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Some History of Clinical Sociology and Sociological Practice, Part I[1]

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ABSTRACT

From the beginning of the discipline, sociologists have used their knowledge to bring about change. This paper reviews the early antecedents of sociological practice, and then concentrates on three areas of practice as illustrative of practice. These are: studies in intergroup relations, before and after World War II; the studies of the morale of soldiers conducted during the Second World War; and the juvenile delinquency and poverty programs. After the end of World War II the focus of sociology shifted from the outside world to disciplinary concerns, and theoretical development was seen as incompatible with the use of sociology. Sociological practice has emerged as a social movement within sociology in response to the problems created by this shift in focus. This article ends with a description of the paradigm shift; a later article will discuss the recent emergence of sociological practice.

Introduction

Clinical sociology is the use of sociological theory, methods, or findings to bring about change at the individual, small group, large organization, institutional or social system level. As such, it is part of the larger emphasis within sociology known as sociological practice. Practice includes the uses of sociology in a variety of settings for a variety of purposes. The uniqueness of clinical sociology is its focus on change, and

it is this focus on change which distinguishes clinical from other forms of sociological practice, including applied sociology.

This paper discusses some of the origins of sociological practice, with a particular emphasis on the aspects currently regarded as clinical. In the historical development of sociology, practice was a normal part of what the sociologist did. It is only in recent years that practice has become a separate sub-field and practitioners separately labeled within sociology. A paradigm shift which took place before and after World War II (Buxton and Turner, 1992) changed the emphasis in sociology to the development of theory without regard for how it was used. Later consequences of this paradigm shift led to (a perceived) decline within the field, and the emergence of the Practice Movement as a way of revitalizing the discipline.

This paper traces sociological practice from its beginnings in sociology to the time when sociological practice began to emerge as a social movement within sociology. Most of the important work cited in this paper was done by sociologists who did not have a separate label of clinical or applied or practicing sociologists; they were sociologists who were doing their work as sociologists.[2] A later paper will discuss this emergence of practice as a separate field within sociology.[3]

European Antecedents

Sociology had it intellectual roots in European philosophy and economics. There can be many arguments about when sociology really started, and which of the early scholars has a current influence on clinical sociology. Certainly Machiavelli (1988), although generally considered a political scientist, can be given credit for applying the systematic study of social relationships to individual and social change.

In his intellectual and social history of sociology, Coser (1977)[4] focuses on six of the early European scholars: August Comte, Karl Marx, Herbert Spencer, Emile Durkheim, Georg Simmel and Max Weber. Of these, it is probably Marx, Durkheim and Weber whose theories are most used today by clinical sociologists. Coser credits August Comte with being the first to use the term sociology. Comte himself "emphasized that theoretical work had to take precedence over reform activities, and that establishing the foundations of the scientific doctrine was more important for the time being than effecting any practical influence," (Coser, p. 16) a viewpoint that was to dominate much of American sociology for many years.

Karl Marx, of course, intended that his writings be the basis for planned change, and was disappointed that during his lifetime the revolution did not arise. Marx made his living outside of academia, when he made a living at all. It is ironic that his intellectual heirs in American sociology, the conflict theorists, are more content to analyze than they are to use their knowledge of social systems to bring about change.

Max Weber, on the other hand, although himself active in a number of political causes, called for a value neutrality in the social sciences, divorcing them from any thought of action. Coser points out that (p. xv), "his appeal for value neutrality was intended as a thoroughly liberating endeavor to free the social sciences from the stultifying embrace of the powers that be and to assert the right, indeed the duty, of the investigator to pursue the solution to his problem regardless of whether his results serve or hinder the affairs of the national state." This view that the sociologist should follow his own values and not be bound by those of the state became transformed by the discipline into the stance that acting on values was antithetical to scientific sociology.

Emile Durkheim, whose writings on social structure and anomie were to become a major influence on intervention programs in the United States, spent most of his career as an academic. While most of his work was primarily theoretical, his work on education was intended to influence the nature of French education in his time. He put his ideas to good use in the administration of the Sorbonne, and in his influence on the French Ministry of Education. "Nothing is so vain and sterile as that scientific puritanism which, under the pretext that science is not fully established, counsels abstention and recommends to men that they stand by as indifferent witnesses, or at least resigned ones, at the march of events" (Durkheim, 1956, p. 104, quoted by Coser, 1977, p. 170).

Early U.S. Sociologists

Early sociology in the United States reflected this conflict between scholarship and action. In Ann Arbor, Charles Horton Cooley, whose theories about the importance of the primary group had a major influence on the clinical sociology of later times, eschewed action, living a relatively secluded life in a quiet University town (Coser, 1977). Interestingly enough, it was Cooley's discussion of the relationship between theory and practice that Wirth (1931) quoted in support of Wirth's ideas of clinical sociology. On the other hand, George Herbert Mead, who taught at the University of Michigan at the same time as Cooley prior to

his move to Chicago, and whose ideas about the development of the self in social interaction became the basis for later theories of individual intervention, was more a person of action. He was involved with Jane Adams at Hull House, and with an association of Chicago businessmen working for social reform (Coser, 1977). The fact that Mead wrote little during his own lifetime, and that most of his major work has come down through the notes of his students, means that little is known of his thoughts on the relationship between theory and practice. However, as a member of the Chicago school, and as an active teacher and collaborator of Jane Adams, it seems likely that Mead was concerned about how his ideas were used in everyday life.

