

# American SOCIOLOGICAL Review

December  
1952

Volume 17  
Number 6

Official Journal of the American Sociological Society

## EXPERIENCES IN INTERDISCIPLINARY RESEARCH\*

DOROTHY SWAINE THOMAS

*University of Pennsylvania*

**T**HIS address will be concerned with experiences in interdisciplinary research that began in 1921, during my junior year at Barnard College, and are still continuing.

Sociology at Barnard, in the early 1920's, was a very insignificant appendage to the Economics Department, and William F. Ogburn served as chairman of the joint curriculum. I took both economics and sociology with Ogburn, and was greatly influenced by his emphasis on the relationships between economic and social phenomena, his tendency to view the economic as independent and social phenomena as dependent variables, and his insistence on objectivity, verification, and measurement. At the same time, I studied and enjoyed elementary statistics under Chaddock and Ross, and was fascinated with the empiricism of anthropology as taught by Boas. Thus, my research orientation came, early and simultaneously, from several disciplines, and my first two research papers,<sup>1</sup> prepared under Ogburn's direction and published in collaboration with him in 1922, were interdisciplinary in the sense that the one, dealing with the incidence of simultaneous inventions, involved explorations into the history of science and into cultural anthropology, and the other combined the data and pro-

cedures of economics, sociology, and statistics to measure the relationships between business cycles and cycles in demographic phenomena (marriages, births, deaths) and in indices of social disorganization (suicide, crime, divorce). In the latter, we applied new techniques of time-series analysis that were then being developed by Harvard economists to an old problem, aspects of which had been approached in other ways by many other investigators, especially in England. The Harvard techniques seemed to promise greater precision than those used previously elsewhere, but their systematic application to all available American data yielded inconclusive results. Attributing their inconclusiveness to discontinuities and other imperfections in American data, I felt that it would be worthwhile to explore the problem further, with English data. Then, too, I wanted more advanced statistical training, and England was notable for its great statisticians. Moreover, I wanted to go abroad. So, in 1922, I entered the London School of Economics to study advanced statistics with Arthur L. Bowley, and to prepare a dissertation, under his and Sir William Beveridge's direction, on social aspects of English business cycles. I immediately discovered that I had too little mathematics to attain proficiency in advanced statistics and it seemed too late to build up this background without slowing down the momentum I already had in empirical research. Moreover, Bowley thought I could get along without it. The important thing in applying statistical methods to social phenomena, he said, was to understand the assumptions underlying the methods and the limitations

\* Presidential address read at the annual meeting of the American Sociological Society held in Atlantic City, New Jersey, September 3-5, 1952.

<sup>1</sup> "Are Inventions Inevitable? A Note on Social Evolution," *Political Science Quarterly*, March, 1922, and "The Influence of the Business Cycle on Certain Social Conditions," *Quarterly Publication of the American Statistical Association*, September, 1922.

of available data in relation to these assumptions. I therefore studied just enough mathematics to be able to follow Bowley's lectures, which were based on Part II of his *Elements of Statistics*, but which were not, by my definition, elementary. My painful experience in following his derivations did, however, make explicit the dangers of my tendency toward too-facile application of statistical techniques. I don't think I ever used a probable error again, and although I continued to apply theoretically inapplicable correlation techniques to observations ordered in time, I began to proceed more on the basis of calculated risk, and less on the basis of faith. In contrast to his lectures, Bowley's seminars dealt almost exclusively with the measurability of data to which statistical methods were applied, rather than with the methods themselves, and in these seminars his students learned by exploring data from many diverse sources how and under what conditions to press for precision and to tolerate imperfection. While studying with Bowley, I also worked closely with T. H. C. Stevenson, of the Registrar General's office, who helped me to collate and organize the statistics for my dissertation and I found that, whereas English data were, indeed, superior to American, they too were subject to underreporting, to systematic biases, and to many compensated and uncompensated human and mechanical errors. In the course of two years, I learned to live with my data as well as with my techniques and I completed *Social Aspects of the Business Cycle*,<sup>2</sup> which was essentially a repetition of the American study, but with the addition of a number of other "dependent variables"—among them emigration—and with much greater specificity in series used as indices of crime and delinquency.

After completing my Ph.D. in economics in 1924, I spent a strictly disciplinary year as statistical assistant at the Federal Reserve Bank of New York, where I fitted trends to economic time series and computed indices of seasonal variation and learned—as I had not learned in statistics—the necessity of arithmetic accuracy. During this year, too, I filled in some gaps in my train-

ing by studying economic theory under Wesley Mitchell at Columbia University. Although I had, and continued to have, low tolerance for formal theory which was not directly related to empirical research, I learned from Mitchell, the great empiricist, to accept theories as part of the data with which social science is concerned.

