that as it may, a revolution has occurred and everybody knows it (Bohannan 1980:512).

We know that there has been a global capitalist postnational revolution. And we know that we must get out there and cope with it. Because, as Gil Scott Heron reminds us, “The revolution will NOT be televised.”

### Notes

<sup>1</sup>In the famous pamphlet of the same name, Lenin (1901) argued that labor unions, working “from the bottom up” could never provoke government reform. Instead, he predicted success for a group of activist professionals, working “from the top down.” Our lecture, which calls for promotion of policy initiatives by a network of applied anthropologists, parallels his argument. For the opposing view see Hackenberg (1997a).

<sup>2</sup>The perceived increasing opposition between the political economy of globalization and the ethnic/fundamentalist or “tribal” alternative is cogently presented by Barber (1996) who employs the terms “Jihad” and “McWorld” to characterize each type of society, e.g., Afghanistan and Israel.

But, in a very provocative piece, Castells (1996:25–31) considers the more complicated case in which tribal and global tendencies appear discordantly in the same society. In Mexico today, for example, the south (Chiapas) is caught up in “Jihad” while the north (Sonora) represents “McWorld”.

An equally challenging discussion reduces the paradox to a microcosm when Kaplan (1998:56–64) argues that both are contained within a single city, Tucson, Arizona. South Tucson is an impoverished ethnic ghetto while the Catalina Foothills to the north is the epitome of winter resort affluence.

<sup>3</sup>The levels of integration concept is Steward’s (1955), but its extension to the “supranational” level was achieved by Alvin Wolfe (1977) in a groundbreaking essay.

<sup>4</sup>COPC is the health care vehicle of choice for those who believe in outreach and prevention rather than intake and treatment. It seeks to use the family and community as a support base for health promotion. Key articles are Mullan and Kalter (1988) and Tolman (1991). The Institute of Medicine (1984) volume describes an early COPC trial at Tohono O’odham. For our own introduction to COPC at Tohono O’odham, see Kukulka et al. (1994).

<sup>5</sup>Robert Hackenberg has an adjunct appointment as Research Scientist, Bureau of Applied Research in Anthropology (BARA), at the University of Arizona. Since 1996 he has been involved with Borderlands studies. See Hackenberg (1997b).

<sup>6</sup>The BECC staff is made up of engineers and architects. Their professional competence is in the area of design, construction, and budgeting. The need to include socioeconomic survey data in proposal preparation remains unrecognized even though the prospects for cost recovery from intended beneficiaries depends upon it. BECC officials maintain that “official statistics” are sufficient for their needs.

The 1990 Mexican census gives the Nogales population as 107,936. Informed sources put the figure for that year at 200,000 or higher. Ingram, Laney and Gillilan (1995:111) observe:

Most people believe that census data are used to support particular political positions.... Understating population allows politicians also to understate the magnitude of existing problems and unmet needs.

As in many developing countries, however, showing disbelief in self-serving government “data” is deeply resented.

<sup>7</sup>Baba (1994) and Partridge (1985) have discussed generating theory from “practice,” i.e., from practitioner activities. But the comparison and analysis in Hill and Baba (1997:14–15) is particularly illuminating.

<sup>8</sup>The Pangue Hydro Project marks the effective entry of SfAA into the world of policy politics. Ted Downing, through his opposition to the original reservoir design which would have displaced the Pehuenche occupants without effective resettlement, brought the situation into public view (see *Practicing Anthropology* 19(4):35–36, Fall 1997). It has since passed into the hands of the SfAA’s International Standards Committee (Ted Downing, Chair) with parallel support from the AAA’s Commission on Human Rights, the AAAS, and 32 other scientific and professional organizations.

The World Bank has ordered an inquiry into the International Finance Corporation’s performance under the Pangue Hydro loan agreement. Furthermore, the ISC has been invited to review a range of bank social and environmental policy statements now under revision. For a full report on this engagement with the policy process, see Winthrop (1998), Young (SfAA Newsletter, Vol. 8, no. 3, August, 1998), and Johnston and Turner (1998).

SfAA now sponsors a Development Policy website, called the Kiosk ([www.policykiosk.com](http://www.policykiosk.com)) which invites comment on policy initiatives including matters mentioned above.

<sup>9</sup>Representatives of leading anthropological associations, working through the International Union of Anthropological and Ethnological Sciences (IUAES), have established a Commission on Anthropological Policy and Practice. Under IUAES sponsorship, Hill and Baba (1997) collected accounts of applied activity in 17 countries spanning the world. Baba (1998) reports on “creating a global community of practising anthropologists.”

Notes 8 and 9 seem to warrant a parting comment, optimistically offered: “We’ve come a long way, baby!”

### References Cited

- Alvarez, Robert R.  
1998 *La Maroma*, or Chile, Credit and Chance: An Ethnographic Case of Global Finance and Middlemen Entrepreneurs. *Human Organization* 57:63–73.
- Alvarez, Robert R., and George A. Collier  
1994 The Long Haul in Mexican Trucking: Traversing the Borderlands of the North and South. *American Ethnologist* 21:606–627.
- Anderson, Benedict R.  
1983 *Imagined Communities*. London: Verso.
- Appadurai, Arjun  
1991 Global Ethnoscapes: Notes and Queries for a Transnational Anthropology. In *Recapturing Anthropology*. R. Fox, ed. Pp. 191–210. Santa Fe: SAR Press.
- Austin, Diane E.  
1998 Cultural Knowledge and Cognitive Map. *Practicing Anthropology* 20(3):21–24.

- Baba, Marietta  
 1994 The Fifth Subdiscipline: Anthropological Practice and the Future of Anthropology. *Human Organization* 53:174–185.  
 1998 Creating a Global Community of Practicing Anthropologists. *Human Organization* 57:315–318.
- Barber, Bernard  
 1996 *Jihad vs. McWorld: How Globalism and Tribalism are Reshaping the World*. New York: Waveland.
- Bohannon, Paul  
 1980 You Can't Do Nothing. *American Anthropologist* 82:508–524.
- Brown, Franklin L. and Helen M. Ingram  
 1987 *Water and Poverty in the Southwest*. Tucson: University of Arizona Press.
- Cassel, John  
 1955 A Comprehensive Health Program among South African Zulus. *In Health, Culture and Community*. B. D. Paul, ed. Pp. 15–42. New York: Russell Sage Press.
- Cassel, John, Ralph Patrick, and David Jenkins  
 1960 Epidemiological Analysis of the Health Implications of Culture Change. *Annals of the New York Academy of Science* 84:938–949.
- Castells, Manuel  
 1983 *The City and the Grassroots*. Berkeley: University of California Press.  
 1996 The Net and the Self: Working Notes for a Critical Theory of Informational Society. *Critique of Anthropology* 16(1):9–38.
- Castells, Manuel and Alejandro Portes  
 1989 World Underneath: The Origins, Dynamics and Effects of the Informal Economy. *In The Informal Economy*. A. Portes, M. Castells and L. Benton, eds. Pp. 11–40. Baltimore: Johns Hopkins University Press.
- Chambers, Erve  
 1989 *Applied Anthropology: A Practical Guide*. Prospect Heights, Ill.: Waveland Press.
- Corwin, Norman  
 1945 *On a Note of Triumph*. New York: Simon and Schuster.
- Dougherty and Holthouse  
 1998 The New World Border: The Maquiladora Industry Has Transformed Nogales, Sonora. *Phoenix New Times*, July 9–15.
- Finan, Timothy J.  
 1996 Anthropological Research Methods in a Changing World. *In Transforming Societies, Transforming Anthropology*. E. Moran, ed. Pp. 301–324. Ann Arbor: University of Michigan Press.
- Firth, Raymond  
 1981 Engagement and Detachment: Reflections on Applying Social Anthropology to Social Affairs. *Human Organization* 40:193–201. (Republished at <<http://www.sfaa.net/>>.)
- Fisher, Julie  
 1996 Grassroots Organizations and Grassroots Support Organizations: Patterns of Interaction. *In Transforming Societies, Transforming Anthropology*. E. Moran, ed. Pp. 57–102. Ann Arbor: University of Michigan Press.

- Foster, George  
1994 Tzintzuntzan: Mexican Peasants in a Changing World. Prospect Heights, Ill.: Waveland.
- Freire, Paolo  
1970 Pedagogy of the Oppressed. New York: Herder and Herder.
- Friedland, Roger, and Deirdre Boden  
1994 Now Here: Space, Time and Modernity. Berkeley: University of California Press.
- Garcia Canclini, Nestor  
1995 Mexico: Cultural Globalization in a Disintegrating City. *American Ethnologist* 22:743–755.
- Giddens, Anthony  
1990 The Consequences of Modernity. Stanford, Calif.: Stanford University Press.
- Greenberg, James  
1997 A Political Ecology of Structural Adjustment Policies: The Case of the Dominican Republic. *Culture & Agriculture* 19:85–93.
- Hackenberg, Robert A.  
1967 Parameters of an Ethnic Group: A Methodology for Studying the Total Tribe. *American Anthropologist* 69:478–492.  
1970 The Social Observatory: Time Series Data for Health and Behavioral Research. *Social Science and Medicine* 4:343–357.  
1995a Setting Up Shop in the Global Market Place: Applied Anthropologists Amidst Immigrants, Insurgents and Corporate Raiders. *High Plains Journal of Applied Anthropology* 15:99–110.  
1995b Joe Hill Died for Your Sins: Empowering Minority Workers in the New Industrial Labor Force. *In* Any Way You Cut It: Meat Processing and Small-Town America. D. D. Stull, M. J. Broadway and D. Griffith, eds. Pp. 231–264. Lawrence: University Press of Kansas.  
1997a Rural Development in the Global Village. *Culture and Agriculture* 19:71–78.  
1997b The U.S.-Mexican Borderlands in Century XXI: Hispanics and Indians Move from Local Ecology into the Global Economy. *Culture & Agriculture* 19:94–101.
- Hackenberg, Robert A., and Beverly H. Hackenberg  
1987 Saving the City: University Research, Political Action and the Squatter Problem in Davao City, Philippines. *In* Collaborative Research and Social Change. D. Stull and J. Schensul, eds. Pp. 179–210. Boulder: Westview Press.
- Hannerz, Ulf  
1990 Cosmopolitans and Locals in World Culture. *Theory, Culture and Society* 7:237–251.  
1992 Cultural Complexity: Studies in the Social Organization of Meaning. New York: Columbia University Press.  
1994 The Nation in the Global Village. *Cultural Studies* 1994:198–209.
- Harvey, David  
1989 The Condition of Postmodernity. Oxford: Basil Blackwell.
- Hill, Carole, and Marietta Baba  
1997 The International Practice of Anthropology: An Overview. *In* The Global Practice of Anthropology. M. Baba and C. Hill, eds. Pp. 1–24. Williamsburg: College of William and Mary.
- Hine, Virginia  
1977 The Basic Paradigm of a Future Sociocultural System. *World Issues* 2(2):19–22.

