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The Fox Project: A Reappraisal [and Comments and Reply]

Author(s): Douglas E. Foley, Raymond Apthorpe, Regna Darnell, Frederick Osmond Gearing, Dell Hymes, Nancy Oestreich Lurie, Robert A. Rubinstein, Murray L. Wax

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The Fox Project

A Reappraisal

by Douglas E. Foley

This paper describes one instance in which Sol Tax sought to fuse applied and academic anthropology into a new anthropological practice that he called "action anthropology." The Fox Project was one of several attempts to create a "clinical science" which produced better anthropological theory as it solved pressing community problems. It broke with conventional applied anthropological projects in several ways. The action anthropologists operated with more autonomy and were, at times, very active political advocates for the tribe. None of the project's cooperative economic and social programs, popular media materials, and educational programs survived their departure, however, and only the scholarship program had a lasting impact. Moreover, the action anthropologists were not as collaborative as they claimed, and their power-brokering with whites may have added to Mesquaki political dependency.

In addition, action anthropology never broke with academic anthropology as dramatically as Tax claimed. Despite his populist rhetoric about the limits of academic anthropology, he retained considerable faith in the reigning constructs of science and cultural analysis of his time. The Fox action programs were never "natural experiments" that generated new theories of culture change. In addition, the project's ethnographic writings essentialized Mesquaki culture and failed to anticipate subsequent tribal activism. Despite its mixed legacy, it remains an intriguing experiment that suggests ways of making academic anthropology more socially and politically relevant. A full appraisal of Tax's theory of action anthropology will require revisiting his other action projects as well.

DOUGLAS E. FOLEY is Professor of Anthropology and Education at the University of Texas, Austin (Austin, Tex. 78712-1086, U.S.A.). Born in 1939, he was educated at the University of Northern Iowa (B.A., 1961) and at Stanford University (M.A., 1967; Ph.D., 1970). His research interests are race relations, ethnicity, acculturation, popular culture, and cultural studies in the United States. His publications include *From Peones to Politicos: Ethnicity and Class in a South Texas Town* (Austin: University of Texas Press, 1989), *Learning Capitalist Culture: Deep in the Heart of Texas* (Philadelphia: University of Pennsylvania Press, 1990), and *The Heartland Chronicles* (Philadelphia: University of Pennsylvania Press, 1995). The present paper was submitted 29 v 98 and accepted 18 vi 98; the final version reached the Editor's office 3 viii 98.

The Fox Project (1948–59) was one of several attempts by Sol Tax to create a "clinical science" which would produce better anthropological theory as it solved pressing community problems. It involved ethnographic work and economic, social, and educational programs conducted by Tax and his students on the Mesquaki (Fox) settlement near Tama, Iowa. It broke with conventional applied anthropological projects in several ways, and despite a mixed legacy it was an intriguing experiment that suggested ways of making academic anthropology more socially and politically relevant.

This reappraisal of the project builds upon a number of previous studies (Høyt 1963, Stucki 1967, Blanchard 1979, Polgar 1979, Washburn 1985, Rubinstein 1986, Bennett 1996), but unlike these assessments it is based on new fieldwork. From 1989 to 1994 I spent 12 months doing participant observation, interviewing, and informant work on the Mesquaki settlement. During the fieldwork, many Mesquaki old-timers shared their recollections of the project. In addition, I worked for six weeks with the Fox Project fieldnotes in the National Anthropological Archives at the Smithsonian Institution and with the Tax papers in the University of Chicago's Regenstein Library. The larger field study, entitled *The Heartland Chronicles* (Foley 1995), is an account of race relations and cultural change on the Mesquaki settlement since World War II.

During an initial interview in 1992, Tax seemed pleased that I planned to use his archives to revisit the Fox Project. He was also pleasantly surprised that I had been a high-school classmate of the Mesquaki youth during the Fox Project years. The irony of a white "native" anthropologist's following in his footsteps did not escape him. Although using the fieldnotes of any anthropologist is tricky business (Sanjek 1990), this is surely what he had in mind.

The Ideological Context of the Fox Project

Before describing the everyday practice of the Fox Project anthropologists, it is useful to locate Tax's approach in the currents of post-World War II anthropological thought. John Bennett's (1996) article introduces Tax's disenchantment with American applied anthropology as paternalistic and mechanistic. Bennett characterizes Tax ideologically as a dedicated political liberal, humanist, and "populist" with a "voluntaristic approach." Both Bennett and Blanchard (1979) stress how much Tax's youthful experiences with dogmatic Marxists and later experiences with elitist academic anthropologists shaped his worldview. Bennett claims that Tax broke decisively with 1950s-style applied anthropology and anticipated, to a degree, the "radical critiques" of the 1960s and 1970s. He portrays Tax as "a kind of contained rebel, distrusting the establishment but at the same time working for it and trying to improve it" (p. 38). Tax portrayed himself in this way during our interviews and in his final retrospective *Annual Review* piece (Tax 1989).

Bennett and several other commentators (Rubinstein 1986, Lurie 1979, Ablon 1979, Polgar 1979) make a reasonable case that action anthropology broke significantly with applied anthropology in the 1950s. Tax generally eschewed serving the managerial interests of colonial, business, and government agents. He saw action anthropologists as community organizers financing their activities with independent academic grants. As we shall see, the project operated far too autonomously from all governmental influences to be a typical 1950s applied anthropology project. It also sought to produce more definitive ethnographic accounts of Mesquaki culture, politics, and acculturation than most applied anthropologists do. Tax's action anthropology was clearly different in several important ways.

Unfortunately, earlier appraisals, which rely heavily on the project's official history and public presentations, do not sufficiently interrogate the project's everyday practices and final ethnographic products. Consequently, these assessments either tend to reproduce the action anthropologists' own idealized view of their practice or are excessively critical (e.g., Hoyt 1961 and n.d.; Washburn 1985). If we are to understand what Tax's action anthropology looked like on the Mesquaki settlement, we must examine its everyday practice and ethnographic products more carefully. Before presenting a brief history of the Fox Project, however, I would like to characterize the notion of science that underpinned action anthropology.

Action Anthropology as a Clinical Science

One commentator on the Fox Project (Rubinstein 1986) pays particular attention to the philosophy of action anthropology. Rubinstein has recently published an interesting account of Tax-Redfield correspondence that shows how reflexive their field practices were (Rubinstein 1991). In his paper on the Fox Project, he asks why action anthropology never developed into an influential tradition or school of thought. To address that question, he reviews the documentary history, interviews Tax and examines the results of a questionnaire sent to Tax's former students. Rubinstein characterizes Tax's view of science as an open-ended process in which theories are taken to be more or less useful rather than true or false. He notes the marked similarity of this view to that of American pragmatists like John Dewey.

Rubinstein concludes that at least philosophically, action anthropology anticipates some contemporary postpositivist, postmodern critiques of anthropological practice. Earlier commentators (Hinshaw 1979) were even bolder in their portrayal of Tax as an innovator and progressive force in anthropology. Nancy Lurie (1979) argues that by the late 1970s the premises and practice of action anthropology had become indistinguishable from modern anthropology. Rubinstein's (1986) survey of Tax's former students concurs with these assessments. Approximately 80% of his students regarded him as an inspiring, creative, open-minded, democratic,

and committed mentor. Although he had his detractors, many colleagues admired him and his efforts to reform academic anthropology.

Tax may well have thought of himself as a philosophical pragmatist. His notion of science does bear a family resemblance to the pragmatist critique of science (Bernstein 1983). In Tax's ideal anthropological world, theories emerged not from the lonely ruminations of academic philosopher-kings but from intense democratic, collaborative, open-ended engagements with those being studied. Theories emerged from the scientist's trial-and-error effort to understand and improve the world. Unfortunately, Tax left no sustained, systematic treatise on the philosophical foundations of his thought. His formal writings on action anthropology as a science include an undergraduate honor's thesis (see Blanchard 1979) and several short talks (Tax 1952, 1975). Besides these few documents, a key basis for portraying Tax as a pragmatist seems to be the casual references to pragmatism in the documentary history and the reflections of other action anthropologists (Peattie 1968, Polgar 1979).

In his writings and talks, Tax relied heavily on the metaphor of "clinical science" to distinguish what he did from "pure science." He saw himself as operating more like a clinician than like a natural scientist. The science of action anthropology was "clinical" in the sense that its theoretical pronouncements were more like "diagnosis" than like scientific predictions. Its theory sprang from what worked in practice. Moreover, clinicians never claim to have discovered some ultimate truth, and they invariably operate on provisional, partial knowledge. The notion of a clinical science also implies that the scientist, after diagnosing the social problem, tries to "cure" or ameliorate it. Such a scientist abandons a value-neutral position to find out what the community wants cured or changed.

Clearly, Tax's notion of a clinical science challenged the positivist notion of social scientists as neutral, emotionally detached, objective recorders of social facts. To a degree, Tax shared some philosophical terrain with the Frankfort School's critique of science. In the 1950s, it too called for a science based on social values rather than on a technocratic logic (Jay 1973). Despite such surface similarities, however, the Frankfort School developed a more systematic critique of science as ideology than American liberal social scientists such as Tax. As we shall see, for all his populist rhetoric Tax ended up embracing many of his era's notions of science, culture, structure, community, and representation.

A Local History of the Fox Project

SETTING UP A FIELD SCHOOL ON THE MESQUAKI SETTLEMENT

The Mesquakis (or Fox, as earlier anthropologists called them), an Algonquian-speaking Woodlands tribe of approximately 5,000, lived in Wisconsin until the 1740s. To preserve their fur trade with the Sioux, the French

reduced the Mesquakis to several hundred and drove their remnants into Iowa (Edmonds and Peyser 1993). After the Black Hawk Wars of 1843, the U.S. government removed the Mesquakis to Kansas, but several hundred tribesmen abandoned their 50,000-acre reservation there and returned to Iowa in 1857. Using allotments saved during the Kansas years, they purchased an old campsite of 80 acres in Central Iowa near the small town of Tama (3,000), which is my hometown. Today, the Mesquakis are a growing tribe (1,500) living on an expanding settlement of 6,000 wooded acres.

The Mesquakis have been subject to intense anthropological investigation since the early 1900s (Michelson 1925, Jones 1939, Hagan 1958, Gearing, Netting, and Peattie 1960, Joffe 1963, Gearing 1970, McTaggart 1976, Torrence and Hobbs 1989, Goddard 1990). Sol Tax entered the picture in the early 1930s, when he was completing his doctoral thesis on Omaha kinship systems (Tax 1935). In 1948, having finished studies of cultural change in Guatemala and Southwestern Indian communities, he returned to the Mesquaki settlement to set up an anthropological field school.

At least one commentator (Washburn 1985) has criticized Tax for creating a training school and the Fox Project without the consent of the elected tribal council. Tax acknowledged to me that he had never sought or received such official approval. Instead, he recollected trying out the idea of a field school on several old Mesquaki friends and informants. After several tribal leaders had expressed enthusiasm, he informed the Bureau of Indian Affairs (BIA) of his plan to set up a field school and action project on the settlement. BIA officials then arranged for summer housing in the settlement's school. The Fox Project began in the summer of 1948 with 6 students—Lloyd Fallers, Lisa Redfield Peattie, Walter Miller, Robert Rietz, Davinda Wolffson, and Grace Gedys. They were the first of 35 graduate students, primarily from the University of Chicago but from the University of Iowa as well, who spent at least one summer at the field school.