Toward the end of 1925, I received a research fellowship from the newly-organized Social Science Research Council to work on a project which I called "Some Economic Factors in Delinquency." What I had in mind was to supplement the analyses of relationships between business cycles and cyclical fluctuations in crime with cross-sectional analyses of the differential incidence of delinquency in population groups classified in terms of various socioeconomic criteria. The project never quite got under way, partly because the statistical data that I needed were not accessible, but primarily because I did not know enough about what was essentially behavioral research to formulate realistic questions or procedures. I discussed my difficulties with Wesley Mitchell, and he advised me to ask W. I. Thomas' help in reformulating the project. Meantime, the Laura Spellman Rockefeller Memorial had commissioned W. I. Thomas to make an appraisal of the various types of standpoint and of research procedures that were being directed towards the study and control of behavior, and Beardsley Ruml, the quantitatively-predisposed director of the Memorial suggested to W. I. Thomas that he employ a statistical assistant. When, therefore, I followed Mitchell's advice and went to Thomas for guidance on my project, Thomas followed Ruml's advice and offered me, in my capacity as statistician, a job on his own project. I eagerly accepted, and out of our collaboration came *The Child in America*<sup>3</sup>—a study so inclusive in scope that it might properly have been called "The Child and Other Matters in America and Elsewhere." It involved first-hand examination and systematic critiques of practical behavioral programs, especially those developed in the community, the school, the court, and the clinic, and of the existing

<sup>2</sup> *Social Aspects of the Business Cycle*, London: Routledge, 1925.

<sup>3</sup> William I. Thomas and Dorothy Swaine Thomas, *The Child in America*, New York: Alfred A. Knopf, 1928.

state of psychiatric, psychological, physiological, and sociological knowledge and research procedures that were being drawn upon or that were potentially applicable to these programs.

The framework of *The Child in America* was W. I. Thomas' famous situational approach, which called for comparative studies of behavior reactions and habit formation in a great variety of situations, which defined the "total situation" as always containing more or less of the subjective and emphasized the necessity of studying behavior "in connection with the whole context, i.e., the situation as it exists in verifiable, objective terms and as it has seemed to exist in terms of the interested persons." The methodology recommended for behavioral research would involve the use of both statistics and personal documents; for example, in studies of delinquency "what is needed is continual and detailed study of case-histories and life-histories . . . along with the available statistical studies to be used as a basis for the inferences drawn. And these inferences in turn must be continually subjected to further statistical analysis as it becomes possible to transmute more factors into quantitative form. Statistics becomes, then, the continuous process of verification. As it becomes possible to transmute more and more data to a quantitative form and apply statistical methods, our inferences will become more probable and have a sounder basis. But the statistical results must always be interpreted in the configuration of the as-yet unmeasured factors, and the hypotheses emerging from the study of cases must, whenever possible, be verified statistically." In the behavior document, which represents "a continuity of experience in life situations . . . we are able to view the behavior reactions in the various situations, the emergence of personality traits, the determination of concrete acts and the formation of life policies, in their evolution. Perhaps the greatest importance of the behavior document is the opportunity it affords to observe the attitudes of other persons as behavior-forming influences, since the most important situations in the development of personality are the attitudes and values of other persons."<sup>4</sup>

It is always dangerous to try to reconstruct the separate contributions of collaborators, but I am reasonably sure that the designation of subjective, documentary materials as the "as-yet unmeasured" and the emphasis on "transmuting" more and more factors "into quantitative form" were mine and that the very positive evaluation of the behavior document *per se* was W. I. Thomas'. For when I joined the staff of the Child Development Institute at Teachers College, in 1927, I was still somewhat distrustful of the subjective and the "as-yet unmeasured" as materials for scientific investigations. I still preferred to work exclusively with the objective, defined in almost mechanistic terms, and to count, measure, sample, fit curves, correlate, test for reliability, validity and the significance of quantitative differences, rather than to utilize descriptive materials or life histories, case records, and other types of personal documents. I hoped, indeed, that the series of observational studies of social behavior which I directed there and continued during the 1930's at the Yale Institute of Human Relations might yield "data as objective as the best of those with which the statistical economists" were dealing. And although I gave verbal recognition to the value of case histories, diary records, and what I called "merely descriptive" accounts of behavior as "hypothesis-forming material for further studies" I made slight use of these materials, on the ground that they "obviously [would] not yield data appropriate for statistical analysis."<sup>5</sup> My associates and I experimented with techniques for recording overt behavior on a time-sampling basis, with particular emphasis on observer reliability, and analyzed the masses of data that we accumulated in methodological terms, with little regard for substance.

