

*Reflections on Action Anthropology: Some  
Developmental Dynamics of an  
Anthropological Tradition*

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"Action Anthropology" was originally conceived during work with the Mesquakie (Fox) Indians in the 1950s by Sol Tax and his students. Despite their enthusiasm and expectations, action anthropology as they conceived of it has not had a great influence on the discipline. Yet, the epistemological, practical, and methodological commitments of their approach offer important alternatives to those accepted by many of the collaborative, advocacy, and "action" research projects in which anthropologists are increasingly involved. This paper explores some of the reasons for action anthropology's lack of influence. Considered are aspects of the epistemological and sociological climates in which action anthropology developed, the reward structure for basic and applied scientific research, and the role of personal characteristics in the leadership and definition of disciplinary traditions. The conclusion of the analysis is that "action anthropology" still has much to offer attempts to better understand how anthropology can be made useful and relevant to policy-makers.

**Key words:** action anthropology, applied anthropology, leadership, Sol Tax

And the end of all our exploring  
will be to arrive where we started  
And know the place for the first time  
—T. S. Eliot  
Little Gidding

In 1952 Sol Tax described an approach to anthropology, and especially to its application in some practical settings, that he called "action anthropology." This approach resulted from the work of Tax and his students, especially from work they carried out between 1948 and 1959 among the Mesquakie Indians (also known as the Fox). This work came to be called the "Fox Project." This group invested much time and effort in the Fox Project and in other action anthropology programs, and, for a time, it was discussed in the anthropological literature (see, e.g., Ablon 1962; Blanchard 1979; Borman 1979; Gearing 1970; Gearing et al. 1960; Lurie 1973; Peattie 1968; Schliesier 1974; Tax 1952, 1956).

Tax used the label "action anthropology" to describe a process during which both general social science inquiry is carried out and practical help toward resolving a group's day-to-day problems is given. The action anthropologist holds

that neither the search for general knowledge nor the solution of practical problems alone is enough. Rather, they form equally important "coordinate goals" (Tax 1952). Thus, during the Fox Project, Tax and his colleagues sought information about social and cultural dynamics and about social organization at the same time that they tried to assist in finding solutions to some very practical problems that arose from the day-to-day interaction of the Mesquakie tribe with the neighboring White community of Tama, Iowa (Gearing et al. 1960; Gearing 1970, n.d.).

Tax and his colleagues took action anthropology to be different from other forms of applied anthropology. Because of this, taking the program of action anthropology seriously required that they think about many issues fundamental to the discipline.

A review of the literature related to their approach, information provided to me by Tax during several interviews and discussions over the past three years, and views expressed by some of his former students and colleagues during interviews and in response to a questionnaire that I sent them, suggest that its adherents anticipated that action anthropology would have a major influence on the development of applied anthropology and general ethnology in the United States (see, e.g., Human Organization 1958; Gearing et al. 1960; Ablon 1979). Lurie (1973:4), for example, noted: "It may be that what we designate action anthropology is what anthropology as a whole is becoming."

Yet, in discussions of the nature and scope of applied anthropology the Fox Project and action anthropology are frequently discussed only superficially. They are often taken as interesting for historical purposes, but dismissed as failures. For example, Partridge and Eddy (1978:37) devote only one paragraph of their 48-page survey of the development of applied anthropology to the Fox Project, and mention action anthropology not at all. Foster (1962, 1969) mentions it not at all or in passing, dismissing action anthropology as an attractive but flawed approach. Action anthropology receives no mention in Goldschmidt's (1979) collection of papers on *The Uses of Anthropology*, or in Arensberg's and Niehoff's (1964) *Introducing Social Change*. Goodenough (1963:44-46) notes, but does not explore, the important action anthropological commitment to people's having "the freedom to make mistakes." Van Willigen (1986) is a recent exception.

Indeed, when anthropological work in the United States, especially in applied settings, is examined, it seems that action anthropology as described by Tax and his colleagues has had relatively little impact. My main goal in this paper is to better understand the reasons for this relative lack of influence of action anthropology, specifically as described by Tax and his colleagues.

I approach this goal by considering two questions: (1) Why did relatively few anthropologists working outside of the action anthropology tradition take up the approach?, and (2) Why did the original group of action anthropologists not form the core of a coherent tradition within anthropology?

To answer the first question, the relations of action anthropology to anthropology in general, and to applied anthropology in particular, are reviewed. Three aspects of these relations are examined: (1) the nature and role of theory building in basic, applied and action anthropology; (2) the nature of accountability in anthropological work; and (3) the

nature of the understanding of science which formed the context in which action anthropology emerged and was evaluated.