- Ingram, Helen, Nancy Laney, and David Gillilan  
1995 *Divided Waters: Bridging the U.S.-Mexico Border*. Tucson: University of Arizona Press.
- Institute of Medicine  
1984 *Community Oriented Primary Care: A Practical Assessment*. Washington: National Academy Press.
- Johnston, Barbara, and Terence Turner  
1998 Censorship, Denial of Informed Participation and Human Rights Abuses Associated with Dam Development in Chile. *Professional Ethics Report* 11(2):6–8. American Association for the Advancement of Science.
- Justice, James  
1994 The History of Diabetes in the Desert People. *In Diabetes as a Disease of Civilization: The Impact of Culture Change on Indigenous Peoples*. J. Joe and R. Young, eds. Pp. 69–128. New York: Mouton de Gruyter.
- Kaplan, Robert D.  
1998 Travels into America's Future. *Atlantic Monthly* 282(1):47–69.
- Kearney, Michael  
1995 The Local and the Global: the Anthropology of Globalization and Transnationalism. *Annual Review of Anthropology* 24:547–565.
- Kopinak, Kathryn  
1996 *Desert Capitalism: Maquiladoras in North America's Western Industrial Corridor*. Tucson: University of Arizona Press.
- Kottak, Conrad  
1997 The New Ecological Anthropology: A Programmatic Statement. Paper read at the Annual Meeting of the Society for Applied Anthropology, Seattle.
- Kottak, Conrad, and Elizabeth Colson  
1994 Multilevel Linkages: Longitudinal and Comparative Studies. *In Assessing Cultural Anthropology*. R. Borofsky, ed. Pp. 396–412. New York: McGraw-Hill.
- Kukulka, Gary, Jon Christianson, Ira Moscovice, and Robert DeVries  
1994 Community Oriented Primary Care: Implementation of a National Rural Demonstration. *Archives of Family Medicine* 3:495–501.
- Laquian, Aprodicio  
1975 Whither Site and Services? *Science* 192:950–955.  
1983 *Basic Housing: Policies for Urban Sites, Services, and Shelter in Developing Countries*. Ottawa: International Development Research Centre.
- Lenin, V. I.  
1901 *What Is To Be Done?* New York: Monthly Review Press.
- Mangin, William  
1970 *Peasants in the City: Readings in the Anthropology of Urbanization*. New York: Houghton Mifflin.
- Marcus, George  
1995 Ethnography in/of the World System: The Emergence of a Multi-Sited Ethnography. *Annual Review of Anthropology* 24:95–117.

- Massey, Douglas  
1987 Understanding Mexican Migration to the U.S. *American Journal of Sociology* 92:1372–1403.
- McGuire, Thomas R., and Gloria Ciria Valdez-Guardia  
1997 Endangered Species and Precarious Lives in the Upper Gulf of California. *Culture & Agriculture* 19:101–107.
- Merton, Robert  
1968 On Sociological Theories of the Middle Range. *In* *Social Theory and Social Structure*. Pp. 39–72. New York: Free Press.
- Moyers, Bill  
1986 One River, One Nation. CBS Reports.
- Mullan, Fitzhugh and Henry Kalter  
1988 Population-Based and Community-Oriented Approaches to Preventive Health Care. *American Journal of Preventive Medicine, Supplement: Approaches to Preventive Health Care* 141–157.
- Nagengast, Carole, and Terence Turner  
1997 Universal Human Rights Versus Cultural Relativity. *Journal of Anthropological Research* 53:269–272.
- Partridge, William  
1985 Toward a Theory of Practice. *American Behavioral Scientist* 29:139–163.
- Peacock, James  
1997 The Future of Anthropology. *American Anthropologist* 99:9–29.
- Perez, Carlos  
1997 Participatory Research: Implications for Applied Anthropology. *Practicing Anthropology* 19(3):2–7.
- Rappaport, Roy  
1971 The Sacred in Human Evolution. *Annual Review of Ecology and Systematics* 2:23–44.  
1993 The Anthropology of Trouble. *American Anthropologist* 95:295–304.
- Roberts, Bryan  
1978 *Cities of Peasants: The Political Economy of Urbanization in the Third World*. Beverly Hills: SAGE Publications.
- Spaulding, Mark, and John Audley  
1997 Promising Potential for the U.S.-Mexico Border and for the Future: An Assessment of BECC/NADBank Institutions. Washington: National Wildlife Federation.
- Spicer, Edward H.  
1957 *Human Problems in Technological Change*. Ithaca, N.Y.: Cornell University Press.
- Stanford, Lois  
1994 Transitions to Free Trade: Local Impacts of Changes in Mexican Agrarian Policy. *Human Organization* 53:99–109.
- Steward, Julian  
1955 Levels of Sociocultural Integration: An Operational Concept. *In* *Theory of Culture Change*. Pp. 43–63. Urbana: University of Illinois Press.

- Tolan, Sandy  
1990 Hope and Heartbreak: The Border Boom. *New York Times Magazine*, July 1.
- Tollman, Stephen  
1991 Community Oriented Primary Care: Origins, Evolution, Applications. *Social Science and Medicine* 32:633–642.
- Turner, John  
1967 Barriers and Channels for Housing Development in Modernizing Countries. *Journal of the American Institute of Planners* 33(3). Reprinted in William Mangin, 1970 *op. cit.*, pp. 1–19.
- Vayda, Andrew P.  
1983 Progressive Contextualization: Methods for Research in Human Ecology. *Human Ecology* 11:265–281.
- Weaver, Thomas  
1985 Anthropology as a Policy Science. *Human Organization* 44:97–105; 44:197–205.  
1996 Mapping the Policy Terrain: Political Economy, Policy Environment, and Forestry Production in Northern Mexico. *Journal of Political Ecology* 3:37–67.
- Winthrop, Robert  
1998 The Pangu Hydro Project Revisited. *Practicing Anthropology* 20(3):33–34.
- Wolfe, Alvin W.  
1977 The Supranational Organization of Production: An Evolutionary Perspective. *Current Anthropology* 18:615–636.
- Yeh, Stephen and Aprodicio Laquian  
1979 Housing Asia's Millions: Problems, Policies and Prospects for Low-Cost Housing in Southeast Asia. Ottawa: International Development Research Centre.
- Yetman, David  
1993 Devastated Lives—For What? Guest Comment. *Arizona Daily Star*, August 8.
- Yeung, Y. M.  
1983 A Place to Live: More Effective Low-Cost Housing in Asia. Ottawa: International Development Research Centre.
- Zedeno, M. Nieves, Diane Austin, and Richard Stoffle  
1997 Landmark and Landscape: A Contextual Approach to the Management of American Indian Resources. *Culture & Agriculture* 19:123–129.



**Chapter 29**  
**Thayer Scudder:**  
**The Environment, Social Problems, and Development**  
**Anthropology**  
**Thomas Weaver**

Thayer Scudder, recipient of the Society for Applied Anthropology's Malinowski Award in 1999, has spent a lifetime developing expertise in long-term community studies and development anthropology.<sup>1</sup> He is an authority on the socioeconomic issues involved in river-basin development and on assessing sustainable resources in Africa and Asia. His major practical contributions have been to solving resettlement and environmental issues of people in China, Laos, Lesotho, and Swaziland. Anthropologists associate his name with Elizabeth Colson, also a Malinowski Award recipient (Colson 1985), and their long-term study of the Gwembe Tonga of Africa, including their contribution to the World Bank's resettlement policy (Scudder and Colson 1982).

Thayer Scudder (1930–) was educated at Harvard University with a bachelor's degree in 1952 (biology and anthropology) and a doctoral degree in 1960 (anthropology). As a second year graduate student he joined Elizabeth Colson in studying the impact of resettlement on people displaced by a dam constructed on the Zambezi River in Africa. Other colleagues have **since** joined them in what has become a long-term study site. This experience also led to similar studies and consultancies in other parts of the world. In 1964 Scudder joined the California Institute of Technology as their first tenured anthropologist where he was Professor of Anthropology at the time of the Malinowski Award presentation. He continued work with the Institute for Development Anthropology in Binghamton, New York, an organization of which he was a Founding Director.

Scudder's Malinowski Award address, entitled "The Emerging Global Crisis and Development Anthropology: Can We Have an Impact?" (1999), answered the question posed in the title affirmatively. There is a global crisis, he maintained, and development anthropology can help. He contrasted his analysis of the topic with Knight Kiplinger's optimistic outlook on global development (1998). Kiplinger argued that globalization and development are good for the world and that problems that arise can be solved by our demonstrated ingenuity. To assess the prevalence of his own less optimistic view of development, Scudder surveyed what he calls "The 53," respondent anthropologists with development-related experiences in different parts of the world. Scudder reported, in the context of his own perspective, the social and environmental issues the respondents most often identified and suggested a number of ways anthropology can address these threatening problems.

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The major environmental concerns identified by “The 53” include, in descending order of frequency, misuse of natural resources (linked to increasing social conflict and poverty), water scarcity, and pollution. Poverty and globalization were among the most important social issues identified. Lower on the scale of concern to the majority of respondents were community unraveling, population pressure, marginalization, and fundamentalism. A surprising issue not given sufficient attention by the group is urbanization. In recent years many have come to recognize urban sprawl as an important factor that leads to environmental degradation.

Scudder called for the creation of new paradigms that involve anthropological experience and knowledge to help correct these environmental and social issues. First, he suggested that anthropologists need to better identify the key problems concerning which they have expertise and knowledge. Second, training in applied and development anthropology must be included in anthropology curricula. Third, anthropologists must bring their expertise to the attention of the public and policy makers more actively. Fourth, anthropologists should rethink their research agendas to better address major global problems, and more emphasis should be placed on long-term studies of how people respond to impacts associated with those problems. Finally, anthropologists must create links with ecologists and their professional societies in both research and educational endeavors.

The list of items identified by Scudder and his respondents as environmental and social issues are symptoms of underlying deeper causative agents. The uncontrolled spread of global capitalism and concomitant problems such as the unequal distribution of income, displaced populations, and urbanization are the real causes of the social and environmental issues described. This is obscured by informational wars by governmental and business agents confronting the findings of science and anthropology through misinformation and persistent advertising. The control of information networks and political systems by business are important elements in this mix.