While I was still at Teachers College, W. I. Thomas revived a dormant plan to make a study of Swedish emigration and of Swedish immigrants in America, which he hoped would parallel and supplement *The Polish Peasant*. He was thoroughly familiar with the 20-volume work on emigration and

<sup>4</sup> Dorothy Swaine Thomas and Associates, *Some New Techniques for Studying Social Behavior*, New York: Teachers College, Columbia University, 1929, pp. 19-20, *passim*.

<sup>5</sup> *Ibid.*, pp. 571-572, *passim*.

"related topics" which Gustav Sundbärg and his associates had prepared for the Swedish Emigration Commission in 1908-1912, and he called my attention to the superiority of these sources and the relevance of the data for studies in social demography.

In 1930 we met Gunnar and Alva Myrdal, and at their invitation visited Sweden and came into contact with Olof Kinberg, psychiatrist and criminologist, with Karl Arvid Edin, who had worked with Sundbärg and was then carrying on pathfinding studies of differential fertility, and with Gösta Bagge and a group of young economists at Stockholm University who were making extensive historical and statistical studies of wages, national income, the cost of living, and internal migration. Together with these Swedish social scientists, W. I. Thomas and I drew up a plan for a major research project on "Behavior and Social Structure" which would have combined our several approaches. We could not get this comprehensive project financed and we gave up the idea of paralleling *The Polish Peasant*, but W. I. Thomas collaborated with Kinberg in his explorations of the behavioral documents available at the Criminological Institute, and when I left Teachers College for Yale, it was with the understanding that I could devote part of my time to population studies in Sweden. During the nine years of my association with the Yale Institute of Human Relations, therefore, I spent part of almost every year in Sweden, where I found the setting, the data, and the personnel admirably suited to interdisciplinary analyses of the relationships between economic development and socio-demographic change. Drawing on Sundbärg's pioneering investigations, working in collaboration with Myrdal and Edin, and exploiting the basic population registers for hitherto unavailable data on internal migration, I was able to carry many of the analyses backward in time to 1750 and thus to observe the patterns of interrelationships for a whole century preceding the Industrial Revolution, and during the period of rapid industrialization that began in Sweden in the 1860's to evaluate structural, cross-sectional, and secular, as well as cyclical factors, in change. In the light of all the available evidence, it became apparent that neither the demographic determinism of most economists (in which

population is viewed as the independent variable and economic development as the dependent), nor the economic determinism which had characterized my approach to *Social Aspects of the Business Cycle*, were adequate explanations of observed interrelationships; that there was a continuous chain of interdependence among demographic variables, none of which, in the long run, "can be considered a completely independent variable," but each of which might in the short run "show an immediate effect (i.e., become a dependent variable) or act as an immediate cause (i.e., become an independent variable)" and that "the economic structure and way of life of the people continually modify and are modified by the chain of demographic events."<sup>6</sup>

The Swedish project was orderly and well coordinated, for it drew upon a vast fund of accumulated data, reexamined problems that had been previously explored in other areas, proceeded along lines that had been well-defined by investigators from several disciplines, and represented a continuity in my own research experience. My next project—a study of the Japanese American evacuation and resettlement was also interdisciplinary, but in contrast to the Swedish study, it extended far beyond the range of my experience, could draw upon no systematically accumulated fund of knowledge, and found few realistic "models" or adequate techniques by which to guide procedures or check conclusions.

I had joined the faculty of the University of California in 1940, and was working on some minor projects in the population field at the outbreak of war. The first plans for a study of evacuation and resettlement were drawn up by Charles Aikin of the Political Science Department and myself early in February 1942, after the Department of Justice had designated a number of small zones, surrounding strategic installations, as areas from which alien enemies were to be evacuated by February 24. The movement would encompass no more than 10,000 aliens (German, Italian, and Japanese) but it was anticipated that these aliens would be accompanied, in many instances, by citi-

<sup>6</sup> *Social and Economic Aspects of Swedish Population Movements 1750-1933*, New York: The Macmillan Company, 1941, p. 3 and p. 351.