To answer the second question I examine some of the social and personal factors that affect the growth of traditions within scientific disciplines as these relate to action anthropology. Among these are: (1) the role of personal characteristics in the leadership and definition of scientific traditions, and (2) the importance for the development of traditions of access to the usual reward structure for work in a discipline.

The result of this work can be briefly characterized. I think that the epistemological objections that have been raised against action anthropology are ill-founded. Furthermore, the sociological constraints on the development of scientific disciplines, together with the personal difficulties involved in doing action anthropology, made its development as a coherent tradition extremely difficult. I conclude from this analysis that action anthropology still has much to offer to our attempts to better understand how anthropology can be made useful and relevant to policy-makers and to our society in general. Finally, our definitions of success in anthropology may be radically altered by reflecting upon the experience of action anthropology.

#### *Action Anthropology Briefly Characterized*

Although there are a number of anthropologists who refer to some or all of their work as action anthropology, I emphasize that the approach about which I am concerned is just that work which conforms to the general outlines adhered to by Tax and his colleagues. Not all of the work that is called action anthropology fits that outline, or is conducted in a fashion recognizably deriving from the action anthropology described by Tax (cf., Tax 1952, 1956; Polgar 1963, 1979; Merrill 1951; Peattie 1968; White 1974; Lurie 1976; Schensul 1974; Paredes 1976; Jacobs 1974). It is in itself interesting that action anthropology as set out by Tax and his colleagues has yet to be given a mature expression that is used as the basis of work for a portion of the anthropological community. That this is the case is clear from the great variation in what the term "action anthropology" is today taken to mean.

The phrase that is probably most often associated with action anthropology is the assertion that anthropologists ought to learn and to help in equal measure. This apparently simple dictum is actually the product of several complex notions about the nature of anthropological work. Two of these, one methodological and one ethical, are particularly important.

The methodological notion is that when working in applied settings (as well as when doing "traditional" ethnographic research) we can—indeed ought to—be concerned with the creation, testing, and elaboration of basic theory. As I will note later, this is *not* a view that usually characterizes discussions of the nature of applied research. Moreover, the view is frequently taken that working on specific practical problems set by interests other than the logic of a developing research program is inconsistent with such theoretical progress.

As a result, when Tax (1975:514) says: "The first thing to make clear is that we [action anthropologists] are theoretical anthropologists . . ." it is easy to suppose that he means either

that action anthropologists *use* theoretical tools in their work in practical settings, or that work on practical problems offers a context in which basic theory can be tested.

Yet Tax reports that his view was, and remains, that in the *normal* process of the development of theoretical anthropology, work on practical problems presents an opportunity for the development of new, more powerful theory. Work on practical problems is likely to give us a chance to develop areas of theory that might not easily be explored in other contexts. These areas are nonetheless important and sometimes even central to the elaboration of theory that has been or could be developed in other contexts.

The ethical notion has two components. First, anthropologists have an obligation to people with whom they work such that our work ought to be imminently useful to them. Second, we should not make decisions for the people we work with. He argues that anthropologists should "not use people for an end not related to their own welfare [and] not only should we [anthropologists] not hurt people, we should not use them for our own ends" (Tax 1952:104). Anthropologists should not decide that a group should take some course of action that they themselves do not desire. Further, where anthropologists have authority, this is also an injunction against their implementing such contrary decisions.

It follows from these ethical notions that while an anthropologist can work with members of a community toward goals they express, the anthropologist ought not to direct that work. In the paper that outlines the philosophical foundations of action anthropology, Tax (1956:175) argues that, "Whether or not the community turns out to have chosen a wise course of action is quite beside the point. Every people if it is free, is free to make decisions, hence must have the right to make mistakes."

These general considerations formed the basis of the original, early formulation of action anthropology. The merits of the action anthropology approach or of particular action anthropology programs usually are treated only briefly in the literature (see, e.g., Foster 1962, 1969; Eddy and Partridge 1978; Turner 1982). These discussions often assert that the responses of these action anthropologists to complex issues like factionalism or the uses of power were naive, or oversimple. Thus these discussions at once note that the approach raises important, tough epistemological, ethical and practical questions, but then beg those questions by failing to continue to explore the potentials of the action anthropology perspective for dealing with those issues. I believe that anthropology is the poorer for this. Seriously considering the issues raised by action anthropology and developing a mature statement of the approach would, I believe, provide anthropology with a special voice in discussions of public policy and contemporary affairs.