In February of 1998 Scudder was named to the World Commission on Dams, an independent body organized through the initiative of the World Bank and the World Conservation Union. The goals of the commission are to formulate new guidelines for dam construction based on a comparison of their social and economic benefits with their adverse environmental, social, and economic impacts. As professor emeritus at the Californian Institute of Technology, Scudder continues to serve in an advisory capacity on panels of environment and resettlement experts for projects in Lesotho, China, Laos, and Swaziland (Scudder 1997).

### Notes

<sup>1</sup>This annual award is given to a senior colleague in recognition of efforts to understand and serve the needs of the world through social science. See Thomas Weaver’s “The Malinowski Award and the History of Applied Anthropology” (2002b) for a brief history of this award and an overview of the recipients and their work, and Weaver’s “Malinowski as Applied Anthropologist” (2002a) for an introduction to Bronislaw Malinowski and his work.

### References Cited

- Colson, Elizabeth  
 1985 Using Anthropology in a World on the Move. *Human Organization* 44(3):191–196. (Republished at <<http://www.sfaa.net/>>.)

Kiplinger, Knight

- 1998     World Boom Ahead: Why Business and Consumers Will Prosper. Washington, D.C.: Kiplinger Books.

Scudder, Thayer

- 1993     Development Induced Relocation and Refugee Studies: 37 Years of Change and Continuity among Zambia's Gwembe Tonga. *Journal of Refugee Studies* 6(2): 123–152.
- 1997     Resettlement. *In* Water Resources: Environmental Planning, Management, and Development. A. K. Biswas, Editor. Pp. 667–710. New York: McGraw-Hill.
- 1999     The Emerging Global Crisis and Development Anthropology: Can We Have an Impact? *Human Organization* 58(4):351–364. (Republished at <<http://www.sfaa.net/>>.)

Scudder, Thayer and Elizabeth Colson

- 1982     From Welfare to Development: A Conceptual Framework for the Analysis of Dislocated People. *In* Involuntary Migration and Resettlement: The Problems and Responses of Dislocated People. A. Hansen and A. Oliver-Smith, eds. Pp. 267–287. Boulder, CO: Westview Press.

Weaver, Thomas

- 2002a    Malinowski as Applied Anthropologist. *In* The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers. T. Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.
- 2002b    The Malinowski Award and the History of Applied Anthropology. *In* The Dynamics of Applied Anthropology in the Twentieth Century: The Malinowski Award Papers. Thomas Weaver, ed. <<http://www.sfaa.net/>>. Oklahoma City: Society for Applied Anthropology.

## **The Emerging Global Crisis and Development Anthropology: Can We Have an Impact?**

**Thayer Scudder**

Before I plunge into some very serious issues, I would like briefly to introduce myself to those of you who do not know me—who I expect are in the large majority. Because I started my career in the natural sciences, I wanted to continue enjoying the company of scientists and engineers after I moved into sociocultural anthropology. So I ended up as the only anthropologist on the faculty of the California Institute of Technology, where I have been based as a “lone wolf” since 1964.

While a graduate student at Harvard in the 1950s, I also took advantage of Boston University’s newly established African Studies Program. There I met Elizabeth Colson, who has had more influence on me as an anthropologist than anyone else. Though I was only a second-year graduate student, who had yet to finish his course work, she invited me to join her as a colleague during 1956–57 in a benchmark study of 57,000 people soon to be involuntarily relocated because of the construction of the first mainstream dam on the Zambezi River in what was then British Central Africa. Our funding also financed a postresettlement study in 1962–63, after which we decided to continue our research among the Gwembe Tonga in what has become one of the most systematic long-term studies in anthropology. Our close collaboration continues today, and I want to thank Elizabeth for inviting me to join her 43 years ago.

As for the Gwembe Study, not only does that continue, but four younger colleagues have joined us to carry it forward into the next century. They are Samuel Clark, currently writing his Gwembe dissertation in the University of Pennsylvania’s Demography Program; Lisa Cliggett, currently assistant professor of anthropology at the University of Kentucky; Rhonda Gillett-Netting, assistant professor of anthropology at the University of Arizona; and Bennett Siamwiza, a Gwembe Tonga whose parents were involuntarily resettled in 1958. With a Ph.D. in social history from the University of Cambridge, Bennett is currently a lecturer at the University of Zambia.

Another Boston University professor in the 1950s who has had a greater influence on my career than he probably realizes was Philip Gulliver. Unable to join a World Bank mission to Africa in 1962 as the bank’s first consulting anthropologist, Phil recommended me instead to participate in a two-year study. That study produced in 1967 John C. de Wilde’s two-volume *Experiences with Agricultural Development in Tropical Africa*, which was the first comprehensive review of smallholder agriculture.

My Gwembe Tonga research led to an invitation to carry out a similar benchmark study in 1961–62 of Egyptian Nubians soon to be relocated in connection with the construction of the Aswan High Dam. That Nubian project was directed by Robert Fernea, now at the University of Texas. The Zambezi and Nile research then led to a large number of annual consultancies dealing

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with involuntary resettlement, agricultural settlement schemes, and river-basin development throughout the world. To give you an example, between January and September 1999, I have been in Laos and Lesotho as a member of advisory panels on environment and social issues dealing with large-dam construction, in the Czech Republic and Brazil at meetings of the World Commission on Dams, and in Sri Lanka and Turkey as one of a three-member team evaluating the International Water Management Institute.

To close these introductory comments, I also wish to thank Cora Dubois. Meeting with me weekly while I was preparing for my oral examinations, she had the greatest influence on me while I was a graduate student at Harvard. And I want to thank David Brokensha and Michael Horowitz, with whom I joined in 1976 to found the Institute for Development Anthropology. David is now retired in South Africa, but Michael continues to effectively direct the institute, which is based in Binghamton, New York. As for anthropology as a field, I have profited immensely by being an academic undertaking fundamental long-term research, the practical implications of which I have had the opportunity to apply on different continents. Today I have been greatly honored, as a recipient of the Malinowski Award, for having the opportunity to share with you some of what I have learned as researcher and practitioner.

In her presidential address at the 1997 Annual Meeting of the American Association for the Advancement of Science, Jane Lubchenco (1998:491) noted “that urgent and unprecedented environmental and social changes challenge scientists to define a new social contract.” Such a contract she argued would require “a commitment on the part of all scientists to devote their energies and talents to the most pressing problems of the day.” As a biologist, her immediate concern related to “a more sustainable biosphere.” However, in defining that biosphere she emphasized not just its ecological soundness, but also it being “economically feasible, and socially just.” Her concern with social justice grew out of her awareness that “inequity within and among all nations has increased” (p. 492). That emphasis links her concerns to the expertise of anthropologists and especially to that of development anthropologists who, I maintain, know more about the causes and impacts of poverty, and how people respond to those causes and impacts, than other scientists and practitioners.

The theme of our 1999 meeting is “Constructing Common Ground: Human and Environmental Imperatives.” In his 1995 Malinowski lecture, Michael Cernea (1995:344) noted that environmentalists constituted a lobby “infinitely more vocal than the social science community” in regard to sociocultural and environmental issues. I would hope, indeed urge, that one result of this meeting would be for us, as a society and as individuals, to join hands with ecologists like Lubchenco to initiate a research and public-information program for better understanding, publicizing, and addressing what some of us believe has become a global crisis.

### **Is There A Global Crisis?**

But is there a global environmental and social crisis? While I am convinced that there is, many argue that today’s problems are no more serious than in times past and our capacity for solving them has never been better. That viewpoint is well presented by Knight Kiplinger in *World Boom Ahead: Why Business and Consumers Will Prosper* (1998). It makes fascinating reading, and I recommend it to you. Knight Kiplinger is the chief executive officer of Kiplinger Books, which publishes for business and other clients the influential weekly, *Kiplinger Washington Letter*.

Dealing with the next century, *World Boom Ahead* reflects the most publicized world futures scenario. Other examples include *Business Week*'s 1998 *Special Double Issue on the 21st Century Economy* and scenario-building by the Global Business Network. As for *World Boom Ahead*'s forecast, it has been well received by reviewers. The president of the National Association of Manufacturers, for example, calls it "a real tour de force of the technological and global changes that are dramatically altering the way industry operates, the way we work, and many other aspects of our lives—all with an eye to the opportunities that lie ahead" (*World Boom Ahead* cover jacket), while former secretary of defense, William Perry, labels it "a convincing, optimistic forecast for a broad, worldwide economic boom over the next two decades" (*World Boom Ahead* cover jacket).

I label these authors "optimistic optimists" in contrast to my being an "optimistic pessimist." As an optimistic pessimist, I believe that, in general, environmental and social trends continue downward to the extent that if we are not already in a crisis situation, we soon will be. But I remain hopeful that the possibility remains that we, as a species, still have the potential to create a global society that not only approximates the vision in *World Boom Ahead* of higher living standards for all societies but that is also sustainable environmentally, economically, institutionally, and culturally.

I have two goals for this talk. The first is to contrast Kiplinger's optimistic view of the future with the more pessimistic one that I and, I believe, many development anthropologists share. That done, I would like to outline what I believe must be done by members of our profession, and more specifically by the Society for Applied Anthropology, to help turn the situation around. I would have enjoyed spending the next 45 minutes telling you why I view current global trends so negatively and what to do to improve the situation. However, that would have been a mistake, for it would have been appropriate for some of you to point out two possibly serious biases that influence my negative perceptions. One is that my most detailed research has been in two areas of the world—Africa and South Asia—that continue to be characterized by serious levels of environmental degradation and conflict as well as poverty for a majority of citizens. The second is that much of my research and consulting have dealt with development-induced involuntary resettlement, which, with few exceptions, has further impoverished already poor people.

So to offset possible biases of my own, in preparation for this lecture, I sent out a brief questionnaire to 89 anthropologists with development-related experience in various parts of the world. I divided them into seven categories. Six dealt with major regions of the world—specifically North America, Latin America, Europe, the Middle East, Africa, and Asia. Getting names from colleagues, I put together a list of at least 10 people born or naturalized as citizens in each of those regions. The seventh category was for people from different nationalities working for international development agencies. Primarily they worked for the World Bank.

The one-page questionnaire included three queries. The first asked colleagues to list those three environmental issues they believed posed the most serious constraints to a sustainable future during the next century. The second question asked them to list three social issues. The third question asked colleagues to list those three opportunities they believed need be taken advantage of if humankind is to enhance living standards in a sustainable fashion during the next century.

