From:
Fieldwork: the Correspondence of Robert Redfield & Sol Tax. Robert A. Rubinstein, editor. Boulder, CO: Westview Press, 1991

Introduction

Reflection and Reflexivity in Anthropology

Robert A. Rubinstein

The years between 1930 and the early 1940s were especially important for the development of anthropology in the United States. This was a time of intellectual fervor and, in relation to the previous decades, unparalleled growth. During this period, the subject matter and methods of anthropology in the United States were reoriented and the discipline was revitalized as a profession. The intellectual outlines of post-World War II anthropology in the United States formed in the concerns and experiences of anthropologists working during this time. Eggan (1968:134) characterizes this as the start of the "modern period" of anthropology in the United States. To some degree, today's anthropological discussions still reflect these developments.

Robert Redfield and Sol Tax played prominent roles in the development of anthropology in the United States. Beginning in the 1930s, separately and together, they developed lines of research and theory that at first anticipated and later guided the growth of anthropological work on culture change, ethnic relations, kinship analysis, world view, and economics. For anthropology in general, and especially for the study of Mesoamerican ethnology, their work supplied many of the empirical themes and theoretical questions which ethnographers subsequently explored. Through their publications and their supervision of the training of younger anthropologists, their work helped define the major outlines of anthropological research. Moreover, their work has a continuing influence as younger scholars expand upon or criticize it.

For helpful comments on earlier drafts of this essay I thank Joan Ablon, Peter M. Ascoli, George Foster, Mary LeCron Foster, Alice B. Kehoe, Sandra D. Lane, Charles Leslie, David Maines, Lisa Peattie, Rik Pinxten, Sol Tax, and Neclâ Tschirgi.

The correspondence of Robert Redfield and Sol Tax during the years 1934–1941 is historically important. The letters are resources for understanding the development of anthropology as a professional community. Reading them also gives the opportunity to analyze the earlier experiences of our discipline and to better understand the nature of anthropological data collection and interpretation. These letters offer us the opportunity to put our exploration of the history of anthropology to an epistemologically revitalizing end.

This introduction sketches the intellectual, personal, and institutional circumstances in which these letters were written. Beyond their historically descriptive importance, the material in these letters contributes directly to current concerns about the nature and status of anthropological knowledge. The later part of this introduction relates the letters to contemporary epistemological discussions in anthropology.

It is useful, before taking up specifics, to note four general domains of anthropological interest on which these letters bear. First, ethnographic fieldwork is the major method through which anthropologists gather data. The "modern" view of what anthropological fieldwork involves crystallized during the years when these letters were written. This correspondence presents a very clear picture of how two important anthropologists thought about and conducted their field research. From their letters we see how they made decisions during field research about which lines of inquiry to pursue, and about how to gather data to test specific ethnographic hypotheses.

Second, Redfield and Tax also corresponded about what they were finding during their research. Thus, these letters contain a wealth of ethnographic data that is useful for understanding how Redfield and Tax arrived at their descriptive and theoretical conclusions. Some of this material, especially their concern with ideational aspects of social life, allows a fresh evaluation of their research programs.

Third, no aspect of professional social scientific life is more difficult to master than the construction of social theory. By letting us "overhear" their discussions, reading these letters allows us to follow Redfield's and Tax's development of theory. These letters, thus, to some extent, make public the usually private processes underlying the intellectual give-and-take of social theory construction.

Finally, besides giving an opportunity for developing insights into the growth of anthropological knowledge, these letters can help us better understand the growth and development of personal relationships within scientific disciplines. Spanning more than half-a-dozen years, the letters allow us to trace the evolution of the relationship between Redfield and Tax from its student-teacher origin to its mature form of valued colleagues and trusted friends.

Anthropology badly needs straightforward reports of what field researchers really do during their work. How do the pragmatic exigencies of extended anthropological fieldwork—including difficulties of housekeeping, personal comfort, funding, and relations with informants—affect the kinds and quality of data we gather? What considerations enter into the choice of a field site, or of the unit of analysis we use? How do the day-to-day activities of fieldwork relate to social theory construction? What is the nature of anthropological apprenticeship, and how does this affect the growth of professional relations in our discipline? What is the relationship between professional publications and fieldwork activities? The material in the correspondence between Robert Redfield and Sol Tax about their fieldwork in Guatemala provides an unparalleled opportunity to explore these, and other, questions.

Redfield, Tax, and Anthropology before World War II

In 1934, the anthropological community of which Robert Redfield and Sol Tax were members was small but growing.[1] Between 1902 and 1941 the membership of the American Anthropological Association grew slightly more than fourfold, from 172 to 696. The numbers of Ph.D. degrees awarded each year during this period illustrate this growth. Between 1900 and 1940, 228 doctoral degrees in anthropology were awarded in the United States; 14 during 1901–1910, 20 during 1911–1920, 40 during 1921–1930, and 154 during 1931–1940 (Frantz 1985:84–85). Fifteen universities awarded these degrees, though 111 of the 154 degrees awarded between 1931 and 1940 came from only six departments (Ebihara 1985:102).

Before 1930, because of the small size of the anthropological community and because most of its members were trained in a few institutions, the discipline was characterized by a set of shared intellectual commitments. Eggan (1968:130) identifies this as the "American historical school," which derived from the work and teaching of Franz Boas, centered at Columbia University. Important for understanding the intellectual context of the correspondence between Redfield and Tax was the Boasian concern with collecting ethnographic information about Native American groups that were then thought to be disappearing. This "salvage ethnography" meant that, especially before the 1930s, in the United States ethnographic fieldwork usually consisted of brief research trips during which a few, mainly older, informants were extensively interviewed. The aim of this work was to record their cultural knowledge so that this material could be used for historical reconstructions of Native American life. Salvage ethnography often focused on a group's material culture, stressing the collection of artifacts (see Stocking 1968,1976). The emphasis on historical reconstruction led to the development of many anthropological ideas, such as the definition of culture-areas based on the distribution of culture-traits which were accounted for by chronologically oriented distributional maps (Eggan 1968:130).

One circumstance affecting the growth of anthropology during the years before World War II was that most professional anthropologists worked outside of academic settings. Not until after World War II did academic employment become the more general pattern for Ph.D. trained anthropologists. Thus, much anthropological research was done by those working in museums, government departments (for example the Bureau of American Ethnology), or research institutions (like the Carnegie Institution of Washington or the National Research Council). As Stocking (1976:9–10) points out, "the customary linkage of archaeology and ethnology in the museum context surely reinforced the historical orientation of anthropological theory, just as the object-orientation of museum collections sustained a particular attitude toward ethnographic data."

The unified Boasian character of anthropology in the United States began to change in the 1930s. The emphasis within anthropology began to shift away from concerns with historical reconstruction based on inventorying culture traits and moved toward a focus on contemporary processes and patterns. Some of this new emphasis came from anthropologists trained by Boas. But the shift away from the American historical approach in anthropology also was stimulated from outside, and did not progress much until the 1930s. By then, Robert Redfield, with his interest in the processes of contemporary culture change and a broad ethnographic database from his Maya research, was a leader (along with Ruth Benedict, Margaret Mead, Edward Sapir, and others) in this reorientation (Stocking 1976:15–17).

Among the things motivating the rethinking of anthropology in the United States at this time was the stimulation provided by Bronislaw Malinowski's and A.R. Radcliffe-Brown's advocacy of their synchronic, functionalist approaches to the analysis of culture and society. They both lectured in the United States in 1926 as guests of the Laura Spelman Rockefeller Foundation. But the effect of their visit was not fully felt until the 1930s. Their lectures provoked considerable interest and controversy. They departed from American anthropological thought in two main ways. First, their work was based on field research that was substantially different from that current in the United States. Malinowski (1922:6), for instance, described his fieldwork among the

Trobriand Islanders, which he had published in *Argonauts of the Western Pacific*, for which he lived in "as close a contact with the natives as possible." This method was significantly different than that used in salvage ethnography in the United States and it provided a conception of ethnographic research which American anthropologists sought to emulate. Much methodological reflection in American anthropology followed from the working out of that model. The letters exchanged by Redfield and Tax during their Guatemalan research reflect this concern with how anthropologists can know other peoples.

Secondly, Malinowski and Radcliffe-Brown each brought to the United States a more theoretical approach than was current among American anthropologists. Radcliffe-Brown especially challenged the American historical school by his insistence that "social anthropology" could discover natural laws of human society by applying a comparative approach to social organization. His rejection as conjecture of the uses to which the American historical school put history, and his use of sociological concepts developed by Emile Durkheim, made him a center of controversy and intellectual excitement (see Eggan 1937).

In 1931 Radcliffe-Brown came to the University of Chicago as Professor of Anthropology. There he concentrated his attentions on theory in the study of social organization. He remained at the University of Chicago until 1937, when he left to become the first professor of anthropology at Oxford University. From Chicago his influence on American anthropology became widespread. At Chicago, Radcliffe-Brown was Tax's doctoral dissertation adviser, and one of Robert Redfield's senior colleagues.

Robert Redfield

Robert Redfield was a remarkable man. Widely admired by colleagues and students for his intellectual abilities and his dedication to high-minded ideals (Hutchins 1958), he had little patience for frivolous conversation. His colleagues and students found him an imposing figure who quickly grasped the essence of even the most specialized theoretical discussions in anthropology. Yet he also always held that the study of humankind meant the study of the whole—the whole person, the whole community (Tax 1958:2). In contrast to this stern demeanor with students and colleagues, Redfield presented a more congenial side of his personality during fieldwork. His collaborator and friend Alfonso Villa Rojas (1958:6) recalled:

> Dr. Redfield was a man of very tender and amiable disposition, especially when dealing with the natives of simple societies. I can remember very clearly how happy and relaxed he was during his long periods of field work, when he had an opportunity to participate in the local activities of the Indians and of being moved by their same feelings and emotions. In this connection he was very different from the man confronting serious matters of theoretical importance with his colleagues and students: then, he was serious, demanding, rigorous, with a devastating logic that made us feel reverence and respect for him.

