

January 1989

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Sociological Practice Editors

Albion W. Small

Herbert Newhard Shenton

Alvin W. Gouldner

Jeffrey G. Reitz

See next page for additional authors

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Recommended Citation

Editors, Sociological Practice; Small, Albion W.; Shenton, Herbert Newhard; Gouldner, Alvin W.; Reitz, Jeffrey G.; Lazarsfeld, Paul F.; Freedman, Jonathan A.; Gollin, Albert E.; Boros, Alex; Lee, Alfred McClung; and Fritz, Jan M. (1989) "Section: Overview of the Field: Definitions and History," *Sociological Practice*: Vol. 7: Iss. 1, Article 1.

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Section: Overview of the Field: Definitions and History

Authors

Sociological Practice Editors, Albion W. Small, Herbert Newhard Shenton, Alvin W. Gouldner, Jeffrey G. Reitz, Paul F. Lazarsfeld, Jonathan A. Freedman, Albert E. Gollin, Alex Boros, Alfred McClung Lee, and Jan M. Fritz

Overview of the Field: Definitions and History

Sociological practice has been part of American sociology since the beginning of the field in the late 1800s. The first American Sociological Society meetings were attended by university teachers as well as sociologists with a variety of jobs in practice settings (Rhoades, 1981). Most of the early sociologists, whatever their affiliations, were interested in social progress and in finding ways to put their knowledge to use within the society (e.g., Diner, 1980:199; Barnes, 1948).

Despite its roots, somewhere after World War II, the main thrust of the field of sociology began to shift away from application and intervention to theory and statistical testing (Franklin, 1979). There were a number of influences involved, but both Mauksch (1983:2) and Gollin (1983) have noted that one important reason was the desire to be accepted as a science. According to Gollin (1983:443):

The search for scientific legitimacy led many sociologists in the early decades of the society to want to put as much distance as possible between its historical roots in social reform and its aspirations to status as an academic discipline.

While the emphasis turned toward science, the field has always included scientists who were interested in application. The articles and excerpts included in this section were selected because they provide a great deal of information about that history. Before reading them, it would be important to understand the meanings of sociological practice, clinical sociology and applied sociology.

Defining the Field

The “practical sociology” of the early 1900s (Barnes, 1948:741) is now referred to as “sociological practice.” This general label includes two areas, clinical sociology and applied sociology.

Clinical sociology. Fritz (1985) details the history of clinical sociology by examining the work of individuals who combined "a scientific approach to social life with an involvement in intervention work." She states (1985:14):

The first linking of the words "clinical" and "sociology" in an important journal occurred in 1931. Louis Wirth's (1897–1952) article "Clinical Sociology" appeared in *The American Journal of Sociology*, the most prestigious sociology journal of its day. Wirth, writing about sociologists working in child guidance clinics, made a strong case for the role "sociologists can and did play in the study, diagnosis and treatment of personality disorders because of their expertise about the varying effects of socio-cultural influences on behavior."

Fritz and Glass (1982:3) also note that Wirth thought the roles of practitioners and researchers were "equally valid and envisioned that both researchers and practitioners would benefit from the emergence of clinical sociology."

In 1944, the term became more firmly established when a formal definition of "clinical sociology" (written by Alfred McClung Lee) appeared in H. P. Fairchild's *Dictionary of Sociology*. Following Wirth's usage and Lee's definition, the term has been used to refer to sociological intervention in a variety of settings. It is the application of a sociological perspective to the analysis and design of intervention for positive social change at any level of social organization.

Clinical sociology is not meant to indicate primarily medical applications (the word "clinical" originally meant "bedside"), nor only a microsociology perspective such as individual counseling or small group work. Instead, it is essential to recognize the numerous roles that the clinical sociologist can fulfill and to recognize that the role of the clinical sociologist can be at one or more levels from the individual to the inter-societal. In fact, the translation of social theory, concepts and methods into practice requires the ability not only to recognize various levels, but to move between the levels for analysis and intervention (Freedman, 1984).

Clinical sociologists have specialty areas—such as organizations, health and illness, forensics, aging, and comparative social systems—and work in many capacities. They are, for example, organizational development specialists, sociotherapists, conflict interventionists, social policy implementors and administrators. In their work they use qualitative and/or quantitative research skills for assessment and evaluation. The field is humanistic and interdisciplinary. Important publications about the history and scope of the field include those by Glass (1979), Glassner and Freedman (1979), Straus (1979, 1985) and Fritz (1982, 1985).

Applied sociology. The applied sociologist is a research specialist who produces information that is useful in resolving problems in government, industry and other practice settings. According to Olsen and Micklin (1981), applied sociologists generally use one or more of the following methods: problem exploration, policy analysis, needs assessment, program evaluation, and social impact assessment.

The term “applied sociology” was used frequently at the turn of the century. In 1906, Lester Franklin Ward, the first president of the American Sociology Society, published a book entitled *Applied Sociology* in which he distinguished between “pure” and “applied” sociology (1906:5–6):

Just as pure sociology aims to answer the questions what, who, and how, so applied sociology aims to answer the questions what for. The former deals with facts, causes, and principles, the latter with the object, end, or program. The one treats the subject-matter of sociology, the other its use. However theoretical pure sociology may be in some of its aspects, applied sociology is essentially practical. It appeals directly to interest. It has to do with social ideals, with ethical considerations, and with what ought to be.

Early publications in the area of applied sociology include Herbert Shenton's 1927 book entitled *The Practical Application of Sociology: A Study of the Scope and Purpose of Applied Sociology* and the *Journal of Applied Sociology*. The journal was in existence from 1921 until 1927. After that time, the name was changed to *Sociology and Social Research*.

Contemporary sociologists continue to examine the meanings and forms of applied sociology (e.g., Boros, 1980; Olsen and Micklin, 1981; Freeman and Rossi, 1984; Iutovich and Cox, 1984). According to Mauksch (1983:3):

In one sense, applied sociology refers to technique and methodology. Unlike the inquiry model which governs pure research, applied sociology starts with the definition and exploration of a real problem or mission. While pure sociology, like all other pure science, seeks to test hypotheses and proscriptions and other abstracts from reality, applied sociology confronts the methodological requirement to translate complex, pluralistic situations into sociologically manageable questions. . . . Applied sociology includes the research model of problem-solving, the research model of formulating and testing action options, and the research model of evaluation.

Overview of the Section

The documents selected for inclusion in this section provide detailed information about the history of the field of sociological practice, suggest a variety of additional sources and/or are not well known. There is an emphasis on contemporary sources because of the overview they provide.

Because of limited journal space, and because they are so readily available in other publications, we did not reprint Louis Wirth's 1931 article "Clinical Sociology" or excerpts from Lester Ward's well-known book *Applied Sociology*. We anticipate persons interested in the history of the field of sociological practice also will want to read those publications.

The items presented here are arranged by date of publication beginning with 1916. We start with excerpts from an article by Albion Small, head of the University of Chicago's graduate program in sociology and the founding editor of *The American Journal of Sociology*. Small (1896, 1913), had written about the importance of sociological practice as early as 1896, had considered moving the University of Chicago's Department of Sociology in the direction of sociological practice and had asked Jane Addams, a well-known community activist, to consider a faculty appointment teaching graduate students in sociology.

Other documents, by Herbert Shenton (1927), Alvin Gouldner (1956), Paul Lazarsfeld and Jeffrey Reitz (1975), Jonathan Freedman (1982), Albert Gollin (1983), Alex Boros (1985), Alfred McClung Lee (1988) and Jan Fritz (1988), trace the history of sociological practice from a variety of perspectives. A review of the items indicates that the definitions have changed over time and continue to evolve and be refined. We are only beginning to understand the history of the entire field of American sociology.

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Fifty Years of Sociology in the United States (1865–1915)

Albion W. Small
University of Chicago

This paper will plot some of the principal points of departure from which to map the main movement of sociological thinking in the United States during the period indicated in the title. It will incidentally write into the sketch certain details of a semi-autobiographical character. . .

. . . No excuses will be offered for rather liberal transgression of the conventionalities of impersonal writing. The years which I have spent in studying the social scientists of the last four centuries have lodged in my mind one indelible impression, viz. that nearly every one of these writers might have done more for the instruction of subsequent generations if each had left on record certain testimony from his personal knowledge, which he probably regarded as trifling and which his contemporaries would probably have pronounced impertinent, than they did by writing much of a more pretentious nature which they actually transmitted . . . So it has seemed to me more and more that one of the traits of developing historical sense should be increasing consideration for the historians of the future, One hundred, two hundred, three hundred years from now there will be students trying to trace the evolution of social science. No one who has sifted the monograph material of a past period can doubt that, so long as the volumes of this *Journal* are legible, here and there a historian will search for clues to interpretation of the period that produced them. . .

. . . Dr Harper (President of the University of Chicago) responded to another true prophetic instinct. He insured from the beginning mutual reinforcement between men who were primarily interested in the theoretical phases on the one hand, and the applied phases on the other, of sociological knowledge.

Excerpts (pp. 721–22, 770–71) from "Fifty Years of Sociology in the United States (1865–1915)," *The American Journal of Sociology*, XXI/6(May, 1916):721–864.

In so far as the University of Chicago has been a factor in promoting the sociological movement, the evidence in my possession leaves no doubt in my mind that, without Dr. Harper, whatever might have been done for sociology at Chicago would have been an exaggeration of one of these phases at the expense of the other, and consequently in the long run to the discredit of both. Dr. Harper brought together, as the nucleus of the Department of "Social Science," two men who were not only strangers to each other, but whose approach to the common problem was from opposite angles. . . Dr. Henderson and the present writer were therefore the sociological staff until it was recruited by Dr. Vincent and Dr. Thomas.

Although Dr. Henderson's center of attention was social betterment, and mine was the methodology of social investigation, we never from first to last had the slightest difference of opinion about the division and correlation of our own work and that of our students. Each of us recognized in the other's program the correlate of his own. I have never had a shade of interest in abstract sociology except as a necessary preliminary to the most intelligent conduct of each and every part, from least to greatest, of the whole range of human life. Dr. Henderson took the same view of the relation between general sociology and concrete applications. While he devoted himself primarily to investigation of concrete conditions crying for immediate relief, he consistently regarded all plans for social betterment as tentative in the degree in which there is uncertainty about the underlying theories of larger social relations upon which the working plans have been based. So long as he lived, he was frequent in generous tribute to the basic importance of the more abstract phases of the work in the department.

How consistently and profitably the department has interpreted human experience in these blended phases of the general and the special is another matter. Moreover, as to both theory and practice, the relations in the country at large between general sociology and social technology still remain in an unsettled and unsatisfactory condition. Inability to do justice to the subject compels me to make this survey partial by omitting the whole history of the technological phases of the sociological movement. I restrict myself, first, to remarking that a comprehensive view of the sociological movement in the United States for the last fifty years would include such a survey as Professor Francis G. Peabody of Harvard, or Professor Graham Taylor, or Miss Jane Addams, or Dr. Devine might supply, and, secondly, to insertion of the personal profession of faith that it will be a grievous mistake, and in its results unfortunate for both as well as for the public whose interests must ultimately evaluate the work of both, if the representatives of the generalizing and of the concrete phases of the sociological movement do not develop consciousness in mutually appreciative and sympathetic co-operation. . .

Applied Sociology

Herbert Newhard Shenton

Applied Sociology and Other Applications of Sociology. The term *applied sociology* as used in this treatise refers to a systematically organized body of sociological knowledge which is practically useful for human, social and societal engineering. It is regarded as a sub-division of sociology. It is not a new science but a development and exploitation of the practical possibilities of objective and quantitative observational sociology. . .

Applied Sociology and Social Arts. Applied sociology is a science and is distinctly different from social practice which is an art. Those who actually apply sociology to the solution of social problems and the effecting of social change, are professional social workers and social artists. Each social art and social profession will undoubtedly develop its own scientific technique. Applied sociology, as herein conceived, is a body of sociological knowledge especially selected, presented, interpreted and organized for those who are endeavoring to use sociology effectively for the achievement of proximate social ends. There may be a general applied sociology and a specialized applied sociology. The former should include such sociology as is generally useful for the solution of all social problems, and the latter will be more intensive and elaborate statements of sociology which are especially applicable to a limited number of specific problems.

A distinction must be made between the development of an applied sociology as an organized body of knowledge and the application of sociology as practice. The former may grow out of the latter and the latter may increasingly depend upon the former but there is need for careful discrimination in the interest of clear and constructive thinking. Both the scientist and the practitioner, under certain circumstances, may lay good claim to the title sociologist;

Excerpts (pp.28, 31-32, 99-108) from Herbert Shenton's *The Practical Application of Sociology: A Study of the Scope and Purpose of Applied Sociology*. New York: Columbia University Press, 1927. Reprinted with permission of Columbia University Press. Copyright by Columbia University Press.

and functions of research and of practice are often performed by the same person. This, however, in no way invalidates the contention that they are two distinct processes. Much harm has resulted from the confusion of the subject and of its practical use. . .

Applied Sociology. The term most extensively used to denote those productions of sociologists which were attempts to make sociology applicable, or actually to apply it, has been *applied sociology*. As early as 1898, Edward Payson wrote *Suggestions Toward Applied Sociology*, an unpretentious volume, but suggestive and not insignificant when the time of its publication is taken into account. The classical work to date with the title *Applied Sociology* is that of Lester F. Ward, of Brown University. Although this work did not appear until 1906, it was almost predicted in his *Dynamic Sociology* in 1888. In 1920 Ward's colleague and successor, James Q. Dealy, published his *Sociology, Its Development and Applications*. In 1916, Henry Pratt Fairchild of Yale produced his *Outlines of Applied Sociology*. These works although they all purport to deal with applied sociology, are varied in subject matter and diverse in their treatments of the subject. Charles R. Henderson used the terms "applied sociology" and "social technology" interchangeably.¹ Although he wrote no books designated as applied sociology, his activity at the University of Chicago and most of his publications dealt with the application of sociology. For several years Emory S. Bogardus of the University of Southern California has been publishing a *Journal of Applied Sociology*.

Edward Payson. This little volume by Edward Payson published over twenty-five years ago is a move in the direction of applied sociology. In his discussion of the nature and function of applied sociology, he writes as follows (p. 143):

Having dislodged old and faulty assumptions, the business of applied sociology as a theory is to replace these with new assumptions, and as rapidly as may be, follow this by a readjustment of practice to theory making use of such deductive and inductive proofs as may speedily show either the uselessness or advantages of the changes proposed.

He makes a valid and necessary distinction between (1) applied sociology as a body of usable sociological knowledge and (2) the applications of the theory (a) to the readjustment of prevailing social practices and (b) to the practical sociological analysis of proposed social changes. The latter chapters of his book are devoted to a "demonstration" of how "criminal law, education and public philanthropy may be taken to illustrate the possibilities of an applied science of sociology, under which these branches may be made to depend upon sensible fact instead of upon fact and assumption inextricably woven."

Lester F. Ward was one of the first and foremost of optimistic American sociologists. He continually and indefatigably urged the possibilities of the modification of social conditions, relations and processes.² The fact that we disagree with his idea of the mutual inclusiveness of ethics and applied sociology must in no way be interpreted as an effort to discredit the fact that Ward has made a valuable contribution to the development of applied sociology. His *Applied Sociology* continues to be unique and widely read. It is regarded by many as the outstanding work on this subject. His discriminating use of the term "applied sociology" as distinguished from "pure sociology" seems to have commenced about 1898 or 1899. Some sociologists contend that he was the first American sociologist to make this distinction. In the opening sentence of his *Pure Sociology* he declares that the terms "pure" and "applied" may be used in sociology in the same sense as in other sciences and that "pure science is theoretical, applied science is practical." With this distinction it seems impossible to find fault. The difficulties come with his actual extension of this idea. No more explicit and condensed statement of his conception of the nature, scope and function of applied sociology can be given than that contained in the following excerpt from the *Applied Sociology*.

Just as pure sociology aims to answer the questions What, Why and How, so applied sociology aims to answer the questions What for. The former deals with facts, causes and principles, the latter with the object, end or purpose. The one treats the subject matter of sociology, the other its use. However theoretical pure sociology may be in some of its aspects, applied sociology is essentially practical. It appeals directly to interest. It has to do with social ideals, with ethical considerations, with what ought to be. While pure sociology treats of the "spontaneous development of society" applied sociology "deals with the artificial means of accelerating the spontaneous processes of nature." The subject-matter of pure sociology is achievement, that of applied sociology is improvement. The former relates to the past and to the present, the latter to the future. Achievement is individual. Improvement is social. Applied sociology takes account of artificial phenomena consciously and intentionally directed by society to bettering society. Improvement is social achievement. In pure sociology the point of view is purely objective. It may be said to relate to social function. In applied sociology the point of view is subjective. It relates to feeling,—the collective well-being. In pure sociology the desires and wants of men are considered as the motor agencies of society. In applied sociology they are considered as the sources of enjoyment through their satisfaction. The distinction is similar to that between production and

consumption in economics. Indeed, applied sociology may be said to deal with social utility as measured by the satisfaction of desire.³

If the ethical implications are eliminated from the above statement there remain some very real contributions to the fundamental difference between "pure" and "applied" sociology. Men's "desires" are not necessarily idealistic or social-ethical. Man may desire to use scientific methods for very selfish and perhaps even anti-social ends. As he desires to use chemistry for wanton human slaughter, so, he may desire to use sociology to devise more efficient collective procedures by which to effect the slaughter. Either individuals or groups may desire to use applied sociology for anti-social purposes and for the immediate satisfaction of self interest.

Sociology, developed and organized so as to be practically useful, will undoubtedly make possible "production" and increase "achievement" and "improvement." But the social "product" may be anything good or bad for which there is sufficient demand. It is true that demand and desire can themselves be changed. This, however, is the task of ethical, educational and religious institutions. These institutions will find general sociology useful for the determination of their objectives, but they will need a specially organized "applied sociology" to work out ways and means of achievement.