I sent out the 89 questionnaires last fall. At 60 percent, returns were far more numerous than I had hoped for. They were also more interesting. To sum up, I received 13 responses from

North America, 9 from Africa, 8 from international development banks, 8 from Asia, 7 from Europe, and 4 each from Latin America and the Middle East. Of the 53 returns, 17 were from women. Some respondents answered the questions in sufficient detail that I have been able to incorporate various comments in this talk. Others listed issues in a few words so as to keep their replies to the one page of the questionnaire. Since all 53 agreed that their names could be used, I have listed them at the end of my paper so those of you who are interested can decide whether they constitute a reasonable opportunity sample.

Having studied all the responses, I have concluded that my views are reflected by the majority of those contacted and, therefore, probably would be supported by a majority of development anthropologists. That said, I cannot deduce from their answers that even a majority would agree with me that we are already in, or moving toward, a global crisis, though a few go beyond me in their belief either that it is already too late to turn things around, or that current downward trends are irreversible. On the other hand, the key issues that all identify, and which obviously concern them, are very similar to mine. So I feel comfortable in contrasting my views and those of my 53 colleagues (hereafter referred to as “the 53”) to those of the author of *World Boom Ahead*.

### **The Future According to Knight Kiplinger**

According to *World Boom Ahead*, “On the eve of the 21st century, the world stands on the threshold of a long, strong surge in economic growth and living standards, unprecedented in world history. Over the next few decades, this boom will bring several billion more people—now toiling in marginal local economies—into a fully integrated world marketplace” (p. 1). “The primary beneficiaries . . . will be today’s least affluent nations—in Asia, Latin America and Africa. But not all of them . . . only those whose leaders choose to create and maintain economic systems hospitable to both home-grown entrepreneurship and foreign investment” (p. 2). “The bywords of the 21st century will be the three Ds: *dispersal* (of technology and production), *decentralization* (of leadership in world affairs and business) and *democratization* (of information, political authority and ownership of capital)” (p. 1).

“This global expansion will be driven by technology” (p. 1). “The dominant technologies of the coming century will be micro-electronics, biotechnology, telecommunications, and information technology” (p. 4). “Over the next decades, today’s two most important technologies—telecommunications and biotechnology—will have a greater effect on more lives, in more countries, in a shorter period of time, than any previous technological impact” (p. 215). “The biological sciences will be the brightest stars” (p. 4). Among other advances, biology will produce “genetically engineered grains and produce, resistant to drought, cold, disease and pests, requiring less water and fertilizer” (p. 12), hence “greatly expanding the land area where grains and vegetables can be profitably grown” (p. 255).

As for more pessimistic scenarios where development is reversed because of population increase and environmental degradation, famine, unregulated flows of capital, and a “very unstable geopolitical climate” (p. 315), caused by “raging nationalism, ethnic and racial conflict,” Knight Kiplinger concludes that “none of these gloomy scenarios is as plausible as our vision of a steadily improving human condition.” (p. 15).

That vision is cogently argued in the 380-page text of *World Boom Ahead*. I was able to find few factual errors, one being a repetition of the myth that Africa is characterized by “rich soils” (p. 103). A more serious weakness was the author’s tendency to downplay legitimate

concerns. His statement that “anxiety about dwindling resources . . . has proven to be unfounded” (p. 37), is, for example, at odds with the international community’s increasing concern about water quality and quantity.

Aside from not foreseeing rising unemployment as a problem, he also pays inadequate attention to employment generation for the world’s low-income majority, in spite of noting that many Latin American countries “can’t create enough jobs for their fast-growing populations” (p. 98). And of major concern to anthropologists, he appears unconcerned about the increasing gap between rich and poor even though he states that: “In the short run—possibly in the next decade—rapid technological advances in the developed nations, coupled with the inability of the poorer nations to afford them, will broaden the existing chasm between rich and poor, educated and uneducated, both within each nation and among nations” (p. 216), while in the United States “the poverty rate . . . will remain in the present range for years to come. And income equality will continue to widen” (p. 145).

Knight Kiplinger’s statement that “We cannot help but be impressed by humankind’s long record of adapting to changing circumstances, finding solutions to problems and inventing devices to make life easier” (p. 17) goes a long way in explaining his optimism; indeed, an almost religious faith in the ability of humankind to prevail against all obstacles. Hence:

1. “As megacities reach virtually unmanageable size, governments will take aggressive steps, in cooperation with private business interests, to disperse populations by locating new manufacturing facilities in less-congested regions” (p. 169).
2. “When environmental degradation and congestion get bad enough, every nation does what it must to make life more comfortable for its citizens.” This has happened in the past in the developed world, “and it will happen in the Third World” (p. 171).
3. “For the past several decades, many of the indices of U.S. social health have declined amid rising prosperity” (p. 161). But “When a situation reaches crisis levels, a public outcry mounts, corrective actions begin, and—a few years later—changes begin to appear” (p. 162).

Throughout he appears to be unaware of the frequency with which preindustrial societies time and again have brought about their own demise after degrading their habitat.

Nonetheless, Knight Kiplinger’s analysis is the most thought through and consistent technology- and business-based scenario for the future of which I am aware. It is a feasible scenario if major constraints, such as those identified by my 53 colleagues, are expeditiously addressed. Moreover, some of his supporting analysis is most interesting. In analyzing opportunities for the future that the 53 wrote, I was surprised by the lack of emphasis on further releasing the potential of women. Not so Knight Kiplinger. In his section titled “Worldwide Empowerment of Women,” he emphasizes that “As they enter the market economy of manufacturing and skilled services, women will be an enormous force in the worldwide rise in living standards” (pp. 39-40). I agree, having told my students in past classes that women-headed businesses in the United States were not only creating record levels of employment but would continue to be a major reason why the United States would continue leading Asia and other countries that are more discriminatory.



Some of Knight Kiplinger's other conclusions are equally interesting. He sees immigration of all income levels into the United States as a continued benefit to the U.S. economy and civil society. Indeed, "without a high level of immigration, the economies of California, Texas, New York City and many other locales would have stagnated years ago. And if immigrants all left tomorrow, those economies would grind to a halt" (p. 184).

Putting their profits from transnational corporations to work, he also believes that "The new generation of global philanthropists—modern day Carnegies, Rockefellers and Fords—will make a deep impact on education, medical research, public health, the arts and humanities, and population control" (p. 4).

And of relevance to academics: "Tenure for professors—the only workers with guaranteed life-time employment in any American enterprise—will come under increasing attack as an expensive and ossified system that stymies the freshening effects of performance accountability and occasional turnover" (p. 143).

### **The Future According to "The 53"**

I would like now to contrast the views presented in *World Boom Ahead* with those of the 53 who responded to my questionnaire. I will also elaborate on the various points made based on my own experience.

#### ***Environmental Concerns***

Before concentrating on social issues, I wish briefly to discuss the main environmental issue among the 18 different categories into which I integrated responses. Misuse of natural resources was emphasized by 74 percent of those who answered the question. Some linked their concern to increasing conflict; others to poverty. According to Cyprian Fisiy (Cameroon and World Bank), "In my work in Sub-Saharan Africa the evidence suggests that as natural resources are diminishing, there has been a strong re-emergence of confrontational ethnicity." Gordon Appleby (United States and World Bank) notes that "As economic conditions deteriorate, more and more people resort to common resources to survive, destroying those resources and causing conflict among themselves."

Actually the percentage would have been higher than 74 percent if several colleagues had not taken me to task for splitting off environmental from social issues, granted the close linkage between the activities of *Homo sapiens*, as the ecologically dominant species, and a wide range of issues with serious environmental implications. Emanuel Marx (Israel), for example, wrote that environmental issues need to be formulated under social issues; "i.e., the fact that they are man-made problems and not natural ones should be borne out." I agree with that critique but made the separation primarily because the theme of this meeting distinguishes between human and environmental imperatives.

In addition to a general statement of concern, under the natural-resources rubric the greatest concern, involving over 50 percent of respondents, dealt with water scarcity and pollution. Hussein Fahim (Egypt) devoted all three of his environmental issues to water: "1) Reduced water supply (water tables are falling; underground aquifers are being depleted . . .); 2) Deteriorating water quality; and 3) Rousing water conflicts and aggression." Ayse Kudat (Turkey and World Bank) mentioned water in two of her three issues; namely, "scarcity of water" and "water pollution."

In recent years, evidence is increasing that there is, indeed, a developing water crisis, with water now generally recognized as our scarcest natural resource. Though a finite resource, global water use has more than tripled between 1950 and the mid-1990s, with Postel noting that already “per capita water supplies are a third lower than in 1970” (1997:23). Yet Serageldin (1995), a senior World Bank official whose positions include having been vice president for environmentally sustainable development, forecasts a 650 percent increase in water demand over the next 30 years due to population growth and increased demand for water in agricultural, industrial, and domestic use.

Despite increasing demand, in areas as diverse as Southern Africa and Western Turkey, river flows, as in the Zambezi and Gediz Rivers, have been cut in half during recent years. Natural climatic cycles are the most likely explanation, which may or may not be influenced by global warming. The Yellow River, which nurtured civilization over thousands of years in North China, now periodically no longer flows into the South China Sea because of drought and poor land and water management upriver. Even more serious, since no known methods currently exist for large-scale recharge, is the rapid drop in water tables in India and Pakistan, where more land is irrigated with ground water than with surface water. Serious drops in water tables also characterize large areas of the Western United States as well as Northern China. Pollution of remaining supplies is an ongoing problem: “All of India’s 14 major rivers are badly polluted,” for example, while “over three-quarters of China’s 50,000 kilometers of major rivers are so filled with pollution and sediment that they no longer support fish life” (Johns Hopkins University 1998:2).

In addition to the risk of water wars in the Middle East, North Africa, Southern Africa, and elsewhere, researchers at the International Water Management Institute have recently concluded that “water has become the single most important constraint to increased food production” (Seckler et al. 1998:16). Yet, “None of the global food projection models such as those of the World Bank, FAO and IFPRI have explicitly incorporated water as a constraint” (Seckler et al. 1998:17).

Occupying approximately 16 percent of cultivated land, irrigation currently provides over one-third of the world’s food production. In China, which with India has 40 percent of the world’s irrigated area, irrigation produces approximately 65 percent of that country’s food grains (Merrey 1997). During the past three decades, 50 percent of the world’s increase in food production has “come from higher yields on an expanding irrigated land area” (SWIM Annual Report 1997:Annex 1). And “in the coming 30 years approximately 80 percent of the additional required food supply will have to be produced on irrigated land” (Vermillion and Merrey 1998:166). Yet, since 1980, not only has the irrigated area per capita declined, while food grain production has stagnated, but the absolute amount of land under irrigation may have also declined due to salination and other causes (Seckler et al. 1998; Seckler 1996).