Much has been written about the Sociology Department at the University of Chicago during the first quarter of this century. Composed of men who formed a core group in American pragmatism, and who appear to have been conflicted about sociology's role in social reform, the Department had a lasting influence on sociology's involvement in real world activities. However, as described by Deegan (1986), social reform was primarily left to a group of women sociologists who did not receive academic recognition for their efforts. Centered around Jane Adams at Hull House, these women were left to 'do good' outside of academia, and without the peer recognition received by their male colleagues. According to Deegan (1988), many of the male faculty of the Sociology Department of the University of Chicago were involved with Hull House. These included Albion W. Small, the founding chair of the Department, reform leader Charles W. Henderson, Charles Zeublin, who made settlement work his own as well, William I. Thomas, George Herbert Mead, Ernest Burgess and Robert Park. Although, according to Deegan, Park was greatly involved with social reform movements, his ideological stance against sociology being involved with action is reflected in his influential writings.

Between the Wars

If many of the intellectual antecedents of clinical sociology were developed in the early days of sociology, the period between the first and second world wars saw the beginning of modern clinical sociology. The first known references to the concept of clinical sociology come in 1930 and 1931. In 1930, Dean Milton C. Winternitz of the School of Medicine at Yale University proposed the development of a Department of Clinical Sociology within the school. This department would have "the responsibility of acquainting the student with methods of ob-

taining a sociological history and of conducting a sociological examination . . . (the student will learn) to approach the social problems of the individual." This examination of the social life of the individual will enable the physician to "piece together the different facets of the many aspects of life that may contribute to the particular indisposition of the patient and that may require adjustment for his future well being" (Winternitz, 1930a, pp. 28-29).[5] This, of course, has not yet occurred. Waitzkin (1991) points out that even today physicians are reluctant to explore and attempt to deal with issues in the patient's life created by social problems, preferring to deal with strictly medical or psychological issues in which the physician can directly intervene.

At about the same time, Wirth (1931) described the role of clinical sociology within child guidance clinics. Quoting Cooley's support of the interconnection between theory and practice, Wirth calls for the sociologist to be involved in studying the social life of the child and in helping to design and implement changes which will bring about an improved life for the child. He also suggests that the training of physicians is deficient in sociology—an issue which Winternitz also addressed as Dean of the Yale Medical School (Winternitz, 1930b). Gordon (1989) suggests that Wirth and Winternitz must have known each other. She also suggests that it was the opposition of Abraham Flexner, who studied American medicine for the Carnegie Corporation, which led to the failure of the Yale proposal to receive funding.

The theme of sociological involvement in the study and change of individual lives has continued to be a major focus of clinical sociology. Among the early writers, Zorbaugh (1939), and Dunham (1972), discuss the appropriate role for sociologists in these endeavors, and recent writings in the *Clinical Sociology Review* suggest modern approaches to changes in individual lives.

For example, Ferguson and her colleagues (1992) demonstrate the need to integrate therapies in the treatment of mentally ill individuals.

Community development was also a major theme at this time. Perhaps the best known advocate of this was Saul Alinsky, (1934; 1984) whose work in the "Back of the Yards" community development organization in Chicago became a prototype for later efforts to involve 'indigenous' people in the war on poverty. The idea of neighborhood involvement was utilized by urban renewal planners in the fifties and sixties, perhaps in an effort to co-opt residents whose neighborhoods were being renewed into supporting these renewal efforts, and later by poverty programs as a way of empowering recipients of program efforts.

Intergroup Relations Before and After World War Two

In 1937 the Carnegie Corporation, whose support of the Flexner commission on Medical Education in 1910 served to exclude women and people of color from medical training (Brown, 1979), hired the Swedish sociologist, Gunnar Myrdal, to conduct a study of "The Negro Problem." Myrdal was chosen because he was a respected sociologist who came from a country without major minority groups; the Carnegie Corporation therefore felt that he would present an unbiased point of view. Myrdal himself perceived the issue as a moral dilemma,

> the ever raging conflict between, on the one hand, the valuations preserved on the general plane which we shall call the 'American Creed,', where the American thinks, talks, and acts under the influence of high national and Christian precepts, and, on the other hand, the valuations on specific planes of individual and group living, where personal and local interests; economic, social, and sexual jealousies; considerations of community prestige and conformity; group prejudice against particular persons or types of people; and all sorts of miscellaneous wants impulses, and habits dominate his outlook. (Myrdal, 1944, p. xvii.)

The sponsors of the study felt that it should "make the facts available and let them speak for themselves . . . (the foundation) does not undertake to instruct the public as to what to do about them" (Myrdal, p. v.). However, the study also contributed to "the need of the foundation itself for fuller light in the formation and development of its own program" (p. v).

