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EPIDEMIOLOGY AND ANTHROPOLOGY : NOTES ON SCIENCE AND SCIENTISM

Robert A. Rubinstein
Northwestern University and
School of Public Health, Health Sciences Center
University of Illinois at Chicago

> Mere knowledge, though it be systematized, may be a dead memory; while by science we all habitually mean a living and growing body of truth. We might even say that knowledge is not necessary to science. The astronomical researches of Ptolemy, though they are in great measure false, must be acknowledged by every modern mathematician who reads them to be truly and genuinely scientific. That which constitutes science, then, is not so much correct conclusions, as it is a correct method. But the method of science is itself a scientific result. It did not spring out of the brain of a beginner : it was a historic attainment and a scientific achievement. So that not even method ought to be regarded as essential to the beginnings of science. That which is essential, however, is the scientific spirit, which is determined not to rest satisfied with existing opinions, but to press on to the real truth of nature. To science once enthroned in this sense, among any people, science in every other sense is heir apparent.
> Charles S. Peirce, *Science and Immortality*

Developments during the last fifteen years have led American anthropologists to begin rethinking many fundamental assumptions about their discipline (1) One catalyst for this rethinking comes from two developments in part external to the discipline itself. First, access to small, well-bounded groups of people who had been the traditional focus of anthropological research became difficult; such groups became fewer in number, and researchers were sometimes also blocked for political reasons from studying some of those that remain. As a result anthropologists seeking research sites increasingly turn their attention to more "modern," "complex" settings in which to pursue their research interests. And, a number of so-called topical specialties that had previously been only casually developed rapidly became areas for systematic exploitation. Included among such areas are urban anthropology, medical anthropology, the anthropology of complex organizations, and the anthropology of educational institutions.

Second, at the same time that new kinds of research sites became increasingly common, the sources of research funding and employment usually open to anthropologists in the United States began to close. Academic job opportunities started to become increasingly scarce, and research support for basic work also became more difficult to find. In response to these kinds of changes in the reward structure for anthropological work, anthropologists have sought to define satisfying nontraditional work roles for themselves.

In the course of this quest, anthropologists have turned back to the topical areas that they developed in response to shifting research opportunities. Thus, ever more anthropologists have turned toward emergent topical specializations as a way of defining their professional selves, and as a way of defining what they have to offer to areas of applied research. Not surprisingly, as anthropologists have tried to move into these alternate areas of work, they found that many were already occupied by other social and behavioral scientists with whom they would have to compete and cooperate.

The outcome of the interaction between anthropologists and other professionals working in applied research areas has not always been satisfactory. My purpose in writing this essay is to examine one area into which anthropologists are expanding their inquiry – the study of social and psychological factors in health and illness and the epidemiology of mental disorders (a field called psychosocial epidemiology) – and to show that the unsatisfactory nature of this interaction stems from the fact that each holds positions about science which are largely inconsistent one with the other.

Briefly, I suggest that deep discontinuities exist between the foundational assumptions taken by anthropology and by psychosocial epidemiology. Although difficulties have often been ascribed merely to differences in idiom and manner of presentation, my thesis is that the anthropological understanding of the nature of the scientific study of human behavior is inconsistent with many non-anthropological views, and that the latter have so thoroughly influenced discussions of the nature, causes, and distributions of mental disorders that a clash between the policy recommendations made by anthropologists and by others working in research on mental disorders is inevitable.

Differing Views of Science : A Basis for Conflict

To address this problem it is necessary to understand the different assumptions that psychosocial epidemiologists and anthropologists make about what constitutes the proper study of human behavior. In characterizing these approaches I take a rather broad view. Certainly the ideas I ascribe to each approach are not universally accepted by everyone who calls themselves an anthropologist or a psychosocial epidemiologist. Clearly, there may be some anthropologists who would be more comfortable working within the parameters set out by the views I characterize as belonging to psychosocial epidemiology. Likewise, there are some psychosocial epidemiologists who would be more comfortable working within the confines of the assumptions I ascribe to anthropology. Thus, for example, psychosocial epidemiologists who work from a symbolic interactionist perspective (e.g., Totman 1979) ought properly be placed in the group that shares the approach I describe as anthropological. And, some anthropologists, particularly those working in materialist traditions (e.g., Harris 1974), might properly be placed in the group I describe as taking the assumptions of psychosocial epidemiology. Since it is not my purpose here to distinguish among coherent groups within the research community, but rather to highlight important epistemological differences between two broadly conceptualized styles of research, this difficulty need not be seen as problematic.