If current trends continue, the situation is grim, but not hopeless. According to the analysis of Seckler and his colleagues, approximately 50 percent of the increase in water demand for the year 2025 can be met by “increasing the effectiveness of irrigation” (1998:17), while Knight Kiplinger puts his faith in desalination of seawater. While both solutions are technically feasible, there is very little public awareness of the nature of the problem and little political will to deal with it.

Anthropologists can play an important role here in several ways other than helping to build public concern. One is to emphasize that water is a multifaceted resource with ecological functions in maintaining the biological and cultural diversity of habitats—not just an economic

resource for human use. In their edited volume on *Water, Culture and Power: Local Struggles in a Global Context*, Donahue and Johnston (1998) make a major contribution. They bring together 22 authors, 16 of whom are anthropologists, who present what is the most comprehensive attempt to describe the cultural context in which water is viewed by indigenous nations and other interest groups within modern states.

Though a distinguished anthropological literature deals with irrigation systems, anthropologists have also begun to study water use and competition within river basins, and to look at recent experiences by a few governments to involve all users, including small-scale farmers, in the management of subcatchments. Examples include Derman's current research in Zimbabwe and van Koppen's involvement in the International Water Management Institute's planned research in South Africa's Olifants River Basin.

### ***Social Issues***

Turning to social issues, I broke those listed by my respondents down into 20 categories, some of which had up to 7 sections. Though I found that categorization useful for analysis, it is important to emphasize the close relationship, and no doubt synergies, between several categories. The most important example is poverty (listed by 57 percent), globalization (listed by 49 percent), and increasing marginalization (listed by 21 percent), with 92 percent listing one or more of those three categories.

#### ***Poverty.***

Of the 20 categories, poverty was the most frequently mentioned. It is an issue that I believe anthropologists know more about than other scholars or practitioners. United Nations' and nongovernment organizations' (NGO) humanitarian workers may have more contact with poverty issues, but anthropologists have studied them more.

My colleagues approached poverty in two major ways. One was to simply mention "poverty" as a single word or to label the poor as the most vulnerable, especially during economic downturns and political strife, and in relationship to environmental degradation and pollution. Gordon Appleby (United States and World Bank) put it as follows: "Poor as the most vulnerable: Globalization and economic development are intended to uplift the poor, but they are in fact the most vulnerable during economic downturns. Yet when it happens, government cannot identify who the poor are—and fail to do so at their own risk since the fragility of economic development as we today understand it can lead to frustration, violence and the loss of trust." Claire Limbwambwa (Zambia) referred to "Poverty and high population densities: these push people to live on marginal lands thus exacerbating the land degradation."

The other was to relate poverty more to perceived causes, including especially the rising gap between rich and poor, due to increasingly inequitable access to consumptive and productive resources within and between countries. Siri Hettige (Sri Lanka) emphasized "the widening gap between the rich and the poor both internationally and nationally," while Hussein Fahim (Egypt) stressed "widening the gap between rich and poor, especially in many parts of the developing world." Robert Hitchcock (United States) emphasized "widespread and growing poverty and inequality in distribution of power and resources, including food, land and capital." Both James Murombedzi (Zimbabwe) and Owen Sichone (Zambia) linked poverty specifically to globalization. Murombedzi referred to "inequitable income and consumption patterns . . . obviously linked to globalization—especially the weakening of the nation state and local communities regarding transnational corporations . . . ; [and] South-North outflows of capital."

As for Sichone, he linked “Mass poverty, especially in Africa” to “failure of global market economy and related neo-liberal ideologies.”

According to Forouz Jowkar (Iran), “The most important social hindrance is the gross inequalities of wealth between haves and have nots. According to the new Human Development File by UNDP the top 225 richest people in the world own as much as the 2.5 billion poorest people . . . . So long as such inequalities remain and strengthen, there will never be . . . sustainability in the next century.”

After noting “abject poverty endemic in most ‘developing’ countries,” Michael Cernea (Romania/USA and former senior sociological adviser at the World Bank) stated “For the last three decades . . . poverty reduction has been defined as the principal objective of development. Yet the statistics of world poverty defy the effort: they have become grimmer and grimmer. Instead of shrinking, the global geography of poverty has swallowed vast new territories even in Europe (its eastern half) and in Asia. A few years ago 1 billion people lived on less than \$1 a day. Today, across the world, 1.3 billion live on less than \$1 a day. Three billion live on under \$2 a day . . . . In Indonesia alone, 17 million people have fallen BACK into poverty.”

### *Globalization.*

The next most frequently mentioned category was globalization itself, at 49 percent of the 53—this was the most glaring contrast between Knight Kiplinger’s *World Boom Ahead* and my colleagues. Leopoldo Bartolomé (Argentina) refers to “the consequences of an uncontrolled and globalized market for commodities and labor” as a major constraining issue. As his first issue, L. K. Mahapatra (India) states that “Iniquitous world trade makes the poor countries even poorer, in spite of the aid and loans (which do not offset the trade losses in unequal trade partnerships), [and] endangers the quality of life of the world population,” while Hari Mohan Mathur (also from India) refers to “Globalization trends hurting smaller economies and their still smaller enterprises.”

Scott Guggenheim (United States and World Bank) notes “the competitive and extraordinary threat posed by uncontrolled globalization,” while Gerardo Otero (Mexico) associates two of his three social issues with the globalization problem. The first is “The growing hegemony of neoliberal ideology, which privileges private interests and enthrones the short-term profit motive of private economic agents.” The second is “The growing power of transnational corporations . . . in shaping national government agendas.”

The most detailed answer was given by Joseph Jorgensen (United States): “The global economy in which ownership and control is located ever more tightly in western nations and Japan, and whose growth in erstwhile socialist and/or colonial nations has been forced through monetarist policies required by the World Bank and the International Monetary Fund . . . . Whereas the absolute number of corporations and persons of great wealth, world-wide has increased, dramatically so, the absolute number of persons and small businesses world-wide who have lost purchasing power, or who are extremely impoverished . . . has increased at a much greater rate.”

Jorgensen’s critique of World Bank and IMF policies corresponds to what I have observed in Zambia, where their free trade policies have enabled South African interests to increasingly dominate the country’s economy at the expense of local businesses. Unable to compete, the latter have failed, reducing in the process the number of Zambians employed in the private sector. In this case the poverty impacts of World Bank and IFC policies are worsened because they are applied on a country-by-country basis, without paying adequate attention to the

relationship of those policies to stronger regional economies—in the Zambian case, that of South Africa.

### *Other Concerns.*

The next four issues in importance are community unraveling (36 percent of the 53), population pressures (23 percent), marginalization (21 percent), and fundamentalism (19 percent). Community unraveling included the widest variety of issues. They ranged from a generalized increase in conflict, including randomness of conflict situations, a crisis in values and spiritual health and destructive nationalism within and between countries to confrontational ethnicity and a loss in people's ability to adapt to habitat. As a category, it requires much more research and understanding, including the relationship between economic downturn, poverty, and societal unraveling at community and household levels. I have found it to be a particularly important category where applied to populations undergoing involuntary relocation. Increased economic impoverishment (Cernea 1999) appears to be closely associated with a wide range of negative social indicators, including social disorganization at the community level, spousal and child abuse, including Dwyer and Bruce's (1988) home-divided syndrome and emergence of confrontational belief systems.

The fourth most frequently mentioned critical issue was population. Ten of the 12 respondents saw increasing human populations to be a major problem—a viewpoint I share. While the other two did not suggest that population increase was not a problem, one believed that environmental degradation was due more to economic and political factors than increasing population, while the other stated the problem to be population imbalances rather than population increase as such. Several others among the 53, who saw urbanization as a critical issue, were also concerned by population imbalances due to rapid rates of immigration.

I suspect that a larger proportion of ecologists than anthropologists would list population increase as a major threat due to their emphasis on an accelerating degradation of the natural resource base. If so, I can think of two reasons which might explain such a difference. One—similar to Kiplinger's reliance on human adaptability for solving problems—is the belief expressed by 4 of the 53, in either their answers to the questionnaire or in covering memos, that human ingenuity will enable people to adapt to changing conditions. Indeed, one colleague saw population not as a bomb but as a resource, provided his forecast of an improved social order during the next century proved accurate. The other reason I suspect may be due to an oversimplistic (indeed, mistaken in my opinion) extension of Boserup's (1965, 1990) theory about the relationship between population growth and agricultural intensification in domestic modes of production to the current relationship between population increase, a globalized, industrializing society, and the ecological support system.

Marginalization was the fifth most frequently mentioned issue. While its emphasis by 21 percent of the 53 is closely related to the concerns of the majority about poverty, several respondents noted a close relationship between marginalization and environmental degradation, on the one hand, and increased conflict, on the other. Environmental degradation resulted when poor people were required to live in marginal habitats. The relationship between marginalization and conflict was more complex. As identified by Paul Baxter (United Kingdom), I believe the key to be “the creation of massive segments of rootless, hopeless and excluded youth in the cities and the country side.”

As I (Scudder 1995) have written elsewhere:

lack of identification with nation or society . . . can . . . foster conflict, especially where national policies and development trends increasingly marginalize citizens. Of special concern here are young men and women who, at considerable sacrifice on their part, and the part of their supporters, have received an education which they find provides them with no appropriate employment. In the Gwembe case, . . . a fair number [of unemployed secondary school leavers] turn to crime either in town or within the village. All might be increasingly susceptible to involvement in some sort of fundamentalistic revitalization movement. Elsewhere similar young people have joined the Muslim Brotherhood in Egypt or in Sri Lanka the ultra-nationalistic Singala-speaking Janatha Vimukthi Peramuna (JVP) or Tamil-speaking Tamil Tigers (LTTE)—both movements that have tried to overthrow the government.

In what is perhaps the most detailed analysis, Paul Richards has analyzed a similar situation as background to the current civil war in Sierra Leone (personal communication, 1995).

Sixth in importance was increasing nationalistic, ethnic, and religious fundamentalism, which I too believe is a serious global threat. Religious fundamentalism relates not just to Judaism, Christianity, and Islam, but to Buddhism and Hinduism also, as well as to new sects that are arising. In addition to the usual threats to a pluralistic global society, in each case the cause of gender equality is set back due to renewed emphasis on the subservient role of women. Hardacre (1994:113), for example, notes that new religions in Japan, in which 25 to 33 percent of the population now participate, “share with fundamentalist groups in other religious traditions the characteristically conservative stance in regard to family, gender, and interpersonal relations.”

It is significant, I think, that the six major concerns of the 53 all relate to topics on which anthropologists have expertise. The question is how to best utilize that expertise so as to enable living standards to rise in ways that are sustainable in the future. Clearly, we must become much more proactive, not just in emphasizing the global threat presented by increasing poverty and the interrelationships between environmental degradation, poverty, community unraveling, and fundamentalism, but in helping policy makers reduce that threat.