Ward's statement that applied sociology relates to the future and that pure sociology relates to the past is even more significant if it means that pure sociology is primarily historical and descriptive and in that sense deals with the past and some of the present (or immediate past), while applied sociology is a science of probabilities and in that sense deals with the future. This interpretation is in direct line with Ward's general practice throughout his writings. Ward also claims that applied sociology deals particularly with artificial social processes. However, his "pure sociology" is primarily a *description* of these artificial social processes and ways and means of accelerating them. Therefore, when he states that applied sociology *deals with* the means of accelerating social processes, it is evident that *deals with* is not the equivalent of *describes*. A general perusal of his works justifies reading into the phrase *deals with* such ideas as *makes possible* or *is practically useful for*. To the extent that these inferences are correct, Ward maintains the thesis that applied sociology must serve the social arts.

In summary, Ward's conception of applied sociology, independent of its ethical connotations, is that it must be practically useful in bringing to pass deliberate artificial accelerations of social change based upon the prediction of the future in terms of probabilities scientifically ascertained from studies of the past and present.

James Q. Dealey, who succeeded Ward at Brown University, and who collaborated with him in publication, does not make a similarly clear distinction

between *pure* and *applied* sociology in his *Sociology, Its Development and Applications*. He speaks of the application of sociology to practical problems as the application of general principles (p. 44) or of teachings (pp. 49–57) of sociology to studies of social conditions. He writes:

If one knows quite fully by observation and comparison a field of social phenomena, and is familiar with the law of its development or evolution, and in addition, comprehends the principles underlying such phenomena, he would then be prepared to go one step further and to show how such principles may be applied in studies of social conditions, so as to produce modifications in these in any desired direction. Like the formulae of chemistry, certain combinations under certain conditions should produce such results. . . . When in any science desired results can invariably be attained at the will of the scientist he has reached the acme of scientific accuracy.

In this statement his use of *desire* carries no ethical connotation. He regards the relation of applied sociology to pure sociology as the relation generally existing between the pure and the applied sciences. His idea of the development of an "applied science of sociology" appears to be limited to the application of the teachings of general sociology to present conditions. On this point he is not clear, for he considers it to be part of the task of sociology (general or at least undifferentiated) "to work out empirically improvements in the situation." A science (pure or applied) does not work out improvements. It may be *used* to work out changes which may or may not be in any ultimate sense improvements.

Henry Pratt Fairchild in his *Outline of Applied Sociology* calls attention to the danger of working out social problems as if each problem were detached. He has endeavored to show the "interrelationships" of social problems and thereby make more of general sociology available for their study. He also takes the stand that the same relation should exist between "pure" and "applied" in sociology as is common in other sciences. He does not distinguish between applied sociology as a specially organized body of sociology and the application of sociology as a practice. Following Ward's suggestion he describes the function of applied sociology in terms of good and bad, better and pernicious. Thus, in a strictly scientific sense, he mars his otherwise excellent statement of the function of applied sociology: "It is not so much concerned with finding out why society is as it is, as with determining how society can be made different from what is—better than it is." There is a nice and fundamental discrimination in this presentation, but it is weakened by the addition of the phrase "better than it is." Applied sociology cannot be limited to producing such changes as are better. Even wishful thinking cannot change the function of the applied sciences.

Journals of Applied Sociology. Three periodicals are now being published, each of which is an effort to work out effective relations between the theory and the practice of sociology. The oldest and the first in the field is *The Survey*.⁴ The other two are recent, and, although originally intended to deal primarily with regional social problems, they have already assumed national importance. One is the *Journal of Applied Sociology*⁵ and the other is *The Journal of Social Forces*.⁶ All three of these periodicals have been and are edited by sociologists who are endeavoring to make sociology practically useful and at the same time to enrich and perfect sociology generally. Various journals, sociological, psychological, anthropological, statistical and ethical and especially the *American Journal of Sociology*⁷ deal occasionally with the numerous problems of the application of sociology. There are, in addition to these, many periodicals treating the application of sociology to particular problems such as the family, community, child welfare, women in industry, etc. Certain of these will be reviewed in the following chapter which is devoted to the formulations of sociology for use in specific problems. The journals mentioned in this paragraph should be considered as factors now effective in the integration of a general applied sociology.

The *Journal of Applied Sociology* is a product of the activities of the Southern California Sociological Society, organized in 1916 for "the increase and diffusion of sociological knowledge through research, discussion and publication." It is edited by the head of the Department of Sociology of the University of Southern California and the associate editors are members of the regular staff of the department. It is a distinct effort on the part of a university department of sociology to develop an applied sociology. According to the president of the Southern California Sociological Society, the journal takes its name and function from the usage of "applied sociology" established by Lester F. Ward.⁸ It is a deliberate "striving to bind all persons who are interested in applied sociology into a closer union," and, as such, is, of course, an active agent for the promotion of research in applied sociology and the assembling and exchange of practically useful sociology.

The *Journal of Social Forces* emphasizes social movement, action, processes and forces. The scope and grasp of its work is contained in its "effective objectives" appearing among the editorials of the first volume.⁹ "The Journal," writes Professor Odum, the editor, "seeks to obtain effective objectives, some more specific, some more general. To make definite, concrete and substantial contributions to present day critical problems of American Democracy, and to make usable to the people important facts and discussion of social life and progress is one purpose." Stating it otherwise, "the *Journal* will seek to contribute something in theory, something in application toward making democracy effective in unequal places." It promises to attempt to discover and to emphasize wherever possible that social theory "which has a content that is

institutional—such theory draws the sociologist, the historian, the economist, the modern psychologist and the modern student of ethics together.” This periodical is an attempt by the sociology department of the University of North Carolina to make sociology practically available especially in North Carolina and wherever similar social problems are found.

The Survey has met an extensive and growing need. The size of its subscription lists (general and student) is a manifestation of the desire for an effective medium for the exchange of practicable sociological information and for a medium of interpretation between the theorizers and the practitioners in the field of social problems. It has aimed to fulfill a synthetic function in the field of applied sociology, (1) by conserving those integrations which are the natural results of the exchange (equilibration) of experience and (2) by relating particular social empiricisms to the theory of sociology. On the occasion of its tenth anniversary, the *Survey* attempted to appraise and to describe its function. This description represents ten years of intimate experience with the problem of using social theory for the analysis and treatment of concrete social situations and is, therefore, worthy of special consideration as an index to the trend of the application of sociology during those years. The following two paragraphs from the *Survey*'s description of its function are especially suggestive:

It is often easier to visualize what is at once a prospect, a problem and a project—by means of comparison. Let us turn to the field of engineering in this instance. There are civil engineers and mechanical engineers, electrical engineers, mining engineers, chemical engineers, industrial engineers. No doubt others. Each branch has its own concerns; all have much in common; and the public has a stake in the larger bearings of the engineering.

The Survey long since gave up endeavoring to serve as a trade journal in the specialized fields of social work comparable to the specialized divisions of engineering which have been named. To do so would have been to attempt the impossible—like an omnibus trade journal specializing at once in chemistry, mechanics, electricity, coal-mining, metallurgy and architecture. Perhaps fifty separate technical journals have grown up to meet the need in our own broad field—Industrial Hygiene, Mental Hygiene, Social Hygiene, the Modern Hospital, the Journal of Nursing, School and Society, the Family, the American City, the Journal of Criminology and Criminal Law and so to the end of the list. . . .

What we seek to do in the *Survey Mid-Monthly* is to serve as a common denominator—to do a synthetic job. . . .

In other words *The Survey* is a journal of general applied sociology (including of course applied psychology, economics, politics, etc.), contributing to all the variegated activities of what is commonly designated as social work. Its experience tends to substantiate four generalizations; first, social work, like all other social art, must be put on a basis of engineering; second, there is a difference between *general* applied sociology and *specialized* applied sociology; third, there is a difference between applied sociology and that technology of social work which depends on the application of many special sciences; and fourth, there is need for "common denominators" that will enable social workers, technologists and sociologists to clear their information.

All three of these magazines stress the need of sociological research for the building up of a practically applicable body of sociology. Their activities in stimulating research and disseminating its results will undoubtedly aid in accumulating sociological data for organization into an applied sociology.

Thus within the last twenty-five years and especially within the last few years notable efforts have been made both to publish in book form and to accumulate in periodicals the data of usable sociology, often with the avowed purpose and generally with at least the implied purpose of developing between the generalizations of pure sociology and the specific and concrete needs of social work that which has been described as "applied sociology."

Practical Sociology. About the same time that the phrase *applied sociology* came into vogue in this country, the term *practical sociology* was used by each of two distinguished statistical sociologists—by Richard Mayo-Smith at least as early as 1895 and by Carroll D. Wright as early as 1899. This practical sociology was another effort to work out a scheme for the use of sociology. Just which of the two terms, *practical* or *applied*, will ultimately prevail is probably a matter which will have to be determined by usage. Both may continue in good use. The really exact term for this body of knowledge would probably be *practicable sociology* but there are too many usages to the contrary to permit the use of this term.

NOTES

1. "Applied Sociology (or Social Technology)," *American Journal of Sociology*, 18:315. Also, "The Scope of Social Technology" in 5:465. Compare Albion W. Small, *General Sociology*, part 9.
2. Even Ludwig Gumplowicz confessed after Ward's visit to Graz in 1903 that he was compelled to admit, on account of the force of Ward's argument, that "the eternal iron laws" of the "social nature process" are modified by the help of the human intellect, itself "also a natural force." See Ludwig Gumplowicz, "An Austrian Appreciation of Lester F. Ward," *American Journal of Sociology*, 10:643-53.
3. Lester F. Ward, *Applied Sociology*, p. 5-6. Compare this concept of "utility" with the discussion *supra*, p. 71.

4. *Charities Review* (1891) monthly. *Charities* (1897) weekly. *The Survey* (1909). At present, *The Survey*, semi-monthly, as "A journal of social, civic and industrial welfare and the public health" and *The Survey Graphic*, monthly, "An illustrated magazine of social exploration, reaching out to wherever tides of generous progress are astir."
5. First published (1916 to 1921) as *Monographs and News Notes*. Since October 1921, 6/1, published bi-monthly as the *Journal of Applied Sociology*, University of Southern California Press, Los Angeles, California.
6. First published November 1922, and by The University of North Carolina Press, Chapel Hill, North Carolina. Published quarterly since September 1925.
7. The official publication of the American Sociological Society, published bi-monthly since July 1985 by the University of Chicago Press.
8. *Journal of Applied Sociology*, 6/1: 1-2.
9. *Journal of Social Forces*, 1/1: 56-7.

Explorations in Applied Social Science

Alvin W. Gouldner

Not so long ago the words “social engineer” were a term of opprobrium. They carried with them the suspicion that such a social scientist had somehow betrayed his vow of dispassionate objectivity and had sold his scientific heritage for a tasteless mess of popularity. This fastidious judgment was congenial to a stable society confident in the capacity of its established routines to cope with familiar tensions. It made sense also in a culture which had an unshaken belief in progress, rationality, and justice, and an optimistic faith that each new generation would automatically outdistance its predecessors. (7) As these assumptions no longer appear transparently self-evident, there emerge such pragmatic disciplines as disaster research, industrial sociology, military sociology, propaganda and communications research, and group dynamics—to mention only a few. Today, the growth of such organizations as HUMRO, RAND Corporation, The Air Forces Institute, and others, indicates the rapid transition to a more honorific and powerful place for the applied social sciences.

The applied social sciences have shifted for themselves, growing rapidly but in a trial-and-error fashion and with little assistance from the theorist. Traditionally, sociological theory has ministered to the needs of pure or basic researches, rather than to those of applied research. Indeed, the casual observer may almost think it a contradiction in terms to speak of a “methodology” of the applied social sciences. Yet the fact is that the applied social sciences are badly in want of such a methodology. For as a result of this deficiency, the very meaning and character of “applied social science” remain obscure and those concerned with it often reflexively reiterate received formulae.

A variety of dubious assumptions, some explicit and some tacit, are now commonly made concerning the nature of applied social science. Unless these

The author wishes to express his appreciation to Helen P. Gouldner for reading and suggesting revisions of an earlier draft of this paper, which was read at the panel on methodology at the September, 1954, meetings of the American Sociological Society in Urbana, Illinois.

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assumptions are given serious inspection they may become sacred items of faith rather than serving as useful guides to work. They can harden into a professional catechism which compulsively shapes future activities in the applied social sciences in ways that prematurely preclude lines of development which could prove fruitful. In the pages that follow several such assumptions will be subjected to re-examination. These are: (a) that an applied social science is one which applies the principles of pure or basic disciplines to practical problems; (b) that there is only one type of applied social science; (c) that applied social scientists cannot specify ends or values for their clients; (d) that resistance to the practical utilization of social science derives mainly from the inadequacy of present day research methods.

Social Science: Pure and Applied

To begin with the first assumption, it is all too commonly held that an applied sociology is "nothing but" the application of generalizations, developed by pure sociology, to concrete and practical cases. For example, in a seminar at Chicago University in 1937, A. R. Radcliffe-Brown commented: "There is . . . a very close relationship between theoretical natural science and applied natural science. Applied science is still science . . . it consists of propositions, but it consists essentially in the application of the knowledge which belongs to theoretical science to the practical problems which are met with in the application of the arts." (1) Fifteen years later essentially the same conception of applied anthropology was advanced by Darryl Forde at The International Symposium on Anthropology. (6) Russell Newman's paper on "Applied Anthropometry" (19) , at the same meeting, was prefaced with an approving reference to Webster's dictionary definition of applied science as "using and adapting abstract principles and theory in connection with concrete problems, especially with a utilitarian aim."

Though much reiterated, it would seem that this conception of applied social science is misleading if not inaccurate. There are in present day sociology few validated laws or broad generalizations; nonetheless, as the above comments indicate, there is a great acceleration of applied social science. There seems to be no close correlation, therefore, between the development of generalizations by the pure disciplines and the multiplication of opportunities for, and varieties of, applied sociology. The applied sciences cannot be fruitfully regarded as springing Athena-like from the furrowed brow of the pure disciplines. Any metaphor which conceives of applied social science as the offspring, and of the basic disciplines as parents, is misleading. It obscures the point that the applied sciences often contribute as much to pure science as they receive from it.

Perhaps the truth of the matter is that the applied social scientist presently makes use of the *concepts* rather than the generalized propositions of pure social

science. For example, anthropologists who have turned to applied endeavors often begin by asking themselves how the concept of "culture" can illuminate their particular problem. This would seem to be the point that George Foster makes in his account of research into Latin American health programs, when he comments, "The research problem was defined in the following general terms: how can the anthropological axiom—'in order to work with a people it is essential to understand their culture'—be translated into terms that would be meaningful to administrators." (7) In like manner, much of market research makes more use of the concept of "social class," to aid it in analyzing differential consuming habits, than it does of specific propositions about the behavior of social classes. Stated differently, applied social science seems to use "general orientations," which focus attention on patterns of behavior and belief that are systematically neglected by practical men, rather than using propositions which could generate specific hypotheses about this behavior. (15)

In the standard view of the relationship between applied and pure social science there is the tacit assumption that the development of the applied social sciences requires no special planning and theoretical analyses. It is assumed they possess no distinctive problems and that, with the maturation of the basic disciplines, all that will be required is to transfer their developments, like carrying bones from an old graveyard to a new one. It is in this vein that Goode and Hatt report that there is a "belief that science has best been able to achieve practical results when no goals other than those of science are considered. Those who hold this position maintain that if scientists are allowed to pursue problems dictated purely by theoretical concerns, the growth of science and hence the growth of its potential applications will be served." (9) The thought is scarcely entertained, however, that the applied and pure disciplines may have differences in their basic interests and thus in their very conceptual roots.

It is an open question whether all theoretical systems or conceptual schemes, in pure social science, have equal relevance and value for applied social science. An applied social science is above all concerned with the prediction and production of social and cultural change. As Thelen has suggested, an applied social science is a technology and, as such, requires "a set of principles useful to bring about change toward desired ends." (26) Eliot Chapple has, in fact, defined applied anthropology as "that aspect of anthropology which deals with the description of changes in human relations and in the isolation of the principles that control them. Perhaps it should also be emphasized that such a definition, by necessity, includes an examination of those factors which restrict the possibility of change in human organization." (3) There is little doubt that the central focus of all the applied social sciences is on the problem of social and cultural change.

In contrast, however, many of the current models of pure sociology have not developed an analysis of change, often having little or nothing to say about

this. Applied social science requires concepts enabling it to deal with change, while much of pure social science today is oriented to the analysis of stable structures in their equilibrium. (17) As a result, the objectives of applied social science often fail to articulate with, or derive little aid from, the models and concepts of pure social science. In this connection, there is a very instructive case in the work of Talcott Parsons, which reflects this disparity between the requirements of applied social science and current models of pure sociological theory. (20)

In Parsons' analysis of "The Problem of Controlled Institutional Change," a work in applied sociology, he attempts to develop a strategy for changing conquered Germany after World War II. In this article Parsons stresses the significance of "internal conflicts" in Germany as a tactical lever for the production of change. While the equilibrium model which Parsons normally uses in his pure theory ignores internal tensions, the problems of preparing a plan for changing German society apparently constrained Parsons to give this concept a much more salient position.

Moreover, in this same article much use is made of "class" concepts—e.g., in appraising the vulnerable position of the Junkers or in planning to modify the recruiting pattern of the German civil service—although such concepts are normally but little stressed in his pure equilibrium theory. There is, then, a strong suggestion in Parsons' work that the conceptual requirements of even his own efforts in applied sociology were not well served by his own model of pure theory.* It seems evident that the needs of an applied social science, which must above all cope with social change, are not met by all models of present-day pure theory. An applied social science cannot, therefore, be regarded as entailing the simple transfer of either the established propositions or the concepts of pure science to practical purposes. Even if a fully mature basic social science existed, the applied social sciences might still be handicapped if the former failed to be organized around concepts and models useful to the applied fields, and particularly if it failed to focus centrally on the problem of change.