Psychosocial Epidemiology

The problems and assumptions about properly conducted research that form the core of traditional psychosocial epidemiology derive from those of infections disease epidemiology. That field seeks to describe the relationships among and between a population, its environment, and some disease agent, such as influenza virus. Often successful work in infectious disease epidemiology requires the combination of techniques from the physical and biological sciences as well as from the social and behavioral sciences. The goal of this work is to make controlled comparisons of groups on particular characteristics (Mausner and Bahn 1974, Lilienfeld and Lilienfeld 1980). This basic goal is carried over into psychosocial epidemiology, with the difference being that psychosocial epidemiology extends epidemiological work to examine aspects of individual and social life as well as biological factors in the causal chain.

In principle the way psychosocial epidemiological work has been conceptualized has been to see it as the application of "the scientific method" to the study of human health and illness. In practice this has meant the acceptance by epidemiologists of a positivist view of science (see, Nagel 1961).(2) When thinking about how to carry out "properly scientific" studies with human groups, epidemiologists have developed their approach by taking the physical sciences as the measure against which they ought to judge their work. Taking such an approach has several consequences, three of which are particularly important to this discussion, and which can be highlighted here.

One consequence is that the subjects of study are thought of as objects existing independently of the researcher. Thus, the scientist's work is conceived of as a process which involves taking observations and measurements on a system that is stable and unconnected to the scientist.

The second consequence is the taking of a particular view of what constitutes scientific explanation. From that perspective, research accounts are considered to be adequate explanations only if they conform to criteria of adequacy which, among other things, require that an explanation "if taken account of in time, would have served as the basis for predicting the event in question" (Hempel 1965 : 249). In order to meet that requirement Hempel (1965) and others (see, e.g., Nagel 1961) have suggested that explanations must take a form such that the event to be explained is presented as the conclusion of an argument which essentially contains in it premises that specify some relevant initial conditions and some statistical generalizations or universal laws. In essense, this is a requirement that scientific explanation give "knowledge that" the phenomena of interest (would) occur (Jeffery 1969, Salmon 1971).

A third consequence of this view has been that researchers studying social life who adhere to it have come to emphasize the importance of techniques and forms of presentation which allow them to meet the formal criteria of the positivist view of good explanation and of observation. Adherence to those methods, not the processes of inquiry or the conceptual significance of a project, has come to be taken as the hallmark of "good science."

One of the ways this tendency to reify method is manifest in the psychosocial epidemiologic literature is in an almost dogmatic insistence that to be taken as "scientific" research must be done using particular data gathering methods (for instance a structured diagnostic interview schedule that is designed to provide results in terms of specific categories of a particular nosological system; see, e.g., Dohrenwend and Dohrenwend 1974), or that it be based upon data collected from a "representative sample."

Some brief citations from the recent literature will serve here to illustrate how method has come to be reified and treated as *the* crucial attribute of the scientific study of mental health and illness. Three quotes from a highly respected recent survey of the field of psychosocial epidemiology will give the flavor of this over-emphasis on method. First, a comment on the importance of work done by Hollingshead and Redlich in the late 1950's illustrates the reliance on sample characteristics :

> The data were obtained from identified patients and, thus, it was hazardous to generalize the findings to the population at large. Earlier studies had already demonstrated that significant numbers of the mentally ill never received treatment and were not included as cases. Therefore, Hollingshead and Redlich *could not draw any scientific conclusions* about the influence of social forces on the the production of mental disorders (Schwab and Schwab 1978 : 164–165, emphasis mine).

The following is suggestive of the importance ascribed to using particular types of measurement techniques :

> Another factor complicating our understanding of stress in psychiatry [sic] is that often stressors are *not scientifically measurable* processes, such as heat or trauma, but, instead can ... (Schwab and Schwab 1978 : 250, emphasis mine).