### **Corrective Measures**

Answers to the third portion of my questionnaire illustrate the major emphases the 53 believe are necessary if our species is to enhance living standards in a sustainable fashion during the next century. As one would expect, most emphasized equity issues and most addressed what my colleagues saw as correctives to their major environmental and social issues. There were also a larger number of issues listed than under the first two questions. First in importance was poverty alleviation (55 percent), followed in order by sustainable development (45 percent), new paradigms (38 percent), equitable access to education (26 percent), and equitable and global access to information (23 percent). The five next most important opportunities were emphasized by only 11 to 8 percent of the 53. They were in order biotechnology, increased internationalism through the UN and other institutional mechanisms, democratization, basic human rights, and social and cultural diversity.

Aside from the nature of the opportunities stressed, of most interest was the emphasis on the need for new paradigms that would lead to the emergence of a civil and sustainable society.

Though 8 of the 53 emphasized the need for such paradigms to reflect knowledge gained from research in the social sciences other than economics, only 4 of the 53 referred specifically to the importance for the future of anthropologically generated knowledge. This is a serious mistake, illustrating not our lack of relevance but rather our lack of self-esteem. In studying the low-income majority around the world, we have accumulated knowledge that can, indeed must, play a major role if today's downward trends are to be reversed.

An important example of such knowledge, derived from anthropological study after study, is that rather than being part of the problem, a majority of the poor respond readily to appropriate development opportunities. "Appropriate," "development," and "opportunities" are all key words. Let me give three examples from my own research.

The first relates to a group of young Dinka men and women I visited in the central Sudan while on a 1970s assignment with the United Nations Development Programme during a lull in the soon-to-recommence civil war. My colleagues and I arrived shortly after dawn, when those visited were still occupying a mound of high ground with their cattle on the floodplains of the White Nile. Dung fires to ward off mosquitoes were still smoking and most men were naked. Some were dashing off the mound singing praise names of their song oxen. Immediately I was reminded of Evans-Pritchard's description of similar scenes among the Nuer in the 1930s. Any idea, however, that no change had occurred among these Nilotic people over a 40-year period vanished as soon as we settled down to discuss their problems. One was the cattle prices they found when they drove their cattle south to Juba near the Uganda frontier or north to Malakal. Another was about the lack of veterinarian care for their livestock and schools for their children. Still another was their difficulty in buying, from the proceeds of cattle sales, the trucks they needed to bring timber for the construction of cattle corrals and consumer goods for the small shops that existed in the various inland communities. And yet another was the difficulty in acquiring parts for their Israeli-supplied Uzi assault rifles. In other words, they wanted development, but of course as defined by them and not by outsiders. And they knew the type of opportunity needed.

The second case deals with Sri Lankans of Vedda background who had been forced to move so that their forest habitat could be incorporated within a national park. They had been relocated to a new village established in connection with the multibillion-dollar Accelerated Mahaweli Project. There, along with settlers from elsewhere in Sri Lanka, they were provided with irrigation for the double cropping of rice as opposed to a former production system based on the gathering and hunting of natural produce supplemented by small-scale swidden cultivation. Yet within several years their expectations for reliable supplies of irrigation water and improved social services were no different than those of their new neighbors.

The third example comes from Elizabeth Colson's and my own research among the Gwembe Tonga in the Middle Zambezi Valley. Like the Vedda case that my colleague Kapila Vimaladharma and I were monitoring throughout the 1980s, it is an example of the importance of long-term research. When we initiated our research in the mid-1950s, we found a small number of the still-isolated Gwembe people experimenting with major economic innovations. They included the purchase of cows and plow oxen in an area still at risk from bovine trypanosomiasis—a fatal disease transmitted by tsetse and other biting flies. Having been connected to the more-developed adjacent plateau by a rough 4-wheel-drive track, one of our study villages also included the first attempt to start small commercial businesses in the form of a tea shop-bakery or to sell surplus maize to a nearby mission.

When we returned for a restudy in 1962–63 we found other people seeking out different opportunities, including secondary school education for children and the purchasing of hand-paddled boats and nets on credit to participate in the gill-net fishery that had developed on the newly created reservoir. Not only did later visits identify still more people responding to new opportunities, but we realized that over a generation people within a majority of households had been willing to take major risks by responding to new opportunities while members of still other households had followed after them. But pointing up the importance of such external variables as government policies, including unfavorable rural-urban terms of trade, and drought and other unfavorable environmental conditions, most of those innovations failed for reasons beyond the control of the individuals, households, and communities involved.

At this point it is important not to confuse willingness to respond to new opportunities with rationality. Human societies have shown their capacity to bring about their demise in the past, a capacity that continues to exist today. Colson's and my research, for example, shows that Gwembe villagers can also be their own worst enemy. In a recent manuscript on long-term research among the Gwembe Tonga we have written (Scudder and Colson, n.d.):

in the mid-1970s when a decline in living standards led to an increase in malnutrition and illness and to general unhappiness, the majority opted to blame the witchcraft of kin and neighbors rather than government policies . . . . Fear of witchcraft and the envy associated with it, along with the increased consumption of alcoholic beverages and associated violence and a high incidence of theft especially in hunger years, contribute to village disorganization and to the break-up of villages whose coherence is already made difficult by the lack of land for expansion and populations increasing beyond old organizational frameworks. It also shifts much of the hostility engendered by failed government policies back into the home community and away from those responsible for making and carrying out policy.

On the other hand, we do not assume that such disorganization will necessarily continue, or should it continue, to “evoke the same explanations.” One lesson well learned by anthropologists involved in long-term studies is that we are dealing with dynamic nonequilibrium systems, the future of which it is impossible to accurately predict.

Another example of important anthropological knowledge relates to the increasing emphasis being placed by policy makers and development agencies on community participation in natural-resource management. That emphasis also arises in good part from anthropological studies. But those studies also warn against the optimism with which policy makers and practitioners think effective community management of resources can occur. We know that it cannot occur in a vacuum. At the national level not only is appropriate legislation necessary, but so is a reliable judicial system for enforcing that legislation. And at the community level, our studies also show serious problems that must be overcome. One is the discovery by policy makers of the importance of indigenous knowledge, residual community rights to land, water, and other resources, and community management institutions, at the very time that such knowledge, rights and institutions are being undermined by external forces. Another is the realization that new management policies must make economic sense for community members and, in making economic sense, they must be gender sensitive. And another is that the assistance of NGOs and funding donors is apt to be necessary for many years in the building of the necessary new institutions (Scudder et al. 1993).



## Putting Anthropology to Work

Before my conclusion, I would like to end this talk by making five proactive suggestions as to how we can put our knowledge and discipline to work. These are not newly formulated, for in recent years many voices stressing similar themes have been raised within the anthropological community. In the Winter 1999 issue of *Practicing Anthropology*, Suzanne Hanchett reviews “Anthropology and Development: The 1998 ICAES [International Congress of Anthropological and Ethnological Sciences] Discussion.” In their 1998 Malinowski Lecture, Robert and Beverly Hackenberg (1999) note other initiatives going on within the Society for Applied Anthropology, including the International Standards Committee’s development policy Website, while the latest issue of the Institute for Development Anthropology’s *Development Anthropologist* is titled “Development and the Anthropological Encounter in the 21st Century.”

The first of my suggestions is to better identify what we anthropologists believe are the key problems we have the expertise and responsibility to address. My opportunity sample of 53 colleagues is only a starting point on which I hope to follow up in collaboration with Michael Burton and Joseph Jorgensen. More important is for the Society for Applied Anthropology to sponsor a more sophisticated assessment of how members see the world and what global issues they believe they have the expertise to address in terms of public education, policy formulation, implementation and evaluation, and further research.

The second is to better integrate applied/development anthropology into the curriculum of anthropology departments. Though not synonymous, I prefer the phrase “development anthropology” to “applied anthropology” in spite of the controversy over the meaning and implications of the word “development.” “Applied,” however, implies that those involved do not do basic research. That is a serious misconception as it relates to those of us who study development issues through time with long-term studies. In creating our own time-series data, frequently we are doing more fundamental research than the large majority of academic anthropologists. That said, there is a critical need to expand the training of sociocultural anthropologists along the lines that Riall Nolan (1998) has outlined in a recent issue of *Practicing Anthropology*.

Third, as already mentioned, we need not only to be more aware of our expertise but also more active in bringing that expertise to the attention of the public and policy makers. And, as Elizabeth Colson has reminded me, we must be sure to emphasize as well our shortcomings. During my teaching career at Cal Tech, I was interested to note that students majoring in astronomy, geology, physics, and biology were more apt to take my courses than engineering and economics majors. Interested in complex systems, my students were willing to tolerate a relatively high degree of ambiguity and uncertainty. Furthermore, they were not too put off when I told them that increasingly we were learning about what kind of systems sociocultural systems were not, but had still much to learn about what kind of systems we were actually dealing with.

In presenting ourselves to the public and policy makers, we also must be aware of confusion as to what anthropology, let alone development anthropology, is all about. When I tell people that I am an anthropologist, the first reaction is to assume I am an archaeologist. After explaining that I am a sociocultural anthropologist, the next assumption is that I study some obscure ethnic group in an out-of-the-way tropical location. The emphasis is on our specific linguistic and cultural knowledge of a single population.

Although we continue to underrate it, we have topical expertise that others value even more than we do. The Institute for Development Anthropology now emphasizes that anthropologists who have studied pastoralists in East Africa have topical experience that can be applied to pastoralists not just elsewhere in Africa but in the Middle East and Central Asia. The same conclusion applies to involuntary resettlement, the World Bank's initial 1980 guidelines on involuntary resettlement in bank-financed projects being based largely on anthropological research undertaken in Africa and the Middle East.

I subsequently developed a four-stage framework for forecasting how both involuntarily and voluntarily resettled people will behave when resettling households are provided with sustainable development opportunities (Scudder 1981, 1984, 1997a and 1997b; Scudder and Colson 1982). That framework—based on comparative analysis of over 100 cases—forecasts the conditions under which a majority of households can be expected to move from an initial risk-adverse stance to a risk-taking one. It also postulates the type of investment and consumption strategies a majority will follow as their living standards improve.

Fourth, as a discipline we need to rethink our research agenda to include more generic research that better addresses major global problems and the linkages between them. An example is the linkages and possible synergies between such issues as environmental degradation, poverty, community unraveling, fundamentalism and globalization. Our discipline also has expertise relating to irrigation communities and natural-resource-user associations, the value of which can be expected to increase in the years ahead.