The suspicion that the applied behavior sciences do suffer from this handicap grows stronger if attention is directed to one crucial case: namely, that what is probably the most successful of the applied psychologies, psychoanalysis, did not develop by way of transferring the established principles of pure academic

*It needs to be pointed out, however, that Parsons' work on "The Problem of Controlled Institutional Change" was completed before the maturation of his equilibrium model. This, however, is not the case with respect to his interesting piece on "'McCarthyism' and American Social Tension" (*The Yale Review*, Winter, 1955), which is also, I believe, vulnerable to a similar interpretation. Moreover, the former article on Germany was also clearly divergent from the pure voluntaristic model which Parsons had earlier formulated in his *Structure of Social Action*.

psychology to clinical problems. It seems instead to have been marked, from its very inception, by conceptual and theoretical innovations.*

Let there be no mistake about the meaning here: it is *not* being said that applied social sciences should not use or have not used the general principles and concepts of the basic disciplines. They may and have done so where they could. The actual relation between applied and basic social science is an empirical problem; we need many detailed case histories describing these relations as they have developed. Such researches, however, would be sorely misguided if they accepted the pat assumption now current concerning these relations as their guiding hypothesis. The following may instead be regarded as more favored hypotheses: (a) Applied social scientists are more likely to use the concepts than the generalized propositions of their basic discipline. (b) Not all concepts or theoretical models of pure social science are equally useful to applied social scientists. (c) Applied social scientists will more likely borrow from their basic disciplines those concepts and theoretical models which aid them in understanding or producing changes. (d) When the basic discipline does not provide theoretical systems or concepts aiding the applied social scientist to deal with change, the latter will develop these himself. (5) These new concepts will, in turn, exert pressure to produce modifications in the theories of the basic disciplines.

What implications follow from this analysis of the relations between pure and applied social science? Among others, it would seem that any discouragement of applied social science on the ground that it should not run too far ahead of pure science, and that its own development should await prior conceptual maturation of the pure sciences, is ill-advised. The applied social scientist cannot assume that theoretical guidance and aid will always derive from the efforts of the pure social scientist; he must be trained and prepared to make his own theoretical innovations. For unless he does so, his work may be in some ways impeded—even if it is in other ways aided—by the pure scientist, and especially by the latter's inclination to neglect the theory of social change.

One such theoretical innovation already attributable to applied behavioral scientists is the concept of "resistance to change." (8, 11) This is a concept which has derived largely from the work of the Freudians in psychology and the Marxians in sociology, both of them preeminently applied disciplines. Similarly, it is notable that the concept of "informal organization" emerged out of work in applied industrial sociology, where it was employed to account for

*Psychoanalysis of course established its own pure theoretical model of substantive psychology, but this was based upon and largely derived from its applied clinical interests. As Freud sometimes stressed, his pure theory derived from his practical experience as a clinician.

resistances to industrial change.* Thus, in the Western Electric study, Roethlisberger and Dickson comment that the social function of the informal organization among the "bank wiremen" served to "protect the group from outside interference by manifesting a strong resistance to change, or threat of change. . . ." (22)

Our analysis also has implications for the pure social scientist as well. Not only does it reinforce him in his efforts to develop a theory of social change, but it also specifically indicates one further way in which this can be done. It has been suggested that applied social scientists are constrained to develop concepts useful in the analysis of social change. It follows, then, that the pure social scientist may well derive some cues, for the formulation of a theory of change, by keeping abreast of and by making a close analysis of developments in applied fields. For by doing so, he may identify useful conceptual innovations which have "spontaneously" emerged there. Indeed, this already seems to have been done by Parsons, who has given a central place to the concept of "resistance to change" in his pure theory of social change. (21)

Engineering and Clinical Sociology

There is a second key assumption which seems to shape the growth of the applied social sciences. While it is never explicitly stated, it is nonetheless of considerable influence. This assumption seems to be that there is but one type of, or one model for, applied social science. In the pages that follow the suggestion will be made that there are at least two significantly different models available for applied social science, the "engineering" and the "clinical," and an attempt will be made to clarify a few of their underlying differences.

The distinction between an engineering and a clinical approach can be considered initially by inspecting a typical case, derived from my own experience, of an engineering research in the social sciences. An industrial concern contracts with a "management consulting" firm to conduct an employee attitude survey among its own employees. The stated aims of this research are to determine whether employees are satisfied with their working conditions, hours, wages, or supervisors. By and large, the consulting firm consents to do this on the terms specified by the hiring company. In the end, the consultant conveys a report to the company which indicates the percentage of employees who are satisfied with their wages, their supervision, or their chances for promotion. Not uncommonly, this report may also include some recommendations for

*I am, of course, aware that the concept of the "informal group" is now widely regarded as a "rediscovery" of the concept of the "primary group." This, however, overstates the continuity between the two concepts and fails to take as problematic the *differences* between the two, differences which are significant precisely in the context of an applied sociology.

changes in the company's labor relations policies. Usually, the company management invites the consultant to a discussion concerning the implications of these findings. Then, after a decent interval, the report may be quietly interred in that great graveyard of creativity, the filing room. Although crudely outlined, this is probably a representative history of the engineering type of applied social research. It is often with such a case in mind that people discuss the "gap between research and policy-making."

Notice that in the above example the consulting "engineer" has conceived and completed his assignment largely in terms formulated by his client. The consultant has failed to ask himself just why it was that the company management requested this survey in the first place; what kinds of problems produced a felt need for such a research among the company people; and will these problems persist even after the proposed survey is successfully completed according to management's prescriptions?

Many industrial sociologists would concur in believing that, underlying a request for an employee attitude survey, there usually exist a number of vaguely sensed tensions. For example, there has probably been some attenuation of informal communication between management and the worker. In short, the employee attitude survey may well serve as a functional equivalent for informal networks of communication which have deteriorated.

Such a survey, however, usually does little to alert the client to the existence of this underlying problem. Still less does the survey mend the ruptured informal channels, however much it supplies reliable data about employee attitudes. Indeed, the survey now makes it easier to continue operation *despite* the breakdown in informal organization. To that extent, then, the survey paradoxically preserves the very tensions which brought it into existence.

Again, an employee survey may also be used as a way of outflanking the union, by making it seem that management is better (because "scientifically") informed than the union leaders about the workers' feelings. In this case, one of the tensions promoting the research was a cleavage between management and the union. Here, once again, the tension is in no way mitigated by the use of the survey. If anything, the union feels increasingly threatened as a result of the research, and labor-management tensions are heightened rather than curtailed.

In contrast with these procedures, we may take a recent study in applied anthropology as a case which approximates, if it does not fully conform to, the clinical model. This is a project reported by Alan Holmberg which involved an Indian community in Peru, Hacienda Vicos. "When we first began to work at Vicos," writes Holmberg, "we soon discovered that one of the principal causes of in-group strife among the Indians was disagreements and fights over the ownership of cattle. . . . In view of this, it occurred to us—as it had apparently not occurred to the Indians—that one of the best ways in which to solve this problem would be to initiate a program of branding. This was suggested to the

Indian leaders who heartily agreed, as did the people themselves with whom we discussed this matter in a general assembly.” (10)

Branding irons were then made and offers of assistance were advanced. At first few takers were found, whereupon the matter was again discussed with the Indian leaders. Only after the wealthier leaders themselves consented to have their own cattle branded did others follow suit. Finally, through this means, community disputes concerning ownership of cattle were eliminated.

Even from this brief account certain contrasts between the clinical and engineering models are already evident. Most importantly, the “clinicians” at Hacienda Vicos did not assume, as had the “engineers” in the management consulting firm, that their clients’ own formulation of their problem could be taken at face value. Instead the clinicians took their clients’ complaints and self-formulations as only one among a number of “symptoms” useful in helping them to arrive at their own diagnosis of the clients’ problems. In the employee attitude study, the engineers studied what they were told to; at Hacienda Vicos, the clinicians made their own independent identification of the group’s problems.

The “Value-Free” Assumption

Although this is only one difference between the engineers and clinicians, it is an extremely significant one. It is significant, above all, because it makes us re-examine one of the most cherished assumptions guiding work in the applied social sciences. This is the assumption that social science, pure or applied, cannot formulate and specify ends for its client group. Legitimated by references to the conceptions of a “value-free” social science, which were advanced by Max Weber and John Stuart Mill, many applied social scientists have claimed that all they can properly do is to study the diverse consequences of different policies, or to suggest efficient means for the realization of ends already specified by their client. (25)

The important questions concerning this assumption are pragmatic ones: To what extent does it truly describe the work of applied social scientists? To what extent does it provide clear and unambiguous directives for their actual operations? Is this assumption likely to be as congenial to engineers as to clinicians? There are many problems which the applied social scientist confronts for which this assumption, treated as a directive, provides no solutions. And there are many operations in which the applied social scientist engages which this assumption, treated as a description, does not accurately portray.

For example, in the event of employment by a client whose values differ from those of the group whom the applied scientist is asked to change, with whose values and to whose ends shall the scientist conform? If the work of industrial sociologists exhibits little uncertainty in this matter, the work of applied anthropologists employed by colonial governments evidences considerable

uneasiness and perplexity. (5) Furthermore, suppose the client does not know what his values are, or suppose he does not know in what priority to order his values? As sociologists very well know, this is a cultural condition which is very likely to give rise to all manner of tensions for the client. Is the applied scientist to deny assistance in these matters, to refuse to help his client formulate his values and goals, under the justification that his is a value-free science? And if he does aid his client in specifying his ends—as evidenced for example by the work at Hacienda Vicos—then is the scientist giving more than “lip service” to the postulate that he should not specify ends for his client?

Again, what of the client who pursues values which may be somewhat incompatible—e.g., desegregation vs. political stability? (28) Should not the applied social scientist somehow indicate that the client's own values may be somewhat incompatible and that this incompatibility may be generating tensions for him? And if the applied scientist does these things, is he not then influencing the values of his client group? If the postulate of a value-free social science is not an accurate description of what applied social scientists do, and, above all, if this postulate is not translatable into clear-cut, unambiguous, operational directives, facilitating the applied scientist's solution of his professional problems, then the postulate itself—if not operationally meaningless—would seem to be in need of consideration respecification. This is not to imply that the postulate, as presently formulated, is totally useless. For the postulate of a value-free social science may be most useful as an ideological mechanism. That is, it may successfully serve the social scientist as an instrument of status defense, deflecting the suspicions of client groups who fear that the social scientist wishes to impose his own values upon them and is a silent competitor for administrative power. (13)

In any event, engineers and clinicians among applied social scientists seem to differ with respect to their interpretation of the value-free postulate. The clinician is less likely to take his client's own values as given, and he establishes a relation with the client in which they may legitimately come up for re-examination in the light of their connection with the client's problems.

There are many other respects in which clinician and engineer apparently differ and, in the remaining space, only a few of these can be examined. It will have been noted that the “clinicians” at Hacienda Vicos carefully consulted with all who would be affected by their diagnosis and proposed remedy of that community's problems. In contrast, the management “engineers” conferred with only one segment of the group, namely, the top echelon; they did not consult with the workers.

One reason for this difference is the differing anticipations which clinicians and engineers have concerning client resistance to their findings, and their differing interpretations of the sources of this resistance. The engineer fatalistically assumes that resistance to his findings is not his legitimate problem and,

at worst, is due to the present deficiencies of his own research methods. He expects that inevitable improvements in research methods will sooner or later dissipate this resistance. (16) The clinician, however, assumes that findings produced by even the most perfect research technologies will continue to meet with resistance. He assumes that this resistance is his problem and that he has a responsibility for coping with it.

Assumptions Concerning Resistance

Without doubt inadequate research impairs the relations between applied social scientists and their clients, leading to many failures in the practical use of social science. But the client's resistance to social science findings is undoubtedly motivated by many considerations. Today no one is able to weight the various factors contributing to breakdowns in the scientist-client relationship. It is well known, however, that there are important cases where this breakdown cannot be attributed to the dereliction of the researcher or to the inadequacies of his research technology. This becomes evident when a research technology is employed in two comparable settings. In one case it is given successful application, and its findings are used by the client. In another very similar setting, however, this same research method will be employed but its findings are ignored and go unused. This seems to have been the case with personnel research which was successfully conducted and fully utilized by the Army Air Force during World War II, while the Navy made very little application of the personnel research which had been conducted for it. (23)

The experience of other applied disciplines also suggests, unfortunately, that the utilization of their findings is by no means entirely dependent upon their validity. It is noteworthy that physicians have sometimes been quite successful in securing acceptance of certain of their recommendations which were far from well validated and which, in fact, they themselves later rejected. For example, American doctors persuaded many parents to feed their infants on a rigorous and regular time schedule, say once every three hours, and even succeeded in diffusing this practice to certain parts of Latin America. Yet, later, the medical professions maintained that infants should be placed on a "demand schedule" and be fed as they wished. It seems evident that, in the case of personnel research, its scientific adequacy was not sufficient to secure its equal utilization in all cases, while the inadequacy of earlier infant feeding research was not sufficient to prevent its utilization.

Pure and applied scientists alike may be relied upon to improve their research technologies and, with this, the scope and reliability of their findings. By itself, however, this will not solve the utilization problem and will not automatically guarantee that these findings are successfully put to use. Applied social science does have to contend with a kind of client resistance which has nothing to do with the deficiencies of scientific research. As suggested by the

situation at Hacienda Vicos, clinicians, unlike engineers, fully anticipate and systematically prepare to cope with such client resistance.

They never suppose that client resistance is solely, or even mainly, reinforced by the researcher's ignorance or incompetence. It is clear, for example, that we do know a great deal about certain fields, for example, about criminology and penology, *not to speak of ethnic discrimination and prejudice*. Nonetheless, it also painfully clear that this knowledge is grudgingly put to use, if at all. Indeed, it may well be true, as some psychiatric clinicians avow, that the *nearer* the social scientist approaches to the nerve centers of his client's problems, the more resistant the client becomes.

There are many reasons for resistance to the findings of social research, other than those residing in the defects of the research itself. One reason may be, as the Freudians and others have insisted, that the client actually derives certain satisfactions or gains from his disturbances. As a result, he is not entirely and singlemindedly ready to accept knowledge which exerts pressure to remedy these problems. Another reason may be that the research itself may serve as one or another form of defense mechanism. In brief, the client sometimes undertakes a research so that he does *not* have to solve certain problems, and so that he need *not* change. In this case, the very conduct of research provides participation in a problem-solving ceremonial. It is a ritual particularly pleasing to the consciences of men reared in a rational tradition. Moreover, it provides a publicly evident token of the client's good faith and of his sincere interest in resolving the problem. But it does not inevitably entail the client's commitment to the conclusions of the research, or to the recommendations for change which may be proposed.

Kenneth Burke, a gifted sociologist who obstinately calls himself a literary critic, has termed this pattern of resistance the "Hamletic strategy." Named after the Great Procrastinator, this pattern of resistance is one in which the very preparations for action are transmuted into devices for postponing action. Nor is this always a matter of *unconscious* resistance. As Burke reminds us, "we may note how legislatures regularly adopt the 'Hamletic' strategy as a way to avoid embarrassing decisions. For if you would forestall a final vote on a measure, and would do so in the best 'scientific' spirit, you need but appoint a committee empowered to find more facts on the subject." (2)

In attempting to account for the resistance to social science findings and the failure to utilize them fully for practical purposes, some emphasis has recently been placed on the status of the social scientist, which is often lower than his client's. The point has been well made that "other things being equal, the amount of utilization is likely to increase with esteem for a science and its practitioners." (23) While this is undoubtedly correct, nonetheless it must be understood that the social scientist has a complex social role which involves much more than hierarchical qualities such as prestige, power, or class. This

role consists of a culturally standardized complex of expectations and definitions of function, which leads the social scientist to develop his relationships with clients in specific ways. To understand properly the failure to use social science findings, it would seem useful to examine not only the social scientist's prestige but the other aspects of his role as well, his role conceptions, and the resultant patterns of interaction with his client. It may be useful, therefore, to examine some of the differences between the clinical and engineering models, in terms of the varying role definitions which they entail.

The Engineering Model

Up to the present, the dominant role definitions of researcher and policy-maker, adopted by most sociologists, have been cast in the classic utilitarian mold. That is, the policy-maker defines his difficulties as deriving from inadequate knowledge. He formally operates on the assumption that, if he only had greater knowledge, his problems would capitulate. It is with this in mind, presumably, that he calls upon the applied sociologist. The policy-maker also tends to assume that the inadequacy of his knowledge is somehow accidental or a matter of neglect. He rarely entertains the dismaying thought that his very ignorance may be functional to him.

The applied sociologist who accepts such a definition of his client's role is more likely to conform to the engineering model and to define himself, in turn, as the bearer of facts and figures. He assumes that the client really wants to solve the problems of which he complains. The engineering sociologist recognizes, of course, that he has a job of "communication" to do. But the engineering sociologist is prone to regard this communication as well done if he reduces his report to fourteen-word sentences and mimeographs it neatly on multi-colored paper. As Wilbur Schramm puts it, "Utilization is sometimes thought of as a process of 'telling people'—writing better pamphlets, drawing better charts, making more and better teaching films, cranking up the transmitters of the mass media. This is clearly an inadequate picture." (23) Inadequate though it is, this is very much the way in which the engineers among the applied social scientists approach the problem of the utilization of social science. It is a fascinating anomaly that, while utilitarianism has been expunged from the *theories* of most sociologists, utilitarian assumptions such as those above still remain deeply embedded in their own role relations with clients. Their heads protrude into the twentieth century, but they shall remain among the half-born so long as their feet are still rooted in the nineteenth century.

The role conceptions of applied social scientists are, of course, still very much in flux and are taking new shapes as they are subjected to new client pressures and temptations. Unaware that the utilization process is, as Schramm calls it, a two-way hook-up, the engineers are particularly vulnerable to an

unwitting redefinition of their roles in ways which obliterate their professional distinctiveness and identity.

Thus one finds the "policy scientists" taking over whole the military language of their clients, or would-be clients, and talking, for example, about the need for "intelligence" rather than for information or data. (14) The general tone of their writing has the atmosphere of a military staff issuing urgent directives, mobilizing resources, and preparing for battle. Their rediscovery that ours is "one world" takes on the flavor of geopolitics; their insistence upon "time factors" is devoid of the humanism of the historian and has, instead, the perspective of the tactician. Their new self-images apparently emphasize tough-mindedness, worldliness, and realism, which are well oriented to the military crisis of our time and well adapted for interaction with a military elite. It is another and more doubtful matter, however, whether these new self-images of the engineering sociologists are equally valuable for the development of an independent and self-conscious social science, pure or applied.