And, finally, this summary comment about an interactive model that seeks to identify why the rate of schizophrenia varies among different strata of society :

> This is an appealing model. *But stress cannot yet be evaluated scientifically*. Hinkle and his colleagues revealed the complexity of the research problem ... (Schwab and Schwab 1978 : 259, emphasis mine).

I am not suggesting here that these sorts of concerns are always misguided; merely that it is inappropriate always to take them as the criteria against which to judge the scientific status of research. Later I suggest that by focusing attention on the products of research *per se* this reification of method in psychosocial epidemiology masks the fact that science is preeminently a process, and that access to scientific knowledge in all cases, but especially when studying humans and other sentient animals, depends upon the sensitive, selective use of different ways of knowing which are appropriate for the questions being asked and not upon the use of a normatively priveleged set of methods (Rubinstein and Laughlin 1977, Rubinstein, Laughlin, and McManus 1984, Wilber 1982). After briefly characterizing the assumptions I ascribe to the anthropological approach, I will give an example of how the two approaches yield different knowledge in response to the same applied research question, and then comment on the importance of that difference.

Anthropology

I turn now briefly to characterizing the assumptions that underlie anthropological work on human social life in general, and on the study of mentally disordered persons in particular. The anthropological approach may properly be seen as allied with a particular analysis of the nature of scientific knowledge in the same way that epidemiologic research derives from a commitment to the positivist view of science. The view from which American anthropology seems to me to follow is the pragmatist analysis of science and of knowledge (e.g., Tax 1960, Almeder 1980, Rescher 1978). The pragmatist view leads to different conclusions about what kinds of research are "scientific" than does the positivist view.

Perhaps the most striking difference in the approaches of anthropology and epidemiology to the study of psychosocial phenomena is that where epidemiology focuses on the products of social life as revealed by particular techniques and methods anthropology seeks to reveal the processes in social life from which those products result. This difference in focus results from fundamental differences between anthropological and epidemiological assumptions about the nature of observation and explanation. While epidemiologic research conforms to the positivist tradition of treating the subjects of research as independent from the researcher, and thus knowable via various standard indices and measures, anthropological research is based on the view that observation is an interactional process. Briefly put, when working with people the researcher,

> can never be an objective outsider not can he be a subjective insider, since (to different degrees) he will always be in a double bias situation : he is biased by his own cultural outlook and he is accepted in a certain role through the bias of the cultural group he is visiting (Pinxten 1981: 59).

As the next section of this paper illustrates, anthropologists addressing questions about "mentally ill populations" respond by examining the processes that those groups use to adapt to their social and physical environments. This involves the explication of both the structure and functioning of a group's conceptual and social systems, as well as an awareness of the demographic patterns that these processes produce (e.g., Spradley 1971).

Thus, although anthropologists have developed and borrowed many data collecting techniques, there is no single normatively mandated set of techniques that must be used to conduct anthropological research. Much of any anthropological research report may be given over to explaining why the use of certain investigatory techniques was deemed useful and appropriate (see, Pelto and Pelto 1978). Further, there is a generally accepted assumption that different types of research settings and problems call for the use, separately or in combination, of a variety of methods.

For purposes of this essay, the major differences between the pragmatist and positivist views of knowledge and of scientific explanation can be stated briefly. Seen from the pragmatist perspective all knowledge is contingent and fallible. Knowledge may be fallible because of incomplete information content or because the structural organization of that information is different (either more simple or more complex) from the phenomenon being explored. It follows from this that we can never know that our models and accounts of the physical or social worlds are actually accurate (Almeder 1973). This is the case no matter how consistently good the predictions from these models might be. Hence, the task of a scientific explanation from this perspective is the development of a mechanism for gaining a better understanding of the phenomenon being explored. That is, the kind of information that should result from scientific explanations is "knowledge of" that which is being studied. This view that explanation ought to reorganize (and, hopefully, increase) our knowledge of that which we are studying does not lead to the criterion of adequacy that explanation be potential prediction (Salmon 1971). Scientific explanation, then, is a process that helps us to increase our knowledge of the world by allowing us to sort the phenomena we study into increasingly homogeneous subclasses.

These different views of science lead to different kinds of information being collected in response to questions about mental illness and the adequacy of services. The next section illustrates these differences for the case of the study of the rehospitalization of chronically mentally ill people.