When he died in 1958, of lymphatic leukemia, Robert Redfield was among the most influential and distinguished anthropologists in the United States. Born in Chicago, Illinois, in 1897, Redfield came to anthropology after he had already begun to practice as a lawyer. After receiving a law degree from the University of Chicago in 1921, Redfield joined his father's law firm. The previous year he had married Margaret Lucy Park, the daughter of Robert Park, Professor of Sociology at the University of Chicago.

Redfield was discontented with law practice. Following a 1923 visit to Mexico, and with the encouragement of Park, Redfield returned to the University of Chicago to study anthropology. He received his Ph.D. from the University in 1928. Fay-Cooper Cole, who had trained under Boas at Columbia University, headed the anthropology section of the joint sociology and anthropology department at the University. Other anthropology faculty members at the University, like Edward Sapir and Manuel Andrade, were also Boas students. Redfield, however, was most greatly impressed with the work of the Chicago sociologists who were developing an ambitious program of research on urban social life, and these scholars had the strongest influence on Redfield's training. Thus, unlike most of his anthropological contemporaries, Redfield was not a Boasian.

In 1926 Redfield returned to Mexico to conduct research for his doctoral dissertation. As a Fellow of the Social Science Research Council, Redfield spent November 1926 to July 1927 studying the Mexican village of Tepoztlan. Pursuing interests he had developed through his contact with Chicago sociology, he sought to better understand the relations of urban and "folk" cultures and the processes by which "primitive man becomes civilized man" (Stocking 1976:17).

In doing this research, he departed from his contemporaries in at least three important ways. First, his study of Mexican peasants was unique for its focus on contemporary life, rather than remembered culture. Second, the manner in which he conducted his study—residence among the people whom he studied, and learning their language—departed from the then more common pattern of brief fieldwork stays

using the aid of an interpreter. Third, he focused on a community embedded in a complex social and cultural context, rather than on a well-bounded, relatively isolated social group.

Redfield was thus among the first American anthropologists to conduct what we consider today to be normal fieldwork—going to a place and immersing oneself in a community in order to learn about it. In addition, by departing from the common tendency to study Native Americans, Redfield became the first North American anthropologist to do a community study in Mexico (Kemper 1985:140).

Between 1925 and 1926 Redfield was an Instructor of Sociology at the University of Colorado. He returned to the University of Chicago in 1927 as an Instructor of Anthropology. In 1928, he became Assistant Professor and was promoted in 1930 to Associate Professor of Anthropology. Soon after, in 1934, he became Professor of Anthropology and the second Dean of the Social Science Division at the University of Chicago, a position he held until 1946, throughout the period of the letters in this book.

The University of Chicago Press published Redfield's first book, *Tepoztlan, a Mexican Village: A Study of Folk Life*, in 1930. During that year he was also appointed Research Associate of the Carnegie Institution of Washington. This appointment ended in 1946. Under these auspices, Redfield directed ethnological and sociological investigations of the peoples of Yucatan and Mexico. In appointing Redfield, Alfred Kidder, director of the Institution's Division of Historical Research since 1929, sought to supplement the Division's archaeological and historical research by expanding its research program to include contemporary ethnological work among the Maya. Redfield set out a broad-based research program covering Yucatan and Guatemala. The letters in this book result from the work done in Guatemala.

Before the Guatemala research was undertaken, Redfield and his colleagues had already completed most of their fieldwork for their Yucatan research. In this program, Redfield tried to clarify the relationship among urban and "primitive" peoples in Yucatan by exploring social change and cultural disorganization. To do this he and his colleagues used a model of rural to urban change to guide their research (see Redfield 1941, Hansen 1976). The Yucatan research resulted in several important publications (Redfield and Villa Rojas 1934, Redfield 1941, Villa Rojas 1945), and his theoretical model of the folk-urban continuum was to become one of his more important, though not always understood, contributions to social science in the United States.

> Redfield's subject was social change and cultural disorganization. In addition his method of analysis was unfamiliar to anthropologists. The

> heuristic use of a model folk society was misunderstood by historically minded ethnologists, who saw discrepancies between the model and particular folk communities as flaws in the model rather than as invitations to analyze the causes of social variations. (Leslie 1968:352)

Redfield planned to study the main urban center in Yucatan and compare it with a series of increasingly more rural and isolated villages. He was helped in this research by two other ethnographers. One, Asael Hansen, worked in Merida, the peninsula's main city (Hansen 1976). The other, Alfonso Villa Rojas, a native of Yucatan, had been a school teacher when Redfield met him in the village of Chan Kom. There he became Redfield's assistant and eventual collaborator. Later, Villa Rojas did an ethnography of the remote village of Tusik in the Department of Quintana Roo. Villa Rojas subsequently became one of Mexico's leading anthropologists. Redfield himself worked in the intermediate villages of Dzitas and Chan Kom.

When he extended the Carnegie Institution's research to Guatemala, Redfield sought to test the model of folk-urban social change that he had developed in Yucatan. There he had observed Indian-Ladino differences and he sought to explore the nature and extent of these differences in Guatemala too. But he was also influenced by his more general concerns with racial equality in the United States. These concerns are evident in Redfield's discussion, in his letters to Tax, of the Social Science Research Council's Committee on Acculturation. They also characterized his professional contributions as an "expert witness" in racial discrimination cases later (Ming 1958).

Throughout his professional career, Redfield was acutely aware of the tensions between the science and the craft of anthropology. Always demanding rigor, precision, and empathy in anthropological work, Redfield self-consciously sought to make explicit the processes by which we come to understand other peoples. When he sent Sol Tax to Guatemala in 1934, Redfield gave him considerable freedom to develop the research program there as he saw fit. Yet despite busy teaching and administrative loads, Redfield was actively involved in defining what kinds of activities ought to go into anthropological fieldwork and in considering, together with Tax, how to use his experience to improve his fieldwork practice.

Sol Tax

Sol Tax's appointment, in 1934, as an Ethnologist with the Division of Historical Research of the Carnegie Institution of Washington was

a bit of professional good fortune. Good academic jobs and suitable research positions were scarce, so the opportunity to continue as a professional ethnologist in a job with some security was one he welcomed. Redfield wrote Tax, on 18 July 1933, "The question of policy has been decided in favor of extending the ethnological work of the Carnegie Institution into Guatemala. . . . I would like to propose an understanding whereby you would begin the study of Spanish with the likelihood . . . that you would begin fieldwork about January 1935." Tax accepted immediately and he and his wife Gertrude (nee Gertrude Jospe Katz) began studying Spanish and the few ethnological materials about Guatemala that were then available.

Sol Tax was born in Chicago, Illinois on 30 October 1907. He grew up mainly in Milwaukee, Wisconsin. As an undergraduate at the University of Wisconsin at Madison, Tax studied anthropology with Ralph Linton, whose friend he became. For his Ph.B. degree (University of Wisconsin, Madison, 1931) he submitted a thesis, *A Re-Interpretation of Culture, with an Examination of Animal Behavior*, which sought to integrate cultural and biological aspects of anthropology.

Following Linton's advice, in 1931 Tax entered the Anthropology Department of the University of Chicago to study for a Ph.D. under the direction of Radcliffe-Brown. Redfield was already an established faculty member in the Department when Tax came to the University and was his principal graduate advisor (Hinshaw 1979:761). Tax was mostly influenced during his graduate studies by Radcliffe-Brown, and by his continuing association with Ralph Linton. During his graduate education, Tax read and commented critically on the manuscript for Linton's influential *The Study of Man*, published in 1936. In this book Linton introduced to American anthropology the concepts of "status" and "role" and insisted that anthropology should separate conceptually the study of "culture" and "society." Tax sought to integrate Linton's and Radcliffe-Brown's views, at one point organizing and chairing a debate between them. Tax had his first experiences of anthropological research as a member of two archaeologically-oriented field schools in 1930 (the Logan Museum Archaeological Expedition to Algeria and the American School of Prehistoric Research in Europe). His participation in the Summer Ethnology Program at the Mescalero Indian Reservation, directed by Ruth Benedict in 1931, marked his first intensive ethnological experience.

Tax conducted research for his doctoral dissertation (1932–1933) among Central Algonquin peoples, focusing on questions concerning the history and meaning of kinship. This research was very much in the mode set by Radcliffe-Brown and during it Tax developed the ego-less kinship chart and the notion that kinship relations were based on accommodation among universal rules and principles present in small societies. Tax was in the midst of his field research for his doctoral dissertation when Redfield's invitation to join the Carnegie's Maya Research Project reached him at the Menominee Indian Reservation.

Redfield's selection of Tax for the research project in Guatemala was a natural choice. Having known Tax as a student at the University of Chicago, Redfield was aware of his enormous energy and enterprise. Tax's professional interests were also broad, an important advantage in a researcher being sent to open an area to anthropological exploration. But perhaps more importantly, Redfield found in Tax an ethnologist who, because of his training with Linton and Radcliffe-Brown, was prepared to think theoretically about the organization of society.

The Taxes left for Guatemala directly after Sol defended his doctoral dissertation. Tax carried with him several letters of introduction to businessmen, government officials, and missionaries whom Redfield had met during a reconnaissance trip the year before. For the next six years they conducted research there, returning to the United States each summer. During this time Tax and Redfield were consistently interested in exploring ways to improve anthropological fieldwork and the knowledge gained by this method. In 1940 they began to plan seminars on fieldwork methods. As Hinshaw (1979:761) notes, "Tax's early and continuing interests in the training of anthropologists and the more central theoretical and methodological concerns of the discipline" are often overshadowed by his later work. Nonetheless, they are clearly evident in the letters to Redfield from Guatemala.