zen members of their families. In any case, the number of persons involved would not be large, and correspondingly it was expected that the range of movement would be relatively slight. Aikin and I therefore applied for a small grant to make a modest study of the development and application of evacuation policies and of their impact on the population groups directly concerned. We were never able to make this modest study, for the simple reason that the officially-announced plan for a small-scale limited-range evacuation was superseded by plans, developed piecemeal during the spring and summer, for a large-scale forced mass migration from a very extensive area. We had continually to reformulate our projected research and to try to adjust our budget and personnel, to meet such radical changes in the evacuation and resettlement program as the following: suspension of the plan for evacuating any Germans or Italians and extension of the plan for evacuating Japanese to include not only "enemy aliens" but all persons who had a Japanese ancestor "regardless of degree"<sup>7</sup> and irrespective of birthplace or citizenship; expansion of the exclusion zone to the whole of California and to the main areas of settlement in Washington, Oregon and Arizona; abandonment of a short-lived plan for voluntary evacuation in favor of one for controlled evacuation, in which the evacuees were moved under military supervision to barbed-wire enclosed camps; the functional reorganization of these camps from "reception centers" for the protective custody of displaced people to "assembly centers" for temporary but enforced detention; and the movement of the assembly center population en masse to larger and more remote camps called "relocation projects," but, like assembly centers, surrounded by barbed wire, designated as military zones, and organized for detention purposes. In 1943, following an attempt on the part of the administration to assess the loyalty of the detained evacuees, by questionnaire and registration, the program was again precipitously and radically changed. Evacuees who refused to

answer the questionnaire or who gave qualified affirmations of allegiance were, along with their dependents and other family members who wished to accompany them, moved to a segregation center for the "disloyal," and the relocation projects then began to function as dispersion centers, from which resettlement to the middle west and east was promoted. At the end of 1945 the exclusion orders were rescinded and within a year all camps were liquidated. The number of persons involved in voluntary evacuation was approximately 10,000; in the initial phases of detention 110,000; in segregation, 18,000; in voluntary resettlement in the middle west and east prior to rescision of the exclusion orders, 36,000; and in the return movement to the West Coast or resettlement in other areas during the period of camp liquidation, 62,000.

As the problems for research multiplied and increased in complexity, members of the faculties of Economics, Anthropology, and Social Welfare were added to the senior staff, and plans for a broad interdisciplinary approach were developed. But as explained in the first volume of the *Evacuation and Resettlement Study* published in 1946, "this ambitious conceptualization was never realized to the full," partly because all of the senior staff members except me were drawn into work with war agencies, partly because we could not use standard techniques to get the data implied in the "conceptualization," but mainly because "the course of events which were to be investigated could not be anticipated." We lamented that we could not sample "either on a time or population basis" or conduct opinion and attitude surveys or use questionnaires. In fact, our evacuee assistants could not, at times, even take notes in public, use typewriters in their barracks, or ask direct questions, from fear of being considered "informers."<sup>8</sup> In this extraordinarily dynamic study, we had to be constantly on the alert to get as complete a record as possible of the changing situations to which the evacuees were exposed and of concomitant changes in their behavior and attitudes. In part we used the so-called "vacuum cleaner approach," and

<sup>7</sup> As described officially by the Western Defense Command. (U. S. Army, Western Defense Command and Fourth Army, *Final Report: Japanese Evacuation from the West Coast, 1942*, Washington, 1943, p. 514.)

<sup>8</sup> Dorothy Swaine Thomas and Richard S. Nishimoto, *The Spoilage*, Berkeley and Los Angeles: University of California Press, 1946, p. ix.

became avid collectors of documents and of statistics, including administrative instructions, camp newspapers, minutes of meetings, school and employment records, results of votes and referenda, petitions to the administration, letters of complaint and correspondence of all sorts, the basic demographic, social, and economic data included in a complete census taken by the War Relocation Authority while the evacuees were being "processed"—the records of intercamp transfers, data from the questionnaire submitted in connection with the "loyalty" inquiry, segregation lists, lists of those who renounced citizenship, vital statistics, transcripts of "leave clearance" permits and so on. This approach, undirected as it was and wasteful as it seemed at the time, paid off in the long run, for when the camps were liquidated, many of the records in the administrative files were either destroyed or buried in archives.