The fieldnotes and letters to Tax of members of this initial group were full of reflections on what they called the "values question." The original action anthropologists wanted to challenge the 1950s notion of a value-neutral social science that simply recorded social reality. They wanted to collaborate with the Mesquakis in studying things that mattered to the tribe and would improve their lives. In the fall of 1948 the students returned to Tax's seminars and urged him to turn the field school into action projects with and for the Mesquakis. Tax remembered this first group as exceptional and especially innovative at articulating a new kind of anthropological practice. As we shall see, this group exerted a lasting intellectual influence on the project.

PUBLICIZING ACTION ANTHROPOLOGY TO COLLEAGUES

Within three years of initiating the project, Tax and his staff began publicizing it through various professional

presentations and writings (Tax 1952). The official Fox Project history was not published, however, until the project ended in 1958 (Gearing, Netting, and Peattie 1960). The project history includes transcripts of most of these initial presentations at anthropological conferences in the early and mid-1950s. Although unpolished and a tedious read, it is a comprehensive portrayal of the project's chronology, theory, and general development. It chronicles the project's high and low points with a mixture of primary documents, theoretical papers, proposals, correspondence, and staff evaluations. The documentary history is more extensive and frank than an applied anthropology program evaluation but less coherent and theoretical than an academic ethnography of the project. Indeed, its hybrid narrative style, neither ethnography nor program evaluation, aptly signifies the project's hybrid philosophy and practice.

After the first group of students had spent the summer in the school, it became obvious that the project needed a more permanent housing facility. Since the settlement had no available rental housing, the University of Chicago purchased a large farmhouse and 36 acres on the edge of it. Thereafter the students lived in the house, under rather spartan, dormitory-style conditions. During the summers up to six students arrived to do fieldwork. In the winter months far fewer students were present, and this apparently necessitated a caretaking arrangement with a Mesquaki neighbor. Students did most of their own cooking, shopping, and cleaning, but one woman from the caretaker's family remembers assisting with various domestic chores. Most Mesquakis remember the house as a lively, friendly place, a view that one gets from the diaries of students as well.

Although located on the edge of the settlement, the University of Chicago house was on the main road within easy biking and walking distance of most Mesquaki houses. The field diaries describe hosting several social events for various Mesquaki community organizations or groups. I do not recall seeing a station wagon with "University of Chicago" emblazoned on its doors, but Mesquakis apparently hitched many rides in it to my hometown. In addition, they frequently used the anthropologists' phone, there being none on the settlement, to make personal calls. The University of Chicago house apparently became a kind of informal hang-out or social center for the youth and veterans involved in the Fox Project. This was partly by design and perhaps partly by the fortuity of their communication and transportation technology.

PHASE I, 1948–53: DIAGNOSING PROBLEMS AND CREATING ACTION PROGRAMS

The documentary history portrays the Fox Project as going through two basic developmental phases. The first phase is characterized as a time of initial experimentation and clarification of action goals. A great deal of energy went into resolving the "values question," and the traditional model of a value-neutral science was found

wanting, as was the traditional means-ends approach of applied anthropology. Both of these models of science were abandoned for the previously described clinical approach, which participants felt was more democratic and open-ended. Phase 1 was also a time of intense ethnographic work and "diagnosis" of the Mesquakis' major social problems. The initial students were struck by three of these.

First, they concluded that the heavy-handed BIA had helped destroy the legitimate, hereditary Mesquaki political system. Consequently, the tribal council was an ineffectual, dependent group of assimilated Indians who barely administered a seriously divided, leaderless, poverty-stricken community. In short, the older generation of traditional leaders no longer provided the tribe with the type of leadership that was built upon harmony, cooperation, and consensus. Stripped of their power and authority, they were left with a dickering role in cajoling favors from powerful whites.

Second, they were impressed with the large number of angry, alienated World War II veterans, who were underemployed and prone to alcohol abuse and, worse still, exercised a negative influence on their younger brothers and sisters. The settlement was also experiencing a rise in adolescent vandalism, ganglike behavior, and low school performance. In short, the younger generation seemed to lack the qualities needed to rebuild and revitalize a dysfunctional, drifting Mesquaki society.

Third, they also encountered a virulent strain of racism in the local white community. Mainstream whites generally felt that the federal reservation system had ruined the noble red man and his ancient culture. They saw the Mesquakis as a dependent community of "welfare freeloaders" who were no better than "white trash." They felt that the only way to save the Mesquakis was through complete assimilation. Prominent local whites wanted to transform the Mesquakis into individualistic, competitive, productive white men. In this regard, the local white elite was in full agreement with the Indian Service's 1950s policy of terminating the reservation system.

Given their "diagnosis" of the problems facing the tribe, these young action anthropologists wanted to address the immediate psychological and social needs of the veterans and youth. They also wanted to address the larger problems of tribal factionalism and leadership and of white racist and assimilationist attitudes. Initially, the project worked with the veterans on the emotionally charged issue of alcohol use. During World War II, nearly every able-bodied Mesquaki male between 18 and 35 had joined the armed forces. Upon their return, they were prohibited from drinking in the local Veterans of Foreign Wars (VFW) Club and the local bars. The action anthropologists quickly discovered that the right to drink publicly was a burning issue among young adult Mesquakis. They saw the need for a veterans' meeting place where young Mesquaki males could come together and socialize, play cards, form sports teams, and watch TV. They hoped that such a place

might help ex-soldiers readjust to the harsh realities of civilian life and local racism.

They also initiated a recreation project with the youth in the hope of addressing youthful vandalism and low school performance. The youth project was a curious mixture of social activities and formal research interviews. Several field journals mention youth dropping in, playing softball, and hanging out at the house. On the research side, Steve Polgar and Adrian Brunel, an undergraduate from Barnard, interviewed a number of the young men and women. The boys' responses seem marked by evasive answers and a good deal of horsing around. Nevertheless, Polgar (1960) wrote an account of "gangs" that highlighted a growing biculturalism or cultural pluralism among Mesquaki youth.

In addition, the project initiated two cooperative economic programs on the University of Chicago's land. Some of the men were organized into a farming coop, and a number of high-school girls were organized into a vegetable garden co-op. The documentary history openly portrays these initial social and economic programs as short-lived, but Robert Rietz (Fox Project archives, box 15) continued to consider such cooperative projects necessary to build civic trust and leadership skills. As we shall see, the action anthropologists hoped to rebuild the broken political structures and psychological spirits of the Mesquakis through cooperative social and economic projects.

According to Fox Project lore, near the end of phase 1 Tax had a great epiphany that reshaped the project. The Tax epiphany tale begins occurring regularly in his public presentations on the project around 1952. During our 1992 interview he was still telling it with considerable enthusiasm. As a young anthropologist he had been surprised by the traditionalism he encountered on the Mesquaki settlement in the 1930s. Some years later, visiting the "conservative" Pueblo societies, he had been struck by their similarities with the allegedly "acculturating" Mesquakis. Over time, his students reported similar findings among other tribes. All this finally made him question the reigning anthropological theories of acculturation. One day, he suddenly realized what the public role of anthropologists should be: "After a good deal of experience with other tribes, it finally hit me that *anthropology* had to take the lead in countering an idea that we had helped perpetuate for years!" Thus, according to the official history, a new theory of assimilation and focus on changing white attitudes was born.

PHASE 2, 1954-59: CHANGING WHITE ATTITUDES AND COPING WITH FACTIONALISM

During its second phase, the Fox Project tried to use popular media materials and an educational and a cooperative economic program to alter white attitudes about the Mesquakis. The project also stated more forcefully the need to be highly collaborative and create programs that the Mesquakis valued and managed. The action anthropologists were to play the role of catalysts and tem-

porary leaders. At this point they were telling anthropological colleagues that they had resolved the values question by aligning themselves with the needs and will of the community. At the same time, they strongly advocated working outside all established power structures other than the university. They saw themselves as independently funded, autonomous social-change agents free from governmental and corporate constraints.

The popular media projects included a series of articles published in the local paper and a pamphlet. Project director Gearing wrote most of the materials, and they were apparently revised and edited in Tax's graduate seminars. Mesquakis seem to have played little if any role in producing them. The articles were designed to convince whites that the Mesquakis were a traditional people who were not going to disappear culturally. They generally portrayed the Mesquakis in a romantic, essentializing manner as a gentle, circumspect, humble, communal, cooperative, religious people (Foley 1995).

How Mesquakis and whites responded to these materials is difficult to assess. The project never systematically surveyed local opinion on any of its popular media materials, but the fieldnotes are suggestive. In the 1950s the tribe generally had little inclination to represent itself to whites beyond the assimilationist-oriented local news columns of a former tribal chairman, George Youngbear (Foley 1995). Consequently, the newspaper articles angered Mesquakis who were less inclined to represent themselves as "traditional Indians." Moreover, the fieldnotes recount a controversy over a pamphlet on Mesquaki history and culture that the project hoped to distribute through the Iowa Women's Club. Various tribal council members apparently objected to selling the pamphlet at the powwow because, among other things, it contained photos of "typical Mesquakis" who were not actually tribal members. Local white views of the articles and pamphlet, recorded impressionistically in the fieldnotes, ranged from curious and positive to negative and stereotypic.

The other major media project, a TV short to advertise the powwow, was planned but never completed. The transcripts of the planning meetings provide a fascinating look at how difficult it was to produce accessible popular cultural materials. The idea was to elicit Mesquaki and white views on representing Mesquaki culture and then produce the materials. The meeting with the Mesquakis was marked by an unvarying pattern: The anthropologists suggested various cultural and historical topics, and the Mesquakis responded with silence or evasive answers. They expressed little interest in educating or informing the whites through any media production.

The two meetings with local whites went very differently. During the first meeting the anthropologists gave mini-lectures on various historical events and cultural practices, and the whites responded with a mixture of ignorance, good intentions, and stereotypes about drunken, lazy, permissive Indians. For example, when the action anthropologists recounted a graphic story

about a Mesquaki woman dying under the care of the local doctor, the whites implored the anthropologists not to put that story on TV. Unlike the Mesquakis, the local whites were quite willing to help develop media material on the Mesquakis, but they wanted to emphasize Indian art, the adoption ceremony, and examples of educational accomplishment. They wanted to portray peaceful, "progressive" things that "Mesquakis could be proud of," not instances of racism and oppression. After several meetings, the media project seems to have died a quiet death. Given the project's new emphasis on changing white attitudes through popular media materials, the fieldnotes are noticeably silent on the short-lived nature of these projects.

The project's second phase was also marked by several highly visible and more popular projects. By 1954 Tax was able to make his action projects more financially independent than most applied projects. He raised \$100,000 from the Swartzhaupt Foundation to create, among other things, a major scholarship program. The program ultimately sent 18 of my Mesquaki classmates to college for two to four years. The fieldnotes portray several prominent settlement families as cooperating with the recruitment efforts and visiting the project house. The interested parents regularly inquired about the status of their children's applications. They wanted to know how cooperative the local schools were, whether the money would come to them directly, what Iowa colleges and schools were acceptable, and so forth.

The program's final report indicates that 6 of the 18 students graduated from college, but this tally does not include 3 others who finished some years later. Unfortunately, approximately half of these scholarship students never returned to the settlement. This trend was particularly evident among males. A majority of the males (7 of 12) who received scholarships married whites and never returned to the settlement. This suggests that the scholarship program may have inadvertently promoted assimilation. Half of the students did return to the settlement, however, and two of the males became prominent political leaders. One of the two led walkouts against the local white schools and helped convert the Indian Service school into a tribally run school. In addition, three of the female graduates who moved away became teachers and nurses for the Indian Service.

The careers of these students suggest the general empowering effects of the scholarship program. The program seems to have launched a trend toward going to college that the GI Bill had failed to produce. In reconstructing which veterans utilized their GI educational benefits, I found that only 4 of the 47 veterans took some form of postsecondary education. In sharp contrast, 18 Mesquakis, many the younger brothers and sisters of the veterans, went to college under the Fox Project. Today, many Mesquaki administrators, who run the tribe's new welfare services and casino, are college-educated (Foley 1995). It would seem that the project's scholarship program started a trend that has produced lasting changes.

