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PRIDE AND PUZZLEMENT: A Retro-introspective Record of 60 Years of Anthropology

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I have been so lucky, in my personal and professional life alike, that I am happy to risk telling as candidly as I can whatever bears on my career. It was in the summer of 1928 that, while supervising a playground in Milwaukee, I read Marett's *Anthropology*. When I returned to Madison to find that the University of Wisconsin had hired its first anthropologist, Ralph Linton, I took his course and after several lectures decided to change my major. Linton was encouraging: "It's a good field," he said. "There are only about 50 anthropologists in the United States." The four fields (physical anthropology, archaeology, cultural anthropology, and linguistics) were then undivided, and I immediately began to prepare myself with courses in geology, evolution, comparative anatomy, embryology, neurology, psychology, and sociology. All the courses in anthropology were taught by Linton (who had just returned from fieldwork in Madagascar) and by Charlotte Gower, a University of Chicago PhD whose thesis had been in Caribbean archaeology and who was fresh from fieldwork in rural Italy. None of us either inside or outside of our small community of anthropologists had any doubts. The "four fields" have since grown tenfold, and my first question is whether it is the same anthropology.

Mainly in Britain and the Americas was "anthropology" the master term. In most of Europe it meant physical anthropology; ethnology had its own terms, and archaeology and linguistics seemed also to be isolated, but in fact a "general anthropology" seems to have been recognized. After the First World War "social anthropology" began to be used, at least by Radcliffe-Brown and Malinowski and their students, who distinguished their work from ethnology in terms of its being nonhistorical and for a time threatened to secede. Partly

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because linguistics became interesting to many of them, this movement slowed just as countervailing forces, especially in North America, were strengthened by government funding for "anthropology" as traditionally recognized. In the summer of 1952, 80 scholars gathered at the Wenner-Gren Foundation in New York for two full weeks of discussion of all aspects of anthropology (7, 41). Although East Europeans were unable to join this first postwar gathering, it was self-consciously international, and it brought into the closest face-to-face contact scholars of every field of anthropology. It made a great difference; Carlos Monge, who studied the effects of very high altitude on Indians in the Peruvian Andes, said to me one day in the course of it, "I have been a biologist; but no more. Now I am an anthropologist."

Reviewing the conference (29), I noted that nonanthropologists had taken for granted or admired the breadth of our subject, but some of the anthropologists had found it problematic. When I attacked the problem historically, I found that anthropology as I knew it had had its beginnings in 1839 in Paris with the founding of a network of societies embracing the four fields and concluded that

> except possibly in Britain . . . anthropology is becoming more rather than less integrated. The danger is not in increasing numbers, or adding specialisms, or permitting free access to our ranks; but rather the contrary is true. If anthropology in the United States disintegrates, it will most likely be because increasing professionalization leads to restrictive definitions of anthropology or anthropologists and to thc counting out of some activities—scientific, applied, or action-oriented. An essential value in the case of anthropology is its historic reaching for new ideas, tools, and subject matters that may further the study of man.

These words might have seemed wishful thinking on the part of one as committed as I was to anthropology. I need to ask myself how I might have become so committed—60 years ago—and why I still am. It happens that from early childhood I had Walter Mitty dreams of greatness that became increasingly sociopolitical and worldwide. By the time I was in high school I was impatient to *do* things, and I thought of law as a career leading to politics and of political science and economics as prelaw majors. But I had also adopted values against which these disciplines would be tested and—I feared—found wanting. In any case, in my first college years I was much more interested in campus political activities, and in writing and playacting, than in required courses, which I quietly neglected. That a sudden return to disciplined study coincided with the turn to a positive, named career—whatever it might be—is understandable. But I came to credit the outlook of anthropology as critically coinciding with philosophical values that, also, I had adopted in my third year of high school. Specifically, I recall one evening reviewing in my mind the different conclusions of great thinkers on such large questions as whether there are knowable absolutes as opposed to conditional truths. It dawned on me then that the very diversity of views among the wisest of men settled the question in favor of relativism, and I have never wavered from that view.

Fieldwork

When I came to the Department of Anthropology of the University of Chicago in the autumn of 1931, I had had a four-month experience with Alonzo Pond's Logan Museum (Beloit College) archaeological expedition in Algeria (15), another two months with George Grant McCurdy's American School of Prehistoric Research in Europe (both in 1930), and then a summer's field school under Ruth Benedict among the Mescalero and Chiricahua Apache in New Mexico. Having learned basic four-field anthropology from Linton and Gower, I felt well prepared.

A. R. Radcliffe-Brown arrived in the department at the same time, and even before I had taken his course in kinship he suggested that I do fieldwork among the Fox because they had an interesting variant of the Omaha type of kinship system. Even the phrase was new to me, and I quickly went to Lewis H. Morgan's *Systems of Consanguinity and Affinity*

Crow types of kinship. In the next three years I studied in depth the history and theory of the study of kinship systems; spent the summer of 1932 with the Fox (Mesquakies) in Iowa and that of 1933 visiting related tribes in Wisconsin, Michigan, Minnesota, and Ontario (none of which had Omaha or Crow systems) for comparison; and, as a research assistant, collected data on the Indian tribes of California and the Northwest Coast. By the summer of 1934, when I returned to the Mesquakies for last-minute checking of data and ideas to write the part of my thesis that later was published as "The Social Organization of the Fox Indians" (18), I had in hand the part that was eventually published as "From Lafitau to Radcliffe-Brown" (30). That fall I worked on the theoretical portion, eventually published as "Some Problems of Social Organization" (17). Robert Redfield had invited me to join his Carnegie Institution of Washington project to do ethnography among the Indians of the highlands of Guatemala, and I finished writing only two or three days before my wife and I were to embark from New Orleans for Guatemala. The faculty was miraculously cooperative in reading and examining me over the thesis, and we were off.