Although the book had been completed in 1942, and hence the basic data had been collected prior to the United State's entry into World War II, the 1944 publication meant that its major impact would come after the war ended in 1945. It was remarkable both for its involvement of many African-American and white scholars of the day, and for its neglect of many other prominent African-American scholars. Although there are many references to the work of W.E.B. DuBois, there is no evidence that he was personally consulted about the study. DuBois was one of the first African-Americans to become a sociologist. As a sociologist and as an activist he made monumental contributions to race relations and to scholarship in the period from 1900 to the Second World War (DuBois, 1944; Aptheker, 1990). He moved from research in which he hoped the facts would speak for themselves to activism as one of the founders of the NAACP and back to scholarship again.

Another neglected black sociologist of the prewar era, George Edmund Haynes started as a scholar, and then for many years headed

the Commission on Race Relations of the Federal Council of Churches in America (Hunter, 1988). In this position, he developed a series of interracial and intercultural clinics to help communities deal with tensions arising from specific local problems (Haynes, 1946).

Although *An American Dilemma* was not intended as a blueprint for social action, it did serve to raise the consciousness of American sociologists about issues involving intergroup relations. But it was not alone in this. The temper of the times, which included the air of optimism which resulted from the end of the Second World War, the demands of veterans, both black and white, for more equal treatment, the desegregation of the Armed Forces by President Truman in 1948 (McCullough 1992), all led to an increase in concern about intergroup relations.

Although many of the leading social scientists of the day were involved with Myrdal's work, many others were not. Charles Gomillion, who taught for many years at Tuskeegee Institute, was actively involved in civic leadership as a teacher, citizen, and sociologist. It was Gomillion, who as President of the Tuskeegee Citizens Association, led the fight against the gerrymandering of the civic boundaries of Tuskeegee to deny effective voting rights to the Negro citizens of the area. The court fight led eventually to a victory in the United States Supreme Court, a decision which later was instrumental in the Court's 'one man one vote' rule (Gomillion, 1962; 1988). In "The Role of the Sociologist in Community Action in the Rural South" Gomillion (1988) discusses the ways in which the knowledge and perceptions of the sociologist can be used to help citizens define the issues to be worked on, the resources needed to change the situation, and the development of appropriate and acceptable solutions.

The concerns raised by *An American Dilemma* led both to an explosion in research, and to a focus on the uses of that research in solving some of the problems thus revealed. At the University of Minnesota, Arnold Rose, who had been one of the major contributors to *An American Dilemma,* embarked upon a program of research on race relations. His reader on *Race, Prejudice and Discrimination* (Rose, 1951) included a major section on "Proposed Techniques for Eliminating Minority Problems." At the Anti-Defamation League of B'Nai B'rith, Leo Srole headed a research department concerned with the development of action projects in which theory would be used as the basis for projects intended to reduce prejudice and discrimination.

At Cornell University a remarkable group of scholars coalesced in a department in which the uses of sociology was an underlying, although unstated theme.[6]

Robin Williams' 1947 *The Reduction of Intergroup Tensions,* commissioned by the Social Science Research Council, summarized what was known about techniques for reducing intergroup conflict or hostility. Its purpose was to codify existing knowledge in a way that it could be used to reduce tensions and hostility between groups, while at the same time advancing theory. *The Cornell Studies in Intergroup Relations,* a community study of the town of Elmira, New York, produced, in addition to its more scientific reports, a remarkable book by Dean and Rosen (1955). Their *Manual of Intergroup Relations* presents a number of propositions about how to improve relations among groups, and briefly reviews the data which support or modify that proposition. Edward A. Suchman, whose later research was in public health, was co-director of the Elmira projects and a sophisticated methodologist.

Also at Cornell, but in the New York State School of Industrial and Labor Relations, was William Foote Whyte, whose introduction of the spindle to solve communication problems between waitresses and cooks still stands as one of the great social inventions of this century (Porter, 1962). Whyte remained concerned with the application of sociological relationships to human problems throughout his career (Whyte, 1947; 1982). In 1982, he indicated that "This is a time for rethinking sociology . . . We must do better in the future to demonstrate the practical relevance of sociology. We can meet the challenge if we reorient the way we do sociology. I suggest that we conceptualize this focus in terms of the discovery, description, and analysis of *social inventions for solving human problems*" (Whyte, 1982, reprinted 1987, p45, emphasis in original).

Nelson Foote moved from Cornell to the University of Chicago where he became involved in family research, and then to the General Electric Company Division of Consumer Affairs. *Identity and Interpersonal Competence* (Foote and Cottrell, 1955) summarizes family studies into a series of hypotheses which family agencies could then use as the basis for their programs of helping families. In this volume he also suggests the characteristics of family agencies which make them more or less able to utilize the research findings in work with their clients.