Tax's research in Guatemala resulted in the definition of the cultural patterning of the Highland Maya. Together with Redfield, Tax helped to define the shape of Mesoamerican ethnology. In influential publications Tax proposed the *municipio* as the central organizational unit of the region, detailed the features of Indian economics, and reported on world view in the highlands (Tax 1937, 1941, 1953). In his work in the highlands, Tax reported "more impersonalism in social interaction than Redfield found in Yucatan communities of comparable size, raising questions about the origins of impersonal, atomistic, and pragmatic social relations in urban, industrial society given the same qualities of social interaction among the highland Maya" (Hinshaw 1979:762). It has been suggested that relations between Tax and Redfield suffered because of Tax's departure from the model proposed in Redfield's folk-urban continuum. In fact, as their letters show, they each took this result as a problem for anthropological theory and method. Throughout their association their relationship deepened, centered in

part on their shared interest in making explicit the processes of fieldwork and ethnographic understanding.

In 1941, with funding from the Carnegie Institution, the Taxes went to Mexico. There Tax spent one year as Visiting Professor at the Instituto Nacional de Antropología é Historía in Mexico, and started fieldwork in Chiapas. Tax's fieldwork and training of Mexican anthropology students in research methods lasted four years. Thus, the interest that emerged in Guatemala in the development and teaching of ethnographic research methods was put to immediate use:

> Aside from his courses on Maya ethnology and his fieldwork seminar, Tax was caught up in the endless round of meetings and arrangements among the acronymic anthropological agencies . . . as well as in frequent briefings at the U.S. Embassy. Not only was he able to learn about a variety of projects, actual and proposed, he also managed to gain support from Mexican and U.S. sources to continue his initial fieldwork project in the Tzotzil area of Zinacantan, Chiapas, beyond the initial season conducted in late 1942. (Kemper 1985:149).

From 1940 until 1944 Tax was Research Associate in the Department of Anthropology at the University of Chicago. In 1944 he was appointed Associate Professor in the Department, and four years later became Professor of Anthropology and Associate Dean of the Social Sciences at the University of Chicago. Tax remained associated with the University of Chicago for the rest of his academic career (Rubinstein in press).

Fieldwork and Reflexivity

Despite their concern with improving fieldwork methods, and their attempt to explicate the nature of ethnographic understanding, Redfield's and Tax's efforts are today largely misunderstood by a younger generation of anthropologists, with whom they had relatively little personal contact. In any discipline, work does not simply "speak for itself." Rather, its status and how it is valued derive from what is said about that work later on. Latour (1987:27–28) describes this process: "*the status of a statement depends on later statements.* It is made more or less of a certainty depending on the next sentence that takes it up; this retrospective attribution is repeated for the next new sentence, which in turn might be made more of a fact or of a fiction by a third, and so on. . . ."

The process of retrospective attribution depends upon claims made about earlier work, not necessarily on the examination of the work itself. Thus, this process may institutionalize partial or inaccurate descriptions of the earlier work. For this reason, it is sometimes useful to question the characteristics attributed to earlier work in contemporary discussions. The characterizations of Redfield's and Tax's work in current discussions of fieldwork and ethnographic interpretation incorporate such distortions. Reexamining their Guatemalan correspondence may help, therefore, to shed new light on contemporary debates about the nature of anthropological fieldwork and the status of anthropological knowledge.

Because of its central place in anthropological discussions, fieldwork is both the most fundamental and least understood aspect of social research. The kind of fieldwork in which anthropologists primarily engage, participant-observation, is one of two activities—the other, the systematic construction of social theory from controlled comparisons among human groups—that give the discipline its distinctive character. Yet anthropological fieldwork, and how it relates to theory construction, remains one of the most uncritically described and inaccurately understood domains of anthropological activity. This is paradoxical because during the past two decades the anthropological literature has been virtually flooded with methods texts and with "reflexive reflections" on anthropological fieldwork and theory.[2]

Part of the critical reaction to Redfield's and Tax's work derives from the intensive critique of anthropology that has been taking place during the last two decades. This critique focuses particularly on the epistemological status of its major research activities: doing fieldwork and writing ethnographies, the vehicles through which anthropological theory develops. During this self-critical movement, anthropology has been declared "in crisis," "at a watershed moment," and in the midst of an "experimental moment" (e.g., Marcus and Cushman 1982, Wilford 1990:16), forcing us to recognize the highly tentative status of anthropological constructs and to confront the disorienting conclusion that there is no stable "cultural-self" on which these constructs can rest (Bellah 1977:ix). The analyses advanced during this discussion have captured the imagination of the general public and of professional anthropologists (for example, see Rabinow 1977, Freeman 1983, Clifford and Marcus 1986, Van Maanen 1988, Wilford 1990).

Central to these analyses is the recognition that anthropological knowledge is incomplete and often contradictory: different ethnographers sometimes report different "realities" (compare Redfield 1930 and Lewis 1951, Mead 1928, 1930 and Freeman 1983). Moreover, issues of power and perspective (Mascia-Lees, et al. 1989), questions of how authoritative knowledge is legitimated (Hufford 1982a, 1982b, 1983), of self-awareness and authenticity of voice in the presentation of data (Geertz 1988, Van Maanen 1988), and of the constraints of the historical and cultural contexts within which knowledge develops (Pinxten 1981, Rubinstein et al. 1984:123–159, Laughlin et al. 1986) complicate the description and understanding of cultural and social life.

There have been a variety of critical responses to the acknowledgment of the tentative nature of anthropological knowledge (compare, for example, Pinxten 1981, Clifford and Marcus 1986, and Roth 1987). Of these, two approaches have been particularly prominent within anthropology. The first seeks to overcome the epistemological difficulties by carefully prescribing methods for anthropological research, thus defining the nature and scope of anthropology. Those taking this approach would define the "minimum standards for ethnography" necessary to ensure the quality of ethnographic data and interpretation (Jarvie 1967, Werner 1984, Werner et al. 1987), and have at times appeared to place considerations of method above other aspects of anthropology, practicing "scientism" or "methodolatry" (Rubinstein 1984:173–178; 1989:26).[3]

More noticeably, however, a self-consciously reflective form of response dominates the last two decades. This approach—often termed postmodernism—bespeaks a deep sense of self-doubt and a mistrust of fundamental aspects of the practice of anthropology.[4] In its baldest form, postmodernist anthropology claims that ethnographies are best examined as imaginative, literary constructions rather than as social science.

Postmodernist anthropology explicitly examines ethnographic representation (as manifest in social theory and in ethnographic writing), and it incorporates within it a peculiar view of fieldwork derived from reflections on individual fieldwork experiences (e.g., Rabinow 1977). In this view fieldwork is portrayed as a hopelessly unsystematic, even haphazard, activity plagued with insurmountable problems of understanding (if by understanding is meant describing and accounting for a "real" world), for which anthropologists are ill-prepared and ill-served by their training.

This view of the inadequacies of anthropological practice is said to emerge from the confrontation of the epistemological and methodological difficulties evident in reflective discussions of anthropological research. In fact, however, this characterization of anthropological practice (and that of those seeking minimal standards for ethnography, for that matter) depends on a particular history of the discipline which emphasizes how little guidance fieldworkers received from their professors before their first field experience.

The trading of aphoristic stories about their own fieldwork experiences has probably been a part of anthropology since its formal incorporation as a discipline. From such story telling, an oral tradition developed among anthropologists in which the storyteller-ethnographer (fearless or frightened, puzzled or self-confident, for instance) confronted and overcame difficulties in his or her fieldwork. Often, these were difficulties said to result from lack of training. This educational deficit was then authenticated by a story about the lack of guidance given by professors. For instance, Radcliffe-Brown is said to have replied in response to a request from his students at the University of Chicago for advice on how to conduct their fieldwork: "Get a large notebook and start in the middle because you never know which way things will develop" (Tax and Rubinstein 1986).

Especially when told by accomplished ethnographers, these stories take on an immediacy and importance for their less experienced colleagues. Yet these stories were apocryphal; they told only partial truths. The storyteller-ethnographers told the story in a context and for a purpose—to reveal something about their teachers, about themselves, or about fieldwork, or simply to pass the time. Few of these stories were told, I think, as a way of describing the teller's full preparation for fieldwork.

Eventually, this oral tradition transmuted into a written tradition and took on a status independent of the storyteller. At first the weight of this tradition was used to account for the need for explicit methodological texts in anthropology (e.g., Pelto 1970:ix). Later, these detached stories became the ground upon which to indict the entire anthropological enterprise as methodologically naive or epistemologically preposterous, requiring a shift in focus away from "the other" and onto ourselves (Rabinow 1977, Clifford and Marcus 1986).

Within scholarly communities, written histories—especially those published in peer-reviewed forums—have a great deal more authority than their oral counterparts (Hufford 1983). Yet these histories may be no more complete than the oral tradition from which they originate. It is only that, reported out of their original oral context, the partial and purposeful nature of the stories is obscured. For example, Sol Tax, who reported Radcliffe-Brown's remark if asked about his fieldwork preparation, would have gone on to say that before Radcliffe-Brown's remark he had taken courses in the logic and method of scientific enquiry as part of his graduate training, and that he had previously participated in an ethnographic field school (Tax 1988:3).