Redfield had suggested that we learn Spanish and read Schulze-Jena's newly published book on the Quiché, and that was almost all we did in preparation for our first Guatemalan fieldwork. We were scheduled to spend eight months in the field and four months back home each year to put together what we had learned, and this we did for three years (1934–1937). Then I had a short field season in the winter of 1938, and then—together now with our newborn daughter—we had six months in 1939 and a full season of fieldwork in 1940–1941 (see R. A. Rubinstein, *Fieldwork: The Correspondence of Robert Redfield and Sol Tax,* in preparation). Our fieldwork consisted of fairly intensive work in Chichicastenango (Quiché) and in the towns bordering Lake Atitlán, particularly Panajachel (Cakchiquel). Part of this time the Redfields lived in nearby Agua Escondida, a Ladino town, and our contact was close. I worked particularly on the world view and the economy of the Indians (19–22, 26, 28, 32, 39). I also had considerable success in helping Antonio Goubaud Carrera and Juan de Dios Rosales to become, in the early 1940s, the first native Guatemalan professional anthropologists. I worked with both of them during the war years on a nutritional survey of rural Guatemala and in the same period (1942–1944) taught in Mexico City and gave students field training among Maya-speaking Indians in Chiapas.

Professional Activities

We returned to Chicago for permanent residency in the spring of 1943. It was wartime. Between trips to Guatemala I wrote and filled in for absent faculty by teaching undergraduates in Robert Maynard Hutchins's famous college. I was a research associate in the Department of Anthropology and met weekly with the faculty. I was also a tenured scientist of the Carnegie Institution of Washington, but when it abandoned its Division of Historical Research it was time to think of a university position. Redfield and I were too close in our interests for me to think of the University of Chicago; but there was no hurry. I was writing and enjoying contacts in the department and the college. I presume that it was at one of our department meetings that we discussed the coming postwar influx of mature students, and the idea came to me of developing a graduate curriculum after the pattern I admired in the college, using a syllabus with selected readings and with lectures by specialists, classroom discussions of issues, and written examinations to measure progress. I was encouraged to develop these ideas, and eventually there was a plan for three year-long courses: "Human Origins," "Peoples of the World," and "Culture, Society, and the Individual." In 1944 I became associate professor, and Robert Braidwood and I worked out the first course, which was followed by Fred Eggan's work on the second and a cooperative effort with the Department of Sociology and the Committee on Human Development accomplishing the third. In 1948 I became full professor and had a five-year term as associate dean of the Division of the Social Sciences, also chairing its Curriculum Committee. These were years of intense interaction with many of my 150 colleagues of every social science discipline. It is probably no accident that these were the years both of the development of action anthropology and of my work toward the racial integration of the Hyde Park community in which the University of Chicago is located (33). These same years also saw publication of the three Americanist volumes (23–25) and of *Heritage of Conquest* (26) and the completion of *An Appraisal of Anthropology Today* (41) and *Penny Capitalism* (28).

The professional activities that occupied my time over the following three decades might be grouped into three large, overlapping categories:

1. Research, teaching, writing and lecturing, and administration.
2. Editing and arranging publication of journals and of books and advising on such matters; organizing conferences on scientific topics and national and international academic congresses; and serving state, national, and international bodies such as UNESCO, the Smithsonian Institution, the Illinois State Museum, the National Science Foundation, the National Institutes of Mental Health, the Office of Education, and (once) the White House.
3. Pursuit of a variety of intellectual problems and practical topics to which anthropological knowledge could be applied, such as (*a*) the (wartime/peacetime) draft (36); (*b*) community development and race relations in Chicago, Mexico and Guatemala, and Bangladesh; (*c*) expansion of the possibilities open to North American Indians through education and the modification of non-Indian institutions and attitudes; (*d*) worldwide population problems; (*e*) concepts of mankind-as-a-whole and means of introducing a worldwide second language; (*f*) ways of living in space colonies, involving futuristic experiments with internationally mixed communities in isolated areas of the world connected only electronically to sister-communities and to their home nations; and (*g*) the idea (38) of substituting for powerful nation-states powerless "localities" of 250,000 people under a democratic constitution ensuring open information and movement but not the possibility of forming alliances (38).

These extracurricular activities represent varying amounts of time and energy over the years. As examples I choose only one from each of two sets: editorial and publication matters and concerns of American Indians.

My editorial activities from the beginning have been associated with organization and community development. In Milwaukee as I passed my twelfth birthday there was a Newsboys' Republic, modeled after the United States government, with which I became associated, winding up as editor of the citywide *Newsboys' World*. The *World* had a circulation of 5,000, and I was on a par with high-school editors at their state convention in my junior year. The innovation I remember was a front cover with a colored cartoon. As an undergraduate in the Milwaukee State Teachers' College one semester I organized a forum on comparative religions and with two friends published a *Forum Free Press* to carry on the discussions. The next year, at the University of Wisconsin, we founded a Liberal Club and published the *Student Independent*, for sale on campus corners in competition with the staid *Daily Cardinal*. Meanwhile, I was active in the new Hillel Foundation, editing its *Bulletin* and serving as president.

As a graduate student at Chicago, and while doing fieldwork in Guatemala, I dropped everything but anthropological research. Not until the early 1950s would I again be editing a journal.