The other major project initiated during phase 2 was Tama Craft, a handicraft production project. Tama Craft was apparently the collective brainchild of Robert Rietz and the Mesquaki artist Charlie Pushetonequa. When Pushetonequa suggested that he and other Mesquaki artists would like to design and sell various painted tiles, greeting cards, and handicrafts, Rietz created a cooperative enterprise that was an improvement on the project's earlier co-ops. The Tama Craft project began in 1956 and initially sold several thousand dollars' worth of handicrafts. When Rietz left as business manager in 1959 he turned the kiln over to a family that had helped manage the project. He apparently tried to give advice and assistance from a distance, but the project quickly fell into debt and left workers unpaid. Overall, Tama Craft was more of a symbolic statement than a financial success. If nothing else, it sparked the imagination of local white businessmen. Within a year after the action anthropologists left, they were floating schemes to revive the project under their leadership.

Reinterpreting the Project's Break with Applied Anthropology

COLLABORATIVE AND COMMUNITY ORGANIZING PRACTICES

Did the action anthropologists involve Mesquakis in the implementation and leadership of various projects? Judging from the diaries, ideas for projects were talked up with key informants. When some Mesquakis became enthusiastic about a proposed project, they were involved, and the project was launched. This was particularly true of the initial all-Indian Legion hall; a number of veterans were quite involved initially. The same was true of the popular youth recreation projects; the fieldnotes show the youth eagerly participating in a variety of sports activities. In contrast, only a few farmers and artists were ever involved in the co-op and handicrafts projects. There is little evidence that Mesquakis initiated any of these projects, although Pushetonequa was clearly the creative genius of Tama Craft.

Overall, the action anthropologists were much more than simply "catalysts" or "transitional leaders," an image they often conveyed in their public academic presentations. Once programs were initiated, they seem to have done all the conceptualization and project development. They raised all of the scholarship money and set up local committees to help select the recipients. They planned, wrote, and revised all the mass media materials to represent Mesquaki culture to whites. They were the chief fund raisers and business agents for the co-op farm and the handicrafts projects. In short, they were consistently the tribe's cultural/power brokers with prominent local, state, and national whites.

Ironically, the one action project marked by extensive collaboration and activism was entirely unplanned. Toward the end of phase 1, the Indian Service sought to hand over the tribal school to state and local authori-

ties. The official history notes that the Mesquakis' swift and outspoken opposition to this plan made them seem less "passive" and more "agents" of their future. At this point, the action anthropologists gained a new respect for Mesquaki activism, and they openly joined them against the Indian Service and local whites.

During the effort to save the tribal school, the tribal council clearly sought the help of the action anthropologists and was grateful for their assistance. In response, the action anthropologists became quite active politically. Behind the scenes, Tax lobbied the assistant commissioner of the Indian Service in Washington, D.C., to stop the school transfer. He also returned to the settlement to attend various tribal council meetings with the regional Indian Service, state educational officials, and local school board members. He and his students pressed vigorously for an autonomous tribal-run school assisted by the American Friends Service Committee. This incident was clearly the action anthropologists' finest hour as political activists. They were much more like "community organizers" than like applied anthropologists employed by a government bureaucracy. During the school affair, they were technical and political advisers and brokers working under the tribal council for the tribe. This was not always the case in the other projects mentioned.

Another important outcome of the school affair bears mentioning. After several years a rift developed between the students and the elected tribal council. The official history portrays this conflict but never provides sufficient context to clarify these events. To put this conflict with the tribal council in perspective, a little Mesquaki political history is necessary.

When the action anthropologists arrived on the scene in 1948, a disgruntled Mesquaki faction called the Oldbears was pressing to reestablish its hereditary chieftainship through the courts and the Indian Service. Since 1897 the Oldbears, the Black Bear lineage of the Bear clan, had been trying to oust the Youngbear faction, the Brown Bear lineage of the Bear clan, from control of the tribal council. The Youngbears generally tended to be more cooperative with the Indian Service and local whites; consequently, the local press consistently extolled them as more "progressive" and acculturated. By 1937 the Youngbears were able to win a hotly contested election to reorganize the tribe's hereditary monarchy into an elected tribal-council system, and in 1948 the Oldbear lawsuits against the Youngbear council were still pending (Foley 1995).

The original research team quickly determined that the two rival Mesquaki political factions did not reflect acculturation differences as neatly as journalists and local whites imagined. Nevertheless, the action anthropologists seem to have been inexorably drawn to the Oldbear traditionalists. The fieldnotes convey their fascination with seeing traditional ceremonies, and like most anthropologists they sought out traditional informants to confirm the survival of the group's traditional culture. Fred Gearing was particularly close to the most outspoken traditional family and even attended several

of the Oldbear faction's meetings. Gradually, the student anthropologists began privately characterizing Tribal Chairman Ed Davenport as an assimilated, authoritarian, ineffective white-style leader who treated them like errand boys. One can hear the political rhetoric of Oldbear conservatives in these judgments, but they also contain the scholarly gloss of Walter Miller's (1955) theory of tribal leadership. Among other things, Miller contended that effective traditional Mesquaki political leaders were never authoritarian or aggressive.

The Youngbear faction definitely noticed the students' negative attitudes. Davenport shows up in several student journals making sarcastic comments that convey his discomfort with the project. How widely shared his views were is unclear. When I mentioned the conflict to several old-timers, they laughed and said, "You mean that deal with Ed?" The ire of nontraditionalists, as some old-timers said, "blew over after a time." Despite the tortured ruminations in the documentary history, the project never became a major political issue. Had sentiment against the project actually been high, the chairman or one of his allies would have initiated a petition drive to ask the project to leave, and many Mesquakis would have signed it. However, the usual political conflict-resolution mechanism of the highly democratic Mesquaki political system was never activated; no petition drives against the project ever developed.

In retrospect, the project's sternest critics (Hoyt 1963, Washburn 1985) have made far too much of the documentary history's ill-advised confessional account. Washburn seems to be grinding an ideological ax when he claims that the incident proves that action anthropology was an arrogant, unethical approach and an affront to pure science. He goes on to claim that Tax's disrespect for elected tribal governments is clear in his subsequent support of radical pan-Indian activists against traditional tribal leaders. None of Washburn's claims square with Lurie's (1979) and Ablon's (1979) accounts of Tax's general advocacy work with American Indians. Tax was far too much the political liberal to advocate confrontational tactics such as armed struggle, picketing, and rent strikes.

Washburn also seems to understand very little about the Mesquakis' political history and how their political system works. As Miller (1955) points out, Mesquakis have never accepted the idea that a tribal council, hereditary or elected, has the power to forbid talk and trade with or acceptance of gifts from outsiders. Washburn is using a European notion of power and governmental authority to chastise the project and Tax. He also conveniently forgets that the Fox Project was located off communal lands and was purely voluntary. The action anthropologists were outsiders who had come to talk and trade with the Mesquakis, to give them scholarships. Most Mesquakis would never have given their tribal council the power to disapprove of such individual exchanges with outsiders. Given these circumstances, Tax's approach of sounding out key traditionalists makes as much cultural sense as obtaining

the formal approval of a much maligned, nondivine, possibly illegitimate elected council of 1948.

In this particular instance, once the tribal council expressed some disapproval of his students' actions, Tax quickly counseled the students to avoid confrontations with the council chairman. The fieldnotes show these young anthropologists going to considerable lengths to work with the latter, and when circumstances offered them an opportunity to make amends they were quick to help the Mesquakis save their tribal school. The field diaries are full of troubled ruminations over their "action" and their "scientific" missions. No one could read these earnest reflections and come away with Washburn's view of them as unblinking, amoral ideologues. They generally come across as dedicated, idealistic young trainees trying to learn their craft and to conduct themselves ethically.

As for Tax, he often illustrated his respect for Indian rights with the story of his aborted attempt to film a Mesquaki peyote ritual (Tax 1988, Gearing, Netting, and Peattie 1960). Tax apparently dreamed up his film project without consulting the entire peyote group. When he showed up at the ceremony with a film crew in tow, several Mesquakis were taken aback. The archival transcript of the meeting shows Tax responding quickly and graciously in an open, extended discussion. Upon realizing that the Mesquakis objected to filming and sharing their sacred ceremony with whites, Tax quickly admitted his error and left. In this instance, as in the earlier tribal council incident, he was sensitive to Mesquaki criticism and quick to acknowledge his missteps. The tale also illustrates, inadvertently, the project's tendency to initiate action projects with little prior collaboration, a failing that Tax rarely acknowledged publicly.

In retrospect, action anthropologists' conference presentations to fellow anthropologists now seem a bit premature and glowing, especially considering that the project never received a systematic independent evaluation. Tax and his students defended the lack of a formal evaluation with the claim that a clinical project's goals were too diffuse, open-ended, and developmental to capture. They were also noticeably silent on how Mesquakis felt about their project. Despite their strong belief in collaboration, they made little effort to collect the views of Mesquakis.

THE FOX PROJECT FROM THE MESQUAKI PERSPECTIVE

Interviews collected by Elizabeth Hoyt (1963 and n.d.) in the late 1950s and by me in the early 1990s (Foley 1995) give us some idea of Mesquaki perspectives on the project. Hoyt, a professor of economics at nearby Iowa State University, arrived on the settlement shortly after the field school and the project ended in 1959. Her "evaluation" was the only formal attempt to collect a Mesquaki view of the project. Her unpublished monograph, dated 1963, is a grim, moralizing commentary on the settlement's growing numbers of illegitimate chil-

dren, delinquents, child abusers, alcoholics, and crimes. It is based on rather pedestrian historical, employment, and occupational data and on 1,000 essays that she collected from Mesquaki and white teenagers. Her comparative essays allegedly reveal Mesquaki youth longing for white jobs and material things but fearing failure. Like most assimilationists, she emphasizes their need to belong to a stable, supportive white society rather than to their dysfunctional traditional tribal society.

Hoyt also claims to have assessed the project's impact, if any, on Mesquaki problems and social disorganization. In a manner similar to that of a muckraking journalist, she presents the anecdotal testimony of various disgruntled tribal and Fox Project members. She reports that Mesquakis felt that the student anthropologists were always "poking into private matters" and asking their youth "strange questions." Several people are reported as saying that the University of Chicago was "making guinea pigs out of the Mesquaki." Others, who were in college when the scholarship money ran out, claimed that they had been "misled and abandoned." Still others expressed anger over the Tama Craft project and accused the action anthropologists of having let the project die, leaving orders unfilled and workers unpaid.

Hoyt portrays the project staff as plagued by dissension and made up of callous academic anthropologists who initiated ill-conceived action projects and left the poor Mesquakis by the wayside. To substantiate this portrait, Hoyt reports a good deal of gossip about individuals. Her key informant was an emotionally troubled graduate student who after leaving the project was jailed on a manslaughter charge. He apparently painted a very negative image of the project's management and of what the students were doing and learning. Hoyt also reports that one student anthropologist was romancing a Mesquaki woman, offending many. The project fieldnotes mention a University of Iowa student's flirtation with a young unwed mother, but whatever indiscretions may have occurred this never became a public issue. The students generally seem to have avoided compromising sexual relationships. The fieldnotes do convey various students' critical views of the action projects, but nothing as extreme as Hoyt's account.

