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SOME REFLECTIONS ON THE GOLDEN AGE OF INTERDISCIPLINARY SOCIAL PSYCHOLOGY

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Abstract

In the 25 years or so that began with World War II, there was a great wave of enthusiasm for interdisciplinary social psychology which resulted in the establishment of interdisciplinary social psychology training and research programs in some of the major universities in the United States. By the mid-1960s however, this seeming Golden Age had largely vanished. This article, by one of the participants in this movement, is devoted to an elaboration of how this Golden Age came about and the forces that led to its demise. Its origins are traced to the World War II experiences of social psychologists in interdisciplinary research on the adjustments of the American soldier under the leadership of Samuel Stoffer and with Rensis Likert on the US strategic bombing surveys in Germany and Japan. Many of the participants in this research were greatly impressed by the fruitfulness of interdisciplinary collaboration and were determined to establish interdisciplinary social psychology programs on their return to their universities. Several of these programs were very successful for a number of years, especially those at Harvard and Michigan, but failed to survive and become integrated into the institutional structure of the American university. The reasons for their failures are complex but at least four factors seem to have been important. First, the threat of these programs to the traditional departmental structure of the university—particularly in light of the relatively weak position of the social sciences in that structure. Second, the lack of adequate and appropriate funding from either university or federal sources. Third, the lack of a major breakthrough in



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social psychological theory. Fourth, advancements in research methods did not produce greatly increased understanding of social psychological phenomena. These factors are examined and contrasted with the situation in the natural sciences, particularly with molecular biology.

Introduction

In a perceptive article, "The Three Faces of Social Psychology," James S. House (1977) pointed out that in the 25 years or so that began with World War II, there was a great wave of enthusiasm for interdisciplinary social psychology. This led to the establishment of several significant interdisciplinary social psychology training programs and research centers in some of the major universities in the United States. By the mid-1960s, however, this seeming Golden Age of interdisciplinary social psychology had largely vanished. By the mid-1970s, it had been almost completely replaced by three separate and largely isolated divisions of social psychology: psychological social psychology now focuses on individual psychological processes as related to social stimuli, emphasizing the use of laboratory experimental methods; symbolic interactionism concentrates on face-to-face social interaction processes, using participant observation and informal interviewing in natural settings; and psychological sociology focuses on the reciprocal relation between social structure and individual social psychological behavior, relying mainly on survey methods. House further asserted that this fractionation of social psychology grew out of the institutional and intellectual contexts in which social psychology originally developed, that the three factions have grown further apart over the last two decades, and that there was great need for more interaction between them, if a vital and well-rounded social psychology were to develop.

For the most part I agree with House's formulations and conclusions, although I still prefer to describe what most of us do with the traditional label, social structure and personality, rather than with his term, psychological sociology. Like Sheldon Stryker (1987), I see a somewhat less clear distinction between the present stance of symbolic interactionism and social structure and personality than House did a decade ago. This is particularly true now that many symbolic interactionists are using formal observation, sample surveys, and multivariate analysis in their research.

This brief review of House's article serves as background for my own reflections on what some call the Golden Age of interdisciplinary social psychology. I wish to elaborate on how it came about and to describe the forces that led to its demise. I agree with House that the intellectual and institutional contexts in which each faction developed probably predetermined its return to its original disciplinary moorings once the interdisciplinary

arrangements faltered. I wish to reflect on the circumstances that may have contributed to the failure of these programs to become part of the institutional structure of our universities, while several post-war interdisciplinary programs in the natural sciences succeeded.

Background

I am neither a qualified historian of science nor a sociologist of knowledge, but I was one of the many actors in the movement and I participated in almost every aspect of it, its successes and its failures. Thus, I feel emboldened to share my reflections on it. Like most other participants, I had completed my graduate training in sociology before World War II, with a major interest but inadequate training in social psychology. There were few places where one could obtain much in the way of training in social psychology in the mid-1930s, and Minnesota, where I did my PhD in sociology, was not one of them.¹ By the time I was called to service in World War II as a reserve officer in the US Navy, I was already a fairly well-established sociologist. I had read widely in social psychology and had done research and teaching in the field. On entering active military service, I was assigned to the staff of the Research Division of the National Headquarters of Selective Service, where along with other social scientists I did research on civilian and military manpower.² During this period, through contacts with Samuel A. Stouffer and members of his staff, I became well acquainted with the interdisciplinary research program of the Information and Education Division of the War Department, most of which involved studies of the adjustment of soldiers to military life during World War II. Much of this research was published, under Stouffer's leadership, in the famous four-volume work *The American Soldier* (1949). I also became acquainted with Rensis Likert and several of his colleagues who directed the Program Surveys Division of the Department of Agriculture which had been doing social psychological studies of the civilian population for several departments of the Government during the war years.