My first contact with book publishers was in 1942–1943 with the then new Fondo de Cultura Económica in Mexico, helping them to get started in anthropology with translations of Linton's *The Study of Man* and Redfield's *The Folk Culture of Yucatan*. Then in 1944, after conversation with Alfonso Caso on how the voluminous data of ethnographic fieldwork might be published, I originated (with the University of Chicago Library) the microfilm series that, after 395 numbers, is still being edited by N. A. McQuown. In 1948, when we needed out-of-print books for students, I negotiated with Jeremiah Kaplan's fledgling Free Press an agreement to reprint books such as Radcliffe-Brown's *The Andaman Islanders* with a guarantee by our bookstore to keep them on order until the initial publication costs were covered. This taught me something about the economics of books. Then it was easy, after the Viking Fund seminar in 1949 that produced *Heritage of Conquest,* to see that book subsidies should be used to buy at wholesale—and for good uses—the books produced. At the same time, when in 1949 I was asked to edit the proceedings of the 29th International Congress of Americanists and told that there was insufficient money to pay for the paperback books promised to members, I learned that one can make good books from selected congress papers and produce hardbound copies for profits that enable a publisher to provide the customary free copies. I do not know if this innovation suggested that I be appointed to the university's Board of Publications (which after a year or two I also chaired), but of course that brought me close to the University of Chicago Press, from which I continued to learn.

When in 1952–1953 the Wenner-Gren Foundation needed to publish quickly the two large volumes from its "Anthropology Today" symposium, I knew how to be helpful to both sides. By 1960, when appointed editor of the Viking Fund Publications in Anthropology, I was prepared to arrange with publishers for a commercial hardcover edition that would pay for the free paperbacks needed for distribution by the Foundation, and we publishcd 21 volumes in both forms. By happenstance the idea could be extended in 1962 to a worldwide audience. The Voice of America asked me to develop a series of lectures taped by 20 anthropologists for an international audience and also made available in pamphlet form. We were permitted to publish the lectures, and Aldine made of them a successful college text (35). Meanwhile, in at least Italy, India, and Colombia, the pamphlets were translated and published as books locally.

With the postwar inflation of the early 1950s, the American Anthropological Association found itself with too little money to publish more than a fraction of what the growing profession was writing, and the thinness of the quarterly journal and the reduced number of issues in its series of memoirs became a serious problem for members, whose numbers declined. I was asked to assume responsibility, with three or four associates; I accepted the challenge and publicly promised (27) to double in four years the capacity of the journal and the number of memoirs. I did so in three years and turned over a thriving journal to a new editor. Doubtless at least partly because of this success I was elected president of the Association two years later. This certainly also led directly to my involvement in the founding of *Current Anthropology*.

The Wenner-Gren Foundation had in 1955 launched a biennial yearbook, but a year later Paul Fejos, the president, approached me to say that the burden on the Foundation had proven too great and the yearbook would be continued only if I would carry it forward outside. Eventually I agreed, provided that I could first discover what the world's anthropologists would find most useful. After a series of conferences in the United States and Western Europe the decision was made to have an open-ended international journal reflecting the changing discipline. Next, through consultation with colleagues in all parts of the world, the form and content of the journal were determined, and in the autumn of 1959 a "pre-issue" was presented that attracted subscriptions from some 3,000 scholars. The first issue was dated January 1960. Editorial and policy decisions were to be made by all "Associates" through "Reply Letters" accompanying each issue. *Current Anthropology* thus became equally a journal and a community, now 23 years old and in its third (editorial) generation.

Out of *Current Anthropology* in 1971 came LARG, the Library-Anthropology Resource Group. To help resolve problems of keeping up with the ever-increasing quantity of reference materials, I asked advice of Jan Wepsich, the Slavic bibliographer of the University of Chicago Library. We decided to call a meeting of anthropologists and librarians from the several universities in the Chicago area to discuss the subject, and this gathering produced the idea of developing reference materials cooperatively. Since then a changing group of us has met monthly and produced three books (6, 9, 13), with a fourth (a biographical dictionary of anthropologists by C. Winters) in preparation. We have worked happily together without funds except as royalties now repay some out-of-pocket costs.

The Ninth International Congress of Anthropological and Ethnological Sciences, in Chicago in 1973, was built on all of these experiences. Responsibility for this congress came to me at the preceding congress in Tokyo in 1968, and as plans for it developed it became apparent that we would need a great deal of money to support them. In some countries governments pay the costs of such congresses, and indeed eventually we received some helpful US government grants for special projects. But we were proposing the largest use of simultaneous translation ever attempted (five languages for six days), the production and mailing of thousands of prepared papers all over the world for advance reading as background for the translated discussions, and, if possible, payment of transportation and living costs in Chicago for scholars unable otherwise to come from far places.

Therefore the congress was designed to make good books, and a publisher advanced $200,000 in potential royalties to make this possible. We produced the 91-volume World Anthropology series, which is expected at least to repay the advance. Sufficient funds were provided for a congress of close to 4,000 participants from every part of the world. The only disappointment was that the volume describing the congress (42), which was to be sent free to all members, was denied publication by the Berlin firm that in the meantime had purchased the original publisher.

Native Americans

My first contact with American Indians was with the Mescalero and Chiricahua Apache people in New Mexico, where in the summer of 1931, as earlier noted, I was part of Ruth Benedict's Laboratory of Anthropology field party. It was a pure-science learning experience, though Blanchard (1) cites my correspondence to show that I encouraged at least one Indian to think of independent reservation development. Again, during three summers (1932 and 1934 with the Mesquakies of Iowa and 1933 visiting mainly Ojibwa communities and then settling for some weeks with the Menominee), I studied kinship systems and remained relatively "pure." In Guatemala and Mexico from 1934 to 1944 I worked on social and community problems mainly to understand the social structure and the economy but also (particularly in Chiapas) to advise colleagues working in their important development programs. It was not until the summer of 1948, however, that six graduate students getting their first fieldwork experiences under my direction—on the Mesquakie settlement near Tama, Iowa, where I had lived 14 years before—urged me to permit them to work on problems of the people as well as on the anthropological problems they had brought with them. Hitler, the War, and the Bomb had all played a part in turning me back to my earlier interest in social action and in the philosophical issues involved in the use of anthropological theory to benefit the people among whom we worked. Thus began what we have since called "action anthropology" (11, 37, 45).

