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Some Reflections on the Golden Age of Interdisciplinary Social Psychology

WILLIAM H. SEWELL*

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

For the most part I agree with House's formulations and conclusions, although I still prefer the traditional label "social structure and personality" to his term "psychological sociology" to describe what most of us do. Like Sheldon Stryker (1987), I also see a somewhat less clear distinction between the

present stance of symbolic interactionism and of social structure and personality than House saw a decade ago, particularly 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 the background for my own reflections on what some consider the Golden Age of interdisciplinary social psychology. I wish to elaborate on how it came about and on 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 eventual return to its original disciplinary moorings when the interdisciplinary arrangements faltered. Therefore, I wish to reflect on the possible reasons why these programs failed to become incorporated into the institutional structure of our universities, in contrast to several postwar interdisciplinary programs in the natural sciences.

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 participated in almost every aspect of it, including its successes and its failures. Thus I feel emboldened to share my reflections. Like most of the 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 really were few places where one could obtain much training in social psychology in the mid-1930s; Minnesota, where I did my Ph.D. in sociology was not one of them.¹ By

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¹ At 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 took 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

the time I was called to service in World War II as a reserve officer in the U.S. 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; there, 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 soldiers' adjustment to military life during World War II. Much of this research was published in the famous four-volume work *The American Soldier* (1949) under Stouffer's leadership. I also became acquainted with Rensis Likert, and several of his colleagues, who directed the Program Surveys Division of the Department of Agriculture. That division had conducted 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 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 *U.S. Strategic Bombing Survey* 1946.) This group included some members of the team that had conducted the German survey and several other social scientists. We drew up preliminary plans for the Japanese survey, including a clearly formulated conceptualization of the aims of the survey, a list of the factors that were to be regarded as the major components of morale, and a series of questions designed to elicit these components.

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 began immediately to review the purposes and the design of the survey, and we 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 took our teams of interviewers into the field and completed the interviewing in three months.

In another month, after returning to Washington, we had developed our coding scheme for the interviews, coded the materials, and completed the statistical processing of the data. By that time several of our members had been released from service and had returned to their academic posts. Those of us who remained, with assistance from some of our departed colleagues, wrote the final report. 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 (*U.S. 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 attention was paid to the disciplinary origin of the idea. I was also greatly impressed with the ability of an interdisciplinary team to mount a study of this complexity and to complete it so expeditiously.⁴ My

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.)

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

³ The 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).

⁴ I wish it were possible to show how the work of the team was influenced by its interdisciplinary composition.

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 (NIH), and later the National Science Foundation (NSF) also supported 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. Throughout that 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 some of the more prominent. These centers were committed primarily 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 missions.⁶ During this period, too, the interdisciplinary research program in social psychology was developed in the Socioenvironmental Laboratory of the National Institutes of Health under the direction of John Clausen and later Melvin Kohn.

Certainly graduate students showed no lack of interest in the interdisciplinary graduate training programs. Social psychology was a challenging intellectual field; students were anxious to learn more about it, including what disciplines other than their own could 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, The National Institute of General Medicine, the National Science Foundation, and other agencies that were interested in increasing the supply of persons trained in social psychology. Thus it seemed that all of the conditions were right for the development and the sustained growth of a new interdisciplinary field of social psychology.

For a decade great progress was made, particularly in the interdisciplinary training programs at Michigan under the leadership of Theodore Newcomb, with a faculty including Angus Campbell, Dorwin Cartright, J.R.P. French, William Gamson, Daniel Katz,

I am sure that it was influenced in this way, 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 took notes 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.

⁵ Among 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.

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

Robert Kahn, Herbert Kellman, Helen Peak, Albert Reiss, Guy E. Swanson, and Howard Schuman, and at Harvard in the new Department of Social Relations headed by Talcott Parsons, with a faculty including Gordon Allport, R. Freed Bales, George Homans, Alex Inkeles, Clyde Kluckhohn and Florence Kluckhohn, Gardner Lindzey, Frederic Mosteller, Richard Solomon, and Samuel Stouffer.⁷ The growth was less spectacular in other universities, but by no means insignificant. With such a good start, why should the interdisciplinary programs in social psychology have nearly vanished by the late 1960s without ever becoming established in the institutional structure of American universities?

WHY DID INTERDISCIPLINARY PROGRAMS IN SOCIAL PSYCHOLOGY FAIL?