From 1962 on, Hoyt seems to have made her criticism of the project into a personal crusade of sorts. What motivated her remains unclear. Initially, she circulated her manuscript to a number of Mesquakis, local whites, and anthropologists. She also became involved with a local group of white businessmen who were lobbying the former Tama Craft project director to turn the project over to them, once writing to Robert Rietz to this end (Tax papers, box 126, folder 4). She fully supported their scheme to convert an old button factory into a plant for mass-producing Indian handicrafts. Rietz initially tried to convince Hoyt that she should assume his old role as adviser and business manager, but she declined. He then told the local businessmen to take their proposals to the Mesquakis. To the action anthropologists' credit,

Tax and Rietz wanted no part of turning the Mesquakis into factory workers under the control of local whites.

Over the next two years, Hoyt continued to contact a number of Native Americanists in an effort to portray the Fox Project as a failure. By February 4, 1964, Bob Thomas was warning Tax that Hoyt, whom he characterized as an "economist out of depth in dealing with anthropological concepts," had "stirred up" various people in anthropological and Indian circles and "insulted us" and "put ammunition in the hands of people who step on Indians' innards" (Tax papers, box 126, folder 4). Meanwhile, Tax heard from Rietz that the editor of *Human Organization* had heard of resentment on the settlement against Tax and the Fox Project and was interested in Hoyt's study but had asked her to do more work with her Mesquaki informants.

Thomas, speaking as a native anthropologist with considerable comparative experience, put Hoyt's criticism in context for Rosalie Wax, a prominent Native Americanist, with the following thoughts (Tax papers, box 126, folder 4):

We should not be surprised that the Fox feel exploited. They aren't Plains warriors. They are an ingrown, threatened, ideological, repressed, close-knit, small woodlands community with a closed tribal outlook. The only way the Sac and Fox can relate to outsiders is in terms of "dependency" relationships, the same way they do to the Indian Bureau or any outside power force. Sol and the University of Chicago for years became their personal powerful whitemen who were going to protect them from all evil. There is no way the project could have avoided this.

He ends this defense of the project with the reassuring but fanciful notion that "Rietz feels certain that his Tama Craft project partially eased this problem."

Around the same time, Hoyt informed Tax that she had sent out 50 prepublication copies of her book. She stated rather boldly, "No one will like it because it shows whites failing to help Indians address their problems." She went on to chronicle local discontent with both the project and various quotations in the documentary history. Tax initially responded to her letter and manuscript with a very strong letter. He told her that she had inadvertently libeled his students, himself, and the university. He added that she had seriously misunderstood what they had tried to do and recounted their efforts to sustain the Tama Craft and scholarship programs. He also articulated a rather surprising and revealing defense of the Fox Project: "The lesson I hope you'll recognize is that even social science well-applied cannot do much . . . a physician can never be permanently successful since patients eventually die" (Tax papers, box 126, folder 3). Tax ended his letter by asking Hoyt to write all those who had copies of the report and "set the record straight."

The correspondence between Hoyt and Tax and other project members continued and grew less heated. Tax

eventually apologized for his initial response and acknowledged that the documentary history was long and hard to read and probably should not have been published, since "it was bound to hurt people." But he staunchly defended Gearing and Rietz, who had run the projects and written the history. In addition, various project members wrote Hoyt apologies and diplomatic explanations of what she had misunderstood about the project. Her key informant was also anxious to explain his role; he claimed that Hoyt had "buttonholed" him after a talk at the University of Iowa but that he had told her very little. He then implicated another student and a University of Iowa professor as her true sources of information. He stressed that even though he was highly critical of the project he had remained silent because he lacked formal data.

Ultimately, Hoyt's study was rejected by the editors of several anthropological publication series and never published. Action anthropology presentations and subsequent commentaries on the Fox Project rarely cited it. The whole Hoyt affair leaves one with the feeling that the documentary history, although more critical than the conference presentations, was also a carefully constructed positive portrayal of the project. Although Hoyt's study was empirically thin and marred by its muckraking tone, it did ask Mesquakis what they thought.

In retrospect, Hoyt's strident letters and manuscript evoked some rather revealing admissions from Tax and his students. They were forced to admit privately to Hoyt and to various colleagues that their claims for the project might have been exaggerated and premature. Rubinstein's (1986) survey of former students suggests one reasonable explanation for their having oversold the project. Several former students noted that many of Tax's colleagues and his own department undervalued applied or action projects. Doing fieldwork in nearby Tama was not nearly as prestigious as doing more theoretical studies on exotic, faraway cultures. Given a hostile political climate, the action anthropologists did what any good entrepreneur with an unpopular idea would do. Despite the early demise of many action projects and some private misgivings, they highlighted their accomplishments to win over a doubting or indifferent conservative anthropological establishment.

Not surprisingly, the Mesquakis today are still telling the same tales of lost college funds and unpaid wages, but they also say many things that Hoyt did not report. I hasten to say that the accounts I collected are hardly definitive or objective. Like all oral history, they are susceptible to memory loss and to exaggeration. Nevertheless, they provide the only insiders' perspective that we have on these matters.

Perhaps the most important thing about these recollections is their tone. They often convey a wry, humorous, detached yet generous view of the Fox Project. Most Mesquakis, if they read all the commentaries and countercommentaries, would be amused by this white man's academic debate. What older Mesquakis remem-

ber most about the Fox Project has more to do with the people than with the action projects. They remember the individual students—the one who played a mean guitar, the one who always took them to town, the one who flirted with Mesquaki women. They recall their kind acts, their special abilities, their personal eccentricities and failings in great detail.

They also remember the projects that were less fundamental in the theoretical schema of action anthropology. The recreational work with youth seems especially important to the Mesquakis. Most Mesquaki youth apparently thought of the young anthropologists as "older brothers/sisters" who kidded around with them and gave them rides to town, advice on girls/boys, tips on batting. Many old-timers recall the youth recreation projects and a host of enjoyable social activities such as trips to swimming holes, dances, and softball games. They remember Steve Polgar as an endearing young man who loved to clown around. Some recalled his strange questions about dating white girls and gangs, but no one had heard of his article on Mesquaki youth (Polgar 1960). When I gave it to several people to read, they recognized the characters but were amused that anyone would call their friendship groups "gangs."

The all-Indian Legion hall is also an enduring source of humorous tales. The aging veterans still recount how it was ruined by "drunkards" and "gossipy women." Although the center may have met some important social needs, it never became a legitimate alternative to the local white VFW. The Mesquakis were still legally prohibited from drinking in their own club, so it actually became a site of clandestine drinking. Mesquakis spoke of feeling trapped in a doubly absurd situation. To do what any white man could do they had to break both town law and settlement law. In retrospect, several old-timers say that the Legion hall was doomed from the beginning to "make the drinking issue worse."

As a Mesquaki activist of the 1960s pointed out, the Fox Project opted for a nonconfrontational social service approach rather than attacking Tama's racist white power structure. Relief on the drinking issue came only when the tribal council openly challenged the local white temperance movement and an unconstitutional, almost comic city ordinance based on a 19th-century state law that prohibited Indian drinking. In the early 1960s some whites also joined the Mesquaki initiative, and within a few months they had opened up the local white bars (Foley 1995).

The various Fox Project schemes to make Mesquakis better farmers were also recalled with considerable amusement. The BIA and local whites have been trying to make Mesquakis into white-style farmers for approximately 100 years. The Fox Project's co-ops were far less grandiose and assimilationist than past BIA schemes, but they generated little more enthusiasm or new income. Tribal members who participated in these economic development schemes told a variety of tongue-in-cheek stories about "cooperatin'" by "not showin' up for the meetin's." In these stories, the wily Mes-

quaki cleverly eludes the earnest student anthropologist and his projects.

Finally, old-timers remember Charlie Pushtonequa's beautiful designs with great pride, but they continue to tell tales about corruption and mismanagement of Tama Craft. It was easy to get the unpaid workers to rail against the Fox Project, but Mesquaki sentiments remain divided over whom to blame for the demise of Tama Craft. Some say the anthropologists, while others say the family that inherited the kiln, which burnt down mysteriously. (The fire is said to have been a sign that the family left in charge was "too greedy.") Others, who have experienced running the new multimillion-dollar casino, say that the handicrafts project was a naive, ill-conceived business scheme. In sharp contrast, several prominent whites brought out their most prized Tama Craft items and regaled me with stories of what the project could have been. White old-timers claim that if the university had allowed the local Chamber of Commerce to create a handicrafts factory, the project could have "put the town on the map."

Listening to the reflections of tribal elders, the project is obviously a tiny blip in the long flow of Mesquaki history. For the Mesquakis, whites come and go. They ask many questions and talk in an earnest, self-important manner, but little of what they say and do is of much consequence to Mesquakis. Consequently, there is little to say about the impact of action anthropology. Even though some of its efforts in education appear to have been quite successful, the project barely makes it into Mesquaki oral history. As one old-timer asked wryly, "Look around, do you see any signs that they were here?"

Reinterpreting the Project's Break with Academic Anthropology

ETHNOGRAPHIC PRACTICE AND THE THEORY-PRACTICE CONNECTION

Sol Tax argued that academic anthropology had created an artificial distinction between theory and practice. He contended that anthropologists deeply engaged in and *with* a community were in a better position to theorize cultural change and assimilation than detached scientific anthropologists. In his view, good theoretical ethnographic work was crucial for initially diagnosing social problems. Once action anthropologists had empirically studied and theorized the situation, they were in a position to create action projects to solve social problems. Moreover, the action projects were supposed to serve as laboratories for "natural experiments," generating data on cultural change and assimilation that would lead to better anthropological theories. For Tax, the systematic interplay between theory and practice was essential for producing deeper understanding and solving real-life problems.

Larry Stucki's (1967) thoughtful reading of the Fox Project history questions whether the interplay of the-

ory and practice was as systematic as Tax claimed. He observes that phase 1's ethnographic studies of tribal factionalism and leadership patterns do not seem to inform the projects of phase 2. He asks rhetorically, if the original ethnographic studies of factionalism were so good, why did the project become embroiled in factionalism and offend the tribal council? To answer this general question it is essential to understand how the project's ethnography informed its action.

A striking pattern that emerges from the fieldnotes is the intellectual prominence of the original 1948 fieldwork team. Several of the original group—Fallers, Miller, Rietz, and Peattie—and two later students, Gearing and Polgar, wrote nearly all of the seminal papers in the documentary history. They generated all of the project's portrayals of Mesquaki society, culture, and personality. Under the tutelage of Gearing and Rietz, the succeeding waves of student anthropologists adopted the founding ideas of the original ethnographers.

There were, however, several exceptions worth mentioning. For example, Marlene Furey raised some interesting reflections on Mesquaki factionalism as merely an anthropological construct, but no one picked up on this fertile suggestion. Lucinda Sangree developed a fascinating set of ruminations on Mesquaki communicative styles. She advocated a Hymesian-like ethnography-of-speaking study of communicative interactions that might have challenged the sweeping characterizations of Mesquakis as a gentle, passive, and circumspect. Larry Fugle, a University of Iowa student, wrote what some Mesquakis considered an interesting article on witchcraft (1961). He suggested that traditional religious beliefs mediated conflict and consensus in Mesquaki society, but this line of argument never threatened the reigning structural-paralysis thesis. Aside from these three commentaries, the fieldnotes contain almost no critical interrogations of the project's romantic portrayal of Mesquaki modal personality or its negative portrayal of Mesquaki community structure.

At the same time, the fieldnotes are full of critical reflections on the philosophy of action anthropology. Paul Diesing's paper on means-ends dilemmas and a value-free social science apparently sparked considerable debate (Gearing, Netting, and Peattie 1960). Steve Polgar's unpublished project evaluation and the fieldnotes of Robert Rietz and Fred Gearing are particularly rich in suggestions for making anthropology a useful, ethical science.