The six students who were doing research on the Mesquakies and, with my permission, also trying to help their newfound friends were warned that they would be facing problems of values. Soon they found themselves divided over what they would like to see as the Indians' future, some favoring an inevitable (but, ideally, happy) assimilation and others hoping for preservation of at least some traditional values. When they returned to the university in the autumn that question remained until, in the course of discussion of substantive issues, it was one evening revealed to all that this question was not ours but the Mesquakies'—that we might only help them to see alternatives. This was perhaps the real beginning of action anthropology; the rest has been legitimizing the relationship of the concepts behind the two words. There followed excellent students and continuing seminars, with participation from other disciplines, and in 1955 a foundation grant made it possible for Robert Rietz (of the "first six") to return to the Fox for four years to help develop several programs (4; C. Tjerendsen, privately published, 1980).

In the autumn of 1949 I was asked to meet with John Provinse, then Assistant Commissioner of Indian Affairs, and Galen Weaver, director of race relations of the National Council of Churches, who were worried about the fate of the Affiliated Tribes on the Fort Berthold Reservation, soon to be flooded by the opening of the new Garrison Dam. Did congressional provisions for the Indians appear to be satisfactory? I agreed to go to Fort Berthold and took with me one of our students, Robert Merrill, a theoretical chemist turned anthropologist interested in economic development. After a few days there I left Merrill to make a thorough study; I had already seen the complex divisions within the reservation community. Merrill's full report was excellent, resulting in the appointment of Robert Rietz to a position on the ground. Rietz's work there for four years was remarkable in that what he did—in most difficult circumstances—was much appreciated by the Indian community, the Bureau of Indian Affairs, and the missionaries; indeed, his greatest problem turned out to be avoiding moving "up" to higher positions in the B.I.A.!

The next summer I accepted an invitation to teach at the University of California at Berkeley, and on our return through Navajo and Hopi country I came to a realization that changed my thinking about American Indians' future. One of our students (John Connelly), a schoolteacher among the Hopi, introduced us to the people of the Mesas. To me the Hopi had always symbolized traditionalism among isolated Indians; we "knew" them well from the work of Fred and Dorothy Eggan, and it had always seemed to me that nobody would ever say, as they said of the Mesquakies, that *these* people were about to disappear into the melting pot. Now, seeing them for the first time in their isolated habitat, I suddenly recognized that they were no different from the Mesquakies or from any of the eastern Indians with whom I had visited. People in Iowa told the Mesquakies that they were temporary; nobody seemed to be saying that to the Hopi. But the Hopi seemed to me no more permanently Indian than the Mesquakies—or the Penobscot or the Iroquois. And I had to conclude that we had no right to count out any of the peoples that are with us today—that existing notions about acculturation and assimilation were not only harmful but quite possibly mistaken. During the next year examination of the data and discussion with our students convinced me of the validity of this simple shift in perception, and three of us prepared papers, given in March of 1952 at the annual meeting of the Central States Anthropological Society, arguing as clearly as we could that the assimilation of our Indian communities was not inevitable. So fixed were Americans on the notion of the melting pot, however, that the first 45 minutes of the discussion that followed was at cross-purposes. Our colleagues took us to be saying that acculturation was proceeding more slowly than we had assumed, and it took that long before they realized that we were saying, rather, that it might *never* occur. The misunderstanding by sophisticated anthropologists was itself a significant cultural datum.

A month or two after this meeting I attended a conference in Washington sponsored by the Association on American Indian Affairs. The topic was assimilation, and the conference was intended to persuade Congress and the B.I.A. that they should slow down the increasingly destructive withdrawal of federal services. For the occasion tribal leaders were brought from Western reservations. The meeting was introduced with the argument that the Indian people would of course eventually assimilate but meanwhile needed help. Then, one after another were introduced the Indian leaders, and each in turn repeated this theme in his own way. When they had finished, I rose and said simply that there was no scientific evidence that the Indian tribes would indeed assimilate and to say that they would expressed the damaging value judgment that those who did so could be called progressive and the others backward. Then each of the Indians rose and recanted vehemently, and a motion was made to change the conference topic from "assimilation" to "integration." This happened so quickly and definitively that I had to conclude that I had been the boy who said that the Emperor wore no clothes.

The melting-pot theory with respect to European immigrants has been weakened since the 1930s by a sentimental third-generation "return"; but Indian *peoples*, in contrast to immigrants from Europe and some individual Indians, have rarely doubted their survival. Indeed, Indians sometimes express a belief that it is only their continuing presence that has protected all of us from destruction. It puzzles me that we anthropologists—even though most of us are immigrants who have ourselves melted into the pot—could have fallen in any degree into what has been an error hurtful to our Indian friends. It is true that acculturation was rapidly changing at least the externals of our ethnographic data-base, and museum-affiliated anthropologists may have thought more than we do today of anthropology as the study of man's *works*. They themselves did not confuse artifacts with people; but the emphasis on the one against the anonymity of the other may well have set in motion the idea that the people were disappearing. If so, perhaps it is no accident that a correction had to begin with anthropologists who study the interrelations of people, requiring understanding of their ideas and emotions. Such a correction would become most necessary when anthropologists came to try not only to understand but to help to improve.