Data on behavior and attitudes were collected both by evacuee members of our research staff, and by other field workers who, with official approval, lived in the quarters assigned to administrative personnel. Among the evacuees who worked on the study, and whose participation in the situations they were observing was a matter over which they could exercise little control, were persons of diverse background and training, including sociologists, an anthropologist, an engineer, an agricultural economist, a psychologist, a social worker, and a journalist, while the nonevacuee field workers included two anthropologists and a historian. Each of them prepared many reports on special topics, which followed outlines developed in terms of our ever-changing "interdisciplinary conceptualization," but more important were the undirected journals kept by most of the participant observers and field workers. These journals included running accounts of "current events," information obtained from wide circles of "participating informants" (both evacuees and administrative personnel), and accounts of the actions and conversations of many persons who did not know either that the study was being made or that they were under observation. Each journal-keeper also recorded the course of his own experiences and his attitudes towards these experiences with the maximum possible

frankness, and appended all documentary material that he could collect. Each brought to his journal something of the standpoint of his own discipline and his own biases, in the very process of selecting events, words, and acts to record. And as the anthropologist Tsuchiyama remarked, whatever his background, each observer and field worker soon found himself functioning more as a "foreign correspondent" than as a social scientist, for good reporting was essential in a study where the "preconceptualized" lines of inquiry were often vague and inadequate. These day-by-day on-the-spot records were a principal and essential source for retrospective analyses. That their extensiveness and detail may be unique in the annals of social science is suggested by the fact that the journal kept by a single participant observer (Charles Kikuchi) covers more than 10,000 typed pages.

After resettlement got under way, in 1943, several of the junior staff members left camp and moved to Chicago, where they again functioned as participant observers in the area which during the next two years absorbed well over half of the total number of resettlers. In the freer conditions of the "outside world," they were able to utilize techniques that had been impossible to develop adequately in the camps, and although they continued to keep journals, they now were able to get a far greater range of experiential records through planned interviews and the preparation of life histories.

The collection of primary data for this study was brought to an end in 1945, and the first volume—essentially a record and interpretation of intercamp tensions and crises—was published in 1946.<sup>9</sup> Then came a "horse-after-the-cart" procedure of examining, collating, and synthesizing all available historical and statistical data, from secondary sources, to provide a socio-demographic frame of reference for the observational and life-history material. This required five more years of hard labor by several of us,<sup>10</sup> and it is to be hoped that this added time gave us the perspective that

<sup>9</sup> *Ibid.*

<sup>10</sup> Dorothy Swaine Thomas with the assistance of Charles Kikuchi and James M. Sakoda, *The Salvage*, Berkeley and Los Angeles: University of California Press, 1952.

the historians told us was necessary for the proper "placing" of contemporaneous events.

When I came to the University of Pennsylvania in 1948 and was, for the first time in my career, associated with a Department of Sociology, I had become so thoroughly conditioned to interdisciplinary research that I soon found myself involved in two new interdisciplinary projects: (1) a study of technological change and social adjustment, as exemplified by the experience of the past fifty years of the people of Norristown, Pennsylvania. Here Thomas C. Cochran, the economic historian and I are trying to build up the record of technological, material, and demographic change, by collating data from surveys and interviews with those available in secondary sources, and, in collaboration with persons from several of the behavioral disciplines, we are exploring the possibilities of developing indices of adjustment; (2) a frontal attack, in collaboration with the economist Simon S. Kuznets, on the shift and redistribution of population and economic resources in space in the process of this country's development since the 1870's. Here we hope that by observing the various migrations and redistributions that have occurred in combination with changes in industrial structure, and finding out who moves, when and under what circumstances, we will get closer to the "whys" of economic growth on the one hand, and the "whys" of demographic change, on the other.

As to possible implications of these experiences in interdisciplinary research—or what I have learned in the course of three decades that will, hopefully, serve as guideposts as I enter the fourth:

- (1) I have not found it profitable to separate economics from the strictly behavioral disciplines—partly, of course, because of the types of problems on which I have worked but also because it does not seem feasible in behavior-situation studies to neglect the realities of economic structure, economic differentials, and economic development.
- (2) I have not found it profitable to approach interdisciplinary research by trying to merge disciplines at the "conceptual" level. It is the data of economics rather than the elaborate systems of the economic theorists that have provided a basis for practicable procedures.
- (3) On the behavioral side, I have not found it profitable to proceed as if all behavior must be or even can be "transmuted" into quantitative terms. And whereas I still push the statistical aspect of all studies to the limit, I no longer relegate the subjective and the descriptive to secondary positions.
- (4) I have belatedly recognized that we are all theorists and all statisticians, and that, on the one hand, underlying theory must be made as explicit as possible, and on the other, the implications of hidden statistical generalizations must be squarely faced.
- (5) I have found it profitable to take occasional and sometimes quite lengthy "disciplinary" leaves of absence from interdisciplinary research, to fill in gaps in training and technique.
- (6) Contrary to the attitudes now being expressed by numbers of my colleagues, I have found interdisciplinary research a rewarding and integrating rather than a "traumatic" experience.