One reason for the general lack of theoretical ferment may have been the way the University of Chicago house functioned. As I have indicated, it seems to have been an informal tribal social center. Tribal members were constantly popping in to use the phone or to hitch rides to town in the project station wagon. Several of the student anthropologists complained about the heavy schedule of socializing and the relative lack of privacy. Students who had hoped to concentrate more on doing ethnography also complained that they were given little direction and supervision.

Another reason the ethnographic work deteriorated may have been the amount of time required to organize action projects. Several students complained that the scholarship program and Tama Craft absorbed most of their time. From 1953 on, inexperienced graduate students were running action projects that required considerable public relations, politicking, organizing, and fund raising. In addition, the project was overburdened with additional graduate students from the University of Iowa, who had their own agendas and supervising professors. Project leaders Gearing and Rietz probably had little time to mentor and guide the students' ethnographic fieldwork. In the end, many situational factors probably worked against the field school's becoming a "community of scholars" thrashing out theoretical and methodological issues. Action projects clearly took precedence over training the students to do good theoretical ethnography.

There was probably far less symbiosis between the ethnographic "diagnosis" and the "clinical" action than Tax had envisioned. The original fieldwork and theoretical models of Mesquaki culture and personality never evolved from the 1948 fieldnotes to the final ethnography in 1970 for two basic reasons. First, subsequent groups of students simply stopped doing intensive, systematic ethnography. The fieldnotes suggest that nearly all the students traveled the well-beaten path to selected informants. Their field experience involved making the rounds and chatting informally with what had become a group of "professional informants," a practice that several students in fact criticized. Consequently, the project's ethnographic enterprise quickly reached a point of diminishing returns. It was as if some invisible hand had organized thousands of hours of participant-observation and interviewing into an endless reaffirmation of the founding ideas.

Second, there is very little evidence that anyone ever systematically collected data on the action projects. They were never "natural experiments" for studying general phenomena such as acculturation, factionalism, leadership, and authority. Nor were any data collected to evaluate the program's impact on Mesquaki or white attitudes and cultural practices. The entire operation had a chaotic, discontinuous character, and the project leaders had a difficult time being both academic mentors and action anthropologists. Rather than reinforcing each other, both the academic and the action goals of the project suffered. Given the importance placed on the interplay between theory and practice, this may have been the project's most fundamental problem.

INTERPRETIVE AND REPRESENTATIONAL PRACTICES

Rubinstein (1986) rightly points out that action anthropologists challenged the notion of a positivistic, detached, value-neutral science, but he exaggerates the extent to which their interpretive practice broke with the scientific anthropological discourses of the 1950s. What he fails to mention is that action anthropologists were good structural-functionalist and modal-personality

theorists. They remained committed to writing a holistic, classical "scientific" portrayal of the cultural other. The strongest expression of this philosophical orientation can be found in their final ethnographic product, *The Face of the Fox* (Gearing 1970).

In the grand style of cultural-ethos theorists such as Redfield, Gearing postulates an ontological cultural difference between Western individualists and traditional non-Western communal peoples. He claims that whites, being Western individualists, have a "final incapacity to recognize oneself in a tribal or peasant other." They can only see themselves mirrored in the face of the Fox. Conversely, the Fox, paralyzed as an Indian community, have become "mere shells, their psyches emptied by the slow removal one by one of all the social things they could possibly be" (Gearing 1970:147-48). Gearing claims that, given their traditionalism and lack of a unique self, the Mesquakis look back at whites in an equally uncomprehending way. The estrangement between Mesquakis and whites is ultimately portrayed as an adaptation to the relentless forces of modernity.

The heavy hand of 1950s-style structural-functionalist and modal-personality thinking dominates the Fox Project's ethnographic interpretation. On one hand, the Mesquaki personality type and "ethos" is portrayed as gentle, generous, circumspect, intensely egalitarian, and nonaggressive. On the other hand, Mesquaki society and "structure" are portrayed as an aboriginal political system ruined by the Indian Service's relentless pursuit of assimilation and termination. The result is a structurally paralyzed community run by an ineffectual, illegitimate tribal council that is severely factionalized (Gearing 1970).

This model of Mesquaki culture and personality led the action anthropologists to theorize that only community organizations based on indigenous organizational and leadership practices would really work. Miller (1955) saw leadership in ancient organizations as relatively weak and powerless, in sharp contrast to that in hierarchical white organizations. It was the action anthropologists' job to help Mesquakis form new voluntary, cooperative organizations that either revived ancient forms of social organization or gently introduced more hierarchical white-style organizational and leadership styles. These new communitywide voluntary associations were supposed to transform a system of narrow kin and tribal loyalties into a more universalistic social system.

Rebuilding the social and civic basis of Mesquaki society also included rebuilding the damaged Mesquaki psyche. Steve Polgar's writings articulate this more psychological role particularly well. His notes and essay in the documentary history spell out a "therapeutic" role for action anthropologists. In his vision, cooperating in the various action anthropology projects would teach Mesquakis to feel good about their tribe and about participating in tribal affairs. Such positive collective activities would help alleviate the individualistic, self-destructive, asocial tendencies of heavy drinkers. In short, the science of social systems and of social psychology

merged in one therapeutic, clinical science that would heal organizational and psychological dysfunction.

Tax's clinical-science metaphor inadvertently retained a good deal of applied anthropology's social-engineering mentality. The action anthropologists were fairly sure that they knew what needed fixing in Mesquaki and white society. They imagined themselves as easing the psychological and organizational pain of rapid cultural change. Like Keynesian economists managing "soft landings" of inevitable recessions, they imagined themselves managing "soft cultural landings" in inevitable processes of acculturation. This dual notion of a scientific ethnographer and a therapeutic scientist is a heroic, grandiose modernist or Enlightenment view of the anthropological mission.

Moreover, history has not been particularly kind to the action anthropologists' scientific account of Mesquaki culture, politics, and personality. Their theories "predict" or anticipate very little of modern tribal history. They greatly underestimated the Mesquakis' capacity for political assertiveness when the opportunity presented itself. If any of the original action anthropologists had returned 30 years later they would have found a new generation of Mesquaki leaders who had emerged from the civil rights movement of the 1960s. These new activists began using the 1974 Indian Self-Determination Act and the 1988 Gaming Act to create greater political and economic autonomy. The tribe is now aggressively fighting racial discrimination, developing a new tribal-run school, writing its own historical, literary, and journalistic texts, and running a multimillion-dollar gaming operation and a tribal welfare state.

It would seem that the action anthropologists underestimated the tribe's cultural strength, for two reasons. First, as good clinical scientists they seem to have become fixated on the plight of Mesquaki veterans. The fieldnotes are full of sweeping psychological characterizations of their disorientation, alcohol abuse, powerlessness, and hopelessness. Their interpretation treats the self-destructive behaviors of young Mesquaki males as if they were ontological traits rather than historical cultural practices.

Working with the original fieldnotes makes it clear that many of the troubled veterans of the 1950s eventually married, settled down, and participated in traditional religious ceremonies. Despite the dire warnings of the action anthropologists, most of them eventually became stable and productive tribal citizens. Moreover, the life histories of subsequent generations reveal the same basic pattern. Most contemporary Mesquaki males from around 15 to 25 still pass through the same 10–15-year psychological minefield observed in the 1950s. Adopting an ahistorical cultural-ethos perspective, the action anthropologies tended to essentialize the behaviors they observed. A more historical view would highlight that post-World War II Mesquakis have apparently created a reasonably well-functioning extended rite of passage. Many individual Mesquakis suffer through a period of identity conflict, but ultimately most become integrated into settlement life.

Moreover, many also become adept "border crossers" who blend white and Indian cultural practices (Foley 1995).

Second, the action anthropologists seem to have misread the frequent contentious Mesquaki political debate over adopting white cultural practices. Working with functionalist notions of social systems and structure, they understood the Mesquaki political conflict between the Oldbears and the Youngbears as a sign of endemic conflict and structural paralysis. The highly democratic Mesquaki political system is marked by the traditionalists' decrying and the progressives' extolling every new white practice. Another way to understand this ongoing public debate is as a healthy political discourse marked by considerable rhetorical flourish. Seen from a discourse perspective, it is actually a sign of cultural vitality. The debate signals that the Mesquakis are much more aware of and engaged in conceptualizing and managing their rate of acculturation than the action anthropologists realized. In effect, the tribe is engineering its own gradual cultural synthesis of white and Indian culture. It does not need "cultural experts" to guide this process.

In the oppressive post-World War II era of termination and unchecked institutional racism, Mesquakis were indeed inactive politically, but the action anthropologists were too quick to create a psychologically and politically dysfunctional "cultural other" that explained their inactivity. In contrast, I understand the Mesquakis' postwar political passivity as a strategic cultural practice rather than a set of cultural traits. A more historical interpretation would be the following: Culturally, the Mesquakis still practiced their core religious beliefs and shared a reasonably stable, evolving hybrid mix of Indian and white cultural practices. In the late 1960s, when American society fell into turmoil and legislated new autonomy for its oppressed minorities, the Mesquakis seized the moment. Ever since, they have taken what the political situation has offered them and made their own history (Foley 1995).

Ultimately, the action anthropologists and I come down on different sides of the venerable theoretical debate over agency versus structure. Using Edward Bruner's (1986) typology of anthropological narratives, action anthropologists were obviously creatures of the pre-1960s anthropological discourses on Native Americans. Even though they were antiassimilationists, they never employed the popular "resistance narrative" that Bruner finds in many post-1960s anthropological studies of Native Americans. Instead, they relied on their era's dominant functionalist discourses of structural and psychological homeostasis. Being a creature of contemporary Marxist and postmodern anthropological discourses (Marcus and Fischer 1986, Clifford 1988, Rosaldo 1989), I rely more on a resistance narrative of agency and counterhegemony to explain Mesquaki culture. The general observation that all interpretations are culture-bound and ideological is hardly news. I emphasize our differences here to underscore that the Fox Project's ethnographic interpretations were based on

the reigning views of cultural analysis of the time. There was nothing particularly innovative or unconventional about their ethnographic products and representations of Mesquaki culture and politics.

Summing Up: The Legacy of Sol Tax's Fox Project

Commentators on the Fox Project (Bennett 1996, Rubinstein 1986, Polgar 1979) are correct to emphasize action anthropology's ideological break with the applied anthropology of the post-World War II era. At times, action anthropologists were much more activist and political than most applied anthropologists of that era. They operated too independently and autonomously, however, to satisfy many contemporary anthropologists who join subaltern movements or do contract research for activist and/or tribal organizations. The Fox Project was marked by less daily collaboration and shared leadership than theorized. Project anthropologists usually planned, initiated, and administered their action projects, and they often acted as independent cultural/power brokers. They worked closely with the tribal council only during the effort to save the tribal school. The tribe actually had little stake in most of the action projects, and therefore these projects died for the same reason that most applied anthropology projects die.

Previous commentaries have also overestimated the extent to which action anthropology broke ideologically with academic anthropology. There were other models of radical social science available that Tax did not follow, such as the Frankfurt School critique of science (Jay 1973), C. Wright Mills's (1956) analysis of power, and African-American critiques of institutional racism (Cox 1948 Walden 1972). Moreover, his formal statements about action anthropology did not constitute a comprehensive, coherent philosophy of science. Some commentators (Rubinstein 1986, Peattie 1979) claim that Tax followed a very democratic, dialogic pragmatist theory of science. But action anthropology as practiced on the Mesquaki settlement was marked by a good deal of social engineering. The young Fox Project anthropologists saw themselves as "clinicians" who were "curing" a dysfunctional culture. They had a strong scientific impulse to play the expert predicting and guiding Mesquaki acculturation and cultural survival.