After the surrender of Germany, Likert asked me to join a group that was

¹At that time the leading centers for social psychology training were Chicago, Columbia, and Harvard, but even at these institutions the offerings were not extensive. At Minnesota I had reading courses with Clifford Kirkpatrick and sat in on a course in social psychology in the psychology department. This course was devoted largely to group differences in ability and attitudes and gave some attention to collective behavior. Fortunately, as an undergraduate, I had had courses in sociology and philosophy at Michigan State in which I had read much of Dewey and Cooley and some of Mead—at that time *Mind, Self and Society* (1934) had not yet been published.

²Others involved in this research were Kenneth McGill, Raymond V. Bowers, C. Arnold Anderson, Harold Faulk, Robert N. Ford, J. Mapheus Smith, and Louis Levine.

making preliminary plans for a study of the influence of strategic bombing on Japanese civilian morale. (Likert had directed a similar study in Germany; see US Strategic Bombing Survey, 1946). This group included some members of the team that had conducted the German survey and several of the social scientists who were to conduct the survey in Japan. We drew up preliminary plans for the Japanese survey, including a clear conceptualization of the aims of the survey, a list of the major components of morale, and a series of questions designed to elicit these.

Within days after the surrender, the interdisciplinary team of psychologists, sociologists, anthropologists, political scientists, a psychiatrist, and sampling statisticians, who were to carry out the study had assembled in Tokyo.³ We immediately began to review the purposes and design of the survey and made many important revisions in both the conceptual guides for the study and the content of the survey instrument. We then pretested the interview schedule on Japanese civilians, using Japanese-American interviewers. These interviewers had participated in many of our meetings and were well acquainted with the purposes of the research. The survey directors and the interviewers then participated in the final revision of the interview schedule.

Meanwhile, our sampling experts had designed and drawn a probability sample of the Japanese adult civilian population consisting of approximately 3000 persons. We then took our teams of interviewers into the field and completed the interviewing in a period of three more months. In another month or so, after returning to Washington DC, we had developed a coding scheme for the interviews, coded the materials, and completed the statistical processing of the data. By then several of our number had been released from service and returned to their academic posts. Those of us who remained, with assistance from some of our departed colleagues, wrote the final report which was then sent to the printers. All of this was accomplished within less than a year after our arrival in Japan. The report was published by the Government Printing Office in 1947 (US Strategic Bombing Survey, 1947).

Throughout this endeavor I was very much impressed with the fruitfulness of interdisciplinary collaboration among bright and willing social scientists. In general the most innovative and insightful ideas were generated as a result of group discussions in which little regard was paid to the disciplinary origin of the idea. I was greatly impressed also with the ability of an interdisciplinary team to mount a study of this complexity and to move it to completion so

³The group included David Aberle, Conrad Arensberg, Jules Henry, and Fredrick Hulse (anthropologists); Donald Adams, Edgerton Ballachey, and Horace English (psychologists); Raymond Bowers, Burton Fisher, and William Sewell (sociologists); Morris Hansen and Harold Nisselson (statisticians); David Truman and Harold Nissen (political scientists), and Alexander Leighton (psychiatrist).

expeditiously.⁴ My colleagues on the Bombing Survey, as well as those who had participated in other wartime interdisciplinary social psychology research projects, were equally impressed with their experiences and were determined to promote interdisciplinary training and research programs in social psychology on return to academic life.

Moreover, the private foundations, especially Ford, Rockefeller, Carnegie, and Sage, along with the Social Science Research Council, were stressing interdisciplinary social psychology. The Office of Naval Research, the National Institutes of Health, and later the National Science Foundation also were supportive of interdisciplinary research and training programs in social psychology. I served on and was chairman of several research grant and training committees during the period of expansion of interdisciplinary social psychology. Through this activity I came to know most of the leaders in this movement, and throughout the period I was involved with them in the promotion of interdisciplinary social psychology on the national level, as well as with others at the University of Wisconsin.⁵

As a result of all of this enthusiasm, activity, and support, interdisciplinary programs for graduate training were developed at Michigan, Harvard, Yale, Cornell, Berkeley, Columbia, Minnesota, Wisconsin, and other leading universities. In addition the National Opinion Research Center was moved to the University of Chicago, with a broadly expanded program under the direction of Clyde Hart; a new national research center, the Institute for Social Research, was established at the University of Michigan under the direction of Rensis Likert; and the Bureau of Applied Social Research was established at Columbia under the leadership of Paul F. Lazarsfeld. More locally oriented survey research centers were developed at Harvard, Yale, Princeton, Berkeley, UCLA, Illinois, Minnesota, and Wisconsin, to mention only the