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

The Threat to the Departmental Structure

First I turn to the traditional institutional structure of the American university and to the relatively weak position of the social sciences in that structure. It can hardly be debated that the physical and biological sciences, in both their pure and their applied branches, are in a superior position to the social sciences and the humanities in most of our universities. This is true with respect to the funds allocated to research, to buildings, to equipment, and to salaries, and particularly

to new ventures such as research centers and training programs. Because of this situation the existing social science departments have to fight 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 local and national sources. Thus social science departments tend to be much less supportive of interdisciplinary programs unless additional funds can be brought in from the outside, particularly when the program is likely to require faculty, scholarships, space, equipment, and operating funds, which are always in short supply and which 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, and secretarial and clerical staff were expected to come from outside grants. Federal funds to meet these costs, although available, were usually inadequate. They were granted for a relatively short period—usually three to five years—and there was no assurance that they would be renewed. Under these conditions it is not surprising that the parent departments found interdisciplinary social psychology programs threatening.

Wisconsin is a classic example of this point. 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 psychology. We met for several months and developed a program consisting mainly of social psychology courses 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 assumed further that the group would seek outside funds for subsidy of graduate students and other costs.

When the plan was presented to the

⁷ To 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 by the departments of sociology and psychology and never had the status of a separate department.

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

departments, neither sociology nor psychology approved of our request for a joint major in social psychology. The most that either department would approve was an interdepartmental minor with psychology requiring that students who were not psychology majors take the psychology proseminar. Sociology established similar requirements for psychology students. The graduate school approved of these arrangements, but the graduate students did not find the program attractive and within a few years it ceased to exist. This did not mean that no interdisciplinary training in social psychology could be found thereafter 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 program was typical, but I know that several others suffered from less than adequate support from their departments and deans. In some institutions interdisciplinary programs prospered only as long as their enthusiastic and powerful founders participated in the program and supported it strongly in their departments. When they were replaced, however, 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 soon were abandoned.

Lack of Funding

Another factor in the decline of interdisciplinary social psychology programs was lack of 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 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 (NIMH). Yet the funds for the support of social sciences in these agencies never accounted for more than 10 percent of

their research budgets. Social psychology received 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 fields involved social psychological research, but in a sense these funds were competing with those for interdisciplinary social psychology research and training programs.

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; they were modest in amount and limited in duration; they provided for only a small number of research assistants or trainees; they did not provide facilities; and usually supported only a limited portion of the salary of the principal investigator or director of the program. As a consequence, these programs were supported inadequately by both the universities and the federal agencies, 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 tended to be underused as a source of interdisciplinary research training in social psychology. These not-for-profit organizations were connected only loosely 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 to 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, but generally this was possible only when faculty members had outside grants for these purposes. Much the same situation prevailed at other university-sponsored survey research centers, with the notable 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 expe-

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

riences in the Bureau's ongoing social psychological research projects.

MODEST ADVANCES IN THEORY

In speculating about the fate of interdisciplinary social psychology, I must point out that no powerful theoretical breakthroughs took place during this period (or for that matter, since then), which might have served as a stimulus to exciting new theoretical developments or new research areas. Advances were made in social psychological theory, but they were modest; although some codification took place, nothing approaching a unified body of social psychological 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; fruitful work was 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, pp. 85-87), consisted of general orientations toward problems and toward the 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 not 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 trend is reflected clearly in the selection of articles included in 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: 1) it must adhere to rigorous canons of scientific procedure; 2) it must draw hypotheses from the relevant psychological and social sciences; and 3) it must bring these hypotheses to bear on systematic research on problems of human importance (Likert 1947, p. v). This final requirement characterized much of the research done during this period. Furthermore a great volume of research was done, enough to fill professional journals in the field and hundreds of pages in the more general journals of the various social science disciplines. 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. Only a small fraction of this research, however, resulted from interdisciplinary efforts.

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

ADVANCES IN RESEARCH METHODS

During this period of great research activity a good deal of 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 a study on sampling under the direction of Frederick 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 known then about interviewing but also reported on a series of careful experimental and observational studies of sources of error in interviews and about 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), which 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 Lazarsfeld and Morris Rosenberg's *The Language of Social Research* (1955), which emphasized the use of partialing to control for the influence of intervening variables in studying causal relationships and the use of contextual analysis to separate individual and group effects (see also Kendall and Lazarsfeld 1950; Lazarsfeld and Menzel 1961).

Stephan and McCarthy's (1958) book discussed 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 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 used widely in survey research methods courses. Other important books on sampling during that period were William Edwards Deming's *Some Theory of Sampling* (1950) and Morris H. Hanson, William Hurwitz, and William Madow's *Sample Survey Methods and Theory* (1958).