#### *External Considerations*

THEORY CREATION IN BASIC, APPLIED, AND ACTION RESEARCH. It is almost universally accepted that the creation and development of theory is the *sine qua non* of scientific research. The nature of "pure" or "basic" scientific work requires that empirical study (observation or experiment) be carried out in the service of theory building. Within scientific

communities prestige and other rewards derive most often from contributions to the advancement of theory.

It is not surprising, then, that activities seen as not directly contributing to the development of new theory get relatively low status. George Foster (1969) argued that because applied anthropology is seen as an area where practitioners gain insight from theoretical work but do little to advance that work, applied anthropologists have low status in the discipline.

Speaking of the situation in which an anthropologist takes an assignment for some organization or agency (the activity that might be contrasted with action anthropology and called "traditional applied anthropology"—see Willner 1973; Thompson 1976), Foster (1969:156) argues that applied research not only makes use of theoretical knowledge, but also tests that knowledge in new and varied settings.<sup>1</sup> As a result it contributes to basic anthropology by extending the range of interests over which anthropological theory might develop, by improving research methods, and by otherwise enriching the discipline.

Foster's view of the relation between theoretical and applied research is characteristic of the general understanding of this relationship (e.g., Karlesky and Smith 1979). In this view applied anthropology is more than the straightforward use of theory for practical ends, it also is a forum in which theory is tested. However, applied work is not seen as an arena in which theory is created.<sup>2</sup>

Action anthropology goes further than this position. Science is seen as an open-ended process in which theories are taken to be more or less useful, rather than as true or false. Because human knowledge is always contingent and incomplete, the "truth" of a theory is always something of an open question. This view of the nature of science is grounded in the work of American philosophers like Peirce and Dewey (cf., Tax 1952, 1975; Gearing et al. 1960; Rubinstein 1984; Almeder 1973, 1975).

Moreover, since scientific judgements and practical judgements are equally and always taken under conditions of uncertainty, emphasizing the structure of activity underlying applied and basic research reveals them to not necessarily be different. What distinguishes science and theoretical innovation from non-science is not the arena in which the work is carried out, but rather how those seeking knowledge react to unexpected, anomalous data (Rubinstein et al. 1984; cf. Pribram 1971). In the presence of what Peirce calls the "scientific spirit," theoretical innovation is no more likely to come from work in a basic discipline than from work in an applied area.

Basic science might work toward theoretical insight by reflecting on the results of experiment or observation and modifying or elaborating theory appropriately. Traditional applied anthropology is generally held to give up involvement in theory development after observation or experiment because of a contractual need to deliver a previously agreed-to product. Thus, rather than a concern with theory creation, traditional applied anthropology takes as its first concern "product delivery" (see Willner 1973).

Action anthropology insists on continuing involvement with a community (Ablon 1979). When their work reveals inadequate theory, the action anthropologist continues to try to discover "whatever works" (Tax, personal communication). The task is seen as "an open-ended, continuing process

of defining community problems and developing skills and techniques for coping" (Lurie 1976:320). Finding "whatever works" is simply an informal way of saying modify one's theory. Modification may well lead to some basic theoretical advance, as it did during the Fox Project.

Since the yield of the Fox Project is often passed over as rather paltry, it is useful here to interject just one example of a basic theoretical advance that came from it. The persistence and importance in the United States of ethnic group identity is now widely taken as a social fact with real consequences for matters of public policy. That the United States is not a "melting pot," where diverse ethnic groups inevitably acculturate to some mainstream standard, first received critical success as an innovation in social science theory with the publication of Glazer's and Moynihan's (1963) study *Beyond the Melting Pot*.

The idea of the melting pot had influenced federal policies about how best to deal with American Indian groups (Bigart 1972). The federal government acted on the premise that Indians were disappearing and would be absorbed as they became acculturated. (Indeed, anthropologists shared this idea and used it as a rationale for getting research funds.) During the course of the Fox Project, Tax and his colleagues concluded that this view was wrong. Rather, they saw the Mesquakie as persisting, and they recognized that ideas about the temporary nature of the Mesquakie held by the surrounding community and by the state and federal governments clashed with the Mesquakie's self-perception. This clash, they argued, was one of the fundamental reasons for the difficulties that characterized Indian-White interaction in Tama (Tax 1957, 1960; Gearing et al. 1960; Gearing 1970, n.d.). This insight helped them to recognize other derivative social processes—like "structural paralysis" and "structural isolation"—that they were able to use to better understand the Fox, other Indian communities, and social processes among other groups (Gearing 1970; Tax and Thomas 1969; Rubinstein and Tax 1981).

It is important that these basic insights derived directly from their work at finding ways to be of practical assistance to the Fox. Moreover, it anticipated by a decade the more general development of this perspective in sociology and in anthropology.