⁴I wish it were possible to produce actual illustrative examples of how the work of the team was influenced by its interdisciplinary composition. I am sure that it was, but my memory of the instances is no longer reliable. I took no notes on our meetings and conferences, nor do I remember that anyone else did except Edgerton Ballachey. His notes on the Tokyo meetings, and those of David Krech on the earlier meetings in Washington, formed the basis for their syllabus, *A Case Study of a Social Survey*, (1948), but they do not discuss the disciplinary sources of ideas, the definitions of concepts, the hypotheses tested, or the analytic strategies of the study. Again, what I remember most clearly is that some of the best suggestions came from persons who were not directly identified with the discipline with which one would normally associate the idea. This is not too surprising because all of us had a strong commitment to social psychology regardless of our disciplinary identifications.

⁵Among the national leaders not otherwise mentioned in this paper were: Gardner Murphy, Richard Crutchfield, Clyde Coombs, Ernest Hilgard, Charles Osgood, Harold Kelley, Goodwin Watson, Muzafer Sherif, Urie Bronfenbrenner, M. Brewster Smith, David Riesman, Leonard Cottrell, Robin Williams, Arnold Rose, Fred Strodtbeck, Angus Campbell, Herbert Simon, Ralph Linton, Fredrick Redlich, August Hollingshead, Warren Dunham, and Robert Faris.

more prominent ones. The primary commitment of these centers was to interdisciplinary research on social psychological topics using sample survey research methods. The Armed Services established similar research centers to investigate problems related to their military mission.⁶ During this period too the interdisciplinary research program in social psychology was developed in the Laboratory of Socioenvironmental Studies of the National Institute of Health under the direction of John Clausen and, later, Melvin Kohn.

There certainly was no lack of interest in the interdisciplinary graduate training programs on the part of social psychologists and graduate students. Social psychology was a challenging intellectual field and students were anxious to learn more about it, including what other disciplines than their own had to contribute to its theory and methods. Moreover, there was a backlog of mature graduate students whose education had been interrupted by military service and who could qualify for financial support for their graduate training under the GI Bill. Still others could be supported by training grants from the National Institute of Mental Health, National Institute of General Medicine, the National Science Foundation, and other agencies interested in increasing the supply of persons trained in social psychology. Thus, it seemed that all conditions were right for sustained growth of a new interdisciplinary field of social psychology.

For a decade or so great progress was made, particularly in the interdisciplinary training program at Michigan, under the leadership of Theodore Newcomb. The faculty included Angus Campbell, Dorwin Cartright, J. R. P. French, William Gamson, Daniel Katz, Robert Kahn, Herbert Kellman, Helen Peak, Albert Reiss, Guy E. Swanson, and Howard Schuman. At Harvard, the new Department of Social Relations was headed by Talcott Parsons, with a faculty that included Gordon Allport, R. Freed Bales, George Homans, Alex Inkeles, Clyde and Florence Kluckhohn, Gardner Lindzey, Frederick Mosteller, Richard Solomon, and Samuel Stouffer.⁷ The growth was less spectacular in other universities but was by no means insignificant. With such a good start why did the interdisciplinary programs in social psychology all but vanish by the late 1960s, without ever becoming established in the institutional structure of American universities?

⁶For information on the work of these and related research centers see Bowers (1967).

⁷To my knowledge only the Harvard program was set up as a separate department. The usual pattern was the one followed at Michigan where the social psychology program resulted from the joint sponsorship at the departments of sociology and psychology; it never had the status of a separate department. I suspect the separate department at Harvard resulted not only from Talcott Parson's interest in social relations but also from his desire to escape Pitirim Sorokin's administrative control.

Why Did Interdisciplinary Programs In Social Psychology Fail?

The reasons for the failure of interdisciplinary programs in social psychology are complex and not entirely apparent. I believe one reason is the traditional institutional structure of American universities and the place of the social sciences in that structure. Another factor closely related to this is the system for funding science that has developed and become institutionalized in the United States and the unfavorable position of the social sciences in this system.⁸ Other reasons may be found within social psychology itself, particularly in the condition of social psychological theory and methods.