The Recidivism of Mental Patients

In the 1960's and 1970's the professions involved in the care and treatment of the mentally ill in the United States experienced a shift in perspective that radically changed the face of their patterns of service delivery. Advocates of the new perspective – called deinstitutionalization – argued that the "warehousing" of psychiatric patients in large, long-term care facilities is inhumane, makes poor fiscal sense, and is inconsistent with good clinical principles. They urged that services for the chronically mentally ill should be provided by community-based treatment facilities in place of long-term care facilities. They reasoned that treatment delivered through community-based mental health centers would be more cost effective (in terms of tangible and intangible benefits and costs) then the traditional long-term facilities, and that such centers also would be consistent with the goals of "modern" approaches to treatment for the chronically mentally ill. These latter goals were described by the Joint Commission on Mental Illness and Health (1961) :

> The objective of modern treatment of persons with major mental illness is to enable the patient to maintain himself in the community in a normal manner. To do so, it is necessary (1) to save the patient from the debilitating effects of institutionalization as much as possible, (2) if the patient requires hospitalization, to return him to home and community life as soon as possible, and (3) thereafter to maintain him in the community as long as possible.

The reforms sought by supporters of deinstitutionalization eventually were written into various national and local legislative programs. To a greater or lesser degree, deinstitutionalization had been implemented throughout the United States by the late 1960's and early 1970's. Naturally, both critics and supporters of deinstitutionalization became interested in assessing the relative success of this policy and of the programs that resulted from it. Both groups reasoned that if treatment in mental hospitals and aftercare facilities was effective in helping patients live productively in their home communities, the incidence of rehospitalization should decrease. Yet, it soon became apparent to service providers and to those responsible for mental health policy that large numbers of the chronically mentally ill returned frequently to mental hospitals. For example, in 1971 the National Institute of Mental Health Biometry Branch reported that some 57 % of all patients admitted to local and state hospitals for psychiatric reasons had previous experiences of mental hospitalization. Such patients became known as recidivists.

Because it is taken as indicating that the programs created in response to deinstitutionalization policy are not working, recidivism is considered a problem. Much research directed at accounting for recidivism has been, and continues to be, carried out. Here I want to highlight the different approaches that psychosocial epidemiologists and anthropologists have taken in their attempts to elucidate this problem.

It is not unfair to characterize psychosocial epidemiologic investigations of mental hospital recidivism as efforts to identify those demographic characteristics of the recidivist population which "predict" rehospitalization. This problem is often interpreted as an exercise in measurement (see, e.g., Rosenblatt and Mayer 1974 and Byers, Cohen and Harshbarger 1977). Whatever might be the processes that give rise to recidivism, the psychosocial epidemiologic approach generally has been to seek generalizable, "scientific" measures of a set of behaviors that has been conceptualized as having independent, constant meaning. Thus, simple rehospitalization figures have been taken as indexing an ontologically real object not because of compelling conceptual reasons, but rather because of the methodological convenience that such an assumption provides :

> It is our impression, however, that readmission statistics are more widely used and find greater acceptance than any other indicator – not because they are necessarily a more revealing measure of hospital effectiveness but because of their methodological characteristics (Rosenblatt and Mayer 1974 : 698).

While the intention of epidemiologic research on recidivism is to uncover its causes, the general strategy is to seek to identify the conditions that "predict" rehospitalization. Thus, research has sought to find relationships between readmission statistics and a variety of "objective" measures that characterize the people who return to mental hospitals. These measures include : prior admission, diagnosis, sex, religion, length of previous hospitalization, frequency of aftercare, and so on (cf., Byers, Cohen, and Harshbarger 1977, Schwab and Schwab 1978). Very little discussion is devoted to considering that these sorts of measures might not be as epistemologically neutral as is ordinarilly assumed in the epidemiologic literature. "Frequency of aftercare," for instance, tells the researcher incredibly little about variations in aftercare services, let alone about how different clients may experience differently (even the same) services and facilities.