Consequently, the action projects never functioned as "natural experiments" that generated new data and new theories of cultural change. The collective efforts of 35 student ethnographers produced far less intellectual ferment and new theory than one would expect from a science based on trial-and-error and dialogue. The theoretical models of Mesquaki culture, acculturation, and change were clearly passed down from the original student ethnographers. Their ethnographic practice neither deconstructed the culture concept nor produced nonessentializing representations of Mesquaki culture.

The project's final ethnography is testimony to the continuing influence of the reigning ideas and conventions of academic anthropology in the 1950s.

Rubinstein (1991) convincingly argues that Tax and Redfield were personally very open and reflexive. Their letters and fieldnotes reveal an ethnographic practice that looks quite contemporary by postpositivist standards. In addition, Tax continued this proclivity by placing the Fox Project fieldnotes in the National Anthropological Archives. For all the present-day rhetoric about reflexivity, few anthropologists are rushing to put their fieldnotes in the historical record. His willingness to share these documents greatly facilitated this reappraisal of his work with the Mesquakis. On that score, Tax leaves an impressive legacy of reflexive anthropological practice.

Ultimately, the Fox Project may not have been as innovative as Tax and some of his more ardent defenders claim, but it was a fascinating experiment. Perhaps what Tax dared to imagine is more important than what he actually accomplished. At the very least, he raised fundamental questions about anthropology. Moreover, we must appraise the full range of his various action projects before we bury his notion of action anthropology. As Polgar (1979) suggests, the original idea may be worth revisiting until anthropologists get it right.

And to do that, perhaps we must listen carefully to the Mesquakis' ironic commentary. On one hand, they express gratitude for the scholarships and the valiant effort to save their tribal school. They are quick to acknowledge that the project helped some tribal members and the tribe in various ways. Many old-timers also express an enduring fondness for Tax and his young action anthropologists. On the other hand, a few Mesquakis are quite critical of the project and Tax, and most tell the kind of humorous stories about the project that folklorists call "trickster tales" or moral parables. The Mesquaki stories about purposely "missing" action project meetings poke fun the indefatigable Tax and his idealistic young charges. They are parables warning all "friends-of-Indians" to resist the siren call of their religion and science to save/modernize indigenous peoples. Mesquakis believe that it is their sacred pact with the creator, not the white man's science and religion, that ensures their cultural survival. They chide us to temper such conceits if we are to be good allies.

Comments

RAYMOND APTHORPE

National Centre for Development Studies, Australian National University, Canberra, A.C.T. 0200, Australia (Raymond.Apthorpe@anu.edu.au). 4 x 98

Foley's absorbing analysis of a famous action anthropology project will, I hope, trigger some comparative or parallel studies of anthropology-in-consultancy, for ex-

ample, in economic development, emergency relief, and institutional governance. If Tax can be described as "a kind of contained rebel," much of today's anthropology-of-development writing could be said to be more about causes than by rebels (as, indeed, I remarked long ago in a review of a text in this field). This is particularly true of consultancy-based work. The anthropologist consultant too tends ideologically to be a "dedicated political liberal, humanist, and 'populist' with 'a voluntarist approach.'"

To what extent, however, when an anthropologist is working as a consultant or an action anthropologist, is it exactly "anthropology" or "social science" that is being applied, "well" or not? "Functionalist notions of social systems and structure" have never been exclusive to anthropology or anthropologists. Functionalism should not bear the whole burden of taking traditionalist as against assimilationist positions anyway. Further, it was not anthropologists only, in Tax's day or before or after, who "create[d] a psychologically and politically dysfunctional 'cultural other.'"

Nevertheless, Foley and others surely are right to look for backward linkages with the academy, provided that this is not taken to be the whole story of approaches in public affairs. The ideological preference he notes could be the driving force, even more than the social science. Perhaps, too, his well-taken conclusion that "action anthropology did not break with academic anthropology" to the extent claimed in that particular case could also be reviewed from an angle he mentions but does not explore: anthropological technique. Arguably there was a greater (or lesser) break with anthropology in terms of technique or field methods than in terms of theory in the case he describes. Moreover, not academic self-identification but the ways in which action anthropologists and anthropology consultants are identified by others perhaps rest more on perceptions of their actual ways of working, choice of language, and so forth, than on their abstract notions.

Finally, to what extent are action economics, action geography, and so forth, like or unlike action anthropology in respect of the dimensions Foley examines for his project? This is another issue for comparative analysis. So far as I am aware, cultural studies have not moved as yet in this direction. It could be seminal.

REGNA DARNELL

Department of Anthropology, University of Western Ontario, London, Ont., Canada N6A 5C2. 16 x 98

It is fashionable these days to apply what is sometimes called postmodernist reflexivity to the production of both contemporary and canonical ethnographies. During the mid-eighties, anthropologists became sensitized to the rhetorical conventions of ethnographic writing, and this was quickly followed by awareness that even our fieldnotes are socially constructed (e.g., Clifford and Marcus 1986, Sanjek 1990). All this seems to have pro-

duced a highly problematic angst on the part of anthropologists whose heroic proportions have suddenly shrunk to human size. We have berated ourselves for conditions of oppression suffered by those with whom we worked and accepted our own collective attributions of guilt.

Foley's paper adds a dose of verisimilitude to this reflexivist genre, revisiting the Fox Project shepherded through a decade of student training exercises in action anthropology by the late Sol Tax. Foley comes to his project honestly: Tama, Iowa, home of the Mesquaki settlement, is his hometown. The racism, progressivist nostalgia, and assimilationist fervor of his high-school years give an intriguing twist to the project's indictment of the local white community for its share in the social ills of the Indians. If anything, Foley's own critiques are sharper than those leveled by Tax and his team.

Sol Tax left a documentary archive rarely matched in the field notes and correspondence of an individual fieldworker. Foley has supplemented these documents with interviews with Tax and other participants and, more important, with Mesquaki who remember the project and its personnel. Oral history is integral to the documentation and updating of Tax's research, a piece of its evaluation, both among the Mesquaki and among the anthropologists. This is good methodology, then and now.

The project gets a mixed report. On the one hand, Tax and his colleagues are acknowledged to have been incredibly naive, by contemporary standards, in their pursuit of unmediated scientism. On the other hand, there is something of an apologetic for their foibles not on the historicist ground that they were ahead of most of their colleagues in the fifties but because they meant well. Tax and his Chicago colleague Robert Redfield undoubtedly were personally reflexive about the project, but their personal motives and intentions are not really the issue. The project's relation to the community was institutional and involved a number of students, who Foley suggests applied their reflexivity more to action anthropology than to their relationship to the Fox.

There were difficulties over what we might now call appropriation of political agency, where the students diagnosed social problems, devised programs to "fix" them, and proceeded with little that would count as consultation by contemporary standards. Tax himself never approached the faction-ridden progressivist tribal council for permission to begin the project; sharing the anthropological obsession with traditionalism which Foley properly highlights as problematic, he consulted influential elders instead. In the 1950s, however, this was a reasonable strategy which it would be anachronistic to read too much into.

The team personnel, finally, seems to have been constructed by the Mesquaki community as bungling but harmless. Uninterested in the theory that might follow from action anthropology and amused at the anthropologists engrossed in such a quest, they recalled particular

individuals interacting with members of the community on a human level—the participants rather than the scientific observers. Students were judged not on their scientific credentials or even utility to the community but on their willingness to interact. One suspects that a “wry, humorous, detached yet generous” verdict is not all that rare among Native American communities well acquainted with anthropologists.

Ironically, the seeds of the political agency taken on by the Mesquaki in the 1970s were invisible to the paternalistic and essentialist models of the project team. In the end, Foley reassures us, the Mesquaki had sufficient sense to realize that the project wasn’t all that important in the longer term of their own history. White folks have been around for a long time, and the oral history of the community doesn’t pay them much attention. That, I suspect, is as it should be. And the paper stands as a corrective for our own self-absorption, alongside Foley’s reminder that our understandings of ethics, epistemology, and reflexivity have come a long way in four decades. That too is as it should be.

FREDERICK OSMOND GEARING

Department of Anthropology, State University of New York, Buffalo, N.Y. 14261, U.S.A. 15 IX 98

The Fox Project: There was Sol Tax, his energy, his brilliance; there was the Fox community; there was (soon after the onset) the outline nature of action anthropology itself, this emergent “clinical” anthropology; and there were 30-plus graduate students over the years, talent and energy in generous amount. The prospects, it would seem, were promising.

Foley says that the project was intriguing but a failure. He reports that in the summer of 1948 six graduate students went to the community simply to learn ethnography; he recognizes that the resulting ethnographies were remarkable. Then, while in the field these students began to express impulses to help in some fashion, this to one another and to their mentor, Tax, and a running discussion continued on campus into the academic year and beyond. At first the discussions were mainly about values and “objectivity,” but they soon spilled over into thought about the nature of knowing itself. Out of this a new sort of field activity, action anthropology, was imagined wherein anthropologists were to help while learning and to learn while helping. This imagined activity rejected the model of applied anthropology and the positivist modes of thought which then prevailed. Then over the ensuing years other graduate students came and went, and sundry actions were put in motion. These were to take the form of clinical interchanges, and through them the helping-and-learning was to occur. The enterprise, Foley says, failed: the “interchanges” were one-sided; with but one exception the results of “helping” were abortive or short-lived; and on the “learning” side there was little in the way of new ethnography and that still locked in the old positivist

language (Foley uses the phrase “essentializing language”).

Foley reviews the project history and its outcomes usefully and well. I will speak here only to the last, the ethnographic products. I can only agree with Foley: after that first summer the products were sparse. The contrast between the several products of the early fieldwork and the scarce products later, after action was under way, is striking, so Foley is at least half-right in that the actions did preempt energies and distract. This suggests to him that the underlying ideas of action anthropology were seriously flawed. Nevertheless, it is well to point out another fact, a matter of mere logistics. Trouble lay as well with the “troops,” those 30-plus students. Almost all were intelligent and serious-minded, and they worked hard, but in general graduate students are always under duress to get on with their own student careers. These careers are diverse at the outset and tend to diverge further. Thus, among these students, project interests diverged, and careers diverged not only within the project but beyond it. (Among the initial six students, only Rietz continued to focus on Indian matters and these mainly at Fort Berthold in the Dakotas and in Chicago; Irv DeVore came to the community later, and his interests shifted to the social organization of baboons; and so on.) All this together, not just the intervention activities alone, made for many half-finished and otherwise abortive field efforts. Small ethnographic jewels did emerge from time to time, but these were usually little-connected with much else and there was little follow-through. I was involved with this project longer than most, but at the point where it was imperative to get on with a dissertation I recognized that any dissertation involving the Fox would have to involve a great deal of the collective experience and that all this, for me, was far too complex; so I fled, and (taking a great deal from Fox studies with me) wrote a dissertation on 18th-century Cherokee political organization. A decade later I managed to get back to Fox matters and wrote *The Face of the Fox*, but this after the project was finished and the people involved had scattered and become otherwise preoccupied, as indeed had I.

I have paused on those logistic matters because, in my judgment, they surely helped cause the failures named. I suppose that with improbably large resources in hand one might hire four or five persons full-time for four or five years and overcome that logistic dimension of the problem. In any event, the basic ideas of action anthropology—the underlying ideas about the very nature of discovery and of knowing, and the possibility of learning while helping—all these, as Sol Tax was able to sketch them, were sound.

Foley says that my Fox book was the culminating ethnography of the project; that was not intended, but I suppose it is so. But the book here serves an additional purpose. Foley says that no contributions to anthropological knowledge emerged from the project, but the book certainly did emerge from it and in my opinion

offers minor contributions in at least three directions, none of which is trivial. Indeed, one might find suggestions here that some sorts of knowledge are likely to emerge from activity of this kind and less likely from anthropology of other kinds.