The Threat to the Departmental Structure

I turn first to the traditional institutional structure of the American university and the relatively weak position of the social sciences there. The physical and biological sciences, in both their pure and applied branches, are in a position superior to the social sciences and humanities in most of our universities. This is true with respect to the funds allocated to research, to buildings, to equipment, and to salaries, but particularly to new ventures such as research centers and training programs. Thus, the existing social science departments have to defend their turf in the face of new interdisciplinary programs that might threaten their claim on the universities resources. This is much less true in the natural sciences where more ample funds are available from both local and national sources. Thus, social science departments tend to be much less supportive of interdisciplinary programs, unless additional funds for them can be brought in from the outside. This is particularly so when the program is likely to require faculty, scholarships, space, equipment, and operating funds, always in short supply, that may draw faculty and students away from the parent departments. To be sure several universities were willing to give limited support to interdisciplinary training programs in social psychology, but for the most part the faculty were part-time in the program and were budgeted to their original departments. Funds for subsidizing graduate students, faculty research, secretarial and clerical staff were expected to come from outside grants. Federal funds to meet these costs, although available, were usually inadequate and were granted for a relatively short period—usually three to five years—with no assurance they would be renewed. Given these conditions it is not surprising that the parent departments found interdisciplinary social psychology programs threatening.

Wisconsin provides a classic example of the point I have been making. Early on, I obtained funds from the Social Science Research Council to set up a faculty seminar made up of psychologists, anthropologists, and sociologists to draw up plans for a graduate interdisciplinary training program in social

⁸For further discussion of this topic see Sewell (1988).

psychology. We met for several months and developed a program consisting mainly of courses in social psychology already being taught in the departments, plus two new seminars: one on current social psychological theory and the other on current research methods in social psychology. The dean of our college supported the plan, subject to the approval of the departments involved, with the understanding that the departments would provide the faculty from their current budgets. It was further assumed that the group would seek outside funds for the subsidy of graduate students and for other requisite costs. When the plan was presented to the departments, neither sociology nor psychology would approve of our request for a joint major in social psychology. The best either department would settle for was an interdepartmental minor, with psychology requiring that students who were not psychology majors take the psychology proseminar, and sociology stipulating similar requirements for psychology students. The Graduate School approved of these arrangements, but the graduate students did not find these requirements attractive and within a few years the program ceased to exist. This did not mean that thereafter no interdisciplinary training in social psychology could be found at Wisconsin; the sociological social psychologists encouraged their students to take courses from psychological social psychologists, and vice versa, but nothing like a true interdisciplinary program emerged. I do not claim that the Wisconsin experience was typical of other programs, but I do know that several others suffered from less than adequate support by their departments and deans. In some institutions interdisciplinary programs prospered only so long as their enthusiastic and powerful founders participated in the program and strongly supported it in their departments. When they were replaced, their successors tended to lack the enthusiasm and often the power and organizational skills of the founders. Conflicts arose; the departments and college administrations withdrew their support, and the programs were soon abandoned.

Lack of Adequate Funding

Another factor in the decline of interdisciplinary social psychology programs was that they never received adequate funding from federal sources. This may seem paradoxical because it was during the period of their ascendancy that funding for social science research and training became institutionalized as a part of the program for the support of science in the United States, particularly in the National Institutes of Health and later in the National Science Foundation.⁹ Social psychology was especially favored in the research grant and training programs of the National Institute of Mental Health. However,

⁹For a brief discussion of the early development and importance of these sources of support, see Sewell (1988).

the funds going to the support of social sciences in these agencies never were more than 10% of their research budgets, and social psychology got far less than other branches of psychology and less than some other subfields of sociology. During this period funds became available for research and training in medical sociology, social problems, urban problems, juvenile delinquency, substance abuse, and aging. All of these involved social psychological research but still were competitive with programs for interdisciplinary social psychology research and training.

In any event, the major source of funds available for social psychology was NIMH. These funds had to be justified on the basis of mental health relevance, were modest in amount, limited in duration, provided for only a small number of research assistants or trainees, did not provide facilities, and usually supported only a limited portion of the salary of the principal investigator or director of the program. The consequence was that these programs were inadequately supported by either the universities or the federal agencies. This is in rather sharp contrast to the ever-increasing funds available to interdisciplinary research and training programs in the natural sciences during this period.

Unfortunately, the national survey research centers at Michigan and Chicago were somewhat underused as a source of interdisciplinary research training in social psychology. These not-for-profit organizations were only loosely connected with the universities, had their own staff, received limited financial assistance from the universities, and had to raise their own funds by doing contract work for government and private business. I do not mean to imply that they provided no support for social psychology programs. A limited number of their members participated in training programs, and both organizations provided part-time employment for a number of graduate students. They also made their research facilities available to faculty members who wished to subcontract with them for data gathering, data processing, and related services. Generally this was possible only when faculty members had outside grants for these purposes. Much the same situation held at other university sponsored survey research centers with the exception of Columbia's Bureau of Applied Social Research, where graduate students received coordinated training in Lazarsfeld's research methods seminar along with first-hand experience on the Bureau's ongoing social research projects.