Yet, once researchers start to treat recidivism as a homogeneous object rather than as an artifact of a set of processes, they may easily forget that at best the demographic information they identify as predictors of recidivism provides only "knowledge that" some phenomenon takes place. Any local variation in the treatment programs of mental hospitals and aftercare facilities, and the social processes that accompany the various physical and social circumstances of people who are chronically mentally ill and which may in significant ways be different in different places and at different times, cease to be foci of interest.(3) Such a shift in focus may be convenient methodologically, but it is not well-grounded theoretically. Nonetheless, review of the psychosocial epidemiologic literature on mental hospitalization shows that it is a shift that is routinely made and pursued vigorously.

In contrast to the psychosocial epidemiologic approach to research on recidivism, which uses a normatively privileged set of techniques in order to find "scientifically measurable" predictive factors, anthropological research in this area typically has sought to understand how the chronically mentally ill negotiate life in particular community or hospital settings (Scheper-Hughes 1982) and has involved the use of a variety of methods (e.g., Strauss *et al.*

1963, 1964, Maines and Markowitz 1979, 1981).

The importance of this difference lies in its implications about the direction in which research is carried out. For the psychosocial epidemiologist the phenomena studied are defined by the methods and techniques that characterize the field. For the anthropologist the research follows the aspects of the phenomena and methods and techniques of data collection are adapted to and defined by that process of inquiry (cf., Dalton 1964).

Spradley's (1971) work with chronic alcoholics, for example, focused on developing an understanding of how this group saw their interactions with the legal and treatment systems. By using a variety of methods – including observation, structured interviews, letters from informants, participation in some activities, and review of legal and medical records – Spradley was able to reveal that these people conduct their lives in general and their encounters with legal and medical professionals in particular in rational but previously unappreciated ways. Moreover, Spradley presented information suggesting that their decisions are based on systematic and explicable, but different than mainstream, ways of categorizing their environments.

More recently, Estroff (1981) studied a group of clients in a psychiatric community aftercare program in order to understand how such identified psychiatric patients live in their community and how this understanding might provide useful information about processes of deinstitutionalization. The range and felxibility of her methods, in contrast to the sorts of methodological narrowness found in the psychosocial epidemiologic literature, is reflected in her statement :

> When I refer to data, I mean primarily volumes of field notebooks filled with verbatim and reconstructed conversations, my own thoughts and feelings, descriptions of events and individual behaviors, synopses of discussions, and miscellaneous information collected from a variety of sources. The other materials I used were notes made by clients (some solicited and some unsolicited) and staff, CAS [Community Adaptation Schedule] responses that were computed, coded, and scored, some transcribed tapes of in-depth interviews with staff members, and veritable mountains of newspaper clippings, books, and scholarly articles (Estroff 1981 : 33).

The results of Estroff's study are a richly detailed description of the lifeways of this group of people and a conceptually compelling account of how the social construction of the roles these people play in their community serve to keep them in stigmatized roles. She says :

> Being a full-time crazy person is becoming an occupation among a certain population in our midst. If we as a society continue to subsidize this career, I do not think it humane or justifiable to persist in negatively perceiving those who take us up on the offer and become employed in this way. As long as we contribute to blocking their exits from this crazy system, it is ridiculously unfair to condemn and reject those who tell us and show us that they cannot leave (Estroff 1981 : 256).(4)

In a real and important sense this emphasis on understanding how particular groups adapt to their social and physical environments is continuous with anthropological work in other areas (e.g., Wallace 1970, Laughlin and Brady 1978, Liebow 1967). In addition to following from traditional anthropological concerns with how group and individual adaptation occurs, the anthropological study of chronically mentally ill people, exemplified by Estroff's and Spradley's work, seems to me to derive also from one of the more important epistemological lessons taught by the anthropological experience : There is reported in the anthropological literature such a vast variety of ways through which people conceive of and interact with their environments and experiences that not even the most common objects or characteristics can be assumed by the researcher to have an invariant, objective existence outside of the context of some specified system of meanings.