I have seen at least one Indian carrying a copy of the United States Constitution with every mention of Indians marked heavily for reference. Indians understand that they had some protection against their neighbors when the Crown was across the ocean that was lost with the Revolution. The Constitution, enforced in federal courts, became their only protection, providing as it did for the making of treaties that remain today the symbol of their special status. The struggle has been to protect these rights against a Bureau of Indian Affairs that might still carry traditions from its time in the War Department and a powerful Congress whose constituents often compete for resources with tribes and communities. There have rarely if ever been "good times" for Indians in relation to government, but the short-lived F.D.R.-Ickes-Collier "New Deal for Indians" had a more liberal spirit than existed before or after. It added to reservation resources in some cases and improved education and health by taking more account of local preferences; but tribal constitutions embodying majority rule rather than consensus and vesting Washington with veto powers vitiated the effort and in many cases induced what among the Mesquakies we saw as "structural paralysis." Congress, meanwhile, had its collective eye on the contrary plan to "solve the Indian problem" by forcing their assimilation into the general society. The way for this had been prepared back in 1946 by the establishment of the Indian Claims Commission, which would "once-and-for-all" compensate with cash for all the chicanery by which whites had obtained tribal lands. The stage was set for the withdrawal of services to Indian tribes and the termination of their special relations to the federal government as expressed in 1953 in House Concurrent Resolution 108 and the program of relocation of Indians to cities, followed in 1954 by Public Law 280, which transferred much jurisdiction over Indians to the states.

I had at most a vague awareness of all this when at the 1952 conference I spoke against the notion that assimilation was inevitable. (The Association had doubtless organized the meeting to counter the congressional pressures for termination of which I was ignorant; evidently an anthropologist at the grass-roots level may never learn what's up!) In any case, I soon became a vocal opponent of "termination," especially since the close-by Menominee were among the first tribes to be cast off (31). In October of 1957 I spoke at the annual convention of the National Congress of American Indians, in Claremore, Oklahoma, putting together what I thought was nationally appropriate policy concerning Indians, and it was so well received that I felt I had finally learned what tribal leaders themselves might want. Meanwhile, since 1953 Indians had been pouring into Chicago under the federal relocation program, and the American Friends Service Committee had been helping local Indians to establish a center to receive the new arrivals. Hundreds—eventually thousands—of individuals and families came from reservations, far and near, on one-way railroad tickets. Most had been recruited because they were having difficulties on the reservation, so it is not surprising that they had difficulties in the city. The federal relocation office in Chicago met their trains, provided funds for some weeks, and found them housing and jobs. Happily settled or not, the Indians were then on their own. Not surprisingly, most of them were unhappy with their housing or jobs or both and lonely in the big city. With new ones arriving, the relocation office could do nothing to help, and soon there were problems for the city's welfare agencies, which, because of the government's rules regarding privacy, could not learn even when or where new people were arriving. Nor was the Indian Center apprised of any facts, but it became a repository for the problems. Many—eventually most—of the relocatees returned to their reservations, but the Center persisted, and my students and I came to know its people well.

The Indian Center depended for funds mostly on grants and employed a variety of tradional Indian techniques that did not compromise Indian values. But it was also an institution without Indian precedent—involving people of very different cultures and even traditions of inimical relations in a strange environment. It was a new challenge for those selected for leadership, and the problems seemed insurmountable until in 1954 Robert Rietz tapered off his work with the Fox Project and became the nondirective director. Rietz helped the Indians to shape, finance, and manage the Center in as Indian a way as was possible in our complex society. In Indian fashion, too, when in 1971 Rietz died and a dissident faction took control, the ousted people quickly established a new and successful organization, the Native American Committee. Its leaders eventually also founded Native American Education Services, Inc., which became NAES College, accredited and operating in Chicago and on several reservations.

In 1956, meanwhile, Galen Weaver, who had brought us to the Fort Berthold problems, called our attention to the plight of young reservation Indians in college, who were not only lonely but embarrassed because they could not answer questions about Indian affairs. Fred Gearing and I suggested a summer-session workshop, and Colorado College agreed to house it (with academic credit provided by the University of Chicago). That summer 25 Indian students from as many tribes began—as it turned out—to make history. They not only produced a book but also discovered together their own interests and problems. The first week of the 1961 workshop was held at the American Indian Chicago Conference, where alumni of previous workshops also organized the American Indian Youth Council, important in the turbulent 1960s and still active. Eventually the workshop moved to the University of Colorado under D'Arcy McNickle's American Indian Development (on whose board I sat), and alumni themselves later developed similar workshops elsewhere.

A detailed map of the 1950 locations of Indian communities in the United States and Canada (43), distributed at the Chicago conference, showed clearly that, except for those removed to Oklahoma, almost all Indian communities still lived on parts of their aboriginal lands. It countered the myth of Indian disappearance and now provides a benchmark for understanding the movement since then of Indians to urban areas. The conference (8), with its more than 800 leaders in national Indian affairs, at least three-quarters of them Indians from all regions, religions, historical perspectives, and political persuasions, was a national media event that attracted international attention, exciting and genuine. It also appears to have been a turning point in modern American Indian history—the beginning of the end, perhaps, of the myth of the disappearance of Indians. It was probably the first time that Indians had ever been asked to express in public their collective hopes for their future. I am proud to have had a part in this, and most proud of the way we anthropologists and other friends of Indians permitted them to do what really became *their* own thing. To me it was the ultimate success—and on a large scale, too—of the philosophy and methods of action anthropology.