Modest Advances in Theory

In speculating about the fate of interdisciplinary social psychology, I must point out that there have been no powerful theoretical breakthroughs that might have served as a stimulus to exciting new theoretical developments or new research areas during this period, or for that matter since. Advances in social psychological theory did occur, but they were modest. Although some codification took place, nothing approaching a unified body of social psycho-

logical theory emerged. Rather, there were improvements in somewhat isolated bodies of special social psychological theories, such as role theory, field theory, attitude theory, socialization theory, theory of interpersonal relations, communication theory, theory of collective behavior, and theory of small group processes. Perhaps the most exciting advances were made in our knowledge of small group processes, with fruitful work being done on group structure, cohesiveness, communication flow, leadership, productivity, deviance, and the construction of social reality. Unfortunately most of the bodies of special theories mentioned above (to paraphrase Robert K. Merton; 1949:85-87) consisted of general orientations toward problems and types of variables to be taken into account, rather than verifiable statements of relationships between sets of specified variables. There was little or no consolidation of these special theories into a general conceptual scheme for social psychology. The fate of social psychology in this regard was no different from that of the social sciences generally. In fact it could be argued that none of the social sciences made spectacular progress in developing general theory during these years.

What did take place was a great burst of research activity on a large number of social psychological topics, often with a view toward shedding light on problems of social behavior, rather than toward theory construction or testing. This is clearly reflected in the articles selected for the influential book, *Readings in Social Psychology*, edited by Theodore Newcomb and Eugene Hartley in 1947 and revised in 1952 and 1957. This book was sponsored by the Society for the Psychological Study of Social Issues. Its editors did not attempt to provide an overall framework for social psychology; instead they stressed three requirements for research in social psychology: It must adhere to rigorous canons of scientific procedure; it must draw hypotheses from the relevant psychological and social sciences; and it must bring these hypotheses to bear on systematic research on problems of human importance (Likert 1947 V). This final requirement characterized much of the research done during this period. It must be remembered that a great volume of research was done, enough to fill professional journals in the field and, for that matter, hundreds of pages in the more general journals of the various social sciences. Most of the pages in the five volumes of *The Handbook of Social Psychology* (1969), edited by Gardner Lindzey and Elliot Aronson, are devoted to summarizing this research record. But only a small fraction of this research resulted from interdisciplinary efforts.

Unfortunately, little of this outpouring of research resulted in powerful ideas that could stimulate further development of theory or research in social psychology. Rather, with few exceptions, the explanatory power of the theories and models in social psychology remained quite modest—often providing only small, though statistically significant, results. This is not the stuff that makes for a stimulating new interdisciplinary field; and it certainly

does not command the long-term, high level of financial support that interdisciplinary programs need to be successful.

Advances in Research Methods

During this period of great research activity much effort was devoted to the improvement of research methods, particularly in sampling, interviewing, questionnaire construction, index and scale development, observational techniques, and statistical methods for the analysis of survey data. Because of the rapid adoption and use of sample survey methods during and after the war, the government and the public were raising questions about the adequacy and dependability of existing methods of sampling, interviewing, and data analysis. Consequently, the National Research Council and the Social Science Research Council jointly sponsored a committee, under the chairmanship of Samuel A. Stouffer, to investigate these questions. The committee in turn commissioned a study on interviewing under the direction of Herbert H. Hyman and one on sampling under the direction of Fredrick F. Stephan and Phillip McCarthy. The results were Hyman's *Interviewing in Social Research* (1954) and Stephan and McCarthy's *Sampling Opinions* (1958). Hyman's book not only brought together what was then known about interviewing, it also reported on a series of careful experimental and observational studies of sources of error in interviews, and their control. This book had a great influence on the work of survey agencies and on the teaching of survey research methods. Hyman also produced another influential book, *Survey Design and Analysis* (1955), that presented a series of detailed case studies of problems encountered in social research. This book grew out of Paul F. Lazarsfeld's well-known project at Columbia, designed to produce materials suitable for advanced training in social research. An equally important book from the Columbia project was Paul F. Lazarsfeld and Morris Rosenberg, *The Language of Social Research* (1955), which emphasized the use of partialling to control for the influence of intervening variables in studying causal relationships, and of contextual analysis to separate individual and group effects (see also Kendall & Lazarsfeld 1950 and Lazarsfeld & Menzel 1961).

The Stephan & McCarthy book (1958) describes the relationship between sampling and other components of survey design, the problems raised when methods do not conform to the underlying mathematical theory, and finally, the problems encountered in actually designing a sample survey and putting it into operation. This book was by no means a primer; it was quite influential in survey research operations and was widely used in survey research methods courses. Other important books on sampling during the period were William Edwards Deming, *Some Theory of Sampling*, (1950) and Morris H. Hansen, William Hurwitz, and William Madow, *Sample Survey Methods and Theory*, (1958).