The Clinical Model

A point has now been reached where some of the characteristics of the clinical model can be brought into sharper focus. There are a great variety of such characteristics which need to be clarified; here, however, the clinical model will only be considered as a social system, particularly as it is expressed in its distinctive role relations with clients. (12, 27) (a) From an engineering standpoint, the problems as formulated by the client are usually taken at face value; the engineer tends to assume that his client is willing to reveal the problems which actually beset him. The clinical sociologist, however, makes his own independent diagnosis of the client's problems. He assumes that the problems as formulated by the client may often have a defensive significance and may obscure, rather than reveal, the client's tensions. Not only does the clinician assume that the client may have some difficulty in formulating his own problems but he assumes, further, that such an inability may in some sense be motivated, and that the client is not entirely willing to have these problems explored or remedied. The clinician, therefore, does not take his client's formulations at their face value, any more than he does comments made by an ordinary interviewee; but he does use them as points of departure in locating the client's latent problems. As Emile Durkheim (who more than any other classical sociologist used a clinical model) remarked: "... a sick man faultily interprets the feelings that he experiences and most often attributes them to a cause which is not the true one. But these feelings, such as they are, have their interest, and the clinician notes them with great care and takes them seriously. They are an element in the diagnosis, and an important one... he is not indifferent as to where they are felt, when they began." (4)

(b) The engineer focusses largely on his relations with those from whom he secures the information necessary to fill his order. He is concerned, for example, about problems of sampling, questionnaire design, or interviewing technology largely as these affect his data collection from respondents. In contrast, the clinical sociologist takes his relationship with his *client* as seriously as he does his relations with interviewees. The clinician does not allow his relationship with his client to be governed by the all-too-common "come back and see me when you've done something" approach. He attempts to arrange his relationship with a client so as to secure the latter's consent to examine the underlying problems of his group.

(c) The engineering sociologist expects his findings to be accepted by his client, and particularly so if they have been acquired in conformity with the best canons of scientific research. The clinical sociologist, however, expects his clients to resist his findings, perhaps because "he that increaseth knowledge increaseth sorrow." The engineering sociologist assumes that his relationship with his client is regulated by the postulate that ignorance is evil, and knowledge power, and that men unequivocally prefer enlightenment to ignorance. Writing in what may be regarded as an engineering vein, E. A. Shils comments, "Truth is always useful to those who exercise power, regardless of whether they wish to share that truth with those over whom their power is exercised. . . ." (24) This is very dubious. Men in power are not merely technicians, concerned solely about the use of effective means to their ends; they are also politicians, committed to morally tinged precepts and symbols, and striving like all other men to maintain a decent self-image. (18) Truths which are inconsistent with their own self-images are demoralizing and thus, in this very real sense, by no means "useful" to them. By assuming that his client wishes to learn the truth, the engineering sociologist has confused an ethical imperative with a description of the learning process. When the applied sociologist recognizes that he has the problem of helping his client *learn* something, and when he recognizes that learning is not accomplished by fact-finding or "communication" techniques alone, then he is on his way to becoming a clinician. Unlike the engineer, the clinician seeks to identify the specific sources of the client's resistance to his findings and he attempts to develop and learn new skills enabling him to cope with his resistance.

It needs to be underscored that these are only a few of the differences between an engineering and clinical sociology. It should also be remembered that there has been a focus on their differences, and a resultant neglect of the similarities which they both share as applied sociologies. What has been attempted were approximate models of the clinical and engineering approaches; any given piece of applied sociology may therefore possess some characteristics of both models. Furthermore, despite this writer's interest in the clinical model, it should not be supposed that he sees no value in the engineering model and

no difficulties in the clinical. If the engineer lacks a sophisticated conception of the client relation and an adequate appreciation of the depth and meaning of client resistance, the clinician typically lacks a sophisticated conception of research design and technology. Moreover, one may well be concerned about the practical possibilities of securing client acceptance of the clinical model in relations with groups—as distinct from individuals—and particularly with large scale organizations. Undoubtedly there are important difficulties here, but as the work proceeding at the Tavistock Institute suggests, not insurmountable ones.

An applied sociology has much to learn from the clinical disciplines. It should not be assumed, however, as is so often done these days, that the only clinical discipline which can usefully serve as a concrete model is psychoanalysis. There is much to be learned from it, particularly if it is constantly borne in mind that psychoanalysis is an applied *psychology*. As sociologists we are interested only in borrowing elements which are properly applicable to the analysis of *groups*, or for the development of change-inducing relations with them.

Physical medicine itself, or bacteriology, to name only two other clinical disciplines, may be just as valuable as psychoanalysis for the development of a clinical sociology. What we happen to know best is not necessarily what we can best use. Nor should it be supposed that a clinical sociology is characterized primarily by the use of one or another therapeutic device, such as “consultative” or “nondirective” methods. Such devices are probably better suited to a clinical than an engineering sociology. The clinicians’ basic commitment, however, is not to a particular therapeutic technique, but, rather, to a distinctive role definition. In short, a clinical discipline is not as such a psychological discipline, nor is it distinguished by a cultish commitment to any specific change-agent.

In fine, then, it has been proposed that applied sociology can profit by deliberately modeling itself, particularly its strategy of client relations, on the *several* clinical disciplines and by adapting them to its own needs. To do so effectively, however, it will have to examine reflectively and to codify systematically the elements of clinical activity in the variety of disciplines where they are presently employed. In this way, we may yet fashion a new branch of applied sociology, a clinical sociology which can aid in mending the rift between the policy maker and the social scientist and in helping groups in their time of trouble.

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History of Applied Sociology

Paul F. Lazarsfeld

Jeffrey G. Reitz

Three Historical Phases

When sociology first came to the United States it was akin to a crusade for social improvement. The story has been repeatedly and ably told, especially by Luther and Jessie Bernard¹ and by Anthony Oberschall. It usually starts with the creation of the American Social Science Association in 1865. Its purpose as told by Oberschall, was

to aid the development of Social Science, and to guide the public mind to the best practical means of promoting the Amendment of Laws, the Advancement of Education, the Prevention and Repression of Crime, the Reformation of Criminals, and the Progress of Public Morality, the adoption of sanitary regulations, and the diffusion of sound principles on the Questions of Economy, Trade, and Finance. It will give attention to Pauperism and the topics related thereto . . . (it will aim to obtain) by discussion the real elements of Truth; by which doubts are removed, conflicting opinions harmonized, and a common ground afforded for treating wisely the great social problems of the day.²

So expansive a program could not easily be maintained. Soon subdivisions were formed, which gave rise to separate organizations such as the Economic Association and the Sociological Society. Early college sociology courses were likely to be taught by Protestant ministers interested in various reform movements. And even when the first graduate department in sociology was created at Columbia University, in 1894, the catalogue carried the following statement:

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It is becoming more and more apparent that industrial and social progress is bringing the modern community face to face with social questions of the greatest magnitude, the solution of which will demand the best scientific study and the most honest practical endeavor. . . It is in the city that the problems of poverty, of meandering, of intemperance, of unsanitary surroundings, and of debasing social influences are met in their most acute form. Hence, the city is the natural laboratory of social science.

Such an announcement is tantamount to an advertisement, and a cynical interpreter could find it amusing that intemperance and other, unmentionable practices were used to entice students to New York City.

The most visible result of this alliance between social reform and early sociology was the so-called social-survey movement. It has been described in great detail by Pauline Young³, whose competent review is still very much worth reading. She describes some of the major surveys in considerable detail, and one can see how the range of topics became more and more subtle. In the beginning, the emphasis was on wages and housing conditions. Soon, social relations in the family were added, subsequently supplemented by descriptive material on attitudes. In 1912, the Russell Sage Foundation created a department of surveys and information. By 1928, the director of this department, Shelby Harrison, was able to review more than two thousand social surveys—some national in scope, others local.⁴

By the end of World War I, a sizable number of sociologists were operating. Some had come out of the social-survey movement, others had acquired systematic training abroad or in the early graduate departments in the United States. Not surprisingly, these new sociologists wanted to win prestige and academic recognition for their work. This effort was characteristic of what we call the second phase.

For an Autonomous Sociology

This phase is much less well documented than the first one. Its beginning is best represented by the creation of the Social Science Research Council in 1923. Characteristically, its main activities were concentrated in a committee on methods. The purpose of that committee was to carve out the specific characteristics of the social sciences and the ways in which the various academic disciplines that founded the Council could be distinguished from and related to one another. To capture the flavor of this period, one should study the first major publication of the Council, published in 1931: Stuart Rices's *Methods in Social Science*.⁵ In the introduction, Rice tells of the involved history of the book. In the end, the following formula was adopted: Major studies were to be

discussed by competent analysts. Robert Park wrote on W. G. Sumner and W. I. Thomas; Floyd Allport discussed cultural change "illustrated" in studies by F. S. Chapin and A. L. Kroeber; Merle Curti discussed the methodological concepts of Frederick Jackson Turner; Laswell described a specific case in which Malinowski tested a hypothesis in one tribe, and so on. Moreover, some of the authors discussed added to accounts of their work. Read today, the book has the flavor of *Who's Who* entries written by somewhat younger men about their slightly older colleagues. All 52 contributions emphasize the methods used by social scientists; as a matter of fact, the majority of the papers have the term "method" in their titles. The division into nine sections shows the same focus on methodology: temporal sequences with and without attempts at causal analysis; relations between measured and unmeasured factors; definition of objects; and establishment of scales, etc.

Stuart Rice was a major figure in the second period. In 1928, he had submitted a dissertation at Columbia on quantitative methods in politics; this paper virtually initiated the so-called behavioral movement in politics. During the New Deal he joined the Bureau of the Budget and became a kind of general adviser on the expanding research activities of the Federal government. He also wrote the introduction to Pauline Young's review of social surveys, which he obviously viewed as a special extension of his own SSRC publication. But the field had expanded so rapidly that the broad coverage of this first effort was insufficient. It seemed necessary to pursue some of the material in more depth.

In 1932, the SSRC asked the various component organizations to propose one major work each for special analysis. The Sociological Society nominated Thomas and Znaniecki's *The Polish Peasant*, and its review by Herbert Blumer set off a whole new wave of methodological concerns. His essay was followed by the transcript of an extensive discussion by well-known sociologists.⁶ The main question was whether the diaries and letters on which *The Polish Peasant* was based adequately supported the main conclusions of the study. The qualitative-quantitative issue moved into the foreground of the efforts to establish sociology as a reputable science. Two further SSRC bulletins, delayed because of the war, were devoted to the value of using "Personal Documents"—a term designed to cover all qualitative material, including detailed open-ended interviews. Robert Angell's measured discussion of the sociological use of such sources can still be read with profit today.⁷

Concurrent with this somewhat defensive discussion of qualitative procedures, a new type of quantitative study began to proliferate. Many of the quantitative techniques had come from England. Thus, sampling procedures had been first used in England before World War I, when Booth's social survey was repeated in London and other cities. These procedures became widely known, and they were used in the United States in connection with market research. Large corporations marketing food or cars or household appliances wanted to

know where they stood with consumers relative to their competitors, and the mass media, especially radio, wanted to demonstrate the size of their audiences to potential advertisers. Both corporations and media turned to consumer surveys. Questions on voter preferences and opinions on public issues were occasionally appended to these surveys and soon acquired a life of their own in terms of public opinion research. Parallel to sampling, techniques of "measurement" were refined wherever respondents had to be classified. Educators became increasingly immersed in tests and their theory. The introduction of tests by the Army during World War I stimulated new interest in general problems of classification. In the mid-thirties, the journal *Psychometrika* and Gallup's Institute of Public Opinion Research made their appearance almost simultaneously. The Social Science Research Council, in an extensive monograph by the psychologist Paul Horst⁸ but supervised by the sociologist Samuel Stouffer, focused methodological interest on the problem of prediction.

While these developments had obvious practical implications, their technical sophistication enhanced the academic respectability of the social scientists and made it easier for them to separate their professional domain from those of the reformers.

Dividing any flow of events into phases can only be done with considerable hesitation. We are inclined to place this period of self-assertion in the period between the two world wars. The history of that period has not yet been written—no monograph exists comparable to the chapters of Pauline Young mentioned above. As a matter of fact, quite a number of elements are still not clear.

For example, it is not obvious why today some authors claim that in that period social science was able to emulate natural science, perhaps because the English word "science" has a restricted connotation; the Germans and the French talk about *Wissenschaft* and *science* in a broader sense. Without surrounding "scientific" with either a laudatory or a derogatory meaning, one might agree that the methods of the social and the natural sciences show differences as well as similarities. Actually, as far as we can tell, the term was rarely used at the time, except perhaps in the rather hilarious debates between Lundberg and Znaniecki, which, incidentally, also have not yet found their proper historical account.

We have no adequate record of the role sociologists played during the Depression. No major figure seems to have been included in Roosevelt's Brain trust, a body composed mainly of economists and political scientists. But sociologists did play a role in the research activities of the various relief organizations, and a number of studies on the effects of the Depression on the family and other aspects of social life were carried out. A detailed annotated bibliography of this material (112 items) was published at the time by the senior author and an assistant.⁹ But its relation to the methodological trends of the period still remains unanalyzed. Most of these studies had a microsociological character,

and this finally raises the question of what had happened during this phase to the broad historical sweep which characterized the origins of sociology. Only in regard to one author is some good documentation available.

W. F. Ogburn was originally connected with reform movements; after a while, however, he became perhaps the most prominent spokesman for the scientific emphasis in social research. He combined these two interests by concentrating on the study of social trends. In certain areas, empirical data extending over quite a long period were available: election returns, information on scientific developments, legislative acts, etc. Ogburn made the quantitative interrelations between these diverse social indicators the main theme of his work. This led to an episode which foreshadowed the transition to the period when the newly autonomous social science and the earlier concern with contemporary problems reconverged. This event, which occurred in the interval between the wars, has recently been analyzed in considerable detail by the historian Barry D. Karl.¹⁰

The starting point is a 1,500-page book which stands unread on the library shelves because it is regarded as a collection of outdated statistics—*Recent Social Trends* published in 1933. There is drama behind this publication. During his 1928 Presidential campaign, Herbert Hoover, who was both a trained engineer and an experienced administrator, advocated the use of social science data in conjunction with the problems of unemployment and old age, his platform issues. Once elected, he appointed a commission on social trends. The commission's executive director was William Ogburn. Disagreements soon developed between the President and the commission. In particular, Ogburn wanted to release only those reports whose every statement was based on solid data; but Hoover, faced with the Depression, was eager to procure any bit of information which would be helpful in legislative social planning.

Karl documents in detail the efforts of Hoover's staff assistant to reconcile the views of the President, the various members of the commission, and the several foundations that financed the enterprise. Ogburn was successful in his determination to delay a report, although some of the commissioners were, in varying degrees, interested in the possible utilization of the social sciences in social problems as Hoover visualized them. In 1932, when Hoover ran against Roosevelt, he still did not have the social data he wanted. The report was transmitted to him in January, 1933, after his electoral defeat but while he was still President. The authors in their introduction stated that "the clarification of given values . . . in terms of today's human life . . . is a major task of social thinking." As the historian puts it, "firm in their faith, they entered oblivion."

Actually, a resurrection of the report is desirable and can almost be predicted. After all, *Recent Social Trends* is practically the cradle of the modern social indicators movement as well as an outstanding example of another issue which commands increasing attention today: the relation between historiography

and sociological data in the broadest sense—from cultural documents to demographic calculations. At the point where we shall discuss the training of “applied sociologists,” let us remember that a paperback reissue of the report, together with Karl’s historical account, would constitute a highly pertinent “case.”

When World War II broke out, in 1939, it became increasingly clear that somehow the United States would become involved. By then, social research activities had become so ubiquitous that the government turned to social researchers almost as a matter of course. In one federal office, the Department of Agriculture, sociologists had played a major role for quite a while, particularly with respect to the land-grant colleges, whose specific task it was to improve the life and work of farmers.¹¹ But once the United States entered the war, new moves followed, at first slowly and then with almost explosive rapidity. In 1939, Roosevelt began cautious support of the Allied side through Lend-Lease and similar policies. The country was in no way united behind this effort, and apparently the President watched public opinion polls rather carefully. Hadley Cantril had left Princeton University to head a special opinion research agency, originally financed by Nelson Rockefeller. Before his death, Cantril published a book in which he tells of several instances of how he provided the Executive Office with information on public opinion here and abroad.¹² At the same time, the United States Army was greatly enlarged and took the unprecedented step of creating a division of research and information.

After Pearl Harbor, all government agencies inaugurated large-scale social research activities. The Office of War Information concerned itself with civilian morale; the armed services worried about training soldiers; the overseas operations of the Office of Strategic Services tried to anticipate enemy moves. These were among the many agencies that called upon social sciences.¹³ They used all the available techniques: content analysis, sampling surveys, detailed interviews, laboratory experiments, group dynamics, etc.*

When the war was over, it was clearly impossible to revert the separation of sociology as an academic pursuit from the problems of governmental and private organizations. The convergence had become a fact, though a troublesome one. Neither the undisputed unity of the first phase nor the enthusiasm of the second could be recaptured. What we call the third phase is characterized by an ever-increasing flow of discussions of the utilization problem. We plan to describe the main issues as they evolved and to locate our book within the network of the various positions which have been taken.

*To the best of our knowledge, the full range of wartime social research has not yet been recorded. The monitoring of German radio broadcasts was substantiated by the fact that after the war, the judgments of the monitors could be compared with the records of the German Propaganda Ministry captured in Berlin. Alexander George, *Propaganda Analysis: A Study of Inferences Made from Nazi Propaganda in World War II* (Evanston, Ill., Row, Peterson, 1959).