The first I merely name: reflexivity. As to the corporate activity of the project, the running self-criticism is painfully evident in the project's paper trail, as Foley notes. And as to the book, it is mainly about looking over one's own shoulder and thereby identifying and assessing the forms of thought which had shaped what had been seen and described and what had not. The book was published in 1970. To record that one dimension of reflexivity—in contrast to the several dimensions recognized today—seemed then, to me, radical.

The second contribution was only a reminder; indeed, Plato invented this idea, and it is surely not new to anthropology today. The book pointed rather severely at reification. Anthropological research yields descriptions of communities (as social organizations, as economic or political systems, and so on). Fine, but too often we slip. Of course: these descriptions are not the community; they are not even true pictures of it. The products of such research are always verisimilitudes—selective readings, true lies—some of which are sometimes useful.

It must be evident that these two perceptions grew out of "action," mainly out of the simple case of "nerves" which always takes over and sharpens observation as events unfold and as the events follow or do not follow a course suggested by one or another of those true lies. I cannot trace the emergence of either. That is the shame. I think again of those logistic matters. But both point toward, not away from, Tax's informing ideas and their wisdom and productive power.

The third contribution is substantive: this is the idea of structural paralysis. Foley seems to have missed the contrast between descriptions of Fox social organization by Miller (1948) and by Rietz-cum-Gearing (a few years later), one put down before "action" and the other after. (It might be noted that Foley merges the first five years of project history as a single phase: thereby he loses key sequences, and these losses seriously distort.) Miller sketched two forms of authority, Fox and Western; this was insightful and accurate, and that description affected virtually all the thinking which followed. If that had served as the theoretical basis of "applied" intervention, it would have suggested bringing about some sort of movement from the first kind of authority to the second (with, ideally, a "soft landing," in Foley's well-chosen phrase). However, over the ensuing months we had seen more community affairs unfold and had been somewhat involved in some of them. And at one moment we had seen authority of that sort and at the next moment we had seen something else which was random-appearing, chaotic (read here Fox involvements in the affairs of the BIA-run school, before the attempt by the BIA to turn the school over to the state, then again during that ruckus and afterward). We had also seen affairs in which something very like that tradi-

tional authority system seemed to run its course quite well (read here their bringing off the annual powwow). And, following Rietz, we had begun to think instead "paralysis." We noted that forces in the wider society had blocked many kinds of the community's affairs, had replaced some of these with new sorts of work, and, above all, had taken over the running of both. And we imagined that as a consequence Fox forms of organization had been disrupted and pushed to one side—and over time had become rusted through little use. And we said to ourselves then that, for all we knew, those traditional forms, if given a chance, might work well in some contexts and might adapt very well in others. These, then, were the ideas which guided the activities we did involve ourselves in: we helped as we could to bring about conditions wherein the community or some parts of it might organize itself and in that realm do the work (read here actions in connection with getting the Legion post established, and, though less persuasively, Tama Craft).

This notion—structural paralysis—deserves elaboration in other places and in several directions. It is interesting in that such description took a small nervous step away from anthropology's fixation on pattern and regularity, then and now so firmly embedded in descriptions of structure and process, and toward a protean dimension of the same thing alongside it. In respect to social organization, both dimensions always exist. (And we have always known it: Need I call to mind Raymond Firth?) There may be forms of anthropological inquiry in which one may often see only pattern and other forms in which one sees mainly something protean. But if the anthropologist is about the business of helping-and-learning, both dimensions will surely surface, and attention to both is surely necessary.

A further step into this protean dimension (perhaps the inevitable next step) is multivocality. In the book, and in the project paper trail generally, there are many white voices talking about Indian voices, but there are no Indian voices, none.

Foley quotes what may have been Steve Polgar's last statement about action: We ought to keep trying until we get it right.

DELL HYMES

*Department of Anthropology, University of Virginia,
Charlottesville, Va. 22903, U.S.A. 14 IX 98*

Foley's account of the Fox Project is admirably balanced. It is sympathetic to the participants and their aims while clear about the limitations and contradictions of what was done. He brings perspective as someone himself once a resident of Tama and from interviews with Mesquaki old-timers, looking back.

I do wonder about the interviews with the old-timers. One would guess that a common history in the area played a part in access and openness. Probably information of this sort is in his 1995 book, *The Heartland*

Chronicles, but it would be helpful to say something about it here.

I wonder also about the fact that a baby born in Phase 1 of the project grew up to become a significant and original poet and prose writer, whose *Black Eagle Child* (1992) is another take on the period of the project and beyond. Young Bear's writing is vibrant with Mesquaki identity and experience. Isn't that also evidence of resources the Mesquaki themselves had, despite appearances at the time?

Foley's account of Mesquaki recollections, including amusement, rings true by analogy with experiences of my wife and myself in Oregon. So does the mistake of taking the state of young veterans as showing their inevitable future. Comparative knowledge of other Native American communities might have shown analogues to the Mesquaki pattern—what may seem to us a late assumption of adult responsibility. I remember my Reed teacher David French remarking on just such a pattern of assumption of adult responsibility at something like early middle age at Warm Springs Reservation.

NANCY OESTREICH LURIE

3342 N. Gordon Pl., Milwaukee, Wis. 53212, U.S.A.
12 x 98

Foley's characterization of the Fox Project as a series of more failed than successful "action projects" is misleading. The Fox Project was a garden-variety field school. It wasn't even an exercise in the then-new field of applied anthropology. As participant observers, fieldworkers have always been asked to help out in one way or another and do so as a matter of reciprocity and rapport. The distinction of the Fox Project and a tribute to Tax's perspicacity is that this more or less routine occurrence gave rise to a new concept, action anthropology.

The field notes that Foley cites from later in the life of the project contributed to the development of action anthropology, but generally these are the very ordinary notes of novice fieldworkers. The retrospective accounts he cites are not so much "idealized" as they are hindsight awareness of what the project augured. The Fox Project was a learning experience for all concerned—Tax, the students, and the Fox themselves.

Foley simply does not understand what action anthropology is. It is not finite action projects such as Tama Craft or helping the Fox to form new community-wide voluntary associations. It is about open-ended processes—enabling communities (broadly defined) to take control of their own destinies and to learn from their own successes and their own mistakes. Foley is dead wrong when he says Tax saw "action anthropologists as community organizers"; he saw them as catalysts in the process of communities' organizing themselves to define and cope with their own problems.

My response is unavoidably quite personal. I was a beginning graduate student at the University of Chicago

when the Fox Project was being planned. By the time it was launched I had left Chicago to complete my Ph.D. at Northwestern University. In 1960 I was only superficially informed about the Fox Project and scarcely at all about action anthropology when Tax tapped me to serve as his assistant coordinator of the American Indian Chicago Conference (AICC) of 1961. This was an action project from the start. It was based on principles inspired by the Fox Project and further developed in subsequent work by Fox Project students, among them Robert Rietz, who also assisted with the AICC. It also reflected matured ruminations on action anthropology that Tax never really wrote down in detail but acted on and imparted by example to his students and associates.

My account of the AICC (Lurie 1961) documents how communication was established among thousands of Indian people across the country to reach consensus on policy recommendations for the new presidential administration, but I identified the underlying principle of action anthropology as "fundamental faith coupled with much patience that the people involved are better able to solve their own problems, given the opportunity, than anyone else" (Lurie 1961:481). Action anthropology is about providing opportunity as needed—running interference, finding funding, supplying useful information, whatever, but not taking charge. Although Tax had the overall format of the AICC in mind in seeking support to coordinate the endeavor, the decision to accept Tax's offer, the document that was produced, the *Declaration of Indian Purpose*, and the way it was produced were thoroughly and ineffably Indian. Besides initiating a united Indian front against the policies of the 1950s and reaching consensus on what Indian people really wanted, the AICC offered an ongoing model for future action for both Indian people and anthropologists.

In 1970 (Lurie 1973:4–15) I tried to set forth systematically what action anthropology entailed, pointing out how similar concepts occurred independently to several other scholars at the time of the Fox Project, and I described two action projects in detail, one directly inspired by the AICC. I observed that "it may be that what we designate as action anthropology is really what anthropology as a whole is becoming" (Lurie 1973:4). Foley correctly attributes this opinion (paraphrased) to me but cites my 1979 article "The Will-o'-the-Wisp of Indian Unity," which is an ethnohistorical analysis of factionalism and doesn't even mention action anthropology. I believe that if he had stepped back from the Fox Project and paid real attention to my 1973 publication and other essays in the 1979 volume that he cites he would not have fallen so wide of the mark in understanding the genesis, nature, and impact of the Fox Project in giving rise to action anthropology and discerning that action anthropology's "very unobtrusiveness is a measure of its effectiveness" (Lurie 1973:4).

Foley's misreading of his sources is not confined to my publications. Although he cites works on Fox history, it is hard to believe he has read them carefully when he speaks of the "Black Hawk Wars of 1843," the government's moving the Mesquakies to a 50,000-acre

reservation in Kansas, and their returning to Iowa in 1857 and using their "allotments" to purchase 80 acres near Tama. The Black Hawk War, involving a dissident Sauk band, occurred during the summer of 1832. Although most of the Sauk opposed it and the Fox remained neutral, these often allied tribes were forced to cede homelands as reparations in 1837 and 1842 when both were moved to the reservation in Kansas. The Fox soon began drifting back to Iowa and in 1856 obtained an act from the state legislature legalizing their residence in Iowa. They raised the money to buy the 80 acres near Tama in 1857 from the sale of their jewelry and ponies and contributing their treaty annuity payments, not "allotments" as Foley claims. Allotments are lands, a nonliquid asset that could not be used as Foley describes; furthermore, the allotment policy dates to the 1880s. Foley ignores the significance of the Mesquakies' unique situation regarding allotment. As the Fox continued buying land, the Indian Bureau paid the taxes until 1930, when the tribe took over, paying with the proceeds of agricultural acreage rented to whites. Because they actually owned their land, it could not be allotted, thus sparing the Fox the enormous land loss and consequent social disruption suffered by the majority of tribes between 1887 and 1934.

ROBERT A. RUBINSTEIN

Department of Anthropology and Program on the Analysis and Resolution of Conflicts, Syracuse University, Syracuse, N.Y. 13244, U.S.A. (rar@syr.edu). 30 x 98

The reappraisal of anthropological work decades after it was conducted is always difficult and delicate. The extensive archives related to the Fox Project facilitate Foley's retrospective, and I find his paper interesting and helpful both in tone and in interpretation. He raises many issues for further exploration and reflection. Rather than focusing on areas of agreement, I will comment on a few places where I think his reinterpretation overreaches the material upon which it is based.

The central theme of Foley's paper is that a careful reading of the Fox Project archives and oral history interviews with Mesquakie and white informants reveals that in its daily practice the Fox Project did not live up to the rather heady, ideal claims made for it by Tax and his students. It is, perhaps, not too surprising to learn that what these anthropologists did was not precisely what they said they did. Indeed, finding otherwise would challenge the well-accepted anthropological observation of this general disparity in human social life.

Foley is correct that Tax's philosophy of action anthropology qua clinical science did not break as fully or as thoroughly with the then-dominant positivist science of the day as did the Frankfort School. In this regard we ought to note three things: (1) The critique of positivist science and the claim for a role for values-based work was a by-product of Fox Project activity, not its main focus. (2) Instrumentally, such a partial break

allowed Tax and his students to carry out applied work toward which their University of Chicago colleagues (and the discipline at large) were at best indifferent and perhaps even hostile. (3) Even when there was considerable variance between their practice and their philosophy, their approach warranted an intellectual project that was much more reflective than was common at the time and might otherwise have been impossible for them to carry out. (Here I depart from Foley in my appreciation of these "ruminations": I am glad that they were recorded for the most part in the private journals and field notes of project participants—later made available through library archives—with only their more worked-out versions being presented to their colleagues rather than becoming the basis of extensive, self-reflectively absorbed ethnographic accounts in professional journals and books as came to be the disciplinary practice just a decade or two later.)