Mention should also be made of the book by Marie Jahoda, Morton

Deutsch, and Stewart Cook, *Research Methods in Social Relations* (1951), which covered research design, observational techniques, survey methods, content analysis, measurement, and data analysis. This book, sponsored by the Society for the Psychological Issues, was widely used in introductory courses in research methods in social psychology. A more advanced text covering much the same subject matter was *Research Methods in Social Sciences* (1953), edited by two prominent social psychologists, Leon Festinger and Daniel Katz. A book used by sociologists teaching research methods in social psychology was *Sociological Studies in Scale Analysis* (1954) by Riley et al. Another important book is R. Freed Bales's *Interaction Process Analysis* (1950) which provided sociological social psychologists with a system that enabled them to observe and rate the behavior of members of small groups. This system was widely adopted by younger sociologists and produced a generation of social psychologists who continue to work on important problems of individual and group behavior.

Considerable progress was also made in the measurement of social psychological variables during this period. Of course, even before the period began, L. L. Thurstone (1928), Rensis Likert (1932), and others had developed useful techniques for scaling attitudes, opinions, and similar social psychological constructs. But early in this period (during World War II) Louis Guttman (1944, 1950) developed Scalegram Analysis for determining rank order. This technique came to be known as Guttman Scaling. Scalegram Analysis, which is easy to accomplish and produces readily understandable results, was widely adopted. It doubtless was a great stimulus to research on attitudes and to studies of attitude change, which was a popular topic during this time of great concern with intergroup relations. Guttman Scaling soon replaced the earlier techniques for scale and test construction and was used to measure a wide range of social science variables. Mention must also be made of Paul F. Lazarsfeld's development of Latent Structure Method (1950) by which the manifest relations between any two items in a questionnaire can be accounted for by a simple set of latent classes, and only by this set. This was an important contribution to scaling theory although it did not gain widespread use by social psychologists.¹⁰

¹⁰During the later part of this period social psychologists began to develop scales and indexes by the use of factor analytic methods. Computer programs were developed to factor analyze and assign factor weights to a large number of items with great speed; this made scaling a quick and cheap process. Scales were produced to measure almost any social psychological variable that anyone could wish for. Unfortunately, according to Otis Dudley Duncan (1984a: 119-155) who provides a number of examples, most of these scales do not meet even minimum measurement requirements and hence they produce misleading results. The matter is made still worse when the numbers produced by these scales are subjected to complex statistical analysis. Duncan concludes that until we are ready to do the hard thinking and careful analysis that the methods developed by Georg Rasch (1968; Perline, et al 1979; Duncan 1984b) require, there is little hope for adequate measurement scales in social psychology or other social sciences for that matter.

Probably the greatest area of advance in this period was in the use of computer technology and methods. We started the period using counting sorters for most of our research. I remember the hours that my wife and I spent in 1938 feeding IBM cards into a counting sorter to get the numbers I used in the analysis of the 123 items from which I selected the 36 most diagnostic ones that finally comprised the farm family socioeconomic status scale (Sewell 1940). By the early 1950s, however, we had computers that, although crude by present day standards, had sufficient speed and storage capacity to enable us to do some quite complicated multivariate statistical analysis. For example, my colleagues and I were able to do a factor analysis of a set of 38 child-training practices to test hypotheses concerning the psychoanalytic claim that the mother's child-training practices reflect her unconscious acceptance or rejection of her child (Sewell et al 1955). Incidentally, the results failed to confirm the hypothesis. The computer proved to be very useful to social psychologists in multivariate cross-tabular analysis based on large samples, such as those my colleagues and I used to parse out the influence of social background variables on educational and occupational aspirations (Sewell et al 1957). Improvements in computer technology also made it possible for social psychologists to begin large-scale longitudinal and panel surveys. One was the Wisconsin study of social and psychological factors in the educational and occupational aspirations and attainments of over 10,000 students who graduated from high school in 1957. (See Sewell & Hauser 1975 for a summary of the early work on this project). Finally, by the time this period came to a close many new computer programs enabled social psychologists to use quite advanced mathematical statistical models in their research. Of course, social psychologists were not the major contributors to these mathematical and statistical techniques or to computer technology but, like other scholars, they were quick to adopt such techniques once they became available.

In closing I must say that although there were important improvements in the research methods used during this period, their main effect was to increase the reliability of our observations rather than to extend our powers of observation. New computers and computer programs did help us to sort out some of the complexities of social psychological behavior that would be almost impossible with earlier techniques. However, none of this was sufficient to bring about major theoretical breakthroughs to fuel great advances in social psychology.