When we sent the *Declaration of Indian Purpose* produced by the conference to our Indian mailing list of some 4,000 names, many Indians who had attended were disappointed not to have received it; apparently not all those who came to Chicago had been on that list. On the evidence offered by this response that on the reservations mail—at least ours!—was appreciated, the Carnegie Corporation gave us funds to experiment with encouraging literacy through such mailings. For five years we then published *Indian Voices,* written mostly by Indian readers. We also had an exceedingly fruitful field project in eastern Oklahoma, where the Cherokee, who in the late 19th century had had excellent academies in which even Greek and Latin were taught, now found themselves among the least literate of people (44, 46). This was our most difficult action program but perhaps the most rewarding, as we helped these Cherokee regain some of their lost independence.

In the 1970s, when the United States Senate investigated American Indian education, Indians gave testimony in Washington, and it appears, as I read the record, that only two of them "spoke up." One was a young Mesquakie and the other a Cherokee from eastern Oklahoma. How this happened I do not know, but it seems more than a coincidence that, out of the scores of Indian communities with problems and grievances, the only Indians to question the status quo should be from two tribes that had felt our influence. Also: in September of 1968 an attorney hired by the Mesquakies telephoned to ask me to testify in federal court in Cedar Rapids, Iowa, in a suit brought against the Bureau of Indian Affairs for closing the Mesquakie day school and even selling its furniture. He said the Indians wanted "only one witness: Sol Tax." Years had passed since my last contact, but I left a conference in Princeton to fly to Cedar Rapids. In the courthouse I found the entire tribal council waiting while a settlement wholly favoring the Indians was being worked out in the judge's chambers. There was no trial, then, and we had time to reminisce;

they said that this victory had been made possible by the unity that we had always urged upon them.

There is another story that is perhaps worth telling. In *Science* (November 30, 1951) appeared a letter signed by five anthropologists, including me, who had studied the peyote religions of different Indian tribes. The letter strongly protested the campaign of "propagandists [who] argued that Peyotists are simply addicted to a narcotic . . . which they use orgiastically" and showed that the "Native American Church" was "a legitimate religious organization [using] peyote . . . sacramentally." In the summer of 1954 I was invited to attend the national convention of the Native American Church, for which the Mesquakie church was to be host. With the issue of legality in the public eye, it occurred to me that a documentary film of the entire convention might someday prove to the courts that the church deserved protection. The film, I thought, would have to include both the mundane business sessions and the drumming and praying around the sacred fire in the ceremonial tepee over the whole of Saturday night. Otis Imboden, a young filmmaker on campus, eagerly accepted my invitation to come along, and the filmmakers at the University of Iowa agreed to bring all the necessary equipment to Tama as soon as I telephoned them that the Indians had consented. The rest of the story, as I told it to colleagues 10 months later (34), follows:

> All Thursday afternoon and Friday morning we were part of the business of the convention. . . . On Thursday I explained at length and carefully the possible importance to them of the film, and the unusual good fortune that made it possible at no cost. There were questions and discussions, and a night to sleep on it. We were optimistic. The next morning the discussion resumed. Again I made explanations and answered questions. I promised that they would help edit the film and would have to approve it before its use. I said it was up to them. Then followed speech after speech, some for me and some against me. It became clear that everybody thoroughly understood that the film, perhaps shown as evidence in court, could someday establish them as a legitimate religion and peyote as the sacrament they felt it to be; otherwise the church seemed to them in danger. The rub came in the prospect of filming their sacred ceremony. The ritual itself would be inevitably disturbed by technical problems, but perhaps more important they could not picture themselves engaged in the very personal matter of prayer in front of a camera. As one after another expressed their views, pro and con, the tension heightened. To defile a single ritual to save the church became the stated issue, and none of them tried to avoid it. Not a person argued that perhaps the church was not in as great danger as they thought; there was no suggestion of distrust of me; they seemed to accept the dilemma as posed, as though they were acting out a Greek tragedy. I sat in front with the president and his wife, facing the assembly. Fascinated, I listened to the speeches, and gradually the realization came that they were choosing their integrity over their existence; that although these were the more politically oriented members of the church, they could not sacrifice a longed-for and sacred night of prayer. When everybody else had spoken, the president spoke, and said if the others wished to have the movie made he had no objections; but he begged then to be excused from the ceremony. Of course this ended the movie, and the sense of the meeting was clear. It was over, and then the realization seemed to come over the Indians that I must be hurt; for all my good and unselfish intentions, and high hopes, and hard work—my reward, and Otis's,

was a clear rebuff. They had suffered through their dilemma, and had made the painful choice that should have relieved their tension. But they realized now that their peace with themselves had been bought at our expense, and they began speeches painfully to make amends.

They were wrong, of course. As their decision was being made I understood that what I had proposed was akin to asking a man to deliver his wife to a lecherous creditor to save the family from ruin. Now, therefore, I arose to speak, and could with genuine sincerity apologize for having brought so painful an issue to them. I had meant to be a friend, but had hurt them. I agreed with their decision. I would be a poor friend indeed if I resented their deciding an issue for their own good.

Relief was great; the euphoria was instantly restored; and it was evident then and in the days that followed that they were more genuinely grateful to me than any Indians had ever been to me for any material or moral help, and felt closer rapport with me.

What I learned that day about the Peyotists' view of their ceremony, about the nature of group discussion in an Indian assembly faced with a real issue, and about the sensitivity of Indians to a situation of aggression against an individual could come in other ways. But never I believe so convincingly. In anthropology we can't prove interpretations of the behavior we see; but in this incident I was so overwhelmingly convinced—and so by the way was my young friend Otis—as to remove the doubt to quite another level.

