Journal Title: Survey research in the United States: roots and emergence 1890-1960

Volume:

Issue:

Month/Year: 1987

Pages: DK

Article Author: Converse, Jean M., 1927-

Article Title: Table of Contents, Preface, Introduction and Conclusion

OCLC Number: 12722783

ISSN/ISBN Number: 9780520053991
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Acknowledgments

The collective generosity of many people made this study possible. I am especially indebted to the individuals whom I interviewed for their considered reflections on survey research and social science and for their many helpful suggestions. I am also grateful to many others who answered queries and suggested ideas, informants, and source materials. The list of these gracious individuals is long:

Acknowledgments

A number of individuals made useful critical comments on various parts of the manuscript: Allen H. Barton, Martin Bulmer, Don Cahalan, Charles F. Cannell, Dorwin P. Cartwright, Otis Dudley Duncan, Jack Elinson, Charles Y. Glock, Patricia L. Kendall, Francis Keppel, Leslie Kish, William Kruskal, Louis Moss, Stanley Presser, Howard Schuman, Paul B. Sheatsley, Tom W. Smith, Charles F. Turner, and Stephen B. Withey. Comments by the anonymous reviewers of the National Science Foundation and the University of California Press were also very helpful. One's counselors are rarely of unanimous mind, but I profited greatly from the quality and the diversity of their criticisms. These readers are, of course, innocent of those errors of judgment, interpretation, or omission that remain in the manuscript.

To the keepers of records and archives who made their treasure easily accessible, I feel very indebted. My special thanks go to Adye Bel Evans, librarian of the Institute for Social Research, University of Michigan, who was ever-patient and resourceful in tracking down elusive materials. F. Thomas Juster, Director of the Institute for Social Research, authorized my use of archival materials. Through Norman Bradburn and Robert T. Michael, successive directors of the National Opinion Research Center, and Patrick Bova, librarian extraordinary, I was given gracious access to NORC archives and documents. By permission of the Harvard University Archives and Ann Stouffer Bisconti, I consulted Samuel Stouffer's professional papers at the Nathan Pusey Library, Harvard University. Patricia L. Kendall authorized my use of Paul F. Lazarsfeld's interviews in the Oral History Collection, Butler Library, Columbia University, and the Oral History Research Office granted me permission to use the George Gallup interview in the same collection. Allen H. Barton and Phyllis Sheridan pointed me to a special collection of materials from the Bureau of Applied Social Research at Columbia. Marion E. Jemmott, Secretary of Columbia University, authorized my use of the University Archives, and Sarah Vos of that office was especially helpful in my search for materials. The American Association for Public Opinion Research granted me permission to use interviews with George Gallup, Elmo Roper, and Archibald Crossley. The Bentley Historical Library of the University of Michigan made available the professional papers of Rensis Likert. The Department of Special Collections of the Joseph Regenstein Library, University of Chicago, granted me permission to use the collected papers of Louis Wirth and Ernest W. Burgess.

I would also like to thank the knowledgeable and helpful archivists in three collections: Michael T. Ryan and Daniel Meyer of the Department
of Special Collections, Joseph Regenstein Library, University of Chicago; Mary Jo Pugh and Nancy Bartlett, Bentley Historical Library, University of Michigan; and specialists at the National Archives in Washington, D.C., and the Washington National Records Center in Suitland, Maryland: Richard Crawford, Mike Miller, Donald Mosholder, Edward J. Reese, Gibson B. Smith, and Helen Ulibarri.

This project was supported by fellowships of the Earhart Foundation of Ann Arbor, Michigan, and research grants of the National Science Foundation (SES 78–11490 and SES 80–28835). I am grateful to Antony L. Sullivan, Director of the Earhart Foundation, and Ronald Overmann, Assistant Program Director of the History and Philosophy of Science Program, National Science Foundation, for their support, encouragement, and good counsel. Stanley Holwitz, Assistant Director of the University of California Press, Los Angeles, and Shirley Warren, Principal Editor, brought their special competence, patience, and good cheer to this endeavor.

Finally, I owe special thanks to Michigan friends and colleagues: to Howard Schuman, for his facilitation of this project and his suggestions at many points over the long course and expansive growth of this manuscript; to Philip E. Converse, A. F. K. Organski, and Stanley Presser for their special encouragement; to Patricia Preston, Michael Bourgon, and James Caffrey for their patient and efficient labors in permission and verification.

J. M. C.

August 1986
Introduction

Themes and Perspectives

This book is about the historical development of “survey research”—an instrument that serves as something of a social telescope in the social sciences. In the pages that follow, social scientists consider other metaphors for this new instrument: should it be called a social microscope, perhaps, or a spectroscopic, or a “demoscope” or social barometer for recording the ups and downs of political and social tensions all over the globe? The image of the “far-seeing” telescope is more apt than the others, for one uses the survey telescope to scan some rim of the social world, without any real hope of resolving great detail, looking instead for large shapes of social geography, movements of populations, flows of information, opinion, and feeling.