Clearly, the unfolding process of looking for whatever works can be creative theory building. Tax urges us to see action work as an opportunity for developing theory. This is a claim that goes beyond the simple taking up of a theory developed elsewhere in order to reach some end, and it is more than testing under new and broader conditions theories developed elsewhere; it is an opportunity for theoretical innovation.

**ACCOUNTABILITY IN ANTHROPOLOGICAL RESEARCH.** Action anthropology is not, in principle, inhospitable to theoretical innovation. How then can the general perception in the discipline that neither it nor other forms of applied anthropology directly contribute to theory building be understood? A large part of the answer is found in the nature of accountability in each type of anthropological work.

In basic research the locus of a scientist's accountability is in the peer review of colleagues. There is no real personal responsibility attached to theoretical work. If a supposed

theoretical advance turns out to be ill-founded, it can pass more-or-less quietly out of the discourse of the discipline. The theoretically active scientist is free to start work on another promising line of inquiry, publish other materials, and seek additional grants or fellowships. Because of the nature of the peer review system, it is likely that the scientist would not suffer personally for the theoretical failure (Cole and Cole 1981; Harnad 1982).<sup>3</sup>

There remains, some argue, the scientist's responsibility to see to it that the uses to which her or his work are put are acceptable. Especially in the late 1960s, a vocal, but unfortunately small, number of people argued strongly for the acceptance by anthropologists of such social responsibility (Berreman 1968; Gjessing 1968; Gough 1968). Yet, such acceptance is not widespread and it is easy to find among our colleagues many who do not accept such responsibility. They decline it by noting that they act as scientist *qua* scientist and are consequently responsible for the production of social theory, but not for its uses.

In traditional applied anthropology the practitioner sells her or his expertise to a client. Usually this involves an agreement to provide a product—report, survey, or research design—for instance. Unless the applied anthropologist chooses to publish some theoretically-oriented paper based on that experience, he or she is accountable only to the contracting authority. That accountability is financial. Failure to deliver the product or service contracted for (or the delivery of one of poor quality) can result in lost income, damaged vocational reputation (but, importantly, *not* necessarily professional anthropological reputation), or even have legal consequences (Willner 1973). Yet, even the applied anthropologist can avoid responsibility for the uses to which her or his efforts are put (Cohen 1984). It is because scientific accountability is not a necessary part of traditional applied anthropology that the widespread (and, mistaken) view that it fails to contribute to the creation of theory has been able to develop.

Unlike workers in basic or traditional applied anthropology, the action anthropologist is necessarily accountable on two levels: to the scientific community and to the local community with whom he or she works. Because action anthropology has mistakenly been characterized as a variant of traditional applied anthropology by many people working outside of it, this joint accountability and its implications have been largely missed. Thus, the assertion of interest in developing new theoretical material through action research has been misunderstood as the more familiar claim for a role in theory testing. This misunderstanding has been reinforced by a failure to face squarely the epistemological and practical implications of the commitments of action anthropology.

**EPISTEMOLOGICAL CONTEXT OF ACTION ANTHROPOLOGY.** Action anthropology got started at a time when models of social science as social physics were taking firm hold in the United States (Suppe 1977; Glymour 1983). Action anthropology, however, was grounded on epistemological views that follow from other philosophies of science, like Peirce's or Dewey's, which have commitments radically different from the commitments of the then increasingly strongly articulated "received view" (see Suppe 1977; Diesing 1982:314–342; Rubinstein et al. 1984).

Thus, where the received view sees science as value-free

and objective, action anthropology sees an essential role in science for clearly articulated value statements (Tax 1958). Where the received view defines the relationship of observer to the observed to be one of separateness, action anthropology insists on the importance and desirability of close personal ties between researchers and the people with whom they work (Ablon 1984; Lurie 1976). When the received view spoke for replicability and objectivity, action anthropology argued for fairness (Tax 1958; Peattie 1970); and when the received view worked toward the development of systems of expert knowledge, action anthropology argued for the authenticity and importance of the experience of a community's members (Blanchard 1979; Borman 1979).

It is ironic that the decade and a half during which Tax and his colleagues were trying to work out more satisfying formulations of action anthropology, and during which they were confronting the hard epistemological questions that resulted, was the period during which the received view of science came to seem most satisfying. During this period there prevailed among social researchers a view that science was neutral and objective, and that recognized data as "scientific" only when they were quantitatively measureable (cf. Nagel 1962; Suppe 1977; Diesing 1982). It was against such criteria that research results were evaluated. These criteria also formed the basis for policy-makers' views of what useful knowledge would look like (Rubinstein 1984).