Written accounts of lack of fieldwork preparation are no less apocryphal than their oral precursors, and no less purposeful; they are written with both a purpose and a point of view. This may be to encourage a particular type of methodological education (Pelto 1970, Appell 1989), legitimate a particular epistemological preference (Clifford and Marcus 1986), or to present oneself in a particular light (Van Maanen 1988, Sangren 1988).[5]

I do not wish to argue that all was or is now well in the instruction of ethnographic technique in anthropology, or that fieldwork is an activity that can be straightforwardly carried out if the proper methods are adequately taught to an ethnographer before he or she goes to the field. On the contrary, it is remarkable that after nearly two decades of self-conscious self-reflection, anthropologists seem little closer than they were in 1970 to understanding the fundamental anthropological activities of fieldwork and comparative theory construction as collective undertakings.

It is remarkable, but perhaps not surprising. For the most part, reflections on fieldwork published in the postmodernist tradition have not principally been about fieldwork at all, but rather about our experience of fieldwork. To the extent that the reflective accounts of fieldwork and ethnographic representation have focused attention on the characteristics and accomplishments of individual anthropologists, they have failed to engage the epistemological quandaries faced by anthropologists as members of a community of practitioners seeking to systematically explore an area of knowledge.

Methods texts have, also, gone to great trouble to show how anthropological research can adapt to issues first identified and elaborated in other social science disciplines, such as face and construct validity, reliability, and generalizability. In pursuing this goal, great importance is placed on developing "systematic, objectifiable research tools" (Pelto and Pelto 1978:36). Such issues are elaborated at length, often at the expense of the examination of the more mundane and less attractive, but equally important, aspects of fieldwork—including the pragmatic details of doing fieldwork and how such arrangements affect the data one gathers and the interpretation of these data.

Moreover, and more detrimental to the discipline, as I show below, is that some of these reflective accounts have distorted our understanding of the history of American anthropology. Because these accounts, written for publication, have treated fieldwork as a necessary if peripheral aspect of the task of understanding ourselves, they have institutionalized the partial and inaccurate picture of fieldwork training that persisted in the oral lore of anthropology. It is because methodolatrist and postmodernist accounts of fieldwork both fail to examine (though for different reasons) the processes and social organization through which anthropological apprenticeship takes place that our understanding of the dynamics of fieldwork and theory construction remains so meager.

The Department of Anthropology at the University of Chicago trained many professional anthropologists now active in the discipline. Given the importance of that Department to American anthropology, and the enduring importance of Robert Redfield and Sol Tax to that Department, the chance to examine closely their professional correspondence about fieldwork in Guatemala provides an opportunity to reconsider the development of anthropological practice in the United States. The yield of this exploration is exceedingly rich. The remaining sections of this introduction point to some of the themes in contemporary discussions of fieldwork and ethnography to which the *Correspondence* relates. When viewed from the perspective of current debates, in my view, the historical material in these letters takes on an immediacy that can help to serve to refocus this "experimental moment" in anthropology and deepen our understanding of our discipline.

Anthropology as Science

Being an anthropologist can be uncomfortable. Most obviously, sometimes fieldwork conditions are unappealing or physically unpleasant (Chagnon 1977). Less concretely, anthropologists are always in some sense outsiders: "marginal natives" (Freilich 1970), and "professional strangers" (Agar 1980). But more profoundly, the complexities of their collective enterprise and the status of their knowledge have always troubled some anthropologists. In response to difficulties inherent in anthropological work, some have attempted to understand the complicated interactions among anthropological researchers and their subject matters through reasoned and measured analyses (e.g., Redfield 1926, Bateson 1936, 1972, White 1938, Tax et al. 1956, Naroll and Cohen 1970, Pinxten 1981, Hufford 1982, Briggs 1986, Fiske and Shweder 1986).

All too often, however, responses to epistemological difficulties in anthropology are refracted through thick polemic lenses, and anthropological unease is expressed by the assertion of one or more dichotomies: Anthropology seeks *either* to produce a body of laws which account for human social relations (thus it is a nomothetic enterprise) *or* to describe the variety of ways in which people organize their social and cultural lives (thus it is an idiographic discipline). Anthropological accounts are *either* stable and independent of time (thus existing in the "ethnographic present") *or else* they are situationally contingent. Anthropologists study objective realities (social facts) *or else* their subject matter is evanescent. Social and cultural behavior results *either* from nature *or* from nurture. Anthropologists are objective observers *or* they are partisans. *Either* society's basic homogeneity renders it understandable *or* intra-societal variation makes such understanding futile. Ethnographies are *either* straightforward narratives, thus showing "naive realism" *or* they are self-conscious, imaginative, literary constructions.

These oppositions turn complex epistemological issues into seemingly simple dichotomies.[6] All are partial restatements of the apparent opposition that has troubled anthropologists: Anthropology is *either* a science *or* it is something else. The issue since World War II has really been: Anthropology is either a science—*like physics*—or the knowledge it produces is not legitimate or reliable or useful.

The question of whether anthropology—or sociology, or political science, or other disciplines that seek to systematically study aspects of social relations—is science or not has been repeatedly asked throughout this century. However, it was not until after the Second World War that the status of the discipline *as* science became invested with the exceptionally heavy load that it still carries today.

Before the Second World War the question of how one could adequately characterize science brought several competing responses, perhaps especially in the United States. These responses fit roughly one of two general trends. One derived from an analysis closely linked to the pragmatist tradition in philosophical thought (for example that of Dewey or Peirce) while the other followed a logical empiricist (also called logical positivist) approach. During this period, neither the pragmatist nor the logical empiricist approach could claim to have adequately accounted for scientific activity. Instead they offered differing answers to questions about the meaning and logic of inquiry, the nature of scientific progress, the status of scientific theories, and the like.

Logical positivism saw the task of science as translating immediate experience into logical categories and relations so that "the meaning of a theory was essentially a function of its logical syntax along with the class of things or objects to which the (non-logical) terms in the theory refer" (Aronson 1984:5). From this emphasis on the formal relations of syntactic structure it followed that meaning and knowledge ought to be stable and independent of the person who discovered them and of their context. In contrast to this, pragmatist analyses emphasized that all knowledge-gathering processes are both privileged and restricted; they direct attention to some aspects of phenomena and away from others. Moreover, this analysis suggested that meaning derived from symbolic relations and thus that all knowledge is polysemic. Therefore a single account is always incomplete in some fundamental way (Almeder 1973, 1975). One result of this is that knowledge is always viewed as contingent on the contexts in which, and the purposes for which, it is acquired.

Eventually, one particular version of the logical positivist account of science eclipsed other accounts (see Suppe 1977) and for a time it appeared to be *the correct* analysis of what science should be like. Yet before World War II there was a pluralism—and tolerance—in discussions of the epistemological status of the knowledge embodied in the social science disciplines. Some scholars rejected the logical positivist program as a standard for judging epistemological questions because it required a very restricted view of what counted as legitimate empirical knowledge, even while they saw it as offering an adequate model of science (Suppe 1977:6).

It was in this atmosphere of epistemological pluralism that Robert Redfield and Sol Tax approached the question of whether anthropology is or is not a science. The epistemology underlying their work was eclectic. Both Redfield and Tax considered anthropology to be a science. But this claim has to be understood in the intellectual context in which it was made.

For Redfield and Tax, science was not a restricted and restricting exercise. Rather, it was a systematic investigation of human social life undertaken with careful attention both to its observable and intangible aspects. This meant that they moved freely among various methods and theories—as these seemed important—in seeking to better understand social and cultural life. In their field research and theory construction, they did not see themselves as facing questions which required discrete choices between two alternatives. They sought an appropriate, though complex and difficult to find, balance among continuous options (Redfield 1926). It was not an accident that one of Robert Redfield's major theoretical efforts was to develop the folk-urban *continuum* (Redfield 1941):

> Redfield maintained that the intellectual structure for scientific studies of mankind was necessarily pluralistic . . . he reasoned that every set of ideas employed some modes for ordering and interpreting phenomena and neglected others, *all* conceptual approaches to human experiences were partial and incomplete views of them (Leslie 1976:152).

The Correspondence of Robert Redfield and Sol Tax presents us with a chance to recapture the deep epistemological commitment to pluralism in our pursuit of understanding human social and cultural life.[7] During and immediately following World War II scholars (and other consumers of research) came to equate science with the technological innovations and successes that were then so conspicuous. These technological

advances derived from the physical sciences which the logical positivist program seemed to characterize quite adequately. This confusion of science with its product, technology, was mistaken (Count 1948a, 1948b), but, nevertheless, widespread. Soon it began to seem obvious that legitimate knowledge is that derived through science, that science is done by researchers who are neutral and objective and adhere rigorously to a process called "The Scientific Method," and that this method led to predictions based on universal laws.

Self-conscious about the status of their work, yet wanting to secure and to expand their claims to the resources they needed to support that work, many anthropologists after World War II were caught up in the enthusiasm surrounding science. Often they sought legitimacy in the argument that their work too was scientific, and they measured their success as science against a positivist conception of science. Some argued that when thus measured, the discipline was found wanting and so it ought to be remade by requiring that anthropologists work in an explicitly positivist fashion (e.g., Jarvie 1967, Fritz and Plog 1970, Schneider 1974). Others argued that there was no point in pretending that anthropology is science; rather than aping the physical sciences, anthropology ought to be seen for what it is—a literary-interpretive enterprise (Clifford and Marcus 1986).

Proponents of each of these sharply drawn alternative views stress that what is lacking in anthropology is an epistemological sophistication, and that this deficiency has been characteristic of the work of earlier anthropologists. Anthropology has neither, on the one hand, the epistemological rigor of the "real sciences" nor the epistemological subtlety of literary-philosophical disciplines, on the other. Each unabashedly offers to supply this needed sophistication.