Aspects of the Third Phase

Before World War II, one man, Kurt Lewin, had already called for a more systematic relation between academic research and the world of action. He coined the term "action research." It led to an important development, but it requires some clarification. Lewin was a prominent member of the Berlin group of Gestalt psychologists, but he had always held a more specialized interest of his own. In Germany, the notion of human action (*Handlung*) had been central to all of the social sciences. Lewin wanted psychology to make its special contribution by conducting experiments in realistic situations. Under the overall title "Contributions to the Psychology of Action," he developed a series of now-famous concepts: level of aspiration, the dynamic pressure exercised by unfinished tasks, goal displacement, etc. In 1933, Lewin had to leave Germany; he finally settled in the United States. In a manner which is still controversial, Lewin merged his ideas with the work of Moreno, who had developed sociometric techniques for the study of small groups. What Lewin did was to add the role of small groups as an influence in the *Handlungen* of their members. The word "action" was still unpopular then with American behaviorist psychologists. Using the mathematical imagery which he had always favored, Lewin gave his work the title *Group Dynamics*. The details are well described in *The Practical Theorist*, a biography by one of Lewin's students, Alfred Marrow.¹⁴

Marrow's family owned a factory which was plagued by morale problems. Persuaded that Lewin's psychological ideas might be of help, management allowed him to conduct experiments on individual attitude changes in small groups of workers.¹⁵ He was successful, and other organizations asked for his help. At one point, Lewin decided to name the whole procedure "action research." The term was a felicitous combination of Lewin's basic interest in human action and his desire to apply his theories to a remediation of the work situation. It is, however, important to remember that during his lifetime (he died in 1947), Lewin gave the term "action research" a very specific meaning: the study of individual attitudes and decisions made under the influence of small groups, which in turn could be organizationally manipulated. Only later, and through a misunderstanding, was the term used by some authors in the broader sense of the use of social research in the pursuit of practical goals.¹⁶ Lewin's best-known work in changing attitudes is probably his effort to revamp American eating habits during World War II. He found, for example, that group decisions were far superior to lectures or individual treatments in inducing housewives to switch to different cuts of meat.¹⁷

The official recognition of what was by then called applied sociology may be dated from a conference called in 1948 by the Social Science Research Council. We asked the Council what records on this conference were available in its file and the archivist was kind enough to reply:

The small conference to which you refer was held in the Council office on March 20, 1948, and the subject, according to our records was "The Expert and Applied Social Science" . . . Pendelton Herring served as its chairman. In a very brief report made at the meeting of the Council's Board of Directors in April, 1948, he noted that the conference was concerned with the possibility of clarifying the relations between the experts and those who use his knowledge in government and business. *Robert Merton, because of his interest in research on this problem, had been asked to prepare the agenda for the conference.* He had written and distributed to the participants a memorandum as well as a longer outline (dated November, 1947) of *a study he was then proposing to undertake.* Mr. Herring reported that the conference was much interested in the research aspects of the subject, and that there might be opportunity for aiding in the development of a project. However, *nothing further appears in our records.* [Emphasis ours.]

Actually, as a participant, the senior author remembers clearly that a review of the collective war experience was one of the reasons for the convocation.

The Social Science Research Council did not follow up Merton's research program. . . . Most likely, this was a situation that was ahead of its time. For beginning about 1950, *three parallel trails can be traced.* The first leads through a rather bizarre landscape: the efforts of sociologists to invent new terms to match the new demands of the third phase. At the same time, the social scientists were scrutinizing their own operations vis-a-vis the natural sciences. . . . Slowly a series of signposts evolved along a third trail, directed toward a really productive analysis of the utilization process. The search for a new synthesis, which characterizes what we call the third phase, consists of the intertwining of these parts. . . .

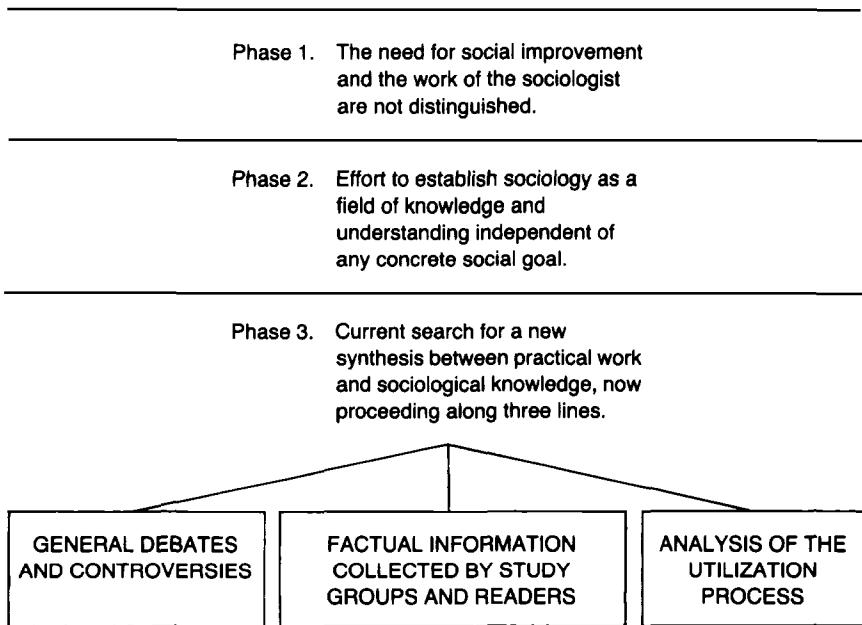


Figure 1

Notes

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Defining Clinical Sociology

Jonathan A. Freedman

Hutchings Psychiatric Center

At this time, anyone in the country can claim to be a clinical sociologist without any challenge to that designation. Persons who already have chosen this title practice as one-to-one, group, family and addictions therapists, marriage counselors, hypnotists, teachers, gerontologists, sociometricians, organizational and community consultants.

Because of this range of practice, it is necessary to explore what clinical sociology is and what it isn't. Any attempt at definition is a thankless task because no definition currently can exclude anyone from choosing this designation. Yet, attempts at clarification are important because clinical sociology is emerging as a response to both employment and ideological conditions within the discipline of sociology . . .

I have been able to locate nine definitional statements about clinical sociology in the literature. There is considerable similarity among these definitions, but not every definer is dealing with the same issues. If presented in a certain order, the statements create a generalized view of clinical sociology.

Clinical sociology is the application of a variety of critically applied practices which attempt sociological diagnosis and treatment of groups and group members in the community (Glassner and Freedman, 1979:5) . . . An analysis of clinical procedure indicates that it has three main characteristics: 1. the attention of the investigator is focused on a "case," i.e., on a person presenting concrete problems; 2. it is a co-operative enterprise and enlists the aid of a number of specialists; 3. whatever may be the theoretical interests of the participants, clinical procedure has an immediate therapeutic aim

Excerpts (pp. 34-38, 47) from J. Freedman, "Clinical Sociology: What It IS and What It ISn't—A Perspective," in *Clinical Sociology Review*, Vol. 1, 1982, pp. 34-49. Copyright 1982 Sociological Practice Association.

and includes, therefore, not merely a study of the "case," but the formulation of a program of adjustment or treatment (Wirth, 1931:50) . . . Clinical sociology is the kind of applied sociology or sociological practice which involves intimate, sharply realistic investigations linked with efforts to diagnose problems and to suggest strategies for coping with these problems (Lee, 1979:489) . . . Clinical sociology brings a sociological perspective to intervention and action for change. The clinical sociologist is essentially a change agent rather than a researcher or evaluator. Clients may be individuals, groups or organizations. The clinical task may involve, for example, a redefinition of the self, role, or situation. Clinical sociology uses a variety of techniques or methods for facilitating change. The field's value orientation is humanistic, holistic, and multi-disciplinary (Glass, 1979:513-4) . . . Clinical sociologists are change agents who use a sociological perspective as the basis for intervention. Many sociologists who teach are "clinicians" in that they try to foster change in students' attitudes and/or behavior as a result of the classroom experiences (Fritz, 1979:577) . . . Rather than adjust people to the "realities" of the "way things are" or "the system" we are committed to helping people cope with their sociocultural and historical situations and institutions and situations in the direction of self-determinism, human value and human dignity (Straus, 1979:480) . . . The sociologist, insofar as he has a point of view and method of approach to problems of personality and behavior, proceeds on the hypothesis that human beings everywhere live in social groups and that the conduct of the individuals, however it may differ from others, is always expressive of the culture of the group (Wirth, 1931:60) . . . The clinical sociologist, however, makes his own independent diagnosis of the client's problems. He assumes that the problems formulated by the client may often have a defensive significance and may obscure, rather than reveal, the client's tensions (Gouldner, 1965) . . . The sociological approach requires the marital and family therapist to understand the conditions, values and relationships which characterize the real world of the society of the American Dream and which affect marital and family interaction. Conditions associated with American society include unemployment and job insecurity. Associated values include extreme individualism, success, racism, and sexism; and associated relationships include aggressive competition and exploitation (Hurwitz, 1979:557).

What themes emerge from this conglomeration? Clinical sociology is:

1. practice oriented
2. focuses on case studies
3. works with individuals, groups, organizations, and communities ;
4. diagnostic
5. change-oriented
6. humanistic
7. tries to comprehend the societal factors which restrict the individual from being effective
8. can move beyond the client's formulation of the problem to consider other factors that affect functioning, especially broad social trends
9. uses insights derived from immersion in the critical sociological tradition; uses sociological imagination
10. leads to behavior change and growth
11. tends to have a liberal/cynical or radical ideological cast

Given what is known about working with people, their groups, organizations and communities, is such an approach valid? The answer is clearly yes. Is it the best possible approach? The answer is highly debatable. Is it an approach that is uniquely sociological? No!

One can also examine what clinical sociology is not. It is not:

1. academic
2. intrapsychic
3. biochemical
4. value-free
5. accepting of the ideological basis of the client's reality
6. culture-free
7. conservative
8. relying on a single ritualistic set of techniques to discover the key factors important in comprehending the situation under study.

The sociological tradition and a good sociological imagination can partially equip some sociologists to work as clinical sociologists. In the textbook *Clinical Sociology*, Barry Glassner and I (1979) present a version of the necessary knowledge base for a clinical sociologist. This includes theoretical grounding in historical, systems, dramaturgical, conflict, and interactional approaches with the ability to develop alternative theoretical perspectives or integrate theoretical approaches; methodological grounding in the basic skills of looking, listening, questioning, reporting and critical thinking, and how these skills are used as methods in participant observation, survey research, interviewing, and documentary analysis; substantive comprehension of ethnicity, stratification, aging, family and sex roles, social change and everyday metaphysics . . .

Sociologists tend to have early knowledge of emerging social problems. Can clinical sociologists develop specific intervention strategies that relate to problems which are emerging, aiding in empowering those who are potential victims of these problems?

... Through critical examination of any problem area of the society, a clinical sociologist can discover situations in which the application of a variety of critically applied practices which attempt sociological diagnosis and treatment of groups and group members in the community can lead to exciting approaches to practice—practice that no other profession is attempting.

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History of Applied Sociology: Some Interpretive Notes

Albert E. Gollin

... The search for scientific legitimacy led many sociologists in the early decades of the society to want to put as much distance as possible between its historical roots in social reform and its aspiration to status as an academic discipline. Several proposals, for example, were presented at the 1931 annual meeting for the purpose of changing the society's public image from one of a "religious, moral and social reform organization" to one of a "scientific society" and of "prun[ing] the society of its excrescences and . . . intensify[ing] its scientific activities." To achieve these goals, tighter control of membership and limitations on programs and publications were urged. But such initiatives toward scientific purification were countered by a concurrent, lively interest in applying sociological knowledge to the social problems of the Depression and in taking up the research opportunities presented by the New Deal. The research committee appointed to broker this dispute noted in a report in 1932 that the proposed changes would hinder the society's function of promoting sociological research and would, moreover, encourage others (presumably nonsociologists) to address the issues posed by the Depression, with an eventual loss of opportunity for and control over sociological work (Rhoades 1981, pp. 25–28).

The twin orientations reflected in these early debates—inward toward the development of sociology as a scientific discipline and outward toward its engagement with problems of the wider society—have continued to influence the course of the discipline and the programs of its professional association. Several objectives were being sought simultaneously during this and subsequent periods: to strengthen sociology's academic legitimacy and multiply opportunities for teaching and research on campuses; to widen the range of job opportunities outside academia, as the Depression and then World War II restricted hiring

Excerpts (pp. 443–446) from "The course of applied sociology: Past and future," in H. Freeman, R. Dynes, P. Rossi, and W. Whyte (Eds.), *Applied Sociology*, San Francisco: Jossey Bass, 1983. Reprinted by permission of Jossey Bass Publishers.

by colleges; and to enhance public recognition of sociology's contributions to knowledge and practical affairs as a means of defending and promoting the wider professional interests of sociologists. These objectives fluctuated in importance over the ensuing decades.

By the 1950s, the battle for academic respectability had largely been won, and sociology entered a period of sustained differentiation in subject matter, theoretical tendencies, and methodological approaches. In time, this differentiation intensified the stresses and conflicts within individual departments and across the face of the discipline over styles of sociological work. The concern with sociology's practical applications became more deeply politicized, with most of the criticism of applied sociology in the period from World War II to the mid-1960s coming not from the "scientific center," worried about the diversion of discipline-building energies caused by involvement with public-or private-sector concerns, but from the "qualitative left," sociologists concerned with the conservative stance and trivial or inhumane uses of an increasingly potent social science (Lynd, 1939, 1940; Mills, 1959; Gouldner, 1965).

On occasion, these tensions were expressed in especially revealing ways. In 1960, Paul Lazarsfeld, as president-elect of the American Sociological Association, was given the opportunity to propose a theme for its 1962 meetings. In line with his long-standing belief in the analysis of case studies as a basis for theoretical and methodological advance and, I suspect, as a direct challenge to those who viewed his interest in applied work critically, he proposed a theme that could be variously entitled "Sociology in Action" or "Applied Sociology." The Executive Council of ASA found the topic "a bit undignified" and changed the title to "The Uses of Sociology." Moreover, Lazarsfeld had to formulate a special justification that session chairpersons could use in soliciting papers, in which the value of this theme as a means of answering doubters or critics of sociology was stressed (Lazarsfeld and Reitz, 1975, pp. 30-31). The whole effort was beset with difficulties, the most significant of which were the problems most authors of papers had in identifying concrete applications of sociological ideas or findings. Eventually, an ASA-sponsored book on the topic appeared (Lazarsfeld, Sewell, and Wilensky, 1967); despite Lazarsfeld's own disappointment with the outcome (Pasanella, 1979), many of the essays deserve careful study, not only for what they tell us about sociology in the 1950s and early 1960s but also for their detailed appraisals of work in various specialty areas or fields of application.

A decade later, in 1972, another ill-starred effort was made to build bridges between the discipline and the practical demands of social policy. In the intervening years, the issue of relevance had shaken and galvanized academic sociology as well as other social science disciplines. Domestically, a long agenda of unmet economic, social, and political needs was posing insistent questions whose urgency was underscored by protest, conflict, and a wave of urban

disorders. Internationally, the Cold War had heated up; confrontations in Berlin, Cuba, and then increasingly in Southeast Asia produced waves of campus anti-war mobilizations in which sociologists often took leading roles. These issues and the heightened visibility of individual sociologists as scholars or activists contributed to an accelerated growth of students and academic programs.

As in earlier times of societal stress—depression, industrial or racial strife, war, urban disorders—sociology's claims of relevant skills in diagnosis and problem solving won for it increasing public interest and support. Federal funding for research and training that was explicitly applied in orientation grew significantly in this period. But demands for accountability accompanied this quickening flow of resources. The case for increased federal financial support had to be made and remade, and a stream of advocacy or stock-taking reports issued forth in response to this need (President's Science Advisory Committee, 1962; U.S. Congress, 1967; National Research Council, 1968, 1969; National Science Foundation, 1969; Lyons, 1969; Orlans, 1969).

As an offshoot of this trend, sociologists in departments with graduate training programs supported by the National Institute of Mental Health (NIMH) were brought together late in 1972 at a conference held under the auspices of the American Sociological Association. The conference was convened partly in response to pressures "to demonstrate the relevance of their work for the public good. Still another consideration was that federal funding agencies appeared to have more interest in research with some practical value than in research with theoretical value alone" (Schuessler, 1975, p. 4). Papers and commentaries were presented on a restricted set of problems in areas that fell within NIMH'S mandate, all of which were devoted to explicating the links between sociology and social policy. Just as a decade earlier, however, the claims of relevance were hard to document. The reasons for sociology's limited contributions to social policy in these and other areas were pinpointed with greater clarity and in greater volume than were the contributions themselves.

Apart from its solidly negative conclusions, another noteworthy feature of this gathering is that not a single sociologist working in an applied setting was invited to attend. To fill the void, a paper by Nelson Foote, presented a year later, that sharply rebuts such conclusions was reprinted in the book of conference papers. (By that time, Foote had returned to academic life after a lengthy career in industry as an applied sociologist; see Foote, 1974.) To be sure, many of the tensions felt by representatives of both the academic and applied sides of sociology were registered during the course of the proceedings (cf. Demerath, 1975). But, unlike Lazarsfeld, who had made an effort in 1962 to include the perspectives of sociological practitioners, believing that they would probably be better able to identify and analyze instances of use, the conference organizers saw no need to go beyond a roster of academic sociologists interested in graduate training issues and programs. Once again, the official disciplinary perspective

on the question of sociological applications was dominated by the experiences and concerns of academic sociologists.

The foregoing sketch of key events in the organizational history of sociology's involvement with issues of application can serve to set the 1981 workshop sharply apart from its precursors. Many of its features were similar to those observed at earlier conferences—reports of worsening academic job shortages, questions about the relevance of graduate training, a concern with the practical applicability of sociology. This time, however, the issues were discussed by both academic and applied sociologists, and the latter were recognized as strategic resources in dealing with the issues raised, a recognition unique in the history of the discipline. That this important advance is, nevertheless, only one step toward the fuller integration of sociological practitioners will presently become clearer. . . .