Thus, Tax and his colleagues had to demonstrate in a hostile evaluative environment the value of their approach, even as they struggled amongst themselves to better understand that enterprise. To some extent, at least, efforts invested on one front would inevitably detract from the other. And, to some extent, action anthropology as a group was unable to stand up to the pressures that resulted.

It is now well accepted that the received view gave an overly narrow picture of science (Suppe 1977), and that the picture of social science as social physics that derived from it is inadequate (Rubinstein et al. 1984; Glymour 1983). Although no alternative philosophy of science has achieved the widespread acceptance earlier accorded the received view, the epistemological issues raised by action anthropology (and by other post-positivist approaches to science) are once again acknowledged as legitimate and important (Pinxten 1981; Rorty 1981; Wilber 1982).

### *Internal Considerations*

Despite the early failure to win widespread acceptance for action anthropology in the discipline, it might have been expected that those involved in its development would continue to interact, to grow, and to set out eventually a more mature statement of the action anthropology program. Action anthropology might then have had the major influence on the discipline expected by its early advocates. That a common, mature program has not been set out, nor a working group of action anthropologists formed in the discipline, may in part be traced to several internal characteristics within the original action anthropology program. This section discusses some of those characteristics.

**LEADERSHIP AND THE GROWTH OF RESEARCH TRADITIONS.** The success of scientific traditions appears to be

TABLE 1. PERSONALITY TRAITS SHARED AND NOT SHARED BY SPENCE AND TOLMAN\*

Traits shared by Spence and Tolman	
1. Inspiring	
2. Creative	
3. In teaching, presented primarily his own position	
4. Critical of others' systematic position	
5. Overly influenced by others' views of his approach	
Traits on which Spence and Tolman differed	
6. Convinced of the value of his own position	
7. Took himself seriously	
8. Expected strong commitment to his approach from me	
9. Authoritarian	
10. Aggressive	
11. Strong-willed	
12. Accepted criticism well	
13. Allowed me autonomy in a choice of research problem	
14. Open-minded	
15. Humorous	

\* After Campbell (1979) Table 1 and Table 2.

determined in large part by social factors (Kuhn 1970; Richter 1972; Brannigan 1981). Among the sociological characteristics of successful scientific traditions is that they are the product of a self-perpetuating community. This community serves as the arena in which beliefs (i.e., the knowledge tradition of a discipline) are developed, improved and passed on from established scientists to novices. Campbell (1979: 113) says:

... before a scientific community can be a self-perpetuating social vehicle for an ever-improving set of beliefs about the physical world, it must first meet the social structural requirements of being a self-perpetuating social system at all. The requirements of achieving this tribal continuity come first, even if they compete and interfere with the cognitive task of increasing the validity of the image of the physical world which the tribe carries.

One requirement Campbell explores is the role of a senior scientist, serving as "tribal leader," in achieving group cohesion and perpetuation. He argues that the personal style with which this leader works affects how successfully the group will cohere and how long it will continue. By "success" he means that students continue to work on problems and in ways that follow recognizably from their mentor's teaching. He contrasts the styles of leadership of two psychological theorists, Tolman and Spence, and compares the success of their "schools of learning theory." Both Spence and Tolman made major theoretical statements in the 1930s and 1940s, were individually successful, and trained many students. Yet, Spence established a tradition within psychology; Tolman did not. (This is despite Campbell's judging Tolman's the better theory.)

As part of a larger study (Krantz and Wiggins 1973), students of each theorist were asked to rate their own mentor on a series of personality attributes. The ratings that resulted (see Table 1) differ only on characteristics that might be labeled dogmatism, open-mindedness, humor, and tolerance.

TABLE 2. PERSONALITY RATINGS OF SPENCE, TOLMAN, AND TAX COMPARED\*

	Spence (N = 53)	Tolman (N = 28)	Tax (N = 22)
1. Inspiring	91%	93%	92%
2. Creative	85	96	81
3. In teaching, presented primarily his own position	64	96	31
4. Critical of others' systematic position	96	61	26
5. Overly influenced by others' views of his approach	10	18	0
6. Convinced of the value of his own position	98	46	80
7. Took himself seriously	98	32	90
8. Expected strong commitment to his approach from me	98	4	36
9. Authoritarian	88	0	14
10. Aggressive	84	4	40
11. Strong-willed	96	36	76
12. Accepted criticism well	13	86	73
13. Allowed me autonomy in a choice of research problem	26	96	81
14. Open-minded	6	100	81
15. Humorous	28	96	90

\* Percentage of raters attributing the characteristic to each person, condensed from the top three categories of a nine-point rating scale in Krantz's and Wiggins's (1973) study. Ratings of Spence and Tolman taken from Campbell (1979).