Useful Knowledge, Science and Scientism

Clearly, the anthropological and epidemiological approaches to the study of mental patient recidivism proceed from very different assumptions. On the one hand, psychosocial epidemiology focuses on apparently objective population characteristics that are measurable through the use of standardized methodological techniques and which can be treated as predictive factors. It is these techniques that define the domain of investigation. The concerns of this approach are grounded in a commitment to the positivist assumptions about observation and explanation. On the other hand, anthropological studies of the chronically mentally ill have tended to be eclectic in method and interpretive in explanatory style. This reflects the basic assumptions that observation is an interactive process (and, indeed, that the researcher must be counted as a scientific instrument), that the goal of explanation is conceptual revision and detailed understanding, and that the abstraction of "events" and "characteristics" from a world consisting of a concatenation of systems with in systems, and ongoing processes depends at least as much upon the researcher's questions and analytic preferences as upon that which may have an independent existence.

These differences in basic assumptions lead not only to the collection and presentation of different material, but are important because they effect whether or not researchers are able to have access to research support and how seriously their research conclusions are taken by others.

In western culture, for many people useful knowledge means "scientific knowledge." Policy makers and people responsible for the design, development, and evaluation of social programs frequently and explicitly require that the information they will consider relevant to their decisions be "scientific." These people fund research (by issuing contracts, developing grant competitions, and by setting research questions), and consume the products of research (by taking account of or disregarding research findings in their planning). As a result, they both influence and are influenced by what the research community investigates and how it conducts its work. Thus, the question of what kinds of work are scientific is important for practical reasons.

During the first half of this century consumers of research came to equate (mistakenly) science with technological innovation (Count 1948a, 1948b). This technological advance resulted from results of research carried out in physical sciences working within a positivist tradition. This positivist view, which appeared adequately to characterize the physical sciences (Suppe 1977), included the notion that scientific researchers were neutral, objective workers who adhered rigorously to a process called "the scientific method" and whose work led to predictions based on the discovery of 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, members of disciplines studying human social life and behavior after World War II also became caught-up in the enthusiasm surrounding scientific knowledge. They frequently sought legitimacy in the argument that their work too was scientific. They measured their success as science against a positivist conception of science derived from its analysis of physical science. Some argued that when thus measured the work of their disciplines was found wanting and so those disciplines ought to be remade by normatively requiring that practitioners carry out their work in explicitly positivist fashion (Skinner 1953, 1969, Jarvie 1967, Fritz and Plog 1970).

The consequence of this enthusiasm for positivist science was often a disavowal of the pragmatist commitment to conduct research in a way that reveals processes in the world using whatever methods work (cf., Rubinstein 1984), and the development of a fascination with methods, like that displayed in psychosocial epidemiology. Just as basic physical science became identified with and confused with technology in the thought of many researchers and consumers, the scientific study of human behavior and social life became identified with and confused with the development and use of particular research techniques.

Ironically, although one impetus for taking this tack was to secure and broaden the place of the social and behavioral sciences in day-to-day work, the endorsement of positivist principles resulted in a serious restriction of the breadth of information collected by these disciplines. This had the effect of normalizing the view that only information derived from positivist research programs is scientific. This view turns out to be incorrect and overly narrow (Suppe 1977, Rubinstein, Laughlin and McManus 1984). Moreover, adherence to it over the last three decades has been detrimental to the development of an understanding of human social life and of the physical world (Wilber 1982).

Criticisms of the adequacy of the positivist analysis of science – even as this view applies to the physical sciences – have come from many sources and from many perspectives. Among the aspects of this analysis that have been tellingly criticized are that : researchers are neutral and objective (Hanson 1958, Kuhn 1960); explanation is potential prediction (Salmon 1971, Scriven 1962); laws have universal structure (Bohm 1977); the scientific method characterizes the progress of science and the process of scientific discovery (Knorr 1981, Kuhn 1960); the phenomenon observed and the observer are independent and do not effect one another (Pinxten 1981, Bohm 1978, Pribram 1971, 1976). These and other criticisms of the positivist program are widely available (see, Suppe 1977) and I will not summarize them here. Rather, I want to discuss briefly some characteristics of a broader view of science and of useful knowledge.

It is now clear that in general the positivist analysis of the nature of science is inadequate (Suppe 1977 : 617ff). In particular many scholars are coming to realize that, even if it had been an entirely adequate account of the physical sciences, it is a mistake to take the positivist description of physical science as the measure against which all science should be judged. As Mayr (1982 : 35) puts it :

> All I wish to assert is that the physical sciences are not the appropriate yardstick of science. Physics is quite unsuited for this role because, as the physicist Eugene Wigner has stated very correctly, "present day physics deals with a limiting case."

