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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. . . .

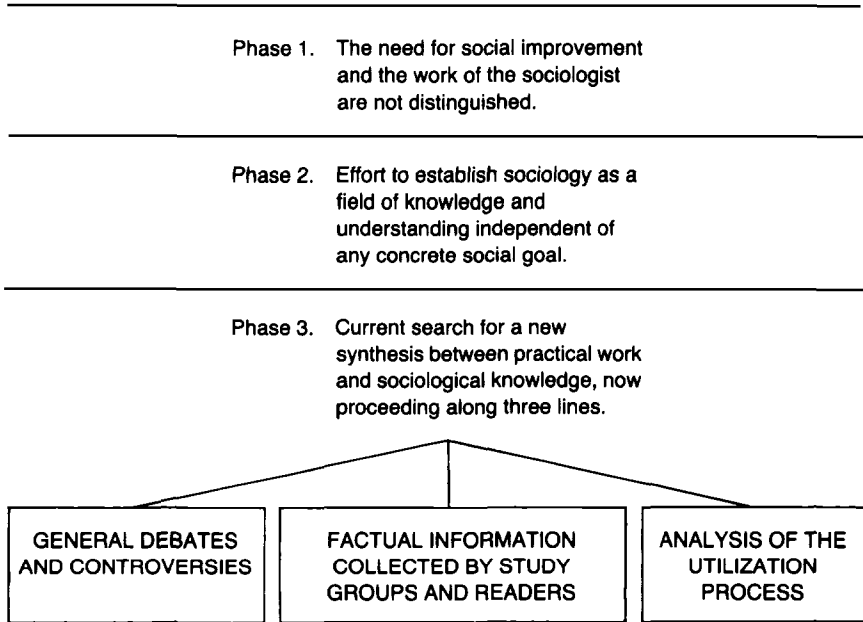


Figure 1

Notes

1. Luther L. Bernard and Jessie Bernard, *Origins of American Sociology* (New York: Crowell, 1943).
2. Anthony Oberschall, *The Establishment of Empirical Sociology: Studies in Continuity, Discontinuity and Institutionalization* (New York: Harper and Row, 1973).
3. Pauline Young, *Scientific Social Surveys and Research*, Part 1 (Englewood Cliffs, NJ: Prentice-Hall, 1946).
4. A. Eaton and S. M. Harrison, *A Bibliography of Social Surveys* (New York: Russell Sage Foundation, 1930).
5. Stuart Rice, *Methods in Social Science* (Chicago: University of Chicago Press, 1931).
6. "Critique of Research in the Social Sciences I," *Bulletin*, 44 (New York: Social Science Research Council, 1939).
7. Robert Angell, "The Use of Personal Documents in History, Anthropology, and Sociology," *Bulletin*, 53 (New York: Social Science Research Council, 1945).
8. "The Prediction of Personal Adjustment" (New York: Social Science Research Council, 1941).
9. P. Eisenberg and P. F. Lazarsfeld, "The Psychological Effects of Unemployment," *Psychological Bulletin*, 35 (1938), pp. 358-90.
10. Barry D. Karl, "Mr. Hoover's Experts," in D. Fleming and B. Bailyn (eds.), *Perspectives in American History*, Vol. 3 (Cambridge, Mass.: Charles Warren Center for American Studies, 1969).
11. This part of the story is well documented; see E. de Brunner and J. Kolb, *Rural Society* (3d ed.; Boston: Houghton Mifflin, 1946).

12. Hadley Cantril, *The Human Dimension: Experiences in Policy Research* (New Brunswick, N.J.: Rutgers University Press, 1967).
13. The history of the Army Research Branch is sketched in the first chapter of Samuel A. Stouffer *et. al.*, *American Soldier: Adjustment during Army Life*, Vol. I *Studies in Social Psychology in World War II* (Princeton, N.J.: Princeton University Press, 1949).
14. Alfred Marrow, *The Practical Theorist* (New York: Basic Books, 1969).
15. These experiments are well described in Chapter 14 of Marrow's biography.
16. Nevitt Sanford, "Whatever Happened to Action Research?" *Journal of Social Issues*. 25(1970), 3-23.
17. Guy Swanson, Theodore Newcomb, and Eugene Hartley (eds.), *Readings in Social Psychology* (New York: Henry Holt, 1952).