They do not differ on characteristics related to perceived creativity, charisma, and intelligence. The Campbell-Krantz-Wiggins analysis is suggestive for action anthropology.

Blanchard (1979) views the development of action anthropology as a direct outgrowth of the working style of Sol Tax. Others of his students whom I interviewed agree with this assessment. They suggest that the philosophy of autonomy of choice, self-determination, open-mindedness, and encouragement of critical discussion that Tax described in "The Freedom to Make Mistakes," is an extension of his own personality and of his style of anthropological work. These attitudes were formalized in the description and conduct of the early action anthropology projects (Tax 1952, 1975; Gearing et al. 1960; Merrill 1951).

I sent the personality questionnaire used by Krantz and Wiggins (1973) to 57 professional anthropologists. These included people Tax identified as having worked with him at some time, or in whose training he considered himself to have been active in some important way (for example, serving on their dissertation committee or directing their dissertation). Twenty-two of these returned completed questionnaires, eight others responded with substantive letters, and five judged their contact with Tax too limited to allow them to respond reasonably to the questionnaire.<sup>4</sup>

These questionnaire responses conform to the Krantz-Wiggins-Campbell analysis (see Table 2). Comparison of them with the responses made by students of Tolman and Spence

reveal Tax to be perceived, on those characteristics that appear to correlate with the formation of traditions within disciplines, much more like Tolman than like Spence.

Campbell says of Tolman that he was the most beloved of the theorists studied by Krantz and Wiggins. And, he says that Tolman was,

... the loving, responsible father to all of us graduate students, concerned about us as persons and about our families, and working hard to get us good jobs. But he was also a father who enjoyed being a fellow child, a lovable scalawag. In ... motion pictures of him ... many taken at conventions in the company of great and serious psychologists—he is invariably thumbing his nose at the camera in a happy, lovable, self-conscious way (Campbell 1979:191).

I include here some excerpts from the letters, comments on the questionnaires, and from my interview notes to fill out the picture of Tax given by the questionnaire responses.<sup>5</sup>

I can be concise by saying that everything I am and have professionally is due to Sol Tax. He has been my intellectual mentor from the first day I began working as his research assistant ... His seminal mind has always been a source of wonder to me. I still learn from him every time we talk on the phone or at meetings. Without his practical assistance in getting me jobs and fellowships, his intellectual guidance ... and his emotional support for me as a terrified, insecure graduate student, I would not have finished graduate school. His action anthropology approach and his philosophy of people's freedom to make their own mistakes, have been the guiding principle of my intellectual and personal life. I realize that to the extent I can I have modeled my style of attacking problems and my teaching and interaction with students much on his ...

One evening each week he'd have a group of students over to spend the evening. Here we would see the "laid back" Tax offering genuine interest and concern in others' problems and interests. He would arrive about 6:00, have dinner and a drink or two, then sit with his family talking about the day and coming events. When students came over they too entered the family circle to be warmed by Tax's somewhat mischievous charm.

Sol encouraged one to think independently which may be why I do not feel indebted to him.

Dr. Tax bothered to show me how to write better and to advise me, in contrast to spending all our time hacking at my divergences from his position as others did.

Sol was a lousy teacher in formal settings, usually unprepared.

He had a point of view, a perspective, and he encouraged my best work, helped me personally, was supportive. He is a fine and open human being, and it was fun to explore ideas with him. . .

I think students generally found him kind and helpful, if occasionally gruff.

Tax was for a variety of reasons relatively unimportant in my life—in part because of WWII and its demands, and in part because of his tendency to avoid and be avoided by left wing students during the 40s and 50s. I should add that my flamboyance left him as cold as his paternalism did me.

... as he came to be one of the major figures in anthropology, it did not change his ego, it did not go to his head. He was always approachable and friendly, and yet always busy and preoccupied with the various projects in which he was invariably involved. He also had a subtle sense of humor!

Sol never did deal with ideas in a flashy intellectual fashion that made you feel you were in the presence of a "Great Mind" ... he didn't give inspired lectures. He was around, accessible, sensible, tolerant, he listened and understood, he organized things, he got things done.

He always instantly understood even the most esoteric statements and always put his finger on the core of the matter. He preferred clear, direct statements of anthropological material to unnecessary

obfuscation and often reduced the latter to substance rather than show.

I have the greatest respect for his professional work and for him as a person. The respect that he has earned among Indians is unusual for an anthropologist, and much admired by me.