I should also mention 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 used widely in introductory courses on 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. It is quite likely that most practicing social psychologists cut their teeth on either or both of these texts.

Another book that must be mentioned 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 adopted widely by younger sociologists and produced a generation of social psychologists who continue to work on important problems of individual and group behavior.

Considerable progress also was made during this period in the measurement of social psychological variables. 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. During World War II Louis Guttman (1944, 1950) developed scalogram analysis for determining rank order; this technique came to be known as Guttman scaling. Scalogram analysis, which is easy to accomplish and produces readily understandable results, was adopted widely. Doubtless it was a great stimulus to research on attitudes and to studies of attitude change, 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. I must also mention Paul F. Lazarsfeld's development of latent structure method (1950), by which the manifest relationships 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 achieve widespread use by social psychologists.¹⁰

¹⁰ During 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

Probably the greatest area of advancement 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 obtain the numbers I used in the analysis of many items to select the 36 most diagnostic ones which finally constituted the farm family socioeconomic status scale (Sewell 1940). By the early 1950s, however, we had computers that were crude by present-day standards but had sufficient speed and storage capacity to permit some quite complicated multivariate statistical analysis. With the help of a computer, 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, Mussen, and Harris 1955). (Incidentally, the results failed to confirm the hypotheses.)

The computer proved very useful to social psychologists in multivariate cross-tabular analyses based on large samples, such as those my colleagues and I used to partial out the influence of social background variables on educational and occupational aspirations (Sewell, Haller, and Straus 1957). Improvements in computer technology also enabled social psychologists to begin large-scale longitudinal and panel surveys. One such survey was the Wisconsin study of social and psychological factors in the educational and occupational aspirations and attainments of more than 10,000 students who graduated from high school in 1957. (See Sewell and Hauser 1975 for a summary of the early work on this project.) Finally, by the time this period came to a close, many computer

programs had been developed which enabled social psychologists to use quite advanced mathematical statistical models in their research. 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 them when they became available.

Although important improvements were made in the research methods used during this period, their main effect was to increase the accuracy of our observations rather than to extend our powers of observations. The new computers and computer programs did help us to sort out some of the complexities of social psychological behavior that would have been almost impossible to analyze with earlier techniques. None of this, however, was sufficient to bring about major theoretical breakthroughs that would fuel great advances in social psychology.

SUMMARY AND CONCLUSIONS

Unfortunately, the rather modest developments that took place in social psychological theory and methods during the Golden Age were not sufficient to serve as the basis for the development of a new interdisciplinary field. This was true particularly because of social psychology's weak position in the university structure and because of the inadequate funding available to this field from university and federal sources. Contrast this situation with the success of the interdisciplinary programs in the natural sciences, particularly 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

large number of items with great speed, thus making scaling a quick, cheap process. Scales were produced to measure almost any social psychological variable that anyone could wish for. Unfortunately, according to Otis Dudley Duncan (1984a, pp. 119-55), who provides a number of examples, most of these scales do not meet even minimum measurement requirements and hence produce misleading results. To make matters still worse, 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 George Rasch (1968, Duncan 1984b, Perline et. al. 1979) require, there is little hope for adequate measurement scales in social psychology or in other social sciences.

¹¹ My 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 10-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, NIH traineeships, or research assistantships. Teaching costs are now borne by the university. Research support comes mainly from federal

This success, 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 be solved only by bringing together the skills and the 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 that were necessary for success in solving the 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 between the parent departments and the interdisciplinary programs was not uniformly high in the early years, but there was no great departmental resistance because adequate funding was available to permit other scholars in those 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 eager 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; in all instances it has had the power to set its own graduate requirements and to grant its own Ph.D. degrees.

One can not help wondering what would have happened if such generous support from the universities and the federal funding agencies had been available to interdisciplinary programs in social psychology. Probably no great new theoretical breakthroughs would have occurred, and no powerful new instruments or techniques of research would have been developed. The nature of social psychological phenomena makes such developments very difficult. Yet adequate and appropriate

funding would have increased the probability of important developments in these areas and certainly would have made for greater progress in the improvement of social psychological theory and methods. I am confident that we would have produced more challenging research and more well-trained social psychologists if we had collaborated with psychologists and other social scientists in interdisciplinary social psychology research and training programs. Moreover, we might well have made much greater contributions to the understanding of some of the common social problems of our time.¹² This development, in turn, probably would have led to greater support for research and training in social psychology.

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sources. The program has never sought departmental status, and maintains great flexibility in its research and teaching functions.

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

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