We need better comparative methodologies that include what Scarlett Epstein (pers. comm.) has referred to as “minimum core data,” which is the responsibility of each investigator and research team to gather. Relevant here is the Hackenbergs' (1999:11) suggestion in last year's Malinowski Lecture that the SfAA “appoint a committee to design a research protocol identifying key variables to guide restudies and assure their comparability.” But we also need our own carefully selected sample of “field laboratories” dealing with global issues in which we can undertake more comparative, long-term research, with results presented in ways to gain the attention of policy makers. Such research, of course, must not be at the expense of other types of research that deal more directly with, for example, multinational corporations, national and international governing institutions, and NGOs, as they relate to the low-income majority, or with various levels of corruption and conflict. However, I am not too worried about such topics being left out. Two things that impressed me about the replies from my 53 colleagues were the breadth and relevance of the topics they were researching as well as their efforts to influence policy outcomes.

In regard to long-term research, sociocultural and development anthropology are well behind long-term agroecosystem experiments and more general long-term studies in ecology. According to Rasmussen (1998:893) and colleagues “long-term agrosystem experiments (LTAEs) exist in many countries . . . and make up the largest temporal and spatial database presently available for determining the impacts of ecosystem change. They provide one of the few means to measure sustainable management systems in agriculture.” As for more general studies, U.S. ecologists currently are undertaking long-term ecological research (LTER) in at least 20 sites in North America and Antarctica—at least two of which involve urban communities <<http://LTERnet.edu>>. More recently, they have begun networking with researchers in other countries undertaking similar research. We need to develop criteria for identifying similar sites and then to seek the necessary funding, perhaps from the National Science Foundation, which initially sponsored the long-term ecological studies.

Fifth, as individuals and as an association, we should join hands with ecologists and their professional societies not just to carry out joint research but also to see if we can work together in public education and in influencing local, national, and international policy makers. I believe we should approach the World Conservation Union's Commission on Ecosystem Management, which is currently wrestling with such problems as the nature and boundary of ecosystems and the meaning of management.

While proactive, those five points are hardly earth-shaking, considering the problems my 53 colleagues outlined. Indeed, if I were in the audience listening to this talk I would be thinking that my five recommendations, while useful, are totally insufficient for dealing with the type of crisis I see threatening our planet.

Kiplinger's *World Boom Ahead* exemplifies a vision of technology and the market as able to advance "progress" and to raise the living standards of the large majority in the process. That vision is unrealistic. It largely ignores the downward trends emphasized by the 53, by ecologists, and by other commentators on the world scene. When I sent the first questionnaires to colleagues last fall, I wrote down my own answers to the three sets of questions. My views appear to be more pessimistic than those of perhaps a majority of the 53, but some colleagues were even more pessimistic, seeing virtually no opportunities for reversing current downward trends.

I agree with the majority that misuse and degradation of the natural resource base is the most serious environmental issue because in Lubchenco's (1998:492) words "the functioning of ecological systems is disrupted and the continued provision of ecological services is threatened." Time and again I have seen that outcome in my own research, while the media daily report new threats or old ones revisited which are more serious than originally believed. Take the 1989 Exxon oil spill in Alaska's Prince William Sound, which was recently the subject of a lengthy article in the *Los Angeles Times* (Murphy 1999). According to the agency responsible for "overseeing scientific studies and habitat restoration funded by Exxon's \$900-million settlement with the government . . . only two of 28 species affected by the spill" are fully recovered (p. A13), while Native Americans and commercial fishers dependent on the sound's bounty continue to be badly hurt. Boosters of technological fixes and unregulated markets continue to ignore such "technological disasters."

My own research highlights the speed with which development-induced resettlement can cause environmental degradation when population densities exceed the carrying capacity of the land under existing systems of land and water use. In the Lusitu area of the Middle Zambezi Valley, I estimate that the carrying capacity of the land was exceeded by at least a factor of three when 6,000 Gwembe Tonga were moved there in the late 1950s in connection with the construction of the Kariba Dam. Today, what was formerly a well-vegetated savanna woodland looks like a degraded Sahelian habitat on the edge of the Sahara. Occurring within a single generation, this transformation was the worst-case of environmental degradation that remote-sensing specialist Eric Lambin has seen during his many years of African research. Ah, you may say, but that is obviously an extreme case. It is. But it forecasts what can be expected during the next century throughout Africa's semiarid zones, which cover the largest portion of the continent, if present trends continue. As for Africa's arid lands, according to BBC coverage of the November 1998 UN-sponsored conference on desertification, nearly three-fourths have already been degraded.

My second environmental issue dealt with increasing water scarcity and pollution—also targeted by many of my colleagues. General awareness of the associated problems has increased significantly, as indicated by such new institutions as the World Water Council and the Global

Partnership for Water. Nonetheless, though corrective actions have begun in the United States and Europe, negative trends in most of the world—and especially in China, India, and throughout Africa—continue to worsen. Even where problems are recognized, as in Israel and Egypt, I do not see the political will to address them in a creative fashion—indeed current Israeli policies tighten their control of water use at the expense of Jordan, while Egypt sees Ethiopia as an adversary in the use of Nile waters.

My third issue dealt with decreasing biodiversity—an issue that to my surprise was only given priority in three of the returned questionnaires. This is another example of how perceptions are influenced by experience. Before switching to anthropology as a graduate student, I had been preparing for a career as a classical biologist who wished initially to be a naturalist working in the tropics and subsequently to be an ornithologist. As an amateur birdwatcher, during periodic trips back to New England where I birded extensively in the 1940s and 1950s, I have been appalled by the decline in songbirds in my former birding locales. According to recent estimates, two-thirds of the almost 10,000 bird species that inhabit our planet “are now in decline, while 11 percent are threatened with extinction . . . . Of the earth’s 4,400 species of mammals . . . 11 percent are in danger of extinction. Another 14 percent are vulnerable to extinction . . . . Of the 24,000 species of fish . . . one-third are now threatened with extinction” (Brown et al. 1999:14). As for flora, the World Conservation Monitoring Centre concludes that of the world’s 10,000 species of trees, 10 percent are threatened by global habitat destruction (Williams 1998:1426), while the U.S. State Department’s (1997:8) first annual assessment of the global environment states that “Every year forests four times larger than Switzerland are lost because of clearing and degradation.” According to Myers (1995:823) in *Science* “tropical forests have lost half their original expanse in the past 50 years,” while the annual loss of boreal forests in Siberia “encompasses an area twice as large as deforestation in Brazilian Amazonia.”

As for social issues, two of my concerns—poverty and globalization—were also stressed most frequently by half or more of the 53. In my case, however, I linked both issues with community unraveling. My other social issue was increasing ethnic, national, political, and religious fundamentalism—an issue shared with 19 percent of the 53. As with poverty, I see such confrontational ideologies increasing. The threat they present was especially apparent in my research in Sri Lanka during the 1980s. There a cabal in the implementing agency joined forces with ultranationalist Buddhist priests in an attempt to use the country’s largest development project as a means for the ethnic cleansing of the Tamil-speaking Hindu minority. Any attempt of the government to accommodate legitimate Tamil concerns intensified the brutal fight between government forces and the ultranationalist Janatha Vimukthi Peramuna (JVP). On one occasion when 5 security personnel had been killed, the government forces retaliated by killing at least 70 detained youths. I was working in the vicinity and saw over 20 decapitated and burnt bodies as well as 40 burnt houses of farmers whose only involvement was that they lived adjacent to where the security forces were killed. Again, during a consultancy in the Indian state of Gujarat, a colleague and I had to navigate on a motorcycle in the wee hours of the morning around barricades established by security forces because of Hindu-Muslim clashes. I mention these two examples to show that a growing fanaticism is a dangerous component not just in Judaism, Christianity, and Islam, but in all the major religions of the world.

*World Boom Ahead* underestimates the seriousness of these issues as well as others. Declining foreign assistance, especially in the United States but also in most other industrial nations, is an indicator that most donor governments have little willingness to play the kind of role necessary to reverse increasing world poverty and environmental degradation. When, for

example, longer-term policies are favored by the executive branch in the United States, as in the case of global warming, Congress tends to balk at making the type of hard choices which subsequently might interfere with short-term economic growth and hence reelection. According to Vogel and Lawler (1998:1684) in *Science*, "Senate leaders have also made it clear that they will not even debate whether to ratify the Kyoto treaty signed by more than 150 nations . . . , which would reduce greenhouse emissions." As for aid from international development banks, even World Bank officials now admit that structural adjustment policies in Africa have intensified poverty, and hence I would add environmental degradation.

While much is made of the increasing volumes of overseas capital provided by private-sector firms seeking to make a profit in less-developed political economies, capital which now dwarfs in quantity international agency and bilateral government assistance, 80 percent of that capital is being funneled into only 12 countries (UNDP 1998). That report also pinpoints another serious problem closely linked with globalization. This is the private sector's expanding ability through advertising to push for more and more consumption of an increasingly widening range of goods. Whether intentional or not, this is closely associated with government policy in the United States, where advertising is tax-deductible and now penetrates even into public radio and television.

What else can development/applied anthropologists do about all this? Quite a bit. As individuals we must increase efforts to influence attitudes, policies, and implementation as they relate to our expertise. Here, however, we must keep in mind that a major reason why anthropology as a discipline has little influence is because we work more as an aggregate of individuals rather than through the type of institutions needed to effect attitudes and behavior of individuals, and the private and public sectors.

Identify a key issue today, and one or more anthropologists are already there studying it—replies to my questionnaire and covering memos, and recent reading in preparation for this talk, have convinced me of this. Telecommunications and biotechnology, which Kiplinger sees as the driving forces during the next 20 years, are both being studied and assessed by anthropologists. In the December 1998 issue of *Anthropology Newsletter*, three San Jose State University anthropologists (Darrah, English-Lueck, and Freeman) report on their research in the communications-linked industry in California's Silicon Valley. Biotechnology is of major interest to Forouz Jowkar, whose questionnaire response emphasized the threat posed by "agrobusiness/chemical conglomerates in agricultural production through the use of chemical-dependent, genetically-engineered seeds." And Otero writes from Australia that he is participating in a UN Convention of Biological Diversity consultancy to assess the new technology for the control of plant gene expression, which sterilizes second-generation seeds. Though such technologies presumably are targeted at increasing yields, I can attest to the havoc they can play among those who are too poor to purchase new seed each year, while Jowkar emphasizes their effect on the disappearance of indigenous species. In the Gwembe Tonga case, a majority of farmers today are too poor to purchase hybrid seed, let alone the next generation of presumably more expensive genetically engineered seed.