Summary and Conclusions

Unfortunately, the rather modest developments that took place in social psychological theory and methods during its Golden Age were not sufficient to serve as the basis for a new interdisciplinary field. This was particularly

true because of social psychology's weak position in the university structure and the inadequate funding available from university and federal sources. Contrast this with the success of the interdisciplinary programs in the natural sciences—particularly with those in molecular biology, where tremendous theoretical breakthroughs, stemming from the work of Watson, Crick, and Wilkins on the structure of DNA, provided the stimulus for a whole new approach to biological studies.¹¹ This, plus the perfection of powerful new instruments for observation and measurement (such as the electron microscope and several complex devices and techniques for studying large and small molecules) spawned complex research problems that could only be solved by bringing together the skills and knowledge of physicists, chemists, geneticists, bacteriologists, zoologists, and botanists. Usually it was the younger scientists in these fields who were willing to engage in this joint effort and to learn the new techniques necessary for success in solving new problems. In the early years of the new programs, most of the scientists involved maintained their departmental connections but did their research in molecular biology teams. The level of cooperation of the parent departments with the interdisciplinary programs was not uniformly high in these years, but there was no great departmental resistance because adequate funding was available to permit other scholars in the departments to continue their established research programs. At the same time there was plenty of new money for the support of molecular biology. In fact NIH and NSF were so anxious to promote interdisciplinary programs in molecular biology that they were willing to provide funds for new buildings, laboratories, and equipment as well as salary support for faculty members and ample stipends for pre- and postdoctoral trainees. Over the years support has continued at high levels for both training and research in molecular biology. In several instances molecular biology has been granted full departmental status and in all instances has had the power to set its own graduate requirements and to grant its own PhD degrees.

One cannot help but wonder what would have happened if generous support from the universities and the federal funding agencies had been available to

¹¹My comments on molecular biology are based on an interview with Robert M. Bock, professor of biochemistry and molecular biology and dean of the graduate school, University of Wisconsin-Madison. The Wisconsin program in molecular biology (now called Cellular and Molecular Biology) began in 1952 with five professors from biochemistry, genetics, and physics, with support from the Graduate School, NIH, and NSF. Funds for a new ten-story building with completely equipped laboratories and offices were provided from federal sources. The program now includes 82 professors from 17 departments. All of its 100 or more graduate students are guaranteed three years support from University fellowships, or NIH traineeships or Research Assistantships. Teaching costs are now borne by the University. Research support comes mainly from federal sources. The program has never sought departmental status and maintains great flexibility in its research and teaching functions.

interdisciplinary programs in social psychology. Probably, no great new theoretical breakthroughs would have occurred or powerful new instruments or techniques of research would have been developed. The nature of social psychological phenomena makes such developments very difficult. But adequate and appropriate funding would have increased the probability of important developments in these areas and would most certainly have made greater progress in the improvement of social psychological theory and methods. I am confident that we would have been turning out more good research and more well-trained social psychologists had sociologists been collaborating with psychologists and other social scientists in interdisciplinary social psychology research and training programs. We might have been making much greater contributions to the understanding of some of the common social problems of our time.¹² This in turn probably would have led to greater support for research and training in social psychology.

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Literature Cited

- Bales, R. F. 1950. *Interaction Process Analysis: A Method for the Study of Small Groups*. Reading, Mass: Addison-Wesley
- Bowers, R. V. 1967. The military establishment. In *The Uses of Sociology*, ed. P. F. Lazarsfeld, W. H. Sewell, H. L. Wilensky, pp. 234-74. New York: Basic Books
- Deming, W. E. 1950. *Some Theory of Sampling*. New York: Wiley
- Duncan, O. D. 1984a. *Notes on Social Measurement: Historical and Critical*. New York: Basic Books
- Duncan, O. D. 1984b. Rasch measurement in survey research: Further examples and discussion. In *Surveying Subjective Phenomena*, ed. C. F. Turner, E. Martin, New York: Russell Sage Found.
- Festinger, L., Katz, D., eds. 1953. *Research Methods in Social Science*. New York: Dryden
- Guttman, L. 1944. A Basis for Scaling Qualitative Data. *Am. Sociol. Rev.* 9:139-50
- Guttman, L. 1950. The basis for scalegram analysis. In *Measurement and Prediction: Studies in Social Psychology in World War II*, S. A. Stouffer, et al, 4:51-65. Princeton, NJ: Princeton Univ. Press
- Hansen, M. H., Hurwitz, W. N., Madow, W. G. 1953. *Sample Survey Methods and Theory*. New York: Wiley
- House, J. S. 1977. The three faces of social psychology. *Sociometry* 40:161-77
- Hyman, H. H. 1954. *Interviewing in Social Research*. Chicago: Univ. Chicago Press
- Hyman, H. H. 1955. *Survey Design and Analysis Principles, Cases & Procedures*. New York: Free Press
- Jahoda, M., Deutsch, M., Cook, S. W. 1981. *Research Methods in Social Relations, Parts 1 and 2*. New York: Dryden