There is more than a little irony in this offer. In place of the "unsophisticated" anthropology of their predecessors—who sought to reconcile the complexities of anthropological research through an epistemology that valued methodological and theoretical diversity—both alternatives advance comparatively narrow—but, we are assured, sophisticated—versions of "good anthropology."

In my view, both alternatives beg the epistemological issues that are raised by squarely facing the question of the scientific status of anthropology. Taking up these issues necessarily means attempting to understand better the nature of science. It has been fairly clear since the 1960s that the positivist model is not an adequate characterization of what it means to be "scientific," either in general (Suppe 1977:619–632) or specifically in relation to anthropology (Rubinstein et al. 1984). Neither the methodolatrist nor postmodernist approaches to the status of anthropological knowledge recognize this. Instead, both positions start by accepting the positivist conception of science as an accurate model of the scientific enterprise against which to evaluate anthropology. One then seeks to legitimate anthropological knowledge by showing that it can conform to this model of science while the other seeks refuge in the claim that anthropology is really a literary-interpretive enterprise (Carrithers 1990).

Neither position does much to advance our understanding of what it means to study human social life, or any other domain, scientifically. During a time when scholars are recognizing the tentative, situational, and reflexive nature of scientific knowledge (Mayr 1982:35, Hufford 1982a:ix–xxiv, Rubinstein et al. 1984:141–159, Bartley 1987:7; Hawking 1988:18), looking with a renewed seriousness at early attempts—like those of Redfield and Tax—to understand anthropology as science will, I believe, help enrich and advance our contemporary epistemological understanding.

Reflection and Reflexivity in Fieldwork Training

In putting forward their "epistemologically sophisticated" accounts, anthropologists following the postmodernist and methodolatrist options have created a partial, and inaccurate, history of their discipline. Both approaches reject as critically naive the work of earlier anthropologists.

Since about 1970, authors often present accounts of their fieldwork, intending to lead us back to examining fundamental aspects of the fieldwork experience and how these affect our knowledge. Such accounts can be very instructive, especially when they direct our attention to epistemological and methodological issues that affect anthropology as a collective enterprise.

Van Maanen (1988) discusses the different self-images projected by authors of "tales of the field." Sometimes, in the course of constructing those self-images, reflections on fieldwork produce idiosyncratic interpretations of a fieldworker's personal experiences, drawing our attention away from the collective enterprise of anthropology even while insisting that their intention is the contrary. Since stories of preparation (or lack of it) for fieldwork are often used as props in the construction of the self-image presented, it is perhaps not surprising that accounts of this preparation are reshaped as they are refracted through the lens of self-presentation.

It is, again, ironic that it is those accounts, offered as explicitly reflexively-reflective about fieldwork, that prove to include the most highly refracted, and historically partial, picture of anthropological training and of the fieldwork process. Paul Rabinow's *Reflections on*

Fieldwork in Morocco, considered by many to be *the* seminal reflexive-reflection on fieldwork, reports fieldwork preparation at the University of Chicago's Department of Anthropology—the same department in which Redfield and Tax worked and taught.

Although it is much lauded by those following the postmodernist approach to anthropology, *Reflections on Fieldwork* is a troubled and troubling work. Like other reflexive essays in the postmodernist genre, it at times seems almost contrived to make a point rather than to accurately reflect the fieldwork experience. Thus, for instance, Rabinow says:

> I went to sleep immediately, but woke up from a fitful night saying to myself that I had probably made a grave professional mistake, because *the informant is always right*. . . .
>
> If the informant was always right, then by implication the anthropologist had to become a sort of non-person, or more accurately a total persona. He had to be willing to enter into any situation as a smiling observer and carefully note down the specifics of the event under consideration. . . . This was the position my professors had advocated: one simply endured whatever inconveniences and annoyances came along. One had to completely subordinate one's own code of ethics and conduct, and world view, to 'suspend disbelief,' as another colleague was proud of putting it, and sympathetically and accurately record events (Rabinow 1977:45–46, emphasis added).

In contrast to this, Redfield's and Tax's letters contain many discussions of how far one ought to go in "subordinating one's code of ethics and conduct" during fieldwork and about how to test the adequacy of informants' reports. For instance, bearing on the proposition that the faculty at Chicago taught that "the informant is always right," Tax, summarizing previous discussions with Redfield, writes on 28 March 1941:

> B. There are informants, yes; but no "informant method."
> C. Ways of stimulating an informant by argument:
> 1. "I don't believe it"
> 2. Citing contrary information by somebody else.
> 3. Pointing out inconsistency of a general statement with a special fact.
> 4. Pointing out a logical inconsistency.

While Rabinow was studying anthropology at Chicago, Tax was a senior member of the department, though Redfield had been dead for some years. Nonetheless, Redfield's field notes and diaries (and

Tax's for that matter) were available in the University Library and students they had trained—using a system of careful supervision described in this *Correspondence*—were teaching on the faculty. It is highly unlikely that the faculty was of one voice in maintaining that "the informant is always right" or, for that matter, in suggesting that researchers abandon their own code of ethics.[8]

Rabinow's is an account written for publication, thus for an audience. Like other postmodernist writing in anthropology, its purpose is personally reflective but not, I think, reflexive in the treatment of fieldwork as a collective undertaking. It is more a personal recounting of, and meditation on, his own reactions to his experiences in Morocco than it is a reflexive contribution to the understanding of the process of doing fieldwork. To be truly reflexive, accounts of fieldwork must apply lessons from the field to the interpretation and gathering of data.

> If discussion of one's relation to the fieldwork process is treated as a task that is independent of interpreting the data, however, the status quo of linguistic and ethnographic reporting will be preserved. The problem of translating awareness of the need for reflexivity into procedures for systematically analyzing the effect of the fieldworker's presence on the data has scarcely been discussed (Briggs 1986:120).

At their best, reflections on fieldwork are also *reflexive* in that they both inform the interpretation of data and offer honest instruction about how particular fieldwork activities affected the kind and quality of the data collected. For instance, by relating the pragmatic arrangements used to overcome difficulties in gathering data about a particular topic to the discovery of theory, Strauss (1987:40–54) is able to explain the crucial role of experience in research.

Briggs (1986:39–60) shows how we might misunderstand data derived from interviews, because the interview is sometimes an artificial communicative event that is subject to differing interpretations by the interviewer and his or her informants. Briggs' analysis rests on the detailed reflective—*and reflexive*—treatment of his own fieldwork in New Mexico. He shows how the interview, as social scientists understand it, can suppress the natural communication norms in a society, replace them with other conflicting norms, and produce data that may mislead the naive researcher.

Separating reflections on fieldwork which contribute to our understanding of fieldwork as a collective enterprise from idiosyncratic recountings of personal experience is not always a straightforward matter. But the following description of how to move back and forth

between the personal and professional sides of field research is instructive:

> . . . resonances between the personal and the professional are the source of both insight and error. You avoid mistakes and distortions not so much by trying to build a wall between the observer and the observed as by observing the observer—observing yourself—as well, and bringing the personal issues to consciousness. . . . You dream, you imagine, you superimpose and compare images, you allow yourself to feel and then try to put what you feel into words. Then you look at the record to understand the way in which observation and interpretation have been affected by personal factors, to know the characteristics of any instrument of observation that make it possible to look through it but also introduce distortion in that looking (Bateson 1984:161).

Such reflections need not be written for publication. Indeed, it may be best if they are recorded without thought of publication. In this latter instance the researchers are freer to record raw, even unappealing, private reactions that they might alter in public presentations.

Reflexivity in Practice: Guatemala 1934–1941

Reflexivity in practice is a feature of good anthropological research. This does not necessarily mean that good anthropological work must be part of an explicitly "reflexive research program." Rather, reflexivity—the active analysis and application of our experience to improve our data collection and interpretation—ought to be an integral part of the everyday practice of professional anthropologists.[9] Such reflexivity is a skill that must be learned. Yet the suggestion in published accounts of anthropological fieldwork is that either such training has not occurred in the past (and it is therefore a recent area of concern), or that we do not have a clear idea of how it develops. This is the problem of anthropological apprenticeship. Accounts of "coming of age" in anthropology too often obscure, rather than clarify, the process of anthropological apprenticeship because they often also have a separate purpose of creating and maintaining a particular professional self-image (Van Maanen 1988).

This unclear picture of the training of professional intuition makes it more difficult to evaluate anthropological research reports. Because the processes through which anthropologists are trained to be reflexive in fieldwork is unclear, instances of the thoughtful application of experience to research problems are not readily apparent.

Instead, accounts of reflexivity in fieldwork become descriptions of moments of insight. Yet such "Eureka!" events are the exception rather than the rule in scientific activity. This problem is especially great when the work considered is relatively old; we are too likely to assume that our predecessors were less sophisticated than we now are. This assumption rests on a faulty understanding of scientific progress as linear and cumulative, which derives from the model of social science as social physics (Glymour 1983). It is not that we do not have improved tools and methods for research and interpretation. Rather, the quality of the use to which any of our methods is put depends critically on the primary instrument of anthropology—ethnographers themselves. We should therefore be more considered than we are in judging earlier work.

Several themes emerge from Redfield's and Tax's letters that are interesting in the context of contemporary discussions of ethnography and ethnology. Throughout the course of their correspondence, Redfield and Tax show concern with: (1) understanding what differences there might be in ethnographic accounts rendered by different ethnographers, especially between "indigenous" ethnographers and non-native ethnographers; (2) understanding methods for establishing the veracity of information collected from informants; (3) the relative merits of focusing on "objective"—material and behavioral—aspects versus ideational aspects of Guatemalan community life; (4) the appropriate unit of analysis and its relation to their theoretical work; and (5) the effect of pragmatic arrangements—like where one lives—on the ethnographic data collected.