A major problem with our discipline, however, is that it has no way to synthesize such knowledge, let alone to publicize it and enter it into theory and practice. It can be done. Drawing on detailed anthropological research in Africa and the Middle East, Michael Cernea, David Butcher, and other anthropologists in the World Bank developed the policy implications of that research, and made it actionable, in the 1980 World Bank Guidelines on Involuntary Resettlement in bank-financed projects. As an association, can the Society for Applied

Anthropology facilitate such a process as it relates to solving global threats to the future of our planet?

The nature of the required changes among the public as well as decision makers and planners at international, national, and community levels is mind-boggling. After noting the many studies on the topic, Corson (1994:207) writes that, “A sustainable future will require shifts in personal values, beliefs, attitudes and goals, and substantial alterations of economic, social and political practices.” More specifically, he states that, “Needed changes will involve every aspect of human activity, including values and world views, ethics, education, the media, political and social institutions, lifestyles, reproduction, gender relations, economics, energy use, transportation, technology and information management, diet, agriculture, and natural resource protection” (1995:38–39). Roodman’s “Building a Sustainable Society” makes the same point: “Restoring sustainability will be comparably complex—a thorough going process involving every sector of society” (1999:170). This change must not just influence policies from global to local levels, but especially the capacity to implement, monitor, and change appropriate ones. No wonder some of my colleagues consider the situation hopeless.

I am an optimistic pessimist, however, and so I wish to end by reflecting on a tantalizing alternative, or complement if you wish, to a technological and market approach to the future. This will require a significant shift in key assumptions about our place in the world and our priorities. Such a shift may require something along the lines of a revitalization movement—revitalization movements being yet another topic best explored by anthropologists. As a scientist I am suspicious of such strongly held ideologies, for all too often they become fundamentalist and totalitarian. Yet, at their best, they show the importance of ideas, as opposed to technology, as a force in history.

Required reading in my course on continuities and change among the world’s low-income majority was Anthony Wallace’s “Revitalization Movements in Development” (1967). Especially intriguing is the question of whether or not what Wallace refers to as the “tremendous energies of the revitalization movement can be placed at the disposal of technical development.” He adds “under such circumstances, great progress can be made in a very short time” (p. 454). Reversing the order, that is, making technical development subservient to the revitalizing energies, could such a movement take on a global character as the instrument for formulating and propagating the necessary changes in values and actions? If yes, I would want anthropologists—with their peerless awareness of cultural diversity in time and space, and their understanding of how symbols can influence behavior—to work with ecologists to translate what we know into a vision for the future. I leave you with that thought.

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### References Cited

- Boserup, Esther  
 1965 The Conditions of Agricultural Growth: The Economics of Agrarian Change under Population Pressure. Chicago: Aldine.  
 1990 Economic and Demographic Relationships in Development. Baltimore: Johns Hopkins University Press.
- Business Week  
 1998 Special Double Issue on the 21<sup>st</sup> Century Economy. August 31.
- Brown, Lester, C. Flavin, and H. French  
 1999 State of the World. New York: W.W. Norton and Company.
- Cernea, Michael M.  
 1995 Social Organization and Development Anthropology. Human Organization 54:340–352.
- Cernea, Michael M., ed.  
 1999 The Economics of Involuntary Resettlement: Questions and Challenges. Washington, D.C.: World Bank.
- Corson, Walter H.  
 1994 Changing Course: An Outline of Strategies for a Sustainable Future. Futures 26:206–223.  
 1995 Priorities for a Sustainable Future: The Role of Education, the Media, and Tax Reform. Journal of Social Issues 51(4):37-61.
- Darrah, C.N., J.A. English-Lueck, and J.M. Freeman  
 1998 Living with Technology. Anthropology Newsletter 39(9):1,4.
- de Wilde, John C.  
 1967 Experiences with Agricultural Development in Tropical Africa. Vols. 1 and 2. Baltimore, Md.: Johns Hopkins University Press for World Bank.

- Donahue, John M., and Barbara Rose Johnston, eds.  
1998 Water, Culture, and Power: Local Struggles in a Global Context. Washington, D.C.: Island Press.
- Dwyer, Daisy, and Judith Bruce, eds.  
1988 A Home Divided: Women and Income in the Third World. Stanford, Calif.: Stanford University Press.
- Evans-Pritchard, E. E.  
1940 The Nuer: A Description of the Modes of Livelihood and Political Institutions of a Nilotic People. Oxford: Clarendon Press.
- Hackenberg, Robert A., and Beverly H. Hackenberg  
1999 You CAN Do Something! Forming Policy from Applied Projects, Then and Now. Human Organization 58:1-15. (Republished at <http://www.sfaa.net/>.)
- Hanchett, Suzanne  
1999 Anthropology and Development: The 1998 ICAES Discussion. Practicing Anthropology 21(1):44-48.
- Hardacre, Helen  
1994 Japanese New Religions: Profiles in Gender. *In* Fundamentalism and Gender. John Stratton Hawley, ed. Pp.111-133. New York: Oxford University Press.
- Institute for Development Anthropology  
1998 Development and the Anthropological Encounter in the 21st Century. Development Anthropologist Vol. 16 (1 and 2).
- Johns Hopkins University Population Information Program  
1998 Solutions for a Water-Short World. Population Reports. Series M, No, 14. Baltimore, Md: Johns Hopkins University Press.
- Kiplinger, Knight  
1998 World Boom Ahead: Why Business and Consumers Will Prosper. Washington, D.C.: Kiplinger Books.
- Long Term Ecological Research (LTER)  
1999 URL: <<http://LTERnet.edu>>.
- Lubchenco, Jane  
1998 Entering the Century of the Environment: A New Social Contract for Science. Science 279:491-497.
- Merrey, Douglas J.  
1997 Expanding the Frontiers of Irrigation Management Research: Results of Research and Development at the International Irrigation Management Institute, 1984–1995. Colombo, Sri Lanka: International Irrigation Management Institute.
- Murphy, Kim  
1999 Alaska Struggles to Recover, 10 Years after Exxon Valdez. Los Angeles Times, March 20:A1, A12-13.
- Myers, Norman  
1995 The World's Forests: Need for a Policy Appraisal. Science 268:823-824.



- Nolan, Riall  
1998 Teaching Anthropology as if It Mattered: A Curriculum For 21<sup>st</sup> Century Practitioners. *Practicing Anthropology* 20(4):39-44.
- Postel, Sandra  
1997 Last Oasis: Facing Water Scarcity. New York: Norton.
- Rasmussen, Paul E., K. W. T. Goulding, J. R. Brown, P. R. Grace, H. H. Janzen, and M. Korschens  
1998 Long-Term Agroecological Experiments: Assessing Agricultural Sustainability and Global Change. *Science* 282:893-896.
- Roodman, David Malin  
1999 Building a Sustainable Society. *In* State of the World. Lester Brown, C. Flavin, and H. French, eds. Pp. 169-188. New York: W.W. Norton and Company.
- Scudder, Thayer  
1981 What It Means to be Dammed: The Anthropology of Large-Scale Development Projects in the Tropics and Subtropics. *Engineering and Science* 54:9-15.  
1984 The Development Potential of New Lands Settlement in the Tropics and Subtropics: A Global State-of-the-Art Evaluation with Specific Emphasis on Policy Implications, Executive Summary. U.S. Agency for International Development Program Evaluation Discussion Paper No. 21. Washington, D.C.: U.S. AID.  
1995 Anthropology's Role in the NSF Initiative on Human Dimensions of Global Change. Unpublished memorandum. Author's files.  
1997a Social Impacts. *In* Water Resources: Environmental Planning, Management, and Development. Asit K. Biswas, ed. Pp. 623-665. New York: McGraw-Hill.  
1997b Resettlement. *In* Water Resources: Environmental Planning, Management, and Development. Asit K. Biswas, ed. Pp. 667-710. New York: McGraw-Hill.
- Scudder, Thayer, and Elizabeth Colson  
1982 From Welfare to Development: A Conceptual Framework for the Analysis of Dislocated People. *In* Involuntary Migration and Resettlement: The Problems and Responses of Dislocated People. Art Hansen and Anthony Oliver-Smith, eds. Pp. 267-287. Boulder, Colo.: Westview Press.  
n.d. Long-Term Research in Gwembe Valley, Zambia. *In* Long-Term Field Research in Social Anthropology. Rev. ed. Robert Van Kemper, ed. New York: Academic Press.
- Scudder, Thayer, with R. E. Manley, R. W. Coley, R. K. Davis, J. Green, G. W. Howard, S. W. Lawry, D. Martz, Peter P. Rogers, A. R. D. Taylor, S. D. Turner, G. F. White, and E. P. Wright  
1993 The IUCN Review of the Southern Okavango Integrated Water Development Project. Gland, Switzerland: IUCN Wetlands Programme. Samara Publishing Company for IUCN (World Conservation Union).
- Seckler, David  
1996 The New Era of Water Resources Management: From "Dry" to "Wet" Water Savings. Washington, D.C.: Consultative Group on International Agricultural Research.
- Seckler, David, U. Amarasinghe, D. Molden, R. de Silva, and R. Barker  
1998 World Water Demand and Supply, 1990-2025: Scenarios and Issues. Report 19. Colombo, Sri Lanka: International Water Management Institute.
- Serageldin, I.  
1995 Toward Sustainable Management of Water Resources. Directions in Development Series. Washington, D.C.: World Bank.

## Systemwide Initiative on Water Management (SWIM)

1997 Annual Report for 1997. Colombo, Sri Lanka: International Water Management Institute.

## United Nations Development Programme (UNDP)

1998 Human Development Report 1998. New York: Oxford University Press.

## United States Department of State

1997 Environmental Diplomacy: The Environment and U.S. Foreign Policy. Washington, D.C.: U.S. Government Printing Office.

## Vermillion, D. L., and D. J. Merrey

1998 What the 21<sup>st</sup> Century Will Demand of Water Management Institutions. *Zeitschrift für Bewässerungswirtschaft* 33:165–187.

## Vogel, Gretchen, and Andrew Lawler

1998 Hot Year, But Cool Response in Congress. *Science* 280:1684.

## Wallace, Anthony F. C.

1967 Revitalization Movements in Development. *In The Challenge of Development: Theory and Practice*. Richard J. Ward, ed. Pp. 448-454. Chicago: Aldine.

## Williams, Nigel

1998 Study Finds 10% of Tree Species Under Threat. *Science* 281:1426.

## World Bank

1980 Social Issues Associated with Involuntary Resettlement in Bank-Financed Projects. Operational Manual Statement No. 2.33. Washington, D.C.: World Bank.