In concluding this account of my relations to North American Indians, I must say that I am always embarrassed to be thought knowledgeable of their ethnology and history. True, for four or five years as a student, I absorbed a great deal of specific knowledge about many groups on some topics and always tried to "keep up," but since I did not teach courses in the subject and did so many other things, this was a losing battle. What I *do* have is a sense of what Indian people feel about themselves and about us. 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.

Many Indians, then, have given me friendship that I want to return. Is that enough to explain these nearly 60 years of trying to be helpful? There is satisfaction and pleasure in any chance to help a friend, and from childhood I sought such opportunities. The associated emotion I knew was anger at injustice—the wrong schoolmate punished, the child beaten by a bully. But I grew up to see that, bad as were the individual acts of injustice that bred in me flashes of anger and dismay, it was *social* injustice that deeply pained the heart and bred lasting resentment. From my first summer at Mescalero, with people giving us firsthand accounts of remembered lives of independence, I could feel the rancor of their loss. And as I came to understand that it was so with all the Native American peoples that I learned to name, and to see the administrative follies that constrained them, I could not but share their dismay. What little I might do to understand those bonds and perhaps to loosen them I had to do. But I soon learned that ultimate success or failure on my part for them, or theirs for themselves, was no measure of the pleasure of the effort, and so I have kept on supporting their long struggle.

Explanations

Since it is the business of an anthropologist to add to and interpret the world's store of anthropological knowledge and to teach it to others, I need now to explain how I permitted myself to engage in the apparently extraneous activities of which those just described are only a sample. Clearly, the first answer is that I have rarely done any of these things alone. But that is no answer; either enlisting others in a cooperative venture or managing helpers takes time and energy. I presume that the list is long because I have always found it difficult to resist what I hope will be opportunities. Since it appears that I have been a willing victim, the question arises how, with a full load of teaching, administration, and research, I managed so many additional things. Two personal characteristics may help explain this.

The first is an almost lifelong reluctance to do only one thing at a time. As a child given routine tasks such as stuffing envelopes for an advertising campaign for my father's business, I took to choosing a subject about which to daydream while my hands did the work. Later, as a newspaper carrier on a regular route I easily delivered my papers with my mind far away. When, still later, I was a telegraph-company messenger I often wondered how I managed to make six or eight or a dozen miscellaneous pickups and deliveries on the busy streets of downtown Milwaukee and return to the office with all the proper receipts and messages in my cap and without any remembrance of the journey. Even today I feel let down when, unable to find at least one other thing, I have to do the single one that is necessary. Foolish as this habit is, it may account for the relative ease with which I have combined (for example) publication with conferences—even congresses. A related tendency is willingness to let things happen or grow—a hospitality to other people's ideas, desires, and needs. Another obvious tendency—shared with most people—is a desire to improve institutions, and in the course of time I have put whatever inventive skills I have to work on institutions with which I have had dealings.

The second characteristic I should mention is a deliberate indecisiveness that comes in part, I suppose, from my generally relativistic philosophy but more specifically from my immersion in action anthropology. At its best, it is "Look before you leap"—learning as much as possible before acting, playing a waiting game when the choices are poor and the consequences of a mistake perhaps irreversible. This all seems wise. But it can also be interpreted as reversing the proverbial expression to read "Never do today what can safely be put off until tomorrow," with the word "safely" always to be questioned. For better or worse, the practice has become habitual with me. It grew out of trying to satisfy the needs and wants of persons who were not sure what they wanted; "trial and error" takes times and patience. But I think that I began to use it also in my personal life, and, noting that I myself was often not sure what I wanted, I became more confident in delaying actions on behalf of others. When working with people, it became my peculiar virtue to be able to accept and quickly internalize and recombine the ideas of others. I recall an extreme case in which, after a lecture in New York to public health workers who had to take into account cultural differences, a number of nondisputatious comments began by referring to something I had said. I was surprised by several that I did not recognize as mine and wished I *had* said! The experience caused me to wonder if indeed I had misunderstood myself. I was reminded of the story of the Chinese philosopher who dreamt that he was a butterfly and ever after wondered if he might not be a butterfly dreaming he was a man.

I have also been influenced by the philosophy and method implied in the way North American Indian communities operate. The distinction between war-party (or hunting-party) behavior and the way decisions are taken and policy made in community affairs is striking. When immediate action appears necessary, followers by definition are committed to a leader they have chosen. I have rarely, if ever, been a leader in that sense. Political leaders of an Indian community, often called "peace chiefs," are successful only if they satisfy the perceived needs and wishes of the people they serve. It is that sort of model that includes anthropologists interested in helping communities long suspicious of officious outsiders who threatened their valued ways and their independence. If I did not actually learn from Indians how to operate helpfully without exercising undue personal influence, they strongly reinforced propensities already there. "Ideas" in profusion come to me, and I express them quickly and enthusiastically and may even appear to be promoting them; but I am saved by my correspondingly quick willingness to accept their rejection happily. Indeed, my "ideas" seem only to suggest others better suited to the situation. Although prone to seek answers to problems posed, I have never been a "planner" in the sense of one who sets goals and then determines means to achieve them. We explicitly rejected this means-end model of thought and action in the Fox Project (3, 10), and though I had doubtless assumed it as part of our common definition of intelligent behavior, I doubt if it had commonly governed my own thought and action even before.