The survey has ancient ancestors. As a straight population count to raise taxes or muster soldiers, the survey can be traced back at least 2,000 years and probably much further. The more complex survey of our own time has been forged in the twentieth century as an instrument of special power for viewing mass populations in industrialized societies, especially in their character as social facts, political publics, and economic markets. It is an instrument serving some purposes of three basic constituencies: elites, social scientists, and the mass public.

Elites are intensely interested in the survey’s yield of information for prediction, planning, profit, and control; for mustering mass support for political programs; and for mobilizing resources to contend against other elites.¹ Social scientists use the sample survey for gathering descriptive data, for making inferences to populations, for building blocks of social theory, and for advancing their own disciplinary and intellectual career interests. Finally, the mass public consumes survey data as something of a mirror of its own time, of its national political opinions, beliefs, and experiences.
Polls and surveys took root in Western societies that were becoming economically industrialized and urbanized, increasingly literate, and politically democratic—for good reason. Industrial society offered the technology and transportation that made a mass constituency accessible to interview in the first place. (Polls and surveys continue to spread to nonindustrialized countries, but they are not a home-grown product there, where physical and cultural conditions offer much resistance and expense.) The literacy distributed by mass public education and the mass media has made a national public intellectually accessible as sources and as consumers of poll/survey data. Most important, a democratic governmental structure has made it politically strategic for powerful groups to gather, assess, represent (or misrepresent), try to influence, and invoke mass opinion.

There is much to support Beniger’s contention that survey research developed as business and government became technically able to communicate to mass audiences and then grew interested in “receiving feedback from this same audience, in order better to control its behavior.” As he writes, survey research arose

[not] from a need to speak one’s mind . . . but rather from the need to find out what is on people’s minds—whether they intend to speak them or not. Attitudinal research arises not out of any need of the holders of attitudes, that is to say, but rather from the needs of an audience interested in the potential exploitation of those attitudes.²

The needs referred to are those of business and government. That survey research did not well up from The People seems pretty certain. Yet it is also the case that polls and surveys have been a natural for democratic political systems, consonant with and reflective of official democratic structures and values. American elites cannot simply and safely pursue their own ends by force or intimidation, without paying some heed to “the holders of attitudes” in their mass constituencies. Whether such elites are seen as the vanguard of democratic reform and liberation or as the oligarchic rear guard of manipulation or repression depends on one’s ideological tastes, utopian hopes, and judgment of the constraints of social reality. In any case, surveys are not useful to established elites alone, as public opposition to the Vietnam war demonstrated, to cite one case. It seems likely indeed that polls and surveys have become as acceptable, ubiquitous, and significant as they have in American life because they can serve some purposes of all of the major constituencies—estab-
lished elites of business and government, aspiring political elites, scientific experts, and large parts of the mass public. On to more specific assumptions.

The history of survey research, like that of the sciences in general and of the arts as well, can be seen from one of two perspectives: internalist and externalist. Now that survey research has achieved a niche in university culture, survey researchers tend to view their own activity from an internalist perspective—that is, as theory and data-driven work carried out by scientists themselves—rather than from an externalist approach, which treats science as still another form of human culture that is most explicable in terms of its economic, political, and social contexts.5

Survey researchers know well enough that they are affected by their external environment. Like all sophisticated social scientists, they try to see the openings and dead ends of the funding environment, but they generally perceive these factors as the surround of their work rather than as the core of their scientific enterprise. They see their work as originating and cumulating in the unique intellectual values of the logic of ideas, the marshaling of data, the canons of evidence—the internal structure of science.

While survey research has taken on something of the character of a quasi-discipline rather than a full-fledged scientific discipline—an argument I shall make in the course of this book—its life history provides an example of the internalizing course of scientific disciplines conceptualized by Kuhn. In that analysis, the content of a science in its early years is close to commonsense concepts and needs of external, practical life. As the science matures, its content is increasingly shaped by the communication of scientists themselves, who become organized into a social system of their own, insulating each other from the language, problems, and—if they can manage it—the pressures of other realms. In Kuhn's view, the internalist perspective is eminently successful not because it actually does fully explain scientific development but because scientists become insulated in their own disciplinary cultures and thus usually think that it does.4 (When Daniel S. Greenberg put together his study The Politics of Pure Science, for example, some of the objects of his research were appalled: How in the world could there be a politics of science?)5) In this book I have tried to blend something of both the internalist and externalist perspectives. My account starts in the wider world of affairs and gradually focuses on the smaller world of science. In one sense, it becomes progressively narrowed and increasingly internal in the course of three sections in the book.
Organization of This Book

In part one (1890–1940), I have traced the origins of polls and surveys to three “ancestors,” two of which were engaged in the practical concerns of business and politics and one in university science. Toward the end of these years, with the crisis of the Great Depression, vast new surveys of national scope were mounted by federal agencies to assess national problems. The power of the new public opinion polls, by Gallup and the others, was not lost on certain actors in the federal administration nor on certain academic social scientists interested in attitude measurement. Survey researchers generally assume an internalist interpretation of these beginnings, seeing their ancestors in science as having been more weighty than those in either politics or business. I see the genealogy as the other way around.