The observation that ethnographers often report "different realities" after having studied the same community is now something of a commonplace. When Redfield and Tax went to work in Guatemala this observation was still new. Two other researchers had made explorations of Chichicastenango, Guatemala, before Tax's arrival there. One, Schultze-Jena, was a German geographer; the other, Ruth Bunzel, an American anthropologist. Before arriving in Chichicastenango Tax had read, in German, Schultze-Jena's work about Quiché social life. Bunzel did not publish her results until 1952, well after Tax's and Redfield's Guatemalan fieldwork. Yet anthropology in the United States was a small enough professional community that Tax and Redfield had some idea of what she found there.

Shortly after he began his Guatemalan fieldwork Tax reports to Redfield that he is finding disparities between his data for Chichicastenango and those reported by Schultze-Jena. In discussing these differences Tax and Redfield puzzle about what their causes might be. In a letter to Redfield on 15 December 1934, Tax suggests that in part the differences between their data may result from differences in their use of informants: Schultze-Jena, he discovers, relied too heavily on a single informant.

Their discussions of these issues suggest that Redfield and Tax were aware of and interested in the sources of potential discrepancies in ethnographic understanding that can emerge when two researchers work in the same place. Rather than treat such discrepancies as a competitive opportunity to topple the interpretations of a fellow anthropologist (as seems to be the practice today, see Hawkins 1984 or Freeman 1983 for instance), Redfield and Tax saw this as a problem in anthropological epistemology and fieldwork method. (It is worth noting that this interest predates by several decades the now famous differences between Redfield's original account of Tepotzlan, based on fieldwork he had done in the 1920s, and the conflicting account presented twenty years later in the restudy of Tepotzlan by Oscar Lewis. For an account of this and of other similar disputes, see Heider 1988:73–81.)

The letters trace Redfield's and Tax's concerns with making methods of field research explicit. Beyond method *per se* they took their work in Guatemala as an opportunity to try to establish the ways in which indigenous ethnography might differ from that made by an outside anthropologist. To this end, their letters trace a concern with working with local people as collaborators in their ethnographic research. They did not phrase their interests as we might today—in terms of power and perspective, of self-awareness and authenticity of representation in ethnography, or in terms of the constraints of the historical and cultural contexts within which they worked.

Yet, in a different idiom, these are the concerns addressed by Redfield and Tax as they discuss the differences that might result from research conducted in a single community by a native researcher as opposed to that of a non-native anthropologist. During their last field season they tried to establish a controlled context to assess such a circumstance by having Benjamin Paul and Juan Rosales both work in San Pedro la Laguna (see Redfield to Tax, 8 December 1940). Although for reasons described in the letters their "experiment" was never completed, they engaged questions about the tentativeness of ethnographic representation and authenticity via thoughtful reflexive action.[10]

How does an anthropologist know that the information he or she gets from informants in the field is "good data?" Have anthropologists until recently, as Rabinow (1977:45) suggests, simply thought that the informant is "always right?" Throughout these letters Redfield and Tax have a considerable amount of discussion about informants. Some of this discussion describes the pragmatics of identifying informants and the circumstances in which research interviews were conducted. Some discussion describes the characteristics of good or bad informants—for example, Tax to Redfield, 24 December 1934: "I hired a mozo to round up Indians from different places—one or two at a time—to come and talk to me. . . . It is true that I had some extremely stupid informants. . . . The men were sometimes very shy, and twice they dashed out and away before I could put them at their ease; it probably would have been a bit better had I come to their homes instead of they to ours. . . . " And they consider the importance of using multiple informants in order to guard against taking idiosyncratic reports as the norm—"I am getting another informant-interpreter next week to replace Tomás, since I want to start on another canton and also I want to be sure that I'm not too much influenced by seeing things through the eyes of one interpreter-guide. Before I finish here I intend that I shall have used all of the intelligent young men of the tribe in this capacity. . . ." (Tax to Redfield, 26 February 1935).[11]

In conducting fieldwork, what are the relative merits of focusing on "objective"—material and behavioral—aspects versus ideational aspects of community life? Recently, Redfield and Tax are faulted for being naive realists because their conception of culture "never became exclusively ideational" (Hawkins 1983:300). Moreover, the critique argues that "Tax thus follows Redfield in characterizing the Indians by traits. Neither Redfield nor Tax sought for the meaning of the traits by considering them as a system of relations to each other that cross both community and ethnic boundaries" (Hawkins 1983:306).

This characterization of Redfield and Tax as seeing only well-bounded groups sharing homogeneous sets of traits bears little relation to the picture that emerges from their letters. Rather, their correspondence has many discussions of heterogeneity within and among communities. In these discussions, Redfield and Tax struggle not to force the appearance of homogeneity, but to come to terms with anomalous information within a conceptual framework that treats such diversity as an important source of information rather than as a problem to be "resolved." As much of this discussion focuses on ideational aspects of Guatemalan social life as it does on the observable behavioral and material aspects. It is clear from these letters that these ideational aspects of social life were the major preoccupation of Redfield and Tax. It emerges that *Penny Capitalism*, the only book-length study of Guatemala published by Tax, which describes the material and economic basis of Panajachel society, was undertaken as a peripheral

project.[12] Tax introduced the term "World View" to American anthropology based on this Guatemalan fieldwork (Mendelson 1968:577).

In their letters Redfield and Tax discuss not only the integration of ideational and "objective" evidence in ethnography, but also the need to combine intensive participant observation with extensive surveys. From these discussions we have the opportunity to see the considerations that go into selecting where and how to carry out ethnography. They chose units of analysis as appropriate for the problem at hand and changed them depending upon the question they were exploring. Rather than failing to see that the *municipios* they often used as their unit of analysis were integrally linked to regional and national structures (Early 1983:75), Tax and Redfield went to great lengths to find the proper balance between local-level and more macro-level analyses. And they acknowledged the need to move freely between these levels as the problem they are investigating requires (Tax 1953:ix–x).[13]

This correspondence of Robert Redfield and Sol Tax can profitably be read from many perspectives. An interested reader can explore the epistemological and practical aspects of anthropology around which many of our discipline's most compelling discussions are today taking place. But first, perhaps, we can read these letters for the pleasure of the opportunity to witness the human exchange between Robert Redfield and Sol Tax, and to share their sense of excitement and fascination in doing anthropological fieldwork with the peoples of Guatemala. From whatever perspective and for whatever purpose these letters are read, I hope others will share the enormous enjoyment and satisfaction I've gotten from working with them.

Notes

1. There are several very helpful discussions of anthropology during this period (see Stocking 1976, Kehoe 1985, Frantz 1985, Ebihara 1985, and Kelly 1985) of Robert Redfield's contributions to social science (see Leslie 1968, 1976, Hansen 1976, Murra 1976, Singer 1958), and of Tax's career (Tax 1988, Hinshaw 1979a, 1979b, Rubinstein 1986, in press), which can be consulted for more information about these topics.

2. Reflection describes those instances when we look back on our experiences in order to form an image of our earlier work. It is a process which involves the construction and management of self-images. Reflexivity, in contrast, requires the active analysis and application of our experience to improve our data collection and interpretation. Reflexivity necessarily involves the critical examination and use of earlier experience to influence future action, and is thus an epistemologically revitalizing activity.

3. Methodolatry results from a scientism in which "the social sciences have tended to rely on the development of highly sophisticated 'scientific' methods to escape the existential determinants readily seen as controlling the subjects whom they study" (Hufford 1985:181). For analyses of scientism and its effects, both theoretical and practical, on the social sciences see Hufford (1982a, 1982b, 1983, 1985) and Rubinstein (1984, 1989, Rubinstein et al. 1984).

4. I follow recent discussions in identifying a "postmodernist" trend in anthropology (Sangren 1988, Mascia-Lees 1989, and Roth 1989). The coherence of, and diversity within, this trend are well represented by the contributions to Clifford and Marcus (1986). David Maines (personal communication) points out that calling this body of work postmodernist is misleading; only some of it "presumes the lack of any central unity in a society or culture." The anthropological work which does not share this core assumption might, Maines (1989) suggests, alternatively be identified as belonging to a post-positivist, interpretive frame.

Whether it shares the core concerns of postmodernism or simply extends the interpretive paradigm, much of this literature substitutes the idiosyncratic interpretation of our professional and personal anthropological experiences for the examination of epistemological issues in anthropology from the perspective of our discipline as a collective enterprise. It is to such discussions that my comments apply. I do not mean to include in this group work which correctly contends that meaning is enacted and communicated, or that anthropological writing is always in part rhetorical.

5. A similar story is told of Kroeber, who is said to have responded to a request for advice about fieldwork by saying, "I suggest you buy a notebook and a pencil." Agar (1980:2) changes this "Berkeley folklore" into "historical fact," and then cites it as evidence of the lack of rigor in methodological training in anthropology. Similarly, Ward and Werner (1984:107) relate Carl Vogelin's report that his total field training involved Kroeber telling him to "get a Model T and a cast iron frying pan." They then use this report to make the point that "Field methods as an important intellectual problem were neglected."

In their descriptions of the historically poor state of methodological training in anthropology, contemporary methodologists willingly accept the veracity of oral reports to the exclusion of observational data. Since anthropologists are keenly aware that what people say they do often varies from what they really do, this selective methodological preference is interesting. Following Latour (1987:25), it is important to explore how it is that statements like "Methodolodical training in anthropology was neglected" have come to be considered "closed, obvious, firm and packaged premise[s] leading to some other less closed, less obvious, less firm and less united consequence." In looking at the issue from the perspective of how the status of such statements changes from problematic to established, we see "why it is solid or weak instead of using it to to render some other consequences more necessary" (Latour 1987:23).

6. This tendency to put complex questions in terms of simple, clear-cut statements reflects a general trend in contemporary social science which is intolerant of ambiguity and treats recognition of it as problematic (Levine 1985).