It occurs to me that although I have lived through history's greatest changes in technological knowledge, they have not destroyed what I take to be a conviction that continuities are at least equally important. I have mentioned continuities in anthropology and of beliefs and motives in my own life. I recall also that I continually emphasized the continuities in the Guatemalan Indian villages and, later, stressed the persistence of North American Indian peoples and cultures. So I come back to fieldwork and to the stuff of anthropology, where I would like to leave the reader. But I cannot without first anticipating what colleagues might wonder: Do I have no regrets? Perhaps I am expected to ask at least about my priorities, but that sort of question implies that I would on principle violate another that I hold dear. I must act at any time according to the situation as I understand it; so clearly I am not the kind of person to set up priorities in advance. But perhaps the question is whether I regret having spent so much time in some ways that I had no time for other things that I might have done. Yes, of course, set aside was my schoolboy ideal of pure science as a means for solving the world's major problems. Even in my first fieldwork in Guatemala I recognized that devoting time to applied science could slow the building of the structure of knowledge that might make application feasible (21), but eventually—as I dealt with human problems themselves—I found myself incapable of giving up present smaller goods for possibly larger ones beyond some horizon. Ironically, however, I fear that my two major contributions to pure science have not served anthropology as theoretically they should have, and I cannot have too many regrets at having sacrificed to "good works" other such abortive successes.

In Guatemala, in happy association with Redfield, I came to understand a kind of society that I described in two complementary books (28, 40) and an article (20). Doubtless it is my fault, but it turns out also that in the end anthropology lost its curiosity about the kinds of types this work explored. *Penny Capitalism* became very influential in economics as presenting what was the first quantitative study of an economy like that conceived but never seen in Adam Smith's Europe, but without capital firms or technology that would permit economic growth (14). I wrote in the preface, "My work falls short of an ideal because I had no model. Here is a pattern from which others may depart." But I should not have been surprised when my colleague the Nobel laureate T. W. Schultz asked me in recent years why it was that other anthropologists had not made similar studies elsewhere. Presumably the answer is that we all like to pursue new problems and methods. But there is also the question whether I would have served science better had I myself pursued the method elsewhere instead of spending my energy on the activities I have described.

The other case is perhaps more interesting because it concerns my first professional work, on the most traditional of anthropological subjects: kinship. My PhD thesis (16) included what I thought was a major breakthrough in studies of kinship terminology. Having noted the sporadic distributions of Omaha and Crow systems (17) I required a theory more widely applicable than any then extant theory of kinship. In working it out I hit upon the sort of structural equivalences (which I called "rules") that Floyd Lounsbury would rediscover as "transformational analysis" some 30 years later. I saw utterly no reference to this until Alan Coult (2) discussed my "rules" in a paper that began "It is commonplace in the history of ideas that certain concepts are too advanced for their time and thereby fail to be accepted, although at a later time their rediscovery is accepted with great enthusiasm." By then I was far too busy with other matters to ask his views on my general theory (and he died soon after), and when, 15 years later, Scheffler (12) made use of the "rules" I had developed, again there was no mention of the theory. Indeed, I would never have heard a word about it had not Greenberg in 1980 called my 1937 article "perhaps the wisest article on kinship ever written" (4).

It happens that in the examination of my PhD thesis back in 1934, the psychologically oriented political scientist Harold Lasswell shocked the anthropologists present by putting to me the ad hominem question "How did you become so antitheoretical?" Since I considered myself a theorist objecting to some of the bad theories that preceded my own, I found this a difficult question to answer, but I never forgot it. Only now do I realize what he might have meant. My explanation of how kinship systems operate in small societies requires intimate knowledge of the intrafamily relations within which the "rules" that I had noticed and systematized were observed; and my explanation involved the ways in which in such societies solutions to problems by individuals can become institutionalized changes. It occurs to me now that if, when Charles Darwin returned to England after his voyage on the *Beagle,* there had not been a belief in Creation, he would have tried to explain the variations in plants and animals he noted and described the processes of adaptation to different environments, showing how individuals with characteristics valuable in changing environments were likely to survive and reproduce more than others. Without the alternative of the biblical Creation, he might very well never have needed the term "evolution" to explain what he saw. Would one then have described his explanation as "a theory"? By analogy, to work through my explanations of changing kinship systems would have required detailed study of variations in kinship behavior in the variety of circumstances to which they could be related in living societies all over the world—a formidable task indeed.

I must, then, forgive us all for bypassing the problem, and I cannot feel myself seriously at fault for choosing problems that people themselves—rather than theoretical anthropologists—feel. It is with the most significant of these human problems that I conclude these comments: the general malaise of most of our people. We feel responsibility for both the increasing economic interdependence of all the nations of the world and the danger to the entire species of any major war with nuclear weapons. It is only the most recent eight millenia of evolution that have brought us to this pass. Every anthropologist knows that the millions still living in small communities—some even in hunting-gathering kinship societies—show that basically humans want more than anything to control their lives and destinies. There are signs that many even in complex urban societies would choose to use our new technologies to help form smaller communities that together we could control, thus beginning a new evolution to replace the course subverted in the Neolithic when villages became subject to nation-states. There is a peaceful and legal model for converting the localities in which we live and work into voluntary communities bound together electronically (38). It happens that in the United States the model size of localities is the congressional district. Thus localities do not cross state lines, and statistics are provided in the census. Each has its representatives in the state legislature as well as in Congress. Any group of residents and organizations can form an educational-philanthropic community open to all and seek support from the existing bodies that collect their taxes. Beginning perhaps in the states of the Northeast, Midwest, and Far West, scores could organize within weeks or months and provide examples for others. The model would spread most quickly in Europe, perhaps, but some such voluntary communities would certainly appear in Asia and Africa as well, everywhere adapted to local conditions. A fresh start now—with our new technologies and our need for community as well as survival—might well permit an evolution fast enough to dissolve the warring nation-states before they dissolve us all.

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