Relegating these fields to the role of ancestor requires a brief apology, for their importance in their own right is thereby diminished. In the account of social surveys, for instance, I consider certain phases of their evolution up through the New Deal, but I do not follow the full story of social fact-gathering on up through 1960. With the exception of some developments of special relevance to survey research, such as probability sampling, I quite neglect the history of the U.S. Census as a subject in itself. Likewise, with commercial research. I have selected material about market research and opinion polls to show their commanding influence as forerunner, indirect facilitator, and sometime (uneasy) ally of survey research. Their wider political and cultural importance is thereby slighted, and their history truncated. The appearance of scientific ancestors is even briefer. The specific focus on attitude measurement is restricted largely to the 1920s and 1930s, although that field has of course thrived and burgeoned ever since. The history of inferential statistics is left largely unexplored, except—again—for those innovations bearing on probability sampling.

To excuse such shabby treatment of ancestors, I offer only my sense that fair treatment would open the horizon to a whole new cast of characters and events, perhaps requiring some three or four other books. To justify the concentration on such a small part of the forest, I offer only the conviction that survey research has become an intellectually powerful and influential part of the scientific terrain. I will proceed to a preview of parts two and three without such extended caveats.

In part two, which spans the years from 1935 to 1945, we see the process by which the example of the polls and the funds of the federal gov-
government facilitated the development of survey research. A protosurvey style of research came to the national scene on the arm of a government growing massively in power and responsibility, mandated first by the Great Depression and then by World War II. In the war years, especially, the expanding agencies of Washington were charged with gathering subjective data—opinions and morale factors that were deemed of consequence to the war effort. A shared community of interest between federal administrators and social scientists lasted for the duration. At the end of the war, congressmen suspicious of the Roosevelt administration and wary of outsiders' information about their own constituencies broke it up, cutting opinion research out of the federal budget.

The wartime experience served to enlarge the potential pool of academic talent for survey research. When most of the Washington social scientists went back home to their universities, some took with them a new professional interest in surveys as well as new technical expertise in putting such surveys together. As we shall see, they later came to share with scientists in general a new system of extracting financial support from the federal government.

Part three (1940–1960) was the period of establishment of survey research in the university. Social scientists interested in survey methods came to academic life (or came back to it) with experience in applied empirical research before and during World War II. As a group, most of the survey researchers were bent on trying to shed these externalist beginnings in order to join the community of scholars and chart an internalist future. Yet they felt the strain between the two cultures, needing the money of applied business and government while desiring the prestige and freedom of basic science.

Their organizations were a poor fit for academic life. Their "number crunching" violated the sensibilities of humanists and of theoretically inclined social scientists, and it threatened the security of the individual scholar. Their organizational division of labor evoked—and still does evoke—the noisy image of knowledge "factories." (While a physical telescope is understood to require capital investment and a complex division of labor, the same cannot be said for social telescopes.) The researchers required more money than universities or foundations were able or willing to find, and their reliance on clients outside the academy raised issues of business and government control of new knowledge itself and threatened the tranquility of the life of the mind. While the researchers often felt on the financial edge, beleaguered, and vulnerable, to some of their colleagues they appeared imperial, aggressive, and too rich by half.
Introduction

Their organizations were not perceived as resources for the university as a whole, on the order of museums, libraries, or (later) computer installations, but were felt by many as threats to the established university order.

The struggle of these researchers to establish a place in the university (and the university's struggle with them) is intertwined with two other stories: the researchers' efforts to consolidate some professional nucleus of their own, and their efforts to gain acceptance and influence in the established disciplines of social science. These subjects will take us, finally, to an evaluation of the contributions of the new survey organizations and an assessment of the particular powers and limits of survey research itself.

On Sources, Designs, and Second Thoughts

To construct this history, I have used archival documents, published and unpublished materials, and interviews—not structured survey interviews permitting quantifiable data, but informal ones without such a yield. I should have used both. Some more pieces of comparable quantifiable data would have enhanced this study, just as the definition of a sample and population would have. The unstructured interviews nevertheless have offered rich detail, interpretation, and useful links to documentary evidence. After the more extensive interviews have been fully edited and