7. For evidence that Tax shared this commitment to pluralism in theory and method see Tax (1988) and Rubinstein (1986:274).

8. There are other points in the book that seem disingenuous as well. Among them are Rabinow's portrait of himself as a lone ethnographer making his way without guidance through fieldwork (when in the introduction he tells us he was a member of a five-person research team, which included his graduate advisor). Also troubling is his insistence that meaning emerges dialectically in fieldwork while employing this lesson only as it corresponds with the image of himself which he seeks to construct in the book. Thus, he "discovers" that he had been "mistakenly" instructed at Chicago about how informants are always right and what to expect of "social facts" (Rabinow 1977:121). He also discovers how easy it is for an anthropologist to misconstrue meaning during cross-cultural communication. At one point he describes the essential dialectic process involved in finding that his refusal to pay for his key informant's expenses during a trip to Marrakech was culturally appropriate, even wise, revealing that after more than a month of close interaction his "typification" of their relationship had been mistaken (Rabinow 1977:28–29).

Yet, later, the construction of meaning via the fieldwork dialectic is apparently unnecessary in order to understand the cultural context of his informant's sensual activities and his own sexual relations with a local woman, because after a brief meeting clearly everyone "knew the score" (Rabinow 1977:64–69).

9. Reflexivity in this sense is a hallmark of competence in all professions (Schön 1983). Moreover, such learning from experience is key to improving research practice and developing new, more adequate frames of reference (Argyris 1980, Argyris, Putnam and Smith 1985).

10. The construction of a controlled comparison of native and non-native ethnographic reporting is a more usefully reflexive response to the issues of authority and representation in ethnography than many of the "sophisticated" postmodernist discussions of ethnography as literature, which sometimes, as Stanley Tambiah says, seem to be "navel watching" (see Wilford 1990:16). Redfield's and Tax's experiment also anticipates Campbell's (1970:71) call for just such a validation of ethnographic knowledge.

11. This apparently simple procedure anticipates the development by those formally interested in methodological issues of the "method of convergent validation" which, in part, requires the use of reports from multiple observers in order to filter out systematic distortions from idiosyncratic reports (see, for instance, Campbell 1970:70).

12. My own reading of Redfield's and Tax's published accounts of their Mesoamerican fieldwork suggests that those who argue that Redfield and Tax were overly concerned with objects and social facts, with culture traits and material culture, and that they paid too little attention to the symbolic and ideational aspects of society (e.g., Tedlock 1983:238, Hawkins 1983:302,306, 1984) are mistaken. For example in the Preface to *Penny Capitalism*, Tax

(1953:ix–x) emphasizes the importance of cross-community interaction and of the need to describe the ideational aspects of Panajachel, the community about which he writes. Tax outlines his intentions to pursue just such a reporting *as an integrated* text with *Penny Capitalism*. Further, Tax invites interested readers to review his field materials about the ideational aspects of Panajachel which then were indexed and available in many libraries on microfilm. It also seems somewhat disingenuous for Hawkins to fault Redfield and Tax merely because they sought *a unit* of analysis with which to deal. It is impossible—even for Hawkins—to make an analysis without constructing *some unit* as appropriate for the problem at hand (Schön 1983).

13. In the introduction to *Penny Capitalism* Tax notes that he has in mind two other books focusing on the non-material aspects of Panajachel society. These were never published, but his *Practical Animism: The World of Panajachel* was in press when he withdrew it to await the results of a restudy. It is available in the University of Chicago Microfilm Collection of Manuscripts on Middle American Cultural Anthropology.

References Cited

Agar, Michael. 1980. *The Professional Stranger: An Informal Introduction to Ethnography*. New York: Academic Press.

Almeder, Robert. 1973. Science and Idealism. *Philosophy of Science* 40(2):242–54.

———. 1975. Fallibilism and the Ultimate Irreversible Opinion. *American Philosophical Quarterly, Monograph Series: Studies in Epistemology* 9:33–54.

Appell, George N. 1989. British- vs American-Trained Ethnographers. *Anthropology Newsletter* 30(6):48, 39.

Argyris, Chris. 1980. *The Inner Contradictions of Rigorous Research*. New York: Academic Press.

Argyris, Chris, Robert Putnam, and Diana McLain Smith. 1985. *Action Science: Concepts, Methods, and Skills for Research and Intervention*. San Francisco: Jossey-Bass.

Aronson, Jerrold L. 1984. *A Realist Philosophy of Science*. New York: Saint Martin's Press.

Bartley, W.W. 1987. Philosophy of Biology versus Philosophy of Physics. In *Evolutionary Epistemology, Theory of Rationality, and the Sociology of Knowledge*, Gerard Radnitzky and W.W. Bartley III, editors. La Salle, IL: Open Court, Pp. 7–45.

Bateson, Gregory. 1936. *Naven: A Survey of the Problems Suggested by a Composite Picture of the Culture of a New Guinea Tribe Drawn from Three Points of View*. Cambridge: Cambridge University Press.

———. 1972. *Steps to an Ecology of Mind*. San Francisco: Chandler.

Bateson, M. Catherine. 1984. *With a Daughter's Eye*. New York: Morrow.

Bellah, Robert N. 1977. Foreword. In *Reflections on Fieldwork in Morocco*, Paul Rabinow. Berkeley: University of California Press, Pp. ix–xii.

Briggs, Charles L. 1986. *Learning How to Ask: A Sociolinguistic Appraisal of the Role of the Interview in Social Research*. Cambridge: Cambridge University Press.

Campbell, Donald. 1970. Natural Selection as an Epistemological Model. In *A Handbook of Method in Cultural Anthropology*, Raoul Naroll and Ronald Cohen, editors. New York: Natural History Press, Pp. 51–85.

Carmack, Robert M., editor. 1988. *Harvest of Violence: The Maya Indians and the Guatemalan Crisis*. Norman: University of Oklahoma Press.

Carrithers, Michael. 1990. Is Anthropology Art or Science? *Current Anthropology* 31(3):263–282.

Chagnon, Napoleon. 1974. *Studying the Yanomamo*. New York: Holt, Rinehart and Winston.

———. 1977. *Yanomamo: The Fierce People, 2nd Edition*. New York: Holt, Rinehart and Winston.

Clifford, James and George E. Marcus, editors. 1986. *Writing Culture: The Poetics and Politics of Ethnography*. Berkeley: University of California Press.

Count, Earl W. 1948a. The Twilight of Science: Age of Dinosaurs? *Educational Forum* 12:199–207.

———. 1948b. The Twilight of Science: But Science is a Humanity. *Educational Forum* 12: 299–312.

Early, John D. 1983. A Demographic Survey of Contemporary Guatemalan Maya: Some Methodological Implications for Anthropological Research. In *Heritage of Conquest Thirty Years Later*, Carl Kendall, John Hawkins, and Laurel Bossen, editors. Albuquerque: University of New Mexico Press, Pp. 73–91.

Ebihara, May. 1985. American Ethnology in the 1930s: Contexts and Currents. In *Social Contexts of American Ethnology, 1840–1984*, June Helm, editor. Washington, D.C.: American Ethnological Society, Pp. 101–121.

Eggan, Fred, editor. 1937. *Social Anthropology of North American Tribes*. Chicago: University of Chicago Press.

———. 1968. One Hundred Years of Ethnology and Social Anthropology. In *One Hundred Years of Anthropology*, J.O. Brew, editor. Cambridge: Harvard University Press, Pp. 119–149.

Fiske, Donald and Richard Shweder, editors. 1986. *Metatheory in Social Science: Pluralisms and Subjectivities*. Chicago: University of Chicago Press.

Frantz, Charles. 1985. Relevance: American Ethnology and Wider Society, 1900–1940. In *Social Contexts of American Ethnology, 1840–1984*, June Helm, editor. Washington, D.C.: American Ethnological Society, Pp. 83–100.

Freeman, Derek. 1983. *Coming of Age in Samoa: The Making and Unmaking of an Anthropological Myth*. Cambridge, MA: Harvard University Press.

Freilich, Morris, editor. 1970. *Marginal Natives: Anthropologists at Work*. New York: Harper and Row.

Fritz, John and Fred Plog. 1970. The Nature of Archaeological Explanation. *American Antiquity* 35:405–412.

Geertz, Clifford. 1988. *Works and Lives: The Anthropologist as Author*. Stanford: Stanford University Press.

Glymour, Clark. 1983. Social Science and Social Physics. *Behavioral Science* 28:126–134.

Hansen, Asael. 1976. Robert Redfield, The Yucatan Project, and I. In *American Anthropology: The Early Years*, John V. Murra, editor. St. Paul: West, Pp. 167–186.

Hawkings, Stephen. 1988. *A Brief History of Time: From the Big Bang to Black Holes*. London: Bantam Books.

Hawkins, John. 1983. Robert Redfield's Culture Concept and Mesoamerican Anthropology. In *Heritage of Conquest Thirty Years Later*, Carl Kendall, John Hawkins, and Laurel Bossen, editors. Albuquerque: University of New Mexico Press, Pp. 299–336.

———. 1984. *Inverse Images: The Meaning of Culture, Ethnicity, and Family in Postcolonial Guatemala*. Albuquerque: University of New Mexico Press.

Heider, Karl. 1988. The Rashomon Effect: When Ethnographers Disagree. *American Anthropologist* 90(1):73–81.

Hinshaw, Robert. 1979a. Tax, Sol. In *International Encyclopedia of the Social Sciences, Biographical Supplement*, David Sills, editor. New York: The Free Press, Pp. 760–763.

———, editor. 1979b. *Currents in Anthropology: Essays in Honor of Sol Tax*. The Hague: Mouton.