There was, for a time, an "American Indian Colloquium" in Chicago. Sol and I both attended—he as an eminent scholar, I as a very raw neophyte. After a particularly abstruse paper was presented, one evening, and as Sol was rising to leave (early, as was his custom), he said, acidly, "I wish there were an Indian here!" Since then I have tried, although not always successfully, to write only things that I could read confidently to an intelligent Indian audience. God bless Sol Tax!

An interesting dilemma appears to result if the Krantz-Wiggins-Campbell analysis of the social prerequisites for success of scientific traditions is correct. On the one hand, action anthropology and Sol Tax's own style of work depend upon a "freedom to make mistakes" philosophy. But, on the other hand, actually following such a philosophy inhibits the successful development and growth of a scientific tradition. Action anthropology appears to be a self-defeating scientific paradigm. Yet this dilemma is more apparent than real. I believe it dissolves when we reevaluate the nature of the reward structure for anthropological work.

### *Rewards and Recruitment to Traditions*

In order for a tradition to grow within a discipline the tradition must attract new members. To do this it must hold out the promise that participants in the tradition will be appropriately rewarded by the discipline. For the most part, the followers of action anthropology did not get these rewards.

Originally conceived of as an academically-based activity, the people working on the Fox Project and on other action anthropology programs soon learned that involvement required that they spend a large part of their private and professional lives outside of academic anthropology departments. For many this alternative was inconsistent with the highly theoretical outlook of the department in which they were seeking their Ph.D. degree (University of Chicago), and it was also unappealing.

In order for there to develop a group of action anthropologists large enough to have an influence on the discipline, there needed to be universities where they could be trained. Those action anthropologists who worked outside of academic departments of anthropology could not routinely contribute to the training of graduate students.

Those with appointments to an anthropology faculty again found the recruitment of students complicated by a number of things. During the time that these action anthropologists were beginning their professional careers, the discipline was very deeply involved in gaining legitimacy as a "social science" (Count 1948a, 1948b). This meant demonstrating anthropology's utility in a forum that used criteria of worth that put anthropology at a disadvantage (Rubinstein 1984). In that intellectual climate the value of traditional applied anthropology, to say nothing of action anthropology, was suspect.

Since in any department an action anthropologist would be clearly outnumbered by other faculty members who had

a commitment to the "science" of anthropology, students who wanted to finish their degrees and to get good jobs were wise not to put too much time into developing their skills as action anthropologists.

This was also true of the anthropology department at the University of Chicago. That department was inhospitable enough to action anthropology that two former students there report that they judged it politically wise to choose a dissertation topic unconnected with their action anthropology experience. Moreover, from the mid-1950s onward Tax was involved with so many other projects (travelling in connection with the founding of *Current Anthropology*; organizing, running, and editing the proceedings of the Darwin Centennial Symposia; serving as president of the American Anthropological Association; organizing the American Indian Chicago Conference, to list some of those) that he provided no haven in which the next generation of action anthropologists could be trained.

Those who continued to work in the action anthropology tradition frequently found themselves, by choice as well as by default, directing their activities and writings to the communities in which they worked. Often, therefore, their constituencies were not other anthropologists. Those whose work was non-academic spoke to the communities they worked in. Those who worked in academic positions outside of anthropology departments spoke to those other disciplines.

While being identified as anthropologists by their constituencies, they came themselves to identify with those constituencies. For example, Lisa Peattie (1981:1-2) says of her recent work:

It is not an attempt to introduce the anthropologist's knowledge of culture and society as a counter balance to economic thinking. Anthropologists have been expert at criticizing development planning for its destruction of native communities and traditional values and for ineptness at taking cultural and social factors into account. I do not think that I can add much to this kind of criticism. And what is more important, and more autobiographical, I joined the planners long ago. Development planners are more my reference group than are anthropologists. But in that world, I am still an anthropologist. I find the anthropological perspective useful in thinking about development.

Taking such a position may provide personal satisfaction, but it also somewhat isolates these people from the larger anthropological community.

This isolation was reinforced by a bias in anthropological publications against reports of what happened (or did not happen) during the course of an action or applied anthropology program. Thus, action anthropologists published relatively little in the anthropology literature of what they might have contributed. Instead, their audience became the groups they worked with—whether self-help groups (Borman 1979), city planners (Peattie 1968), Native Americans (Ablon 1962), university communities (Hinshaw and Young 1979), people with socially stigmatized characteristics such as dwarfism (Ablon 1984), or others.

One result of this situation is that the kinds of very critical issues raised by attempts to use anthropology to seek solutions to practical problems never were discussed fully from the action anthropology perspective. In a sense, action anthropology was never able to achieve "critical mass" as a research group or as a domain of discourse within the discipline.