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A Workable Sociology

Alex Boros

... Starting at the turn of the century, the development of sociology included debates about the relationship between applied work and basic scientific theories. One group of sociologists believed in the cooperative contributions of both applied and basic researchers in producing a valid and useful sociology (Ward, 1906). To provide a publication outlet for this integrated approach, Emory Bogardus founded and managed the *Journal of Applied Sociology* from its inception in 1922 until its termination in 1927. During the same period, a major drive to promote an independent scientific sociology was made by a group of sociologists that led to a memorandum distributed during the 1931 Annual Meeting of the American Sociological Society (Rhodes, 1981). From this period on, the majority of sociologists sought acceptability in academia by stressing the objective research aspects of basic sociological theories. With each decade, the basic sociologists in academia became more dominant and applied sociological interests waned. Sociology developed along the lines predicted by Ellwood:

Every historical movement starts with some new enthusiasm, or hope, which reaches out in every direction and brings everything within the movement which may in any way serve its purpose. When the first enthusiasm is spent the movement settles down into fixed habits which are supported by strong traditions. Gradually, there grows up an orthodoxy regarding what the movement stands for, and, in order to hold their lines more securely, some leaders of the movement make the orthodoxy a very narrow one (1929).

By the 1950s the orthodoxy for sociology was narrowed down to the core of basic science objectives, eliminating applied interests as being outside of its

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purview. Applied sociologists became a minority within the discipline that they were instrumental in forming.

In the 1960s this basic orthodoxy came under attack by prominent sociologists such as Paul Lazarsfeld, C. Wright Mills, Alvin Gouldner, and Irving Horowitz. Olsen (1981) summarized their criticism:

- a. Much of what passes for basic empirical research in this field is merely trivial data manipulation.
- b. A great deal of our "theory construction" is really just meaningless categorizations and other mental gymnastics.
- c. Pursuing pure science without any concern for its applied relevance is intellectually and morally indefensible.
- d. The public will not continue for much longer to tolerate or support a field that makes no appreciable contribution to the welfare of society. . .

. . . Even though from its beginning sociology was an interventionist discipline (Bailey, 1980), today's sociologists have to defend their craft against charges of irrelevancy for solving problems of social life. It is not until people are convinced that the products of sociology are relevant to their concerns that they will begin to worry about whether they are true. To be relevant, sociology has to be workable. Who could provide better feedback on the workability of sociological perspectives in producing social betterment than applied sociologists?

In its present operational mode, our discipline, along with other social sciences, has been found inept in practical problem-solving for the following reasons (Special Commission on the Social Sciences, 1969):

- 1. Most professional social scientists are employed in academic institutions where their nonteaching activities are focused on basic theoretical research.
- 2. Empirical research tends to be exploratory, or for the purpose of testing theoretical propositions, rather than for practical problem-solving.
- 3. Even when social science work is directed to application, it often produces fragments of knowledge that need to be joined with other fragments to present a program of action.
- 4. Social scientists fail to communicate effectively with laymen about their expertise.
- 5. When faced with a specific problem that has no ready-made conceptual answer, social scientists frequently retreat to the laboratory for more research and more facts.

To overcome these criticisms, it is obvious that applied sociologists are the best link between their discipline and the policy makers. However, Denzin (1970) identifies major limitations of current applied sociology in the connector role:

1. Much of applied sociology is not theoretical with little lasting impact upon the discipline.
2. Applied sociologists are apt to become supporters instead of critics of social policies.
3. The applied sociologist has little control over the work he or she does.
4. Applied research is often just data collection for "program justification."

In the fifteen years since Denzin published his critique, applied sociology has become more professional in outlook, with better opportunities within the discipline to provide feedback to colleagues on the workability of sociological propositions in real-life settings. Much more has to be done. . . .

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The Convergence of Science and Humanistic Intervention: Practitioners in the Sociological Struggles

Alfred McClung Lee
Drew University

Sociology, like all other intellectual disciplines, has its treasured myths. With many variations, those myths justify professional orientations that can be grouped under the labels abstractionism, scientism, commercialism and humanism. All four derive directly from nineteenth century roots in the social science movement, and that movement, in turn, has many more ancient sources that still benefit and haunt us.

The industrialization, urbanization and mass migrations of the nineteenth century disrupted many ways of life. As one consequence, innovative intellectuals perceived that existing conceptualizers were not providing "the answers" to many pressing social problems.

Scholars' reactions to the pressing problems of social life varied markedly. Radicals like Karl Marx highlighted abuses of the masses and pointed to remedies. Reformers demanded changes that would help brush aside such outrageous notions as Marx's call for a cataclysmic revolution; they wanted to make the middle class continue to feel comfortable. Defenders of the status quo saw the need or advantage of developing fresh rationalizations for upper class interests. And then there were the curious-minded and practical participant observers who walked the streets, talked with all sorts of people and delved clinically into social problems and concerns, organizations and family life. These folks did not distinguish between theory and practice. They were interested in both and the way in which they were integrated with each other. These sociologists

This paper is a revised version of the Keynote Address entitled "Practitioners in the Sociological Struggles" that was given at the Sociological Practice Association's "Celebration of Practice" on 31 August 1986.

brought a degree of realism and verification to findings that often upset many of the traditional sociologists.

With sociologists having such different motivations, how could a discipline evolve that would be sufficiently "respectable" to gain acceptance among both policymakers and academics in spite of existing entrenched viewpoints and vested interests?¹ As Louis Wirth (1953:53) noted: "When sociology made claims for academic recognition it did so under the great handicap of lack of clarity of the term and wide difference of opinion among its proponents concerning its subject matter and scope." Decking out theories in the garbs of philosophical abstractionism and of scientific terminologies and methods were available choices. In such ways, pro-establishment research proposals, findings, and textbooks took on some of the authority and even glamour of the other sciences. As an illustration of my point, let me mention Lester F. Ward. When Ward (1893), a paleontologist, invaded sociology about a century ago, he brought with him such biologicistic terms as "sympodial development," "social karyokinesis," "social synergy," and "social telesis."²

Another illustration of this tendency is the work of Franklin Henry Giddings (1900, 1915, 1918, 1920, 1924). Giddings seized upon Spencerian doctrines plus statistics to provide his work with "scientific" responsibility. In spite of this, Stern (1931:654) has noted Giddings "was inclined to base his judgments . . . on immediate impressionistic reflections" often on apparently opportunistic considerations. Thus, from the late 1890s, he welcomed the imperialism of the Spanish-American War and the militarism of World War I.

After World War I, Giddings' devotion to the status quo led him to crusade against any tendencies he suspected of being socialist. His influence through his texts and his students—fifty Columbia University Ph.D.s—has been a significant influence in American sociology.

Those to whom such camouflage was repugnant persisted in pursuing their humanist concerns even though many times they annoyed or embarrassed the established. In spite of the tactics of the established, sociologist Harry Elmer Barnes (1948:741) introduced a history of sociology by noting that the "largest group of sociologists are what are usually called 'social economists' or 'practical sociologists,' namely, those chiefly interested in social work and amelioration." Viewed in historical perspective, it has been the humanist observers and clinicians who have given sociology the vitality it has exhibited.

Another part of the garb of respectability that should also be mentioned is machismo. Barnes' 1948 history of sociology, for example, mentions among "well-known personalities" in the field Jane Addams, Edith Abbott, Mary van Kleeck, Mary E. Richmond, and Jessica Peixotto, but his book contains no further reference to any of these outstanding *female contributors* to social thought and action. He does not even mention Richmond's *Social Diagnosis*

(1917) that Howard Becker (1952:624) insists "still remains one of the best systematic treatments of social case work as a scientific procedure."

Jane Addams' (1911, 1960) Hull-House, founded in Chicago in 1889, and Albion W. Small's Department of Sociology, founded at the University of Chicago in 1892, had related interests, but they were also separated, especially by male sociologists' need for "scientific respectability" unsoiled by the "uplift" attitude. Small sponsored a "drive toward objectivity," assured by the importation of European social theories. Thus sociology became "macho" not only in personnel but also by stressing theory and methods rather than participant observation. Social work, in contrast, was hospitable to female workers (especially volunteers) and was looked upon as "feminine" because of its humanitarian and moralistic "uplift" orientation (Deegan, 1987).

When the University of Chicago organized its own settlement in 1894, Mary Eliza McDowell became its first head resident, but she was not a member of the sociology department (Wade, 1958). One of the department sociologists, Charles R. Henderson, was said to be more "humanitarian" than "objective scientist," and his successor in 1916, Ernest W. Burgess, did make contacts and send students to study in social work and other community agencies. This was excused by the more pretentious because it made possible "great data-gathering efforts" (Faris, 1967:12, 52). The social workers gathered the data.

W. I. Thomas and Robert E. Park from 1913 through 1918 and then Park and Burgess on into the 1930s humanized the department and gave it its great days, but the department remained short of women (Bulmer, 1984; Matthews, 1977). A historian (Faris, 1967:126) also tells how the sociology department at Columbia University was similarly distorted by "old-boyism" as well as scientism.

As the foregoing suggested, the genealogy of clinical or practical or applied or humanist sociology is more accurately traced to social workers, reformers, and explorers than to the vaunted philosophical sociologists of earlier periods. Actually it also would be wise to include among our forebears, as well as among our current stimulants, socially conscious novelists and investigative journalists such as Charles Dickens, Lincoln Steffens, Sinclair Lewis and Gore Vidal. The influential and scientific Karl Marx often is spoken of as an abstract theorist, but he was a perceptive observer and an investigative journalist as well as a scholar.

Clinical studies of the past that are too often neglected in sociological histories include ones by Engels, Booth, Kellogg and Galpin. In the early 1840s, Friedrich Engels (1976:323), as an immigrant in England, sought "more than mere *abstract* knowledge" about the underprivileged there. As he told those social victims later in his *Condition of the Working Class in England in 1844*, he "wanted to see you in your own homes, to observe you in everyday life, to

chat with you on your condition and grievances, to witness your struggles against the social and political power of your oppressors."

Even though Charles Booth was the owner of a successful shipping line out of Liverpool, by the 1890s he had "developed the habit of exploring the East End of London, mingling with the people and becoming familiar with their lifestyles" (Kent, 1981:53). In consequence, he decided to undertake a comprehensive survey of the *Life and Labour of the People of London*, eventually published in seventeen volumes (Booth 1902-03). As the historian Raymond A. Kent (1981:59, 61-62) asserts, this was "a gigantic undertaking, unparalleled in its time and unsurpassed by modern empirical sociologists. Yet this work generally has been dismissed as mere fact gathering and unrelated to sociology proper. Such views, Kent insists, "are mistaken."

Booth's analysis contains "the pervasive conception of class as a 'style of life' involving a multiplicity of criteria and as a force in the community having considerable impact on various types of social institutions." Booth's work contains "no shortage of sociological insight and much of what he said was suggestive of what would now be regarded as in the best tradition of sociological research."

A similarly significant investigation in the United States, Paul U. Kellogg and associates' *The Pittsburgh Survey*, published in six volumes in 1909-14, "revealed to that community and to the nation at large the dangers to workers and citizens inherent in a community of rapid and uncontrolled industrial expansion" (Klein, 1938:xi). Its penetrating generalizations about city life are similarly neglected by sociologists, to their loss.

C. J. Galpin's 1915 publication, *The Social Anatomy of an Agricultural Community*, based on studies at the University of Wisconsin, is one of a number of important clinical contributions of rural life ordinarily ignored by the typically urban-minded sociologists.

W. I. Thomas and Florian Znaniecki attracted more attention with their five-volume clinical study of *The Polish Peasant in Europe and America* published in 1918-20. Later, the popular acceptance achieved by Robert S. and Helen M. Lynd's *Middletown* in 1929 and *Middletown in Transition* in 1937 helped to convince more sociologists that such observational reports and analyses could provide more dependable knowledge than philosophical disputes and mechanized surveys.

The abstract and/or scientific establishment in sociology was far from being entirely academic. Many of its members have always had a strong commercial orientation. Especially beginning in the 1920s, the increasingly organized public relations concerns of financiers and industrialists resulted in support for research projects in sociology and social psychology by foundations, advertising agencies, and public relations firms. During the depression of the 1930s, commercially-minded social scientists turned away from individual research with a

welfare or reformist or just academic emphasis and toward so-called "rigorous empirical research" carried on under "provision of large-scale research by staff" and aided by "graduate study linked to ongoing research programs," to quote the social science historian Martin Bulmer (1982:191).

Bulmer rejects the idea that these tendencies imply "principally a reflection of the class interests of philanthropists" or that "foundation officials simply molded American social science in their own image." How protective of our dignity Bulmer apparently tries to be! What other class interests have been and are served by typical foundation grants or contracts? How do foundation officials manage to select recipients who do not share their aims and values—if they do?

Beginning just fifty years ago in 1936, the first of four organizations came into existence with which social psychologists and sociologists sought to rehumanize their disciplines. A group of controversial idealists, led by such people as David Krech and Goodwin Watson, organized the Society for the Psychological Study of Social Issues (SPSSI). Writing in 1937, Watson (1937:26) called "our SPSSI one manifestation of a more general determination of our ablest social scientists to be participant observers at the most strategic point of reconstruction." Many of us who were sociological social psychologists felt the lack of such organization and became active in SPSSI.

The three other organizations that have related goals are the Society for the Study of Social Problems (SSSP), brought together in 1950–51, the Association for Humanist Sociology (AHS), dating from 1975–76, and the Sociological Practice Association (SPA), which began in 1978 as the Clinical Sociology Association. These organizations do not compete; they are complimentary and enjoy friendly working relations. Through these groups a great deal is being done to keep sociology relevant and vital in today's problem-racked society.

SSSP focuses on the realities of the passing scene plus their origins and possible consequences. AHS denies the possibility of so-called value-free research and analysis and advocates a commitment to human values. The SPA brings together those who are taking humanistic sociology into a variety of workplaces.

New social wisdom will come out of combining humanistic intervention and science. These associations are meeting the challenge identified by Nelson Foote (1974:128): "The best management consultants and the best organizational theorists ought really to be almost indistinguishable. Yet at present it is as if they inhabit two different worlds, or at least speak two different languages. And organization theory is only one example of the present gulf." As John Glass and Jan Fritz (1982:5) have pointed out, SPA is defining "problem areas where sociological skills and knowledge can be utilized." We can expect, as Glass and Fritz have anticipated, "the redefining of sociology to include recognition and acceptance of an interventionist role and a revitalization of the whole field."

Notes

1. Annoying, but not instructive, to the established have been the significant contributions of such controversial people as Karl Marx, Charles Booth, W. G. Sumner, Jane Addams, Jerome Davis, Mary E. Richmond, Harry Elmer Barnes, W. I. Thomas and C. Wright Mills.
2. Ward's optimism about the human lot was contagious but he lacked contact with social realities. He became a professor of sociology at Brown University and his artificializing influence has continued to affect the discipline.

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The History of Clinical Sociology

Jan M. Fritz

The origins of sociology are found in many times and places. Sociologists typically write that their field developed in Western Europe during the mid-1800s. They mention the early sociologists' interest in understanding society and making the world better and then they cite the same names—Auguste Comte, Emile Durkheim and Max Weber. Most mention the contribution of Karl Marx although the amount and kind of coverage generally clearly indicates the sociologist's (usually unstated) theoretical view. Quickly, then, a sociologist moves on to a rather lengthy discussion of whether sociology is a science. The conclusion is always in the affirmative.

There are other histories, however. These views of the field are not yet researched very thoroughly or so widely known but they take nothing away from the view of sociology as a science. Instead, they add to this picture by showing there are other threads running through the general history of the discipline. The threads to be discussed here are humanistic, multidisciplinary and clinical and emphasize some of the contributions of women and black clinical sociologists.¹

The Roots of Clinical Sociology

Clinical sociologists create systems and intervene in existing ones to assist with assessment and change. Clinical sociologists are scientists who are humanistic and multidisciplinary in approach. They engage in planned social change efforts by focusing on one system level (e.g., interpersonal, community, international) but integrate levels of focus in their work and do so from a sociological frame of reference. Clinical sociologists may be involved in sociological practice in a variety of ways including teaching and action research.

The history of this broad field begins with individuals who combine a scientific approach to social life with an involvement in intervention work. We

This article is a revised version of "Making Tracks: The History of Clinical Sociology" which appeared in *The Clinical Sociology Handbook* (New York: Garland, 1985.) Copyright 1988 Jan Fritz.

begin here, as Alfred McClung Lee (1979:487) has done, with the Arab historian and statesperson *Abd-al-Rahman ibn Khaldun* (1332–1406).

Ibn Khaldun has been described as a “thinker and doer” (Rosenthal, 1958:lxvi). In his *Muqaddimah*, he provided numerous clinical observations based on his various work experiences. In addition to being a scholar and professor, Ibn Khaldun also was Secretary of State to the ruler of Morocco, Prime Minister and a statesperson who headed political missions. As Chief Judge of Egypt, he was known as a reformer.

It has been said (Rosenthal, 1958:lxvii) that many of the ideas discussed in the European West long after Ibn Khaldun’s death were known “in their rudiments at least, to (Ibn Khaldun), the northwest African of the fourteenth century who founded a ‘new science’ in his *Muqaddimah*.” Ibn Khaldun has been mentioned (see Schimmel, 1951:xvii) as the forerunner of many Western scholars—including Machiavelli, Vico, Montesquieu, Condorcet, Tarde and Comte.

The history of sociology often begins with *Auguste Comte* (1798–1857), the French scholar who coined the term “sociology.” Comte’s life began in turbulent times; he was raised in the aftermath of the French and Industrial Revolutions. Comte, like the other founders of sociology, grappled with the problem of how to change the society to meet the demands of the Industrial Age. As he strongly believed that the scientific study of societies would provide the basis for social action, we certainly would want to include him in a history of clinical sociology.

So too would we include *Emile Durkheim* (1858–1917) and *Karl Marx* (1818–1883.) Durkheim’s groundbreaking work on the relation between levels of influence, e.g., social compared to individual factors, led Alvin Gouldner (1965:19) to say that “more than any other classical sociologist (he) used a clinical model.” Marx’s work is based on archival research but his writing came alive with a “grasp of human affairs only possible through extensive involvement in praxis, in social action, in agitation and in social organization.” Along with Engels, Marx’s work affected conservative as well as revolutionary thinking (Lee, 1979:488) and his theory is basic to the work of many practitioners.