¹²For a helpful discussion of some of the important social problems that might be better studied by interdisciplinary effort, see House 1977:172-74

- Kendall, P. L., Lazarsfeld, P. F. 1950. Problems of survey analysis. In *Studies in Scope and Methods of the American Soldier*, ed. R. K. Merton, P. F. Lazarsfeld, pp. 133-96. Glencoe, Ill: Free Press
- Krech, D., Ballachey, E. 1948. *Study of a Social Survey: U.S. Strategic Bombing Survey (Japan)*. Silabus Series T. G. Berkeley: Univ. Calif. Press
- Lazarsfeld, P. F. 1950. The logical and mathematical foundations of latent structure analysis. In *Measurement and Prediction: Studies in Social Psychology in World War II*, ed. S. A. Stouffer et al, 4:362-412. Princeton: Princeton Univ. Press
- Lazarsfeld, P. F., H. Menzel 1964. On the relation between individual and collective properties." In *Complex Organizations: A Sociological Reader*, ed. A. Etzioni, pp. 426-35. New York: Holt, Rinehart & Winston
- Lazarsfeld, P. F., Rosenberg, M. eds. 1955. *The Language of Social Research*. Glencoe, Ill: Free Press
- Likert, R. 1947. Foreword v. In *Readings in Social Psychology*, ed. T. M. Newcomb and E. Hartley. New York: Holt
- Likert, R. 1932. A technique for the measurement of attitudes. *Arch. Psychol.* No. 140
- Lindzey, G., Aronson, E. eds. 1968-1969. *The Handbook of Social Psychology*. 5 volumes. Reading, Mass: Addison-Wesley. 2nd ed.
- Merton, R. K. 1949. *Social Theory and Social Structure: Toward the Codification of Theory and Research*. Glencoe, Ill: Free Press
- Morris, C. W. ed. 1934. *Mind, Self and Society: From the Standpoint of a Social Behaviorist*. Chicago: Univ. Chicago Press
- Newcomb, T. M., Hartley, E. L. eds. 1947. *Readings in Social Psychology*. New York: Holt
- Perline, R., Wright, B. D., Weiner, H. 1979. The Rasch model as additive conjoint measurement. *Appl. Psychol. Measur.* 4: 1-7
- Rasch, G. 1968. An individualistic approach to item analysis. In *Readings in Mathematical Social Science*, ed. P. F. Lazarsfeld, N. W. Henry, pp. 89-108. Cambridge, Mass: MIT Press
- Riley, M. W., Riley, J. W. Jr., Toby, J. eds. 1954. *Sociological Studies in Scale Analysis*. New Brunswick, NJ: Rutgers Univ. Press
- Sewell, W. H. 1940. *The Construction and Standardization of a Scale for the Measurement of the Socio-Economic Status of Oklahoma Farm Families*. Stillwater, Ok: Agric. Exp. Stat.
- Sewell, W. H. 1988. *The changing institutional structure of sociology and my career*. In *Sociological Lives: Social Change and the Life Course*, ed. Matilda W. Riley, 2:119-43. Newbury Park, Calif: Sage Publ.
- Sewell, W. H., Haller, A. O., Straus, M. A. 1957. Social status and educational and occupational aspirations. *Am. Sociol. Rev.* 22:67-75
- Sewell, W. H., Hauser, R. M. 1975. *Education Occupation and Earnings: Achievement in the Early Career*. New York: Academic Press
- Sewell, W. H., Mussen, P. H., Harris, C. W. 1955. Relationships among child training practices, *Am. Sociol. Rev.* 20:137-48
- Stephan, F. F., McCarthy, P. J. 1958. *Sampling Opinions: An Analysis of Survey Procedure*. New York: Wiley
- Stouffer, S. A. et al. 1949. *The American Soldier: Studies in Social Psychology in World War II* 4 vols. Princeton, NJ: Princeton Univ. Press
- Stryker, S. 1987. The vitalization of symbolic interactionism. *Soc. Psychol. Quarterly* 50:83-94
- Thurstone, L. L. 1928. Attitudes can be measured. *Am. J. Sociol.*, 33:529-54
- US Strategic Bombing Survey. 1946. *The Effects of Strategic Bombing on German Morale*. Washington, DC: USGPO
- US Strategic Bombing Survey. 1947. *The Effects of Strategic Bombing on Japanese Morale*. Washington, DC: USGPO