### *Action Anthropology and the Measure of Success*

This discussion has focused on what has not happened to action anthropology: it has not become a "school" within the discipline. This is due to its creation in a period during which its epistemological views were unwelcome, and because it did not meet the sociological requirements of disciplinary development. Viewed from this perspective, action anthropology has not been a particularly successful program.

Yet, the judgement can be different if we consider what action anthropologists have individually accomplished using the approach. Action anthropology programs have developed models of community self-determination that elsewhere have been taken up and put to use. This effect has ranged from providing an approach to economic self-determination among American Indians (Ablon 1962; Lurie 1973), to the setting up of mutual aid and self-help groups (Borman et al. 1982; Lieberman and Borman 1979), from influencing the way urban planners approach their subject (Peattie 1981), to helping urban communities participate in the design of their own future (Tax 1959, 1968), and to helping find ways that the members of a community can participate in the planning of health care programs (Kelly 1984; Rubinstein et al. 1985).

During its 35-year history, action anthropology has been useful to people seeking ways to deal with problems in their day-to-day life. In the course of giving this help we have also learned much about the processes of social life. If action anthropology's success is measured by how well it has served to facilitate other people's attempts to improve the quality of their own lives, and not simply by how much of an effect it has had on the discipline, then our estimation of the value of action anthropology may indeed be different.

The social demands on anthropology, and the changing nature of its epistemological environment, require that we make clear just what our contribution can be to public policy. The increasingly high priority given to anthropological work on practical problems challenges us to reconsider the role that our discipline can play in better understanding and enhancing our own society. If we are to arrive at models of professionalism in anthropology that are at once more personally satisfying and scientifically rigorous, I believe we must again explore seriously the ethical and epistemological imperatives of action anthropology.<sup>6</sup>

### NOTES

<sup>1</sup> There are, of course, many variations in approach and emphasis within "traditional applied anthropology" (see van Willigen 1980; Goldschmidt 1979). Yet, because to some degree each shares the characteristics described here, they can legitimately be treated together for this analysis. Likewise, there are a number of applied anthropologists who consider their work to be action research, but who do not share the particular set of commitments held by the approach defined as action anthropology by Tax and his students.

<sup>2</sup> George Foster tells me that he too finds applied research to provide a setting in which basic theoretical insights can regularly be gained.

<sup>3</sup> Failure at basic research may also have personal consequences. This is especially so if the theoretical position advocated by a researcher is heretical. Paul Kammerer's suicide after some of his work on the inheritance of acquired characteristics was found to be fraudulent is one case (see Koestler 1971; Rubinstein et al. 1984).

<sup>4</sup> These people were selected from a listing of all people who received Ph.D. degrees from the Department of Anthropology at the



University of Chicago between 1949 and 1982. Some of the addresses available in the department and other records were quite old, and perhaps inaccurate.

<sup>3</sup> I can discover no characteristic common to all of those who did not respond to the questionnaire. It may be that they did not reply because they had only unflattering things to say about Tax. In any event, the anecdotal material contains very few negative assessments of Tax as a person, scholar, or teacher.

<sup>6</sup> After completing this paper I circulated it to all of the people who responded to the questionnaire (even if they declined to answer it). Several wrote back. While some disagreed about the relative importance of epistemological, economic, personality and other factors for the success of action anthropology, all found the analysis generally accurate. One of these replies is especially revealing:

"I believe that I dismissed Tax as having had any kind of significant impact on my theorizing anthropologically—which is why I declined to answer the questionnaire. I believe still that nothing he said is built into how I think about anthropology as such. What I came to realize in reading your paper, however, is that as a human being, Tax's teachings probably had a more profound effect on me than anyone or anything I learned at Chicago. I speak of Tax's dictum that 'People have a right to make mistakes.'

"... I have followed it [this dictum] always in dealing with my students, insisting that they take responsibility for their ideas. I have followed it scrupulously when dealing both with native peoples and with the governments charged with administering them. Sol Tax's teachings have never been just anthropology to me, they've been my life.

"I have found adherence to the principle of being unprincipled somewhat costly . . . I have frequently been disappointed that my students did not see that I had some major input into their thinking . . . Governments (and granting agencies) have found me very difficult to deal with because of having no evident and defensible "position" on most issues—one which they could either support or oppose. One becomes either unpopular or invisible for them, depending on the context.

"So I guess I have to change my mind and confess that Tax more than any other had some lasting impact on my way of thinking . . .

"What bothers me now is that he must sometimes feel what I have felt when struggling to make people take responsibility (and credit) for their lives—underappreciated! . . . I seriously underappreciated Tax. Probably just as well I never told him—he'd probably be embarrassed."

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