Early American Sociology

American sociology developed in the late 1800s² as a response to the industrialization and urbanization of the post-Civil War era. The courses that emerged—such as pauperism, charity, unemployment, migratory labor, child labor, women wage-earners, insanity, illness, crime, temperance and race relations—focused on social problems.

Many of the well-known sociologists prior to 1920 came from religious and rural backgrounds or had studied in divinity schools; they were concerned with

ethical issues and social reform.³ At the University of Chicago, the prominent center of sociological thought, most social thinkers rejected Herbert Spencer's "laissez-faire attitude" toward social development (Rosenberg, 1982:36-7.) Most of the sociologists had read Auguste Comte's work and "followed Comte's view of progress as susceptible of acceleration by purposive, rational intervention in society (Hinkle and Hinkle, 1954:7.)

At the University of Chicago in 1896, *Albion Small* (1854-1926), Chair of the Graduate Department of Sociology, founding Editor of *The American Journal of Sociology* and one of the first Presidents of the American Sociological Society (1912-13), published his article "Scholarship and Social Agitation." Small thought the primary reason for the existence of sociology was its "practical application to the improvement of social life" (Timasheff and Theodorson, 1976:2). The following passage from Small's (1896:564) article shows his interest in sociological practice:

Let us go about our business with the understanding that within the scope of scholarship there is first science, and second something better than science. That something better is first prevision by means of science, and second intelligent direction of endeavor to realize visions.

I would have American scholars, especially in the social sciences, declare their independence of do-nothing traditions. I would have them repeal the law of custom which bars marriage of thought with action. I would have them become more profoundly and sympathetically scholarly by enriching the wisdom which comes from knowing with the larger wisdom which comes from doing.

Small (1896:581-2) thought it was a "betrayal of . . . social trust . . . for the sociological scholar to withdraw from affairs, and attempt to grow wise by rearranging the contents of (one's own) personal consciousness." He said he had found that "action not speculation was the supreme teacher."

According to Small (1896:582), every sociologist should be involved in two kinds of "concrete work:"

work which the thoughtful and careful prosecute for the benefit of the thoughtless and careless . . . (and) work which the enterprising and efficient organize for the better security of their own social interests.

Small (1896:582) thought this concrete work should be a central interest for a professional social science organization. As he noted, sometimes the intent did not match the reality:

(I refer) to the career of a certain reputable society of which many teachers of the social sciences are members. The declared object of the association is commendable, viz., the improvement of city governments in the United States. The programme into which the society has gravitated is discussion rather than action. Its accomplishments up to date very naturally amount to ocular proof of the futility of talk. A scientific label for this respectable body would read: A National Association for the Propagation and Enjoyment of Melancholy over the Misdoings of the Municipalities.

The first of five editions of the *Outline of Practical Sociology* by sociologist Carroll Wright (1840–1909) appeared in 1898. Wright was a member of the Massachusetts senate, a U.S. Commissioner of Labor and President of Clark College. Wright chaired the Presidential Commission appointed in 1894 to investigate the Pullman strike in Chicago and was a member of a similar commission appointed by President Roosevelt to investigate and then arbitrate the anthracite coal strike of 1902.

Wright (1899) wrote that “practical” sociology deals with actual, pressing social questions.” He went on to say that the sociologist:

may advocate reforms, he may insist upon changes in legislation, upon the adoption of new systems of finance or commerce, but he does all this because to his mind the ascertained facts lead to his conclusions.

While Wright stressed the role of the scientist throughout his book, he didn’t preclude roles in government or private enterprise and that is particularly evident when one looks at Wright’s own career in government and educational administration.

In 1906, Lester Ward (1841–1913), the first President of the American Sociological Society, published *Applied Sociology: A Treatise on the Conscious Improvement of Society by Society*. In his book, Ward (1906:8) clearly indicated that applied science is not the same as art . . . because “if it is art it is not science.” Ward (1906:8) stresses the importance of field work but only for the practice of applying “principles directly to nature.” He said this “is almost always done in miniature, or on a small scale, for practice only, and without expectation of any practical result.” Again he brought home his point (1906:9–10):

Applied sociology is not government or politics, nor civic or social reform. It does not itself apply sociological principles; it seeks only to show how they may be applied. It is a science not an art.⁴

But there were sociologists in Europe and within the American Chicago school—or, more accurately, the Chicago network—who combined science and art. They were concerned with social problems, they used their skills as scientists to collect and analyze pertinent information and they developed the skills, a combination of science and art, that were needed to practice as clinical sociologists.

Clinical Sociologists at the Turn of the Century

In England, *Beatrice Webb* (1858–1943) and her husband Sidney Webb were working as activist social scientists. Beatrice Webb's education and work experiences clearly qualify her as a clinical sociologist.

Beatrice Webb's comfortable family status had been such that eminent visitors frequently were at her childhood home. Among the guests—Herbert Spencer. As Beatrice was given little formal schooling, she was taught primarily through her own interests and by Spencer. As a result she "learned no mathematics but read a great deal of stiff and serious work." Like Spencer, and primarily because of him, she developed "a passion for ascertaining facts and discovering their relevance to theories of society and of human and animal behavior" (Cole, 1946:13–14).

Beatrice Webb worked as a social investigator for several years with Charles Booth. The conservative Booth was an "amateur" social scientist, a ship-owner and businessperson who became skilled in the scientific study of social conditions. Booth "wanted to give some definite quantitative meaning to the term 'starving millions'" (Bulmer, 1982:11) and did so through his 20 years of research on poverty and work in England. Booth, who introduced the idea of a "poverty line," published his lengthy studies in seventeen volumes between 1899 and 1903.

Beatrice Webb and Sidney Webb "had formidable influence upon twentieth-century British social policy" in part because of their historical analysis of policy but primarily because of their work "as politically engaged social scientists, institutional innovators, members of (official) committees and (in Sidney's case . . .) as politician and minister." They were founders of the Fabian Society and instrumental in founding the London School of Economics and Political Science. Beatrice Webb and Sidney Webb "blended social science and political action" (Bulmer, 1982:17,21).

Beatrice Webb learned her sociology through Spencer, Booth and her independent study; she was not formally trained as a sociologist. However, it is very difficult to talk about formal training or active employment as a sociologist during her formative years. According to Bulmer (1982:22), "those who did research typically worked in a non-academic setting, often doing research in their spare time and with their own money." There were no positions labeled

“social scientist,” sociology was not taught within the university and no one used the label “sociologist.”

In the United States, the Chicago network was developing during the late 1880s. Included among its members were faculty at the University of Chicago such as George Herbert Mead, W. I. Thomas and Marion Talbot. Also central to this network were activist-scholars like Jane Addams.

George Herbert Mead (1863–1931) and *W. I. Thomas* (1863–1947), teachers at the University of Chicago, are part of the history of clinical sociology. Mead, a pioneer of the symbolic interactionist approach, joined the Department of Philosophy at the University of Chicago in 1894 and remained there until his death in 1931.

According to Deegan and Burger (1978:362), Mead’s writings and his work on social reform issues generally are not well known. For instance, scholars usually don’t mention that in 1910, when 40,000 garment workers in Chicago went on strike, Mead headed a citizen’s committee investigating conditions and workers’ grievances. Mead, working with others, was able to bring the workers’ interests to arbitration.

Mead was a supporter of women’s equality. He spoke at a suffrage meeting in 1912 and around 1918 he marched for women’s suffrage along with John Dewey, Jane Addams and other prominent Chicago citizens. In 1920 he was President of the Chicago City Club and took part in the civic organization’s committees which were attempting to eliminate corruption in the city.

W. I. Thomas received one of the first doctorates awarded by the University of Chicago and taught there in the Department of Sociology until 1918. Thomas, a President of the American Sociological Society, was a major influence on American sociology and well known as the co-author of *The Polish Peasant in Europe and America* (Thomas and Znaniecki, 1951).

Thomas wrote about the need for applying social science to daily life in his “Methodological Note” in *The Polish Peasants*. This “concern with the practical aims of science is found in most of Thomas’ writings (though) his (work) on social reform and his active participation in the progressive movement have been ignored” (Deegan and Burger, 1981:116,114).

Thomas had close personal and professional ties to Jane Addams and her colleagues at Hull House. He lectured there and his work on juvenile delinquency and on Polish peasants was due, in large part, to his connections with Hull House (Deegan, 1987).

Thomas was a member of the Chicago Vice Commission and, along with George Herbert Mead and others, he participated in the Rudowitz Conference to affirm the idea of political asylum. As Deegan and Burger (1981:122) concluded, Thomas “was committed to improving society and acted on his concern.”

In 1892 *Marion Talbot* (1858–1948) left Boston to become Dean of Women

for the Colleges and Assistant Professor in the Sociology Department⁵ at a new school—the University of Chicago. Working with her mother, Emily Talbot, and a few other women, she already had helped found the Association of Collegiate Alumnae (ACA), the forerunner of the American Association of University Women, to encourage women to go to college and to open opportunities for women graduates (Storr, 1971:423).

Talbot was promoted to Dean of Women in 1899 and in 1905 was appointed full professor in the new university Department of Household Administration. She held these positions until her retirement in 1925. During her tenure at the University of Chicago, Talbot also was, for over twenty years, an Associate Editor of *The American Journal of Sociology*.

Talbot became a central figure in Chicago's growing community of scholars and activists. She directed many students to work in Chicago's urban laboratory—Hull House—and through her the women in the university and the scholar-practitioners working in the reform movement maintained close contact. According to Rosalind Rosenberg (1982:34), Talbot became "a kind of chief of employment for Chicago's women students and academic dean for Chicago's reformers."

In 1889, three years before the Department of Sociology was founded at the University of Chicago, sociologist *Jane Addams* (1860–1935) and her good friend Ellen Starr established a settlement house in the decaying Hull Mansion in Chicago. Hull House had many aims not the least of which was to allow privileged, educated young people contact with the real life of the majority of the population. The core of Hull House members were well educated women who were bound together by their involvements such as the labor movement, the National Consumers League and the suffrage movement (Fish, 1981:30–36). During the next 45 years Jane Addams would travel widely but "Hull House remained her home and the reflection of her thought and personality" (Scott, 1971:22).

During the founding years of sociology in the United States from 1892–1920, Jane Addams was the "foremost female sociologist."⁶ She headed a network of women, including clinical sociologists Sophonisba Breckinridge and Edith Abbott, working in reform activities and influenced "all of the men in the Department of Sociology at the University of Chicago as well as John Dewey, George Herbert Mead and the other American pragmatists" (Deegan, 1981:18.)

Jane Addams' involvement in the major issues of the city of Chicago (e.g., factory inspection, child labor laws, improvements in welfare procedures, recognition of labor unions, compulsory school attendance and her work as an arbitrator in labor disputes) catapulted her to national prominence. Intellectuals from around the world, including Beatrice and Sidney Webb, came to Chicago to meet her and her colleagues.

Jane Addams considered herself a sociologist (Deegan, 1981:19) and has

been referred to as "a virtual adjunct professor in sociology at Chicago." In documenting the relationship between the university and the settlement house, Rosenberg (1982:32-4) has written:

Most of the Chicago social scientists participated in some way in the work of Hull House, leading seminars, giving lectures or just having dinner with the exciting group of people who always gathered there . . . Hull House became a laboratory for sociologists, psychologists, and economists, who helped to transform it from a home for moral uplifting of impoverished immigrants to a center for systematic social investigation and an agency of political and economic reform.

In 1895 *Hull House Maps and Papers* was published. This pioneer study dealt with tenement conditions, sweatshops and child labor. It was the "first systematic attempt to describe the immigrant communities in an American city" and it was patterned in some ways after Charles Booth's 1899 publication *Life and Labour of the People of London* (Fish, 1981a:28-29).

Addams was definitely an organization development specialist. Within five years of the establishment of Hull House, some forty clubs were based there,⁷ eleven kinds of community activities were connected with the settlement and over 2,000 people came into the facility each week. Hull House (Addams, 1893), for example, hosted meetings of four women's unions, offered social clubs to immigrants, held economic conferences to bring together businessmen and workers and ran a coffee house. The Working People's Social Science Club held weekly meetings there beginning in 1890 and the College Extension course, as it was known, offered courses and lectures in the evenings to two hundred neighborhood residents. Two university extension courses were held there in connection with the University of Chicago and the Chicago Public Library established a branch reading room in Hull House.

Addams was a well-known lecturer and her articles, on a variety of important topics, were widely read. Her most successful book was her moving autobiography, *Twenty Years at Hull House* (1910.) Addams years of work and writing in the interest of peace earned her the Nobel Peace Prize in 1931.⁸ This amazing woman's central role in founding American sociology is documented in Mary Jo Deegan's (1987) book on Jane Addams and the men of the Chicago School. There are, unfortunately, a number of reasons why Addams has not been remembered as a sociologist. Emily Balch (1935:200) mentioned one such reason in a tribute written shortly after Addams died—"I think her greatness has been veiled by her goodness."

There were also American scholar-practitioners operating outside of the Chicago network who are an important part of the history of clinical sociology.

Among them—Emily Greene Balch, Jessie Taft and W. E. B. Du Bois. They were all affected in some way by the activities and interests of individuals in the Chicago network.

Clinical sociologist *Emily Balch* (1867–1961) is one of two American women who have been awarded the Nobel Peace Prize. She received the award in 1946. The head of the Nobel Committee introduced her that year by saying that her name was probably unfamiliar to many in the audience and that there were probably few in Europe who knew her. Unfortunately, she also is generally unknown today among sociologists.

Balch wrote over 100 articles on labor, social settlements and women as well as a number of books. Her *Public Assistance of the Poor in France* (1893) is one of the “earliest sociological studies of care for the poor and disabled” and her *Our Slavic Fellow Citizen* (1910) is the “first major sociological book on immigration” (Deegan, 1983:102, 104).

Balch was not part of the Chicago network but there are connections. Balch’s close friendship with Jane Addams began when both attended the 1892 Summer School of Applied Ethics, held in Massachusetts. Balch later studied for one quarter at the University of Chicago and, while there, visited Hull House.

Balch was a member of the Wellesley faculty from 1897 until 1918 and was the second Chair of the Department of Economics and Sociology. In 1892 Balch had been one of the founders of Boston’s Dennison House, one of the first settlement houses. While at Wellesley she became a charter member of the College Settlement Association, a group organizing settlement houses across the United States.

In 1915 Balch and Addams were delegates to the International Congress of Women at the Hague. Gray (1976:201) has described Balch’s prominent role at the Congress:

founding . . . the Women’s International Committee for Permanent Peace, later named the Women’s International League for Peace and Freedom; preparing peace proposals for consideration by the belligerent nations; and serving on a delegation to Russian and Scandinavian countries to urge their governments to initiate mediation offers.

From 1915 until her death in 1961, Balch’s primary concern was her work for international peace. After returning from the 1915 Congress, “she campaigned actively against America’s entry into the war . . . worked on the liberal weekly, *The Nation* . . . and wrote a successful pacifist book, *Approaches to the Great Settlement* (Gray, 1976:201).

In 1919, when Addams became President of the Women’s International League for Peace and Freedom, Balch became International Secretary-Treasurer.

Balch worked closely with the League of Nations on many projects—such as disarmament and drug control—and with its successor, the United Nations. In reviewing Addams' and Balch's lives, Deegan (1983:107) has written that they stand "as heroic standards far outdistancing the achievement of other early, American sociologists."

Jessie Taft (1882–1960) received a Ph.D. in philosophy from the University of Chicago in 1931. She, like her advisor George Herbert Mead, was interested in psychology and sociology and had begun her work in Chicago with sociologists W. I. Thomas.

Taft held a variety of positions before joining, in 1934, the faculty of the Pennsylvania School of Social Work (which later became the University of Pennsylvania School of Social Work.) She was Assistant Superintendent of the New York State Reformatory for Women in 1913 and then Director of the Social Services Department of the New York State Charities Aid Association's Mental Hygiene Committee.

In 1918 Taft moved to Philadelphia as Director of the new Department of Child Study at the Seybert Institution, a shelter for children awaiting placement. While connected with Seybert, she became well known as a therapist and mental hygiene consultant.

In 1924 Jessie Taft met psychologist Otto Rank, who, like Mead, became a major influence on her work. Taft is known for her functional casework, an approach which places the client at the center of a growth process which is fostered by a therapist. In her writing, Taft "combined the concepts of G. H. Mead and Otto Rank into a powerful theoretical framework for interpreting problems in daily living" (Deegan, 1986:35).

There has been very little examination of Taft's theoretical work on the part of sociologists and yet Taft's writings certainly provide a theoretical basis for the work of many clinical sociologists, particularly those involved in counseling and therapy or who undertake role analysis in other settings.

When *W. E. B. Du Bois* (1868–1963) was at Harvard, sociology was not a separate discipline. He received a Ph.D. in history but had taken many courses in the social sciences. Du Bois credited his Harvard advisor, Albert Bushnell Hart, for directing him "to the social sciences as the field for gathering and interpreting that body of fact which would apply to my program for the Negro" (Du Bois, 1968:148). In reviewing his own background, Du Bois has written that his "course of study would have been called sociology" (Du Bois, 1940:39), and he is considered one of the pioneers of clinical sociology.

In a ten-year period from 1895–1905, his book, *The Philadelphia Negro*, the subsequent Atlanta University publications and his study of rural Negroes in Farmville, Virginia, provided the first reliable information about Negroes in America based on empirical sociological research" (Broderick, 1974:3).

Du Bois was familiar with Booth's (1899–1903) study of poverty and work

in London and with *Hull House Maps and Papers* (Addams, 1895.) Du Bois and his sponsor, reformers in the Philadelphia settlement house movement, wanted a similar empirical study to document the situation of Philadelphia's Negroes. In addition to the expected empirical work, Du Bois gave specific suggestions for the advancement of Blacks. As Rudwick (1974:28) has said, "Du Bois enthusiastically played the dual role of social scientist and social reformer."