Hufford, David J. 1982a. *The Terror That Comes in the Night: An Experience-Centered Study of Supernatural Assault Traditions*. Philadelphia: University of Pennsylvania Press.

———. 1982b. Traditions of Disbelief. *The New York Folklore Quarterly* 8(3/4):47–55.

———. 1983. The Supernatural and the Sociology of Knowledge: Explaining Academic Belief. *The New York Folklore Quarterly* 9:21–29.

———. 1985. Reason, Rhetoric, and Religion: Academic Ideology Versus Folk Belief. *New York Folklore* 11:177–194.

Hutchins, Robert Maynard. 1958. Remarks by Robert Maynard Hutchins. In *Robert Redfield*. Chicago: Privately Published.

Jarvie, I.C. 1967. *The Revolution in Anthropology*. London: Routledge and Kegan Paul.

Kehoe, Alice B. 1985. The Ideological Paradigm in Traditional American Ethnology. In *Social Contexts of American Ethnology, 1840–1984,* June Helm, editor. Washington, D.C.: American Ethnological Society, Pp. 41–49.

Kemper, Robert Van. 1985. From Nationalism to Internationalism: The Development of Mexican Anthropology, 1934–1946. In *Social Contexts of American Ethnology, 1840–1984,* June Helm, editor. Washington, D.C.: American Ethnological Society, Pp. 139–156.

Kendall, Carl, John Hawkins, and Laurel Bossen, editors. 1983. *Heritage of Conquest: Thirty Years Later*. Albuquerque: University of New Mexico Press.

Latour, Bruno. 1987. *Science in Action*. Cambridge, MA: Harvard University Press.

Laughlin, Charles D., John McManus, Robert A. Rubinstein, and John Shearer. 1986. The Ritual Transformation of Experience. *Studies in Symbolic Interaction* 7(A):107–136.

Leslie, Charles. 1968. Robert Redfield. In *International Encyclopedia of the Social Sciences*, E. Shils, editor. New York: Macmillan and Free Press, Pp. 350–353.

———. 1976. The Hedgehog and the Fox in Robert Redfield's Work and Career. In *American Anthropology: The Early Years*, John V. Murra, editor. Saint Paul: West, Pp. 146–166.

Levine, Donald. 1985. *The Flight from Ambiguity: Essays in Social and Cultural Theory*. Berkeley: University of California Press.

Lewis, Oscar. 1951. *Life in a Mexican Village: Tepoztlan Restudied*. Urbana: University of Illinois Press.

Maines, David R. 1989. On the Liminality of Post-Positivism and the Conceptualization of Culture: An Editorial Introduction. *Cultural Dynamics* 2(1):1–8.

Malinowski, Bronislaw. 1922. *Argonauts of the Western Pacific*. New York: Dutton Reprint.

Marcus, George E. and Dick Cushman. 1982. Ethnographies as Texts. *Annual Review of Anthropology* 11:25–69.

Mascia-Lees, Frances E., Patricia Sharpe, and Colleen Ballerino Cohen. 1989. The Postmodernist Turn in Anthropology: Cautions from a Feminist Perspective. *Signs: Journal of Women in Culture and Society* 15(11):7–33.

Mayr, Ernst. 1982. *The Growth of Biological Thought*. Cambridge: Harvard University Press.

Mead, Margaret. 1928. *Coming of Age in Samoa*. New York: Morrow.

———. 1930. *Social Organization of Manu'a*. Honolulu: Bishop Museum.

Mendelson, E.M. 1968. World View. In *International Encyclopedia of the Social Sciences*, E. Shils, editor. New York: Macmillan and Free Press, Pp. 576–579.

Michlovic, Michael G. 1978. On Social Science and Science. *Current Anthropology* 19(2):379–380.

Ming, William. 1958. Remarks by William R. Ming, Jr. In *Robert Redfield*. Chicago: Privately Published.

Naroll, Raoul and Ronald Cohen, editors. 1970. *A Handbook of Method in Cultural Anthropology*. New York: The Natural History Press.

Nash, Manning. 1984. Foreword. In *Inverse Images: The Meaning of Culture, Ethnicity, and Family in Postcolonial Guatemala*, by John Hawkins. Albuquerque: University of New Mexico Press, Pp. xii–xv.

Pelto, Pertti J. 1970. *Anthropological Research: The Structure of Inquiry*. New York: Harper and Row.

———, and Gretel H. Pelto. 1978. *Anthropological Research: The Structure of Inquiry, Second Edition*. Cambridge: Cambridge University Press.

Pinxten, Rik. 1981. Observation in Anthropology: Positivism and Subjectivism Combined. *Communication and Cognition* 14:57–83.

Rabinow, Paul. 1977. *Reflections on Fieldwork in Morocco*. Berkeley: University of California Press.

Redfield, Robert. 1926. Anthropology, a Natural Science? *Social Forces* 4(4):715–721.

———. 1930. *Tepoztlan: A Mexican Village, A Study of Folk Life*. Chicago: University of Chicago Press.
———. 1941. *The Folk Culture of Yucatan*. Chicago: University of Chicago Press.
———. 1955. *The Little Community: Viewpoints for the Study of a Human Whole*. Chicago: University of Chicago Press.
——— and Alfonso Villa Rojas. 1934. *Chan Kom: A Maya Village*. Chicago: University of Chicago Press.
Rescher, N. 1978. *Peirce's Philosophy of Science*. Notre Dame, IN: University of Notre Dame Press.
Roth, Paul A. 1987. *Meaning and Method in the Social Sciences: A Case for Methodological Pluralism*. Ithaca, NY: Cornell University Press.
———. 1989. Ethnography without Tears. *Current Anthropology* 30:555–561.
Rubinstein, Robert A. 1984. Epidemiology and Anthropology: Notes on Science and Scientism. *Communication and Cognition* 17:163–185.
———. 1986. Reflections on Action Anthropology: Some Developmental Dynamics of an Anthropological Tradition. *Human Organization* 45:270–279.
———. 1989. Culture, International Affairs, and Multilateral Peacekeeping: Confusing Process and Pattern. *Cultural Dynamics* 2:21–41.
———. in press. Sol Tax. In *International Dictionary of Anthropologists*, C. Winters, editor. Chicago: Library Anthropology Resource Group.
———, Charles D. Laughlin, and John McManus. 1984. *Science as Cognitive Process: Toward an Empirical Philosophy of Science*. Philadelphia: University of Pennsylvania Press.
Sangren, P. Steven. 1988. Rhetoric and the Authority of Ethnography: "Post-modernism" and the Social Reproduction of Texts. *Current Anthropology* 29:405–424.
Schneider, Harold. 1974. *Economic Man*. New York: The Free Press.
Schön, Donald. 1983. *The Reflective Practitioner*. New York: Basic Books.
Stocking, George. 1968. *Race, Culture, and Evolution: Essays in the History of Anthropology*. New York: The Free Press.
———. 1976. Ideas and Institutions in American Anthropology: Thoughts Toward a History of the Interwar Years. In *Selected Papers from the American Anthropologist, 1921–1945*, G. Stocking, editor. Washington, D.C.: American Anthropological Association, Pp.1–53.
Strauss, Anselm L. 1987. *Qualitative Analysis for Social Scientists*. Cambridge: Cambridge University Press.
Suppe, Frederick, editor. 1977. *The Structure of Scientific Theories, Second Edition*. Urbana: University of Illinois Press.
Tax, Sol. 1937. The Municipios of the Midwestern Highlands of Guatemala. *American Anthropologist* 39:423–444.
———. 1941. World View and Social Relations in Guatemala. *American Anthropologist*. 43:27–42.
———. 1953. *Penny Capitalism: A Guatemalan Indian Economy*. Washington, D.C.: Smithsonian Institution.
———. 1988. Pride and Puzzlement: A Retro-introspective Record of 50 Years of Anthropology. *Annual Review of Anthropology* 17:1–21.

———, et al., editors. 1953. *An Appraisal of Anthropology Today*. Chicago: University of Chicago Press.
Tax, Sol and Robert A. Rubinstein. 1986. *Archival History Program Videotaped Interview of Sol Tax*. New York: Wenner-Gren Foundation for Anthropological Research.
Tedlock, Barbara. 1983. A Phenomenological Approach to Religious Change in Highland Guatemala. In *Heritage of Conquest: Thirty Years Later*, Carl Kendall, John Hawkins, and Laurel Bossen, editors. Albuquerque: University of New Mexico Press, Pp. 235–246.
Van Maanen, John. 1988. *Tales of the Field: On Writing Ethnography*. Chicago: University of Chicago Press.
Villa Rojas, Alfonso. 1945. *The Maya of East Central Quintana Roo*. Washington, D.C.: Carnegie Institution of Washington.
Ward, Jeffrey J. and Oswald Werner. 1984. Difference and Dissonance in Ethnographic Data. In *Epistemology and Process: Anthropological Views*, Robert A. Rubinstein and Rik Pinxten, editors. Ghent, Belgium: Communication and Cognition Books, Pp. 91–115.
Wax, Rosalie. 1971. *Doing Fieldwork*. Chicago: University of Chicago Press.
Werner, Oswald. 1984. Minimum Standard for Ethnoscience Ethnography. Paper presented at the annual meeting of the American Anthropological Association, 15–18 November, Denver, Colorado.
Werner, Oswald and G.M. Schoepfle (with associates). 1987. *Systematic Fieldwork, 2 Volume*. Beverly Hills: Sage Publications.
White, Leslie. 1938. Science Is *Sciencing*. *Philosophy of Science* 5:369–389.
Wilford, John Noble. 1990. Of Legends: Whose Culture is it Anyway? *International Herald Tribune* Wednesday, 21 February, No. 33,277:16