Foley faults the project for not having anticipated (being unable to predict) the fluorescence of the Mesquakie tribe a couple of decades after the project's end. He attributes this to project members' confusing "strategic cultural practices" with "ontological cultural traits" and having "operated too independently and autonomously to satisfy many contemporary anthropologists who join subaltern movements or who do contract research for activist and/or tribal organizations." He suggests a "less essentializing, more historical interpretation" which focuses on Mesquakie agency and resistance. About these observations, three telegraphic points: (1) It seems to me inappropriate to compare the strategic public relations representations of the Mesquakie made to a popular white audience with the professional ethnographic products of anthropologists produced decades later in a very changed disciplinary milieu. (2) Tax valued the independence of the action anthropologist as a practical means to manage questions of values and legitimacy that face committed action research. Such questions are not settled in contemporary forms of committed applied anthropology. (3) Prediction in any precise sense still eludes anthropology and perhaps particularly eludes the subaltern and contract anthropology to which Foley contrasts the Fox Project. And, in any event, I believe that Tax's view of the science of anthropology did not privilege prediction as a criterion of success.

I am particularly struck in Foley's recounting of the Fox Project that, despite the project's failure most of the time to be as collaborative as claimed (perhaps, desired), there is an iterative nature to its practices. Several of the episodes that Foley recounts suggest a developing convergence of theory and practice among the Fox Project anthropologists. These events also helped, however modestly, to change the social and political arenas within which the Mesquakie operated. The two most important of these are the school control issue and the "Tax epiphany tale." In regard to the latter episode, it is worth noting again that the recognition of the persistence of Indian tribes in general and of the Mesquakie in particular was radical for the period. During this time

the myth of the homogenizing influence of America upon the people living in it was pervasive both inside and outside of academia. I believe, from my own conversations with him, that Tax's view was that this persistence depended largely upon what we would now call the agency and resistance of native peoples. I believe that this view is also reflected in his testimony to Congress on this matter.

As Foley observes, a full appreciation of action anthropology as an intellectual and professional project would require examining other action anthropology efforts, both those conducted by Tax and those of other anthropologists. The reappraisal of the Fox Project offered by Foley in this paper is a good and welcome start.

MURRAY L. WAX

*Department of Anthropology, Washington University,
Campus Box 1114, St. Louis, Mo. 63130-4899, U.S.A.
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Reviewing the situation of the Oglala Sioux, Vine Deloria has remarked that the Civilian Conservation Corps (CCC) saved many lives during the Great Depression of the 1930s. Yet this program has been little noted amid the countless retellings of the Battle of the Little Big Horn or the massacre at Wounded Knee. Given the poverty of most Indian reservations and the discrimination suffered by their members, it has always been valuable to bring in benevolent projects, together with their moneys and staffs of outsiders. My guess is that the Fox Project brought these benefits to the Mesquaki.

Another shrewd observer, Nancy O. Lurie, has characterized Plains Indian communities as oriented toward herds of buffalo and other game animals. When they located a herd, the tribe feasted; in their absence, they starved, suffered, and died. Benevolent projects appeared to these communities as if each constituted a herd of game animals. In disregard of the ideology and nominal purposes, members feasted. While the older generation were grateful for that sustenance, their descendants may not be aware of the efforts involved, and even the next generation of anthropologists may be ignorant or reluctant to claim credit.

Sol Tax became dedicated to righting the wrongs suffered by North American Indians. Gifted at organizing people on a quasi-voluntaristic basis and at gathering funds from private foundations, he made an enduring contribution to Indian welfare in general and—so I would infer—to the Mesquaki in particular.

In the wake of crusaders, others may suffer. This was a pattern throughout Tax's efforts. A major paradox was that the men most gifted at working with Indians—notably Robert Rietz, Robert K. Thomas, Richard Pope—were never properly rewarded; each merited a doctorate. The credential would not only have benefited them personally but also have lent authority to their expertise. Unhappily, Chicago's Department of Anthropology was not set up to confer a doctorate for achievement in action anthropology. To so formalize the achievements,

Tax would have had to be as concerned about the welfare of his graduate students as he was about Indians, but there is much evidence that he left these students to fend for themselves.

As Foley notes, the research activities of academically successful students were seldom harmonious with the ideological aims of the Fox Project. Under the foregoing circumstances, theirs was a sensible strategy for academic survival. Fallers, Gearing, Ablon, Polgar, et al. were persons of marked potential (and ideological commitment), but in the absence of an organized program to coordinate and reward achievement in the Fox Project of action anthropology they had to find their own ways. Persons like Gearing did so by a contribution to Fox ethnohistory and ethnology, using the academic tools at their disposal. Overall, one is left with a vision of an aggregate of students who, like cats, could not be herded together. Amusingly enough, their loose informal organization mirrored that of the Mesquaki themselves and must have fostered a mutually congenial relationship.

Given Tax's crusading temperament and his scorn for "applied anthropology," the Fox Project appears one of a kind, but in fact it was but one of a number of anthropologically inspired projects intended to benefit mid-century reservation life. Most were directed toward North American Indian communities, although a few (e.g., Vicos) had a southern orientation. During the lives of the projects, they seemed to be having significant beneficial effects; nevertheless, they have been too quickly forgotten by both subsequent generations of anthropologists and Indians. Anthropologists should have longer institutional memories and should pride themselves on the dedication of their ancestors, but they have allowed themselves to be shamed by internal critics and Indian militants. A half-century later it has become difficult to assess the substantive effects of these projects, but while in process they involved great dedication on the part of a number of scholars and allied practitioners (often physicians), and they often provided concrete benefits. It surely would be worthwhile to try to assemble a balanced assessment, and one must be grateful to Foley for his efforts with one crucial project.

Reply

DOUGLAS E. FOLEY

Austin, Tex., U.S.A. 9 XI 98

The comments on my paper live up to the CA tradition, heading out in various directions and defying any simple, straightforward reply. Most of the reviewers seem to find something redeeming in the article. Hymes, Darnell, and Apthorpe are particularly generous with praise. Darnell recaps very well what I was trying to do and is sympathetic. Hymes anticipates remarkably well other points not in this article: It did help to be a local,

and I said so in *The Heartland Chronicles* and in a separate methods article. The poet/novelist Ray Youngbear is indeed an excellent example of Mesquaki vitality, and he is one of several "organic intellectuals" in the *Chronicles*. I am also planning another piece that explores the differences between his brilliant novels and my ethnographic portrait. Other Native Americans may indeed also assume their adult responsibilities later in life, and one only captures this through life histories. I have elaborated on that idea in an article about adolescent rites of passage and sports. Finally, Apthorpe's call for comparative or parallel studies of other action disciplines is important. All academic disciplines do need to critique how personal and disciplinary ideologies influence their action practices.

Rubinstein also finds the piece interesting and helpful but offers a series of "yes but" defenses of Tax and action anthropology. He continues to believe that the project helped change some political and social areas of Mesquaki life and that Tax had an appreciation of native people's agency and resistance. What we have here is a gentle, commonsense apologia for action anthropology. It is hard not to agree with most of it, and in fact I made most of these points myself.

Gearing also says I do a decent job of chronicling the project's history and basic action outcomes, and his defense of Tax and action anthropology is as restrained as Rubinstein's. But he would not characterize my account as balanced. Gearing says that I see action anthropology as intriguing but a "failure." True, my account is critical, but I hope that most readers will see the gray hues and mixed results.

The main thing that Gearing contests is my view of the project's ethnographic training and products. After acknowledging the project's chaotic ethnographic work and missed opportunities, he argues that its ethnographic products were highly reflexive, a critique of reification, and innovative theoretically and thus contributed to knowledge. I agree that the documentary history was an interesting, reflexive piece of writing. Although it leaves out various controversial matters in the fieldnotes and exaggerates the project's success, it conveys the ethical, philosophical, and political ferment of the project. And, as Rubinstein has pointed out elsewhere, Sol Tax was, even by contemporary standards, a very reflexive fieldworker. Gearing's ethnography, however, is not particularly reflexive. It has a strong personal, confessional tone and laments the estrangement that he and other whites feel toward Mesquakis. He then theorizes/explains these feelings as originating in the cultural difference between modern, individualistic whites and traditional, communalistic Indians. Rather than reflexively interrogate his subjectivity, theoretical constructs, relationships with and representations of Mesquakis, and the intellectual milieu that produces his interpretation, Gearing retreats into a grand cultural ethos theory. This is good post-World War II anthropology, but it is not reflexive in the contemporary sense.

The claim that his ethnography enhances our understanding of reification is also puzzling. Gearing invokes

Plato and the literary construct of verisimilitude rather than the usual Marxian or sociology-of-knowledge perspectives on reification. I do not associate *The Face of the Fox* with reification, so I have no idea what he is arguing here. Finally, Gearing also feels that the project's theory of structural paralysis was a major contribution—"a small step away from anthropology's traditional search for patterns and regularity." In sharp contrast, I see the focus on structural paralysis as the epitome of searching for patterns, regularity, and homeostasis. We go past each other like the proverbial ships in the night.

I do, however, agree that what Gearing calls "logistics" helps explain the project's demise. He highlights how the shifting careers and interests of the students disrupted the publication of some ethnographic gems in the rough. As Wax aptly puts it, organizing the project must have been like herding cats. Lurie opines that the Fox Project was a "garden-variety field school. It wasn't even an exercise in the then-new applied anthropology." All these insiders seem to be acknowledging the enormous difficulty of running an experimental project through a field school with part-time students. I tried to make that point, but it probably needs underscoring. Gearing's notion of logistics is helpful on that score.

Lurie's defense of Tax is more pugnacious and personal than the previous two commentaries. She says that I do not understand action anthropology, that I rely on questionable data and have not read widely enough, and that I seem not to know much about Mesquaki history. I read both of her articles but, regrettably, cited the wrong one. I also got the date of the Black Hawk War wrong. I will let others judge if these points are sufficient to discredit my argument. Lurie claims that I misunderstand Tax's philosophy because I say that he saw action anthropologists as community organizers. In fact, I also point repeatedly to Tax's idea of being democratic, collaborative "catalysts," and it becomes my criterion for judging the action anthropologists' work.

Finally, Wax is also fairly contentious in tone, but he is more interested in commenting on the unfulfilled promise of action anthropology than on the specifics of my argument. He is much more critical of Tax than I am. He represents Tax as a gifted organizer and fundraiser who left some of his best students out in the cold professionally. He then uses my study to corroborate his view that the academically successful students were at odds with the project and had to find their own ways. None of Tax's students expressed such views in their field journals, and I do not say that they did. Given Rubinstein's survey and what Gearing told me in a long interview, I find this view of Tax unsubstantiated and suspect.

All in all, given Tax's legendary status and the controversy that swirls around action anthropology, I expected the reviews to be more contentious than they are. Tax's former students and colleagues are still defending him, but not without reason. Although I am critical of the project, I must add that I probably would have been a Tax loyalist in the 1950s. Back then he was the best

thing going for those who wanted to be activist anthropologists. When the whole story of his action projects is told, his legacy may be quite impressive, but until then I suggest that we not exaggerate matters and glorify that legacy. I stand by my reassessment of the daily practices of the Fox Project, and I hope that it will help us do action anthropology better.

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