Studies in Social Psychology in World War II:
The American Soldier Series

The years 1949 and 1950 saw the publication of one of the most important and controversial reports of studies in which sociological and social psychological research was used as the basis for important policy

decisions which affected the lives of millions. The four volumes of *Studies in Social Psychology in World War II* (more familiarly known as *The American Soldier Series)* reported on the work of the Research Branch of the Information and Education Division of the United States Army. According to former Major General Frederick Osborn, wartime Director of the Information and Education Division,

> A major purpose of the Research Staff was to provide a base of factual knowledge which would help the Director of the Army Information and Education Division in his administrative and policy decisions. The Army gave little weight to personal opinions; but when these opinions were supported by factual studies, the Army took them seriously. For the first time on such a scale, the attempt to direct human behavior was, in part at least, based on scientific evidence. (Stouffer, et. al, 1949, Vol 1, p. ix)

Osborne had been President of the Carnegie Corporation, (Clausen, 1984) although apparently not at the time it sponsored Myrdal's study. However, his location in a pivotal position in social science enabled him to recruit a knowledgeable research staff of both military and civilian sociologists and psychologists.

The Branch conducted studies on morale and on the effectiveness of training materials. Among the important accomplishments were the studies which led to the development of the point system by which decisions as to who would be discharged from the army were made. The data gathered in worldwide studies indicated the factors which should be considered, the weight that should be given to each factor, and the acceptability of the system to the men involved. Important methodological contributions included the development of techniques for the analysis of paired comparisons, the development of Guttman scaling techniques and of latent structure analysis.

The four volumes were highly controversial. While many sociologists praised the work for its actual and potential contributions to theory, for its practical utility to the war effort, and for its demonstration of the utility of social research for policy analysis, many others criticized it as being nothing but rank empiricism with no theoretical value, adding nothing to sociologists' understanding of theory. A later symposium which reviewed the work, concluded that it indeed had many important theoretical contributions to make, particularly in the area of the effects of membership and reference groups. The concept of relative deprivation, which has both theoretical and practical utility in a variety of situations, arose from this work. (Merton and Lazarsfeld, 1950).

In truth, the work must be considered both outstanding and flawed. Perhaps what is most amazing is not the paucity of theory, but that a series of studies, constructed under pressure and conducted under field conditions during wartime for an Army which required immediate information which would be useful for practical policy decisions, was done with such a strong theoretical base and methodological sophistication.

If there is a major lack in the series, hindsight suggests that too little attention was paid in these volumes to how the Army took the research results and turned them into policy decisions. The chapter on the point system for determining who would be discharged from the Army first, indicates that there was some considerable opposition to it. This opposition came from some in the military who wanted to use a system which would maintain fighting units for the war against Japan. A number of studies were done in order to demonstrate the support the point system had among enlisted men and other Army personnel.

When the Army began to consider changes in the point system to be used following VJ Day, the Research Branch mobilized its forces to examine the effect that such changes might have on the soldiers' perception of the fairness of the system. This chapter states matter of factly, "On August 9, 1945, then, the report quoted below was given to General Marshall within a few days of the final decision as to the VJ point system" (Stouffer et. al, Vol II, p. 540). At the time, General George C. Marshall was Chief of Staff of the Army. The report does not provide any information on how the decision was made that the report would be given to General Marshall, what process had to be used to insure that it was brought to his attention, and so on.

To say that the work was controversial in sociology is a major understatement. While many sociologists praised it for its contribution to research and to sociology, many others vilified it as rank empiricism which should never have been published, and which did not belong within sociology.

In many ways, it seems possible that the dichotomy of response to this series was one of the major factors which led to the labeling of all applied and clinical sociology as inferior science and inferior sociology. Although some of the country's leading sociologists had been involved, most of them (with the exception of the Cornell group) moved on to more theoretical, sociological mainstream, activities. It may be also that the criticisms leveled at *The American Soldier Series* was at least in part responsible for the reluctance of most mainstream sociologists to become involved in the War on Poverty in the 1960s.[7]

It should be noted that even Samuel Stouffer, the major author of the volumes, was concerned about the effect of the studies on social science. Writing in the *Continuities* volume, (Merton and Lazarsfeld, 1950) he says,

> The greatest danger of applied social science lies in our reward system. In many respects, the most valuable people on our staff were those who could write quick, simple, lucid reports, who could make simple figures 'talk' so a general could understand them. This may be true in industry, too. The rewards are much more likely to go to the man who has the knack of seeing clearly, if superficially, the practical problem of the consumer and writing a report which appears to smack the problem on the nose, than to the reflective analyst. Sometimes, these skills are combined in one man and there lies the greatest threat. For the salesman in him will be rewarded far above the analyst in him. Yet the very gifts of clarity and lucidity, when combined with technical competence, integrity, and intellectual depth, are precious jewels for social science.
>
> That is why I have said, on several occasions, that the very success of social science in application is also a grave danger. I believe that the universities—and especially the foundations—have a social obligation to counteract the tendency to drain off into applied research so many of our best trained minds. (Stouffer, in Merton and Lazarsfeld, 1950, p. 202)

Stouffer then goes on to lament the sterile nature of academic research, in which theory is not studied empirically, and empirical studies are not sufficiently theoretical. He does not consider the possibility that sociological practice can lead to reformulation of research questions, the results of which will require a modification of dearly held theories.

Daniel Lerner, also writing in the *Continuities* volume, suggests on the basis of his informal content analysis of the first reviews of the series that most social scientists were favorable to the work, but many humanists were not. Furthermore, he suggests that much of the humanist dislike was tied to a concern over who would be given first choice of available research funds. Lerner also suggests that the "conception of 'social engineering' which in principle can serve any master (is) a useful one for American social scientists. Most (reviewers) seemed agreed that the profession has a job to do *for* democratic thought and practice. Yet there is no consensus on *how* to do the job." (Lerner, in Merton and Lazarsfeld, 1950, p. 245. Italics in original.)

Stouffer was not alone in his concern over 'social engineering' taking the best minds away from universities and basic research into the real world and useful activities. A major group of sociologists took the

position that the job of sociologists was to study the world and develop theory, and if the best minds are to be protected from applied and clinical activities, then those who are involved in them must be second rate.

Talcott Parsons, whose vehemence about the proper place of sociology being in basic research in academia, was highly influential in setting the direction of the profession after the Second World War, and was one of the major articulator's of this position. In his 1949 Presidential address to the American Sociological Association, Parsons discussed the "proper balance between fundamental research and applied engineering work" to the detriment of the latter. He noted that "It is not a question of whether to try to live up to our social responsibilities, but how." He took the position that while work on practical problems might do some good, it would be at the expense of greater usefulness to society in the future. He made an impassioned plea for "systematic work on problems where the probable scientific significance has priority over any immediate possibility of application" (Parsons, 1949). The implication is that attention paid to real world problems would not contribute to the science of sociology, and hence would be less valuable than work aimed at increasing theoretical sophistication, no matter how arcane the theory.

The Post War Years

At the same time, the discipline did not need to be concerned with practice. In the years immediately following the Second World War, universities were expanding at a rapid rate, in order to meet the demand for education created first by the returning citizen army, and then by the increased prosperity which permitted more and more young people to delay entry into the labor market through continued education. The G. I. Bill, which made it possible for hundreds of thousands of military veterans to attend college, was one of the great social inventions of its time. (Incidentally, the high demand for education supported by the G.I. Bill had been predicted by the *American Soldier* studies.) The explosion in the demand for college professors in all fields meant that there were academic jobs for most new Ph.D.s. in sociology, as well as in other fields. Practice was not needed as a way to make a living in sociology.

An additional barrier to the development of sociological practice, including 'applied' research was the relatively easy availability of research funds for basic research, first from the National Institute of Mental Health and then from the National Science Foundation. While NIMH funding needed to be 'relevant to mental health,' the emphasis was on

the development of theory, with the assumption that the theory might be relevant to practice, or at least to the understanding of mental health. The study of practice itself, or applied contributions to practice, if funded, were the domain of psychiatrists, psychologists, social workers, and nurses. The National Science Foundation was even more basic; anything that suggested a practical project was regarded as not relevant to the mission of the Foundation.

This stance was echoed by the major foundations. At the time of Brown vs. Board of Education, the Supreme Court decision which led to the desegregation of public schools in the United States, the Ford Foundation decided that it would not support studies of school desegregation programs, on the grounds that it was the role of the government and not private foundations to support such research. When the National Institute of Child Health and Human Development opened in 1962 as the Institute in NIH which would study normal behavior, it was difficult to get sociologists to apply for support because of the close relationship between NIMH and the discipline.

This is not to suggest that the support of NIMH and NSF was not enormously important to the discipline, or that a great deal of important knowledge was not gained through studies supported by them. Assistantships on NIMH research grants or training programs supported many graduate students in sociology while they obtained their degrees. But the clear focus of research and training on basic research led to the development of an ethic that other types of sociological work was inferior.

Graduate training was concentrated in a relatively small number of departments which had interlocking ties with each other. For the most part, the student was socialized into the belief that the only proper role for sociologists was research and teaching in a top ten or top twenty graduate department and publication in the *American Sociological Review*. If the young sociologist did not get his (or, more rarely, her) degree from a top twenty department, then he or she would not receive a faculty appointment in a top twenty department. Primarily teaching departments were regarded as second, or third or worse rank, someplace graduates ended up at if they were not quite good enough. Accepting an applied job was regarded as a clear sign of failure as a professional. Now, not all sociologists believed this, but the common norms held this to be true.

The discipline also became Balkanized in two important ways. First, the increasing number of specialties within sociology created separate career lines for specialists in the sociology of this or the sociology of that, all of whom had to demonstrate that their specialty was the true,

scientific sociology. Second, subgroups within the discipline split off and formed their own, independent specialties. Criminal Justice went from courses to divisions with departments to departments of their own to whole schools within universities. Family therapy, which had been developed by sociologists under the rubric of marriage counseling, became its own clinical profession. A new discipline of students of the family, known as famologists, developed research, intervention, family life education, and other divisions within the National Council of Family Relations. Survey research centers developed on campuses, with only indirect ties to sociology departments, and public opinion researchers formed their own organization. Schools of Business taught organizational analysis and organizational development to students who needed to use the knowledge in their jobs.

The result of these splits was that many of those who might have maintained a clinical or applied presence within sociology found friendlier environs in more specialized areas. The movement of sociological scholars who had a practice orientation to these more specialized subgroups further weakened the role of practice within traditional departments, and hence within graduate training as well as within the American Sociological Association.

The Control of Juvenile Delinquency and the War on Poverty

In 1960 Richard Cloward and Lloyd Ohlin published *Delinquency and Opportunity: A Theory of Delinquent Gangs*. Using a largely Durkheimian perspective, Cloward and Ohlin summarized their hypothesis as follows:

> The disparity between what lower class youth are led to want and what is actually available to them is the source of a major problem of adjustment. Adolescents who form delinquent subcultures, we suggest, have internalized an emphasis upon conventional goals. Faced with limitations on legitimate avenues of access to these goals, and unable to revise their aspirations downward, they experience intense frustrations; the exploration of nonconformist alternatives may be the result. (p. 86)

Although this formulation owes much to that of Robert Merton's (1957) statement on "Social Structure and Anomie," the specific application of the idea that juvenile delinquency may be the result of social structural opportunities and constraints had not been stated in this form

before. Clearly implied in their work was the idea that the solution to juvenile delinquency would be to provide more acceptable opportunities to participate in the rewards of American society to those young people who, for structural reasons, had chosen unacceptable pathways.

Lloyd Ohlin became the first director of President John F. Kennedy's Committee on Delinquency and Youth Crime. Ohlin and Cloward were involved in "the conceptualization of the action program for Mobilization for Youth; for the latter undertaking designed originally as a delinquency-prevention program effort in New York's lower East Side, was to become a youth-development and then community-development program as the opportunity concept was broadened and popularized" (Kahn, 1967, p. 483).

This office supported two types of activities: university research centers on juvenile delinquency, and demonstration programs intended to reduce juvenile delinquency through providing potential delinquents with legitimate opportunities to participate in the economic benefits of American life. These demonstration projects were well funded, frequently with a combination of Federal, local and foundation funds. The approach developed for Mobilization for Youth became the blueprint for demonstration projects in other cities, including ABCD (Action for Boston Community Development) in Boston.

Kahn summarizes the ambivalent stance of the discipline towards this effort:

> Sociologists and sociologically influenced foundation executives, officials, and social workers have wrought a major change in delinquency-treatment programs and have played a significant role in the shift in the prevention field to broad concern with poverty and urban community development. It seems clear, however, that their theory-oriented concepts ("opportunity structure") were quickly popularized and became vehicles for a variety of goals, derived out of other experiences and interests. Nevertheless, studies of youth gangs, delinquent subcultures, and the lives of the poor have influenced overall goals , program philosophy, and action strategy.
>
> While sociologists may deplore the ensuing imprecision and the conversion of concepts into slogans (one could write an essay about the reification of "community power structure"), a significant contribution has been made. To some observers American sociology has become *relevant* as never before. Others may regard all this as a departure from the responsibility to develop and test knowledge. Those who would make their contribution in this latter domain might now turn quite profitably to the as yet little addressed tasks of studying the choices which American society has been willing to make—and those which it has skirted—in noting, defin-

ing, and coping with deviance. They might also seize the opportu-
nity now around us for the conceptualization and study of a mas-
sive effort at planned social change." (p. 502)

The massive effort referred to by Kahn was, of course, the War on
Poverty. The story of the discipline's involvement, or more correctly,
lack of involvement in this major social experiment has yet to be writ-
ten. It started with the general reluctance of most established sociolo-
gists to be involved with the juvenile delinquency demonstration pro-
grams—in contrast to the number of present and future leaders in the
field who had been involved in the work on *The American Soldier.* The
controversies over the *American Soldier* may have had a negative effect
on sociologists' willingness to be involved in another major, controver-
sial, messy endeavor. The relatively full employment within the field,
and the easy availability of research support for purer, methodologically
easier, research was certainly a factor, as was sociology's increasing
self-conception as a theoretical and not a clinical or applied discipline.[8]

Certainly research associated with the poverty program was not
'pure'. Much of it was action oriented, as were many of the studies of
the *American Soldier*, with the need to obtain quick results which would
be immediately useful for program planning. Many of the evaluation
studies were under some pressure to be 'success oriented,' in order to
insure future funding for programs which people agreed were socially
good, even if their effectiveness could not always be clearly demon-
strated. But mainstream sociology also created a self-fulfilling proph-
ecy; if the self-defined best minds in the field were not to be put to the
tasks of the poverty program, then it is the not-best-minds who will do
the research. But if it is the not-best-minds who are involved in the
research, then the research is defined as second rate, and no best-mind
wants to be involved in second-rate research. In truth, a great deal of
excellent work was done by sociologists involved in poverty programs,
and much of what was learned was eventually fed back to theory devel-
opment. But the perception remained.

It seems possible that another major determinant of the discipline's
reluctance to be involved in the War on Poverty was the war in Viet-
nam. Not only were the country's energies being absorbed by this war,
but sociologists, and other intellectuals, became reluctant to become
involved with a government which was pursuing this war. Involvement
in applied and clinical research and activities associated with the War
on Poverty became unacceptable because of the government's involve-
ment in Vietnam. It is almost as if many sociologists thought, a war is a

war, and the victims may be Vietnamese or they may the poor, but in any event, involvement in the war is not the responsibility of sociology.

This is not to say that no sociologists were involved. Many were, including Irwin Deutcher, the past president of the Society for Applied Sociology, and S. M. Miller, currently of Boston College, who will appear on the program on the future of sociological practice in Washington this summer. James S. Coleman's (1966) study of *Equality of Educational Opportunity* provided a blueprint for programs of school desegregation, although Coleman never thought of himself as either a clinical or an applied sociologist.

Changing audiences and changing self perceptions of sociology

Buxton and Turner (1992) suggest that in response to changing definitions of its goals and responsibilities, sociology changed its audience and thus became more isolated within itself as a discipline. Although these trends had existed for many years, a major shift in the paradigm took place during and after the Second World War under the leadership of Talcott Parsons. They point out that the early American sociologists were dependent first on a book-buying and lecture-attending public and then on private foundations for the funding of their research. Efforts during the Second World War to make sociology relevant centered in part on Parson's efforts at Harvard to make the study of social institutions the centerpiece of education, training and intervention in occupied countries. Parsons did feel that the bringing of democracy to Germany after the War would depend on institutional changes in the country, This, in turn, would require support for the study of the social institutions of the to-be-occupied countries. Sociology should provide the studies and the information; others would do the applied work.

The audience of sociology thus changed from the knowledgeable public to sociology itself, and the rewards became those from within the discipline. The reward structure of universities supported this change; decisions about promotion and tenure were made by peers who shared the same conception of proper sociological activity. As Buxton and Turner point out, government and foundation grants and textbook publishers took the place of the lecture audience and the reading public.

Parsons was concerned with "the differentiation of sociology as a science from practice" and the emergence of a "proper relation" to "applied fields" (Buxton and Turner, p. 399). Social reform was no longer central to

the profession of sociology, and professional schools such as, law and medi-
cine, would be left to consider the applications of what sociologists knew
and studied. This effort failed, despite the funds which supported it. Buxton
and Turner point out that one reason for this is as follows:

> Medical sociology was the product of grants; the 'experts' it pro-
> duced needed to establish a stable relationship with an audience of
> professional practitioners. This never happened, at least to the ex-
> tent that was originally envisioned, and as a consequence, funding
> ultimately diminished. The reasons physicians never took sociol-
> ogy seriously are many, but one that is of general significance in
> connection with the 'professional' model is this: the methodolo-
> gies and explanatory paradigms that proved so successful in estab-
> lishing a domain for sociology in medicine, for example, by show-
> ing that certain medical outcomes were statistically associated with
> 'social' variables, proved to be poorly adapted to the policy prob-
> lems they revealed. The research paradigm of demonstrating a sta-
> tistical relationship between some social attribute or socially dis-
> tributed condition and some undesirable outcome, such as infant
> mortality, rarely pointed unambiguously to solutions. The solutions
> tried by well-meaning physicians and public health officials pos-
> sessing this knowledge rarely were very effective: not only were
> the correlations between policy-mandated inputs and demonstrable
> outcomes often very low, but the character of the failures raised
> questions about the validity of the implicit causal reasoning that
> had motivated the policies. In most cases there was a great deal of
> redundancy or over-determination built into the 'social problems'
> that policies sought to eliminate: eliminating one 'cause' simply
> meant that another 'cause' would produce the same outcome. So-
> ciologists never overcame this deficiency or successfully adapted
> their methods to the practical demands of the audience, and it is
> unclear how they could have done so—in any case, the bond be-
> tween the 'experts' who were created by the grants to medical so-
> ciologists and their putative audience never gelled.

It is probably also true that medical sociology never accepted its
task of providing concrete, clinical assistance to physicians. The sub-
field of medical sociology made a clear distinction between 'sociolo-
gists of medicine,' who studied, produced theoretical papers and books,
conducted basic research about medicine, and remained pure, and 'soci-
ologists in medicine' who attempted to apply sociological insights into
the real problems which physicians and patients faced every day. Fur-
ther-more, there were no training programs for clinical or applied soci-
ologists; the learning of how to translate basic sociological knowledge
into clinically useful activities was learned the hard way—by doing it

until either it was successful or the sociologists left the field and turned back to 'basic' research.

Thus the early seventies saw a discipline which was increasingly isolated from practical affairs, and in which its members wrote and spoke primarily to each other in a private language, the jargon of sociology which even the educated lay person had difficulty understanding. The members rewarded each other for these contributions, and the discipline began to go into decline. The paradigm which had been so successful in the 1950's and 1960's was no longer working. It was under these circumstances that sociological practice began to emerge as a new social movement within sociology.

Conclusions

. This paper takes the position that the uses of sociological theory, knowledge and methods to bring about change has been a normal part of the sociological endeavor for most of its history. From its early beginnings in Europe through the early days in the United States, sociologists worked on practical as well as on theoretical issues. Some sociologists worked to improve the lives of individuals on an individual basis, while others focused more on social change, particularly in the area of race relations. Major efforts such as the study of the *"An American Dilemma"* by Gunnar Myrdal and the work of the Research Branch of the Information and Education Division of the United States Army were large and organized efforts to use sociology to bring about change. But sociology has always been ambivalent about practice; the controversial nature of the publications arising from the War Branch research led to a reluctance to be involved in the War on Poverty. Sociology shifted its focus from the general public to itself; its audience became other sociologists rather than the educated public. Specialties within sociology, such as criminology, marriage counseling, and organizational development moved into professional schools and out of sociology departments. The result was a discipline which became increasingly isolated, speaking to itself in language only it understood.

It was under these latter circumstances, when there was an apparent decline in sociology, that sociological practice re-emerged. The timing of the re-emergence seems to be related to the perceived decline in the field. The story of the development of the social movement of sociological practice will appear in another article. In the meantime, the events discussed in this paper should provide some background of the proud tradition which practice has in sociology.

NOTES

1. Parts of this paper were originally presented at the Annual Meeting of the Sociological Practice Association in Denver, Colorado in June, 1993; at the meeting of the Southwest Social Science Association in Dallas, Texas in March, 1995, and as the Presidential Address at the Annual Meeting of the Sociological Practice Association in Scottsdale, Arizona, June, 1995.

In this paper, I rely heavily on the work of others, as cited in the text, and on the historical section of the *Clinical Sociology Review*, edited by Jan Fritz. However, in many ways the paper reflects my own experience with sociology and my own viewpoint about the importance of certain events in sociology. Because my early experience with sociology made no distinction between sociology and practice, I present some of it in the following paragraphs.

I was an undergraduate sociology major at Cornell University from 1947 to 1951. At that time, I became acquainted with Myrdal's (1944) *An American Dilemma*, and with the four volume *Studies in Sociology in World War II* (Stouffer, et al, 1949, 1950), and with Robin M. Williams 1947 *The Reduction of Intergroup Tensions*. Equally important, I studied with Williams, (including an independent study reading course in which I read many of the early American Sociologists), and E. A. Suchman. I spent the summer of 1950 working on the Elmira Project, a community study of intergroup relations.

My advisor my freshman year was John Clausen. Leonard S. Cottrell Jr. was Dean of the College of Arts and Sciences. Williams, Suchman, Clausen and Cottrell had all been part of the *American Soldier* work. Nelson N. Foote was a faculty member in sociology, and William Foote Whyte was a faculty member in the School of Industrial and Labor Relations. Nelson Foote later went on to the University of Chicago, and from there to the Department of Consumer Affairs at the General Electric Corporation. Cottrell later became President of the Social Science Research Council; Clausen the founder of the Laboratory of Socio-Environmental Studies at NIMH and then Director of the Institute of Human Development at the University of California, Berkeley. Thus, my introduction to the field was to a sociology which made no distinction between the development of theory, the development of knowledge, and the uses of that knowledge for human betterment. As a graduate student at the University of Michigan I worked with Ronald Lippitt, whose whole research program combined the development of theory with the uses of knowledge.

My first job after getting my Ph.D. was as research director of the Health and Welfare Council of the Baltimore Area, where I had to learn to translate what I knew about sociology into language that community leaders and social workers would find useful. Later, while working at the National Institute of Mental Health and the National Institute of Child Health and Human Development I had a working relationship with the juvenile delinquency program and the poverty program. At Michigan State University I have spent my time in the Department of Pediatrics/Human Development, a clinical department within the College of Human Medicine, where I have been expected to take what I know as a sociologist and make it useful. The Department has been generous in providing me the freedom to do this, and supportive of my work with the Sociological Practice Association. These experiences clearly have shaped my understanding of the nature of sociology, and have informed my approach in this paper.

2. This was despite the fact that from its very beginning the American Sociological Association distinguished between 'pure' and 'applied' or 'practical' sociologists, (Clark, 1990) rather than the differential uses of sociology by people who called themselves sociologists. In 1965 the Association was still making a distinction between 'theoretical' and 'applied' sociologists, (Kallen, 1965) and it was not until some years after the formation of the ASA Section on Sociological Practice that the ASA employment census recognized Practice as a legitimate field within sociology. At the same time in the early days of the ASA there were many voices for the uses of sociology.

3. This history makes no pretense at completeness. There are many important events which deserve to be part of this history. However, I have chosen to focus on a limited number, which have influenced my own point of view.

4. In this section I rely heavily on the personal and intellectual biographies provided by Coser (1977). The interpretation of these biographies, and the parts emphasized are mine.

5. For further discussion of these events at Yale see Fritz, (1989) and Gordon (1989).

6. See footnote 1 for a discussion of this Department.

7. The psychologists who studied attitude change for the Branch went on to distinguished careers in academia, while maintaining their concern with ways of changing attitudes (Lumsdane, 1984). The sociologists also went on to distinguished careers, often in the same institution. Clausen (1984) notes that seven of the members of the Branch went on to become foundation executives, where they were in a position to influence research and teaching in the social sciences, although the foundations they worked for did not continue to support clinical and applied studies.

8. In response to this section, Jonathan Freedman pointed out that jobs in poverty program agencies were short term, and that most established sociologists were unwilling to leave secure university positions to work in them. Hence, the sociologists who did work in the program tended to be graduate students or recent Ph.D.s. However, although many established sociologists served as consultants to the Research Branch of the Information and Education Division of the United States Army, few were willing to serve as consultants to poverty program agencies.

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