Citing Simpson (1964), Mayr notes that biological study involves the examination of physical processes *and* processes that do not apply to the subject matter of physics :

> The point is that *all* known, material processes and explanatory principles apply to organisms, while only a limited number of them

apply to nonliving systems (Simpson 1964 : 106).

In regard to the study of human social life and behavior some additional processes that must be accounted for include : symbolism (Foster and Brandes 1980, Sahlins 1976, d'Aquili, Laughlin, and McManus 1979); intentionality (Searle 1978), consciousness (Tart 1975), various other aspects of mind (Greene 1978), and the like.

In· a social milieu that identifies useful knowledge with science it is of practical importance for those studying human social life and behavior to argue for the legitimacy of their work by asserting that it too is scientific. However, it was and remains a mistake for us to acquiesce in the popular view that this means showing that our work conforms to the model of physical science research described by "the scientific method." Indeed, to accept this as the arena in which the scientific legitimacy of our work is argued is self-defeating. It is precisely this move that leads to the drawing of derisive distinctions like that between the "hard" physical sciences and the "soft" social sciences. The further result of that specious bifurcation is the increasing delegitimization of the work of people who study human social life and behavior. The practical consequence of this process is that we are increasingly denied access to the resources our desire for which led originally to our acquiesence in the overly narrow view of science presented by positivism (Rubinstein, Laughlin, and McManus 1984).

The goal of science is the continual refinement of our understanding of our world. Progress in science mainly depends upon conceptual revision and improvement. And, contrary to the popular view that it is the collection of new facts that drives this conceptual development, it turns out that it is actually the conceptual reorganization which leads to the discovery of new and different kinds of data (Knorr 1981, Hull 1978, Mayr 1982, Toulmin 1972, Almeder 1980, Westfall 1973).

Thus Mayr (1982 : 24), for example, argues that,

> One can take almost any advance, either in evolutionary biology or in systematics, and show that it did not depend as much on discoveries as on the introduction of improved concepts.

And, writing in an historical study of evolutionary theory, Hull (1978 : 137–138) notes that,

> What really determines the success of failure of new scientific theories is how advocates of these views conduct themselves. ... Scientists can only succeed if they are willing to break a few me-

> thodological rules -- sometimes every rule in the book. However, they cannot finagle at all costs. ... Successful scientists are those that master the art of judicious finagling.

It is because scientific advance depends most heavilly upon conceptual development, rather than upon the piling up of more and more "new facts," that it is a mistake to take as the chief feature of science the use of a privileged set of techniques that more-or-less automatically provide data consistent with the current intellectual milieu. When the continual engagement of methodology that characterizes science is replaced by an insistence that particular research methods and techniques be used, the result is scientism.

An unfortunate outcome of the growth of scientism in disciplines concerned with aspects of human social life and behavior (like psychosocial epidemiology) is that the range of information that is considered legitimate and scientifically important is constricted and it becomes overly narrow (Laughlin, Shearer, and McManus 1983, Wilber 1982). The insistence in some of the social and behavioral sciences that researchers restrict their work by using methods that provide "objective" data has had the additional effect of denying the importance of the active role of the scientist ***qua*** scientific instrument. Thus, reports of social researchers that incorporate, for example, their emotional reactions to the people they work with or their "sense" of the social setting and processes effecting those people are often taken as "interesting anecdotal material" not as useful "scientific data."

In practice this narrow view of what counts as legitimate data in the human sciences frequently leads to the development of social policies and to the design and evaluation of social programs based on only these "objective" data. Because the legitimacy as scientific data of researchers' qualitative observations, and their intuitive, experiential, and other such engagements with the research process has been denied, valuable information about the social processes engendered by social policies and programs has often been ignored. This has had the ironic result that programs intended as humane and compassionate remedies to social imbalances frequently establish and maintain structural arrangements the human consequences of which are negative (Tax and Thomas 1969, Tax 1960, Estroff 1981, Spradley 1970, Liebow 1967, Rubinstein 1979a, 1979b).

The inadequacy of the positivist analysis of science as a general model of the scientific enterprise has been clear for several years (Suppe 1977). Its corollary view of the scientist as a neutral, objective observer is also coming to be widely recognized as at least problematic, if not completely mistaken. Especially in the human sciences, scientists are actively part of the observations which they make; they are "instruments" through which data are selected. As such, reports of their subjective experiences with and their reactions to their subject matter form data which are critical for the proper understanding and reasoned evaluation of their research (Stent 1975, Blackburn 1971, Wilber 1982). My view is that part of the alternative to scientism in the study of human social life and behavior is for us to take up again and to further develop the broad view of science offered by pragmatism. One aspect of this work is the development of ways of knowing in science that, by respecting the active, selective role of researchers allows us to gather information about processes in social life. Blackburn (1971; also Toulmin 1982, Pinxten 1981) has pointed out the need for science in general to achieve such "trained intuition," and Estroff's (1981) study may be taken as a preliminary example of what this work would look like when applied in particular to questions related to social policy (see also, Rubinstein 1983, Laughlin, Shearer, and McManus 1983).

The further development of such ways of knowing in science seems to me to be among the most important and urgent tasks facing anthropology and the other human sciences. To acknowledge in the abstract the failure of positivism but because the alternatives to it are not yet clear or easy, to continue to work in the positivist style is to default on our scientific responsibility.(5) Further, it is morally suspect to argue, in the face of mounting evidence that the social policies and programs based on data from positivist human sciences perpetuate the social imbalances they are intended to mitigate, that we must continue to work in that tradition because doing so provides access to funding in a way that alternative approaches do not.

It is important to review periodically the foundational assumptions we accept since the practical results of the epistemological choices we make are realized in the human consequences of the social programs that are designed using information collected in ways consistent with those choices. The positivist approach to the human sciences has been shown to be overly narrow and to rely too heavilly on the "rational" to the exclusion of other synthetic forms of knowing (Blackburn 1971, Stent 1975). Indeed, if the human sciences continue to work in the positivist tradition and to provide the "objective data" upon which are based social programs that perpetuate the denial of human dignity rather than foster its affirmation, we will have paid a dear price in human suffering in order to learn with Rubashov (Koestler 1941 : 210–211) the bitter lesson that "perhaps reason alone was a defective compass, which led one on such a winding, twisted course that the goal finally disappeared in the mist."

NOTES

(1) Preparation of this paper was supported by National Institute of Mental Health grant MH-15589. I thank David R. Maines and Sol Tax for helpful discussions of this material and for their comments on an earlier draft of this paper.

(2) Despite arguments among themselves concerning various technical aspects of positivism there was reached by the 1960's general consensus about its major tenents. Suppe (1977) calls this consensus the "received view of science," and he describes its development, the technical disputes among its proponents, and the reasons for its demise.

(3) One recent study (Brenner 1973), for instance, examined the relationship between selected economic indicators and rates of mental hospital admissions in New York State for a period of 127 years. What is remarkable about the study is that it effectively ignores examining the consequences of changes in many factors (for example, changes in the service delivery system and case identification methods, shifts in diagnostic categories, or the effects of the Civil, First, Second, or Korean Wars) during this period (see, Rubinstein 1983b).

(4) This conclusion derives from Estroff's detailed analyses of how various aspects of the aftercare environment establish and maintain such paradoxical processes. She shows how various aspects of aftercare support intended to allow the program's clients to be reintegrated into the community actually serve as barriers to that reintegration. Regarding, for example, the medications used to control the clients' psychiatric symptomology, she notes that the fact that they know they must take medication for the rest of their lives serves for them to symbolically set them apart from "normals" who are not bound to such drugs. Further, the physical side effects of some of the drugs that successfully control the psychiatric symptoms are so pronounced that they are perceived by both clients and nonclients alike as making them look "sick," "wierd," or "crazy."

(5) In connection with this point, Sol Tax (see his 1968 : 10–11) reminds me of "the old story of the inebriated gentleman wandering around beneath a street lamp in the dark of night. When approached by a policemen who asked what he was doing, the drunk replied that he was looking for his ring. The policeman helped him search for a while and then asked, "Are you sure that this is where you lost it ?" "No", replied the drunk,

"I lost it over there across the street." "Then why are you looking here ?" inquired the pliceman. "Because the light is better."

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