Like many of the early women sociologists in the Chicago network, Du Bois' work was given little attention by the white, male sociology establishment. He had established a successful research base at Atlanta University where he published monographs and held annual conferences to discuss the relevance of the Atlanta University papers to the advancement of the Negro. He was disappointed that he was unable to develop connections for his research base with the eminent, established universities. Du Bois became very discouraged with his primary work as social scientist/teacher and, as lynchings "called—shrieked—for action," (Du Bois, 1920:21–2) he left the academic world.

Du Bois was a founder of the National Association for the Advancement of Colored People (NAACP) and became an internationally known spokesperson as the Editor, from 1910–1934, of the NAACP publication, *The Crisis*.

The Appearance of the Label "Clinical Sociology"

A discussion of "clinical sociology" or the "clinical" approach appeared in the literature at least every few years between 1931 and 1969. The term "clinical sociology" generally has been used to refer to sociologists doing intervention work in a variety of settings.

The first linking of the words "clinical" and "sociology" in an important journal occurred in 1931. Louis Wirth's (1897–1952) article "Clinical Sociology" appeared in *The American Journal of Sociology*, the most prestigious sociology journal of its day. Wirth, writing about sociologists working in child guidance clinics, made a strong case for the role "sociologists can and did play in the study, diagnosis and treatment of personality disorders because of their expertise about the varying effects of socio-cultural influences on behavior." Wirth thought that roles of practitioners and researchers were "equally valid and envisioned that both researchers and practitioners would benefit from the emergence of clinical sociology" (Glass and Fritz, 1982:3.)

In 1934, Saul Alinsky, a staff sociologist and member of the classification board of the Illinois State Penitentiary, published his article, "A Sociological Technique in Clinical Criminology" in the *Proceedings of the Sixty-Fourth Annual Congress of the American Prison Association*. Here he discussed an interviewing technique that he developed for working with prison inmates. Alinsky, a clinical sociologist, became well known in the 1960s for his work

in community organizing. His early work in corrections "led to a focus on community as the unit for investigating crime and on community organizations as a means of crime prevention" (Reitzes and Reitzes, 1982:48.)

In 1941 Walter Webster Argow's article "The Practical Application of Sociology" appeared in the *American Sociological Review*. Argow (1941:38) noted that Giddings, Wirth and Fairchild had offered "a program of an 'applied' or 'clinical' sociology."

In 1944 the first formal definition of clinical sociology appeared in H. P. Fairchild's *Dictionary of Sociology*. Alfred McClung Lee (1944:303), known as one of the founders of the Society for the Study of Social Problems, the Association for Humanist Sociology and the Sociological Practice Association, defined the term as follows:

sociology, clinical. That division of practical or applied sociology that reports and synthesizes the experiences of (a) social psychiatrists with functional problems of individual adaptation and (b) societal technicians with functional problems of institutional adjustment. Chiefly in the first group, at least in emphasis, is the experience of social workers, personnel managers, psychiatrists, career guidance experts, etc., and chiefly in the second group is that of public relations counselors, professional politicians, sentiment and opinion analysts, propagandists, advertisers, etc.. Clinical sociology thus stresses the development of effective manipulative and therapeutic techniques and of accurate functional information concerning society and social relationships.

In the following years, Lee used the word "clinical" in the title of two articles—his 1945 "Analysis of Propaganda: A Clinical Summary" and his 1955 "The Clinical Study of Society."

Also appearing in 1944 was an article in *Sociology and Social Research* called "An Approach to Clinical Sociology." The author, Edward C. McDonagh (1944:382), knew Lee's definition of clinical sociology but had not read Wirth's 1931 article.⁹ McDonagh (1944:379–80) proposed that sociology departments establish social research clinics. He thought the clinics should be "composed of representatives from the social sciences with a person trained in sociology serving as director." Among the topics McDonagh thought the clinic might deal with:

regional housing standards and conditions, probable post-war employment, juvenile delinquency and health indices . . . (concerns of) draft boards . . . (and) constructive public works.

McDonagh (1944:376–7) said “the clinical approach as a means of sociological research is essentially a group way of studying and solving problems.” After mentioning the “intellectual eclecticism” of sociology, McDonagh said he was puzzled as to why sociology had “not adopted and incorporated the advantages of clinical thinking.”

In 1946 George Edmund Haynes’ “Clinical Methods in Interracial and Intercultural Relations” appeared in *The Journal of Educational Sociology*¹⁰ Haynes, the first black to receive a Ph.D. from Columbia University, was a co-founder of the National Urban League (1910) and the first black to hold a sub-cabinet post (Director of the Bureau of Negro Economics, U.S. Department of Labor, 1918–21). His 1946 article was written while he was Executive Secretary of the Department of Race Relations of the Federal Council of the Churches of Christ in America and discusses the Department’s urban clinics which were set up to deal with interracial tensions and conflicts by developing limited, concrete programs of action.

The July, 1949 issue of the journal *Philosophy of Science* included a symposium on applied social research in policy formation. E. A. Shils (1949:225), in his article “Social Science and Social Policy,” briefly mentioned that some social scientists were policy-makers although his examples were economists with the exception of one political scientist.

The symposium also included an article by David Ulrich (1949:247) entitled “A Clinical Method in Applied Social Science.” Robert Merton (1949:163), in the lead symposium article, had said that “all applied social science involves advice (recommendations for policy).” Ulrich responded that “advice-giving” may, at times, be an “inadequate frame of reference for applied social science.” He suggested a “combined research-consulting operation of seeking out management and employee interests and stimulating their participation in the development of a plan which will fit their needs and which they can regard as their own.” Ulrich said this practical consulting would be useful to an organization “whether it be business, government or some other form.”

In 1956 Alvin Gouldner’s “Explorations in Applied Social Science” appeared in *Social Problems*.¹¹ In this paper he examined the differences between engineering and clinical sociology. Gouldner was interested in the development of a clinical sociology in which clinicians made “their own independent identification of (a) group’s problems.” The clinician also wouldn’t take the client’s values as given and would work with the client in re-examining values in light of the client’s problems.

In 1957 Marie Kargman’s “The Clinical Use of Social System Theory in Marriage Counseling” appeared in the August issue of the journal *Marriage and Family Living*.¹² Kargman stated that marriage counseling was practiced by individuals in many disciplines—including sociology. She used a case presentation

and discussion to show the "effective clinical use of social system theory for marriage counseling."

In December 1957 James Schellenberg discussed clinical sociology in his article "Divisions of General Sociology" in the *American Sociological Review*. According to Schellenberg (1957:661), "clinical or concrete sociology deals . . . with a total situation within restricted limits of time and space." He said that the term clinical meant "a general and diagnostic mode of analysis" which "does not necessarily imply . . . solving social problems." Schellenberg thought that clinical or concrete sociology was one of three divisions of the subject matter of sociology. The other two were (1) historical and cultural sociology and (2) logico-experimental sociology.

In 1963 James Taylor and William Catton, Jr. published "Problems of Interpretation in Clinical Sociology" in *Sociological Inquiry*. The authors concerned themselves with the issues confronting the clinical sociologist who works as a consultant to organizations. Taylor and Catton (1963:44) noted that clinical sociology "has not as yet a crystallized set of occupational norms" but went on to advocate a consultant role as part of the "role repertoire of the sociologist."

Also appearing in 1963 was another article on work in clinical sociology. This piece, published in a French journal, discussed contracts, ethics, the object and methodology of socioanalytic art and the norms of that art (van Bockstaele, van Bockstaele, Barrots and Magny, 1963).

In 1964 Marshall Clinard's book *Anomie and Deviant Behavior* appeared. H. Warren Dunham (1964) had written a chapter on anomie and mental disorder for that book and one section of that chapter was "Clinical Sociology and Personality Vulnerability." In this section Dunham said that to develop adequate explanations of deviancy, we must develop the field of clinical sociology. Dunham (1964:155) also mentioned that the field of clinical sociology had "been most startlingly neglected during the past two decades."

In 1965 Frederick Lighthall and Richard Diedrich published "The Social Psychologist, the Teacher, and Research" in *Psychology in the Schools*. The authors discussed the school psychologist's research as an example of "what can only be called clinical sociology."

In 1966 Julia Mayo's "What is the 'Social' in Social Psychiatry?" appeared in the *Archives of General Psychiatry*. The final section of her article was on the transition from psychiatric caseworker to clinical sociologist. Mayo believed that the social work practitioner no longer had restricted functioning but had developed into a clinical sociologist having distinctive diagnostic skills.

Finally, *Patterns in Human Interaction: An Introduction to Clinical Sociology* written by Henry Lennard and Arnold Bernstein, was published in 1969. The book was about how social contexts influence social behavior. In the

introduction, "Clinical Sociology: A New Focus," Lennard and Bernstein (1969:3) stated that their "application of research methodology and sociology theory to the data of the 'clinical' situation and to subject matter traditionally falling within the fields of psychiatry and clinical psychology seemed to us to deserve a new characterization, to which the term clinical sociology seems ideally suited.

Heightened Interest in Clinical Sociology

Presentations about the field of clinical sociology—labeled as such—began to appear at professional sociology meetings during the early 1970s. By the late 1970s, presentations and training sessions, as well as publications appeared with some regularity. These publications began to document the earliest contributions in the field as well as encourage contemporary work.

In 1978 Hugh Gardner published an article about clinical sociology in the magazine *Human Behavior* and in 1979 a special issue of the *American Behavioral Scientist* (Straus, 1979) was devoted to clinical sociology. The book *Clinical Sociology*, by Barry Glassner and Jonathan Freedman (1979), also was published that year. Numerous articles were now appearing including ones by Charlotte Schwartz (1978) on teaching, Billy Franklin (1979) on the history of the field, Estelle Disch (1979) on sociological psychotherapy, Alex Swan (1980) on the emergence of the field, Drukker and VerHaaren (1980) on consulting and Black and Enos (1980) on counseling.

This activity was spurred on in large part by the establishment of the Clinical Sociology Association (now the Sociological Practice Association) in 1978. Clinicians now belonged to a network of sociological practitioners and the organization began to develop forums and publication projects for its members. In 1982 the Association published the first issue of its annual journal, the *Clinical Sociology Review*, and in 1985 sponsored the volume *Using Sociology: An Introduction from the Clinical Perspective*, edited by Roger Straus. In cooperation with the American Sociological Association, two volumes (Fritz and Clark, 1986; Clark and Fritz, 1984) were published on courses and programs in clinical sociology.

The interests and activities of Sociological Practice Association members, acting individually or on behalf of the Association, have been major factors in the development and acceptance of publications about clinical sociology. Iowa State University Press, for example, published Harry Cohen's (1981) book on theory and clinical sociology, Schenkman published Alex Swan's *The Practice of Clinical Sociology and Socioterapy* in 1984 and in 1985 Garland published Fritz's *The Clinical Sociology Handbook*. The journal *Free Inquiry in Creative Sociology* sponsored a series of articles on clinical sociology and in 1987 the

American Sociological Association announced plans to establish a sociological practice journal which should cover both clinical and applied sociology.

Conclusion

It has taken a long time for the history of clinical sociology to begin to be pieced together. The reasons for this include the following:

- The early work often is not identified as “clinical sociology” and so it’s difficult for contemporary writers to locate that information and analyze it.
- Some chroniclers and reviewers consciously or unconsciously have rejected information about the intervention role of sociologists even when the information was provided.
- Information about clinical activities often was published in places not usually read or catalogued by contemporary sociologists.
- The earliest American clinical sociologists didn’t publish much about how they may have integrated practice with teaching.

Numerous examples might be given of clinical work that generally has been overlooked. In addition to the earliest sociologists mentioned throughout this paper, I would point to some of the early work of William Foote Whyte and the contributions of Charles Gomillion. Neither of these men identified their work as “clinical.”

William Foote Whyte wrote a little known article called “Solving the Hotel’s Human Problems” which appeared in a 1947 issue of *The Human Monthly*.¹³ Here Whyte described his work as a consultant both to the staff members in human relations research at a Minneapolis hotel and to the hotel’s executives who were working on change initiatives. The editor of *The Hotel Monthly* (1947:37) indicated at the time Whyte’s article appeared “that the policies and practices instituted through the human relations activities headed by Professor Whyte (are) largely responsible for reducing labor turnover by 66%.” The editor said this was an “impressive demonstration of the value of the work.”

Another clinical sociologist whose work generally has been neglected is Charles Gomillion.¹⁴ Gomillion was an educator community-activist affiliated with Tuskegee Institute who “organized the Tuskegee Civic Association in 1947 and launched a program of political activism in the town and surrounding rural areas” (Hunter and Abraham, 1987:xxv.) In 1960 the *Gomillion v. Lightfoot* case came before the U.S. Supreme Court. Gomillion’s successful suit stopped the local gerrymandering which had excluded all but about ten blacks from voting in town elections (Smith and Killian, 1974:205; Gomillion, 1987.)

We should make the effort to learn the history of our field. As part of this initiative, we need to identify those women and men who have been scholar-practitioners in economically developed as well as developing countries and research thoroughly their contributions. If we do this, we will have a more accurate picture of the history of the entire field of sociology and be in a better position to discuss some of the historical currents that have encouraged sociologists to value clinical sociology at some periods while tolerating or denying it in others.¹⁵

This is a period of sustained interest in clinical sociology. During this time we will continue to write the field's history and discuss the strengths of and barriers to sociological practice. If national and international events encourage the development of the field and if a strong organizational structure for the field can be put in place, clinical sociology will blossom.¹⁶ At that point this humanistic, multidisciplinary field—so much a part of the history of the discipline—finally will be recognized as a mainstream area of professional competence and as an important consideration in projects involving planned social change.

Notes

1. Little has been written about the history of the field of clinical sociology. Like histories of the general field of sociology, what has been written does not do an adequate job of covering the contributions of women and people of color. Also like histories of the general field, this history does not adequately reflect contributions of those outside of Western Europe and the United States.

2. It is difficult to establish an exact date for the beginning of sociology in the United States. We could start, for instance, with the 1880s when publications on sociology first appeared, with the 1890s when sociology courses were given in academic institutions or in 1905 with the establishment of the American Sociological Society.

3. According to Diner (1980:199), eleven of the fifteen members of the University of Chicago's Sociology and Anthropology Department, over 73%, were involved in reform activity. Diner was assessing faculty involvement for the years 1892 through 1919.

4. This point also is central to Robert MacIver's 1931 volume *The Contribution of Sociology to Social Work*. He sees sociology as science and social work as art.

5. Talbot (1936:4) described her position as Assistant Professor of Sanitary Science while Rosenberg (1982) says she was Assistant Professor of Sociology. Departments were interdisciplinary at this time. Sanitary Science (Public Health) was in the Sociology Department until 1904 when a separate Department of Household Administration was established.

6. The 1930 issue of *The National Cyclopaedia of American Biography* identifies Jane Addams as a sociologist. In White's *Conspectus of American Biography* (1937), Jane Addams is listed under sociology and is not listed under social work. As late as 1948 Harry Barnes said the largest group of sociologists were "social economists" or "practical sociologists." He included Addams in this group.

The American Journal of Sociology gave a great deal of coverage to Addams' ideas and activities. In addition to her articles (e.g., 1896, 1899, 1912, 1914), there were solicited comments (1908), reviews of her books (e.g., Mead, 1907) and an article on a day at Hull House (Moore, 1897.) Thomas (1910:550) began her review of Addams' "The Spirit of Youth and the City Streets" in

The American Journal of Sociology by saying "One lays down (the book) with the feeling that sociology has published a classic."

7. The Chicago branch of the Association of Collegiate Alumnae was among those holding meetings at Hull House. Marion Talbot and Sophonisba Breckinridge were very active in this group (Talbot and Rosenberry, 1931.)

8. Julius Rosenwald, a wealthy Chicago business leader, was an active Hull House trustee and supporter of Jane Addams' work. It was not surprising that the President of the Julius Rosenwald Fund wrote to several prominent individuals to ask them to support Jane Addams' nomination for the Nobel Peace Prize. Yale University President James Angell (1928) responded to the invitation by saying in part: "I find your request extremely difficult to deal with. I have known Miss Addams for nearly forty years and have in many ways the greatest admiration for her character and accomplishments. I am frank to say, however, that I could not understand, and I find it even difficult wholly to forgive, her attitude during the early part of our entry into the war. She was, from my point of view, so altogether irrationally pro-German, veiling her actual procedure under the guise of her Tolstoian pacifism that, in common with many of her other life-long friends, I found myself deeply hurt and alienated. I doubt whether I could write the type of letter which would really be helpful in connection with the Nobel Prize, assuming that any weight attached to the letter at all, and I think that, under these circumstances, perhaps I had better not make the effort."

9. Information about Edward McDonagh can be found in the introduction to his article which was reprinted in the 1986 *Clinical Sociology Review* (Fritz, 1986).

10. I am indebted to Herbert Hunter for calling this article to my attention.

11. Gouldner's article is reprinted in his 1965 volume *Applied Sociology*. The first section of Gouldner's book was entitled "A Clinical Approach" and the second was "Practitioners and Clients."

12. Kargman's article is reprinted in the 1986 issue of the *Clinical Sociology Review*. The introductory article (Fritz, 1986, 11-13) gives information about Kargman's work.

13. Whyte's 1947 article is reprinted in the 1987 issue of the *Clinical Sociology Review*. The introductory article (Fritz, 1987) gives information about Whyte's clinical work.

14. Gomillion's work is discussed in Smith and Killian's (1974) "Black Sociologists and Social Protest" and in Butler Jones' (1974) "The Tradition of Sociology Teaching in Black Colleges."

15. Billy Franklin, in his 1979 article in *Psychology*, identified the following historical currents: depressions and unemployment, war or the threat of war, status seeking and revolutions (in thought, act or technology).

16. It is important—because of size, resources and historical role—to have the active support of the American Sociological Association. It is also important for sociology departments at established universities to offer clinical sociology programs or concentrations. These programs, when reviewed as a whole, need to cover the range of intervention levels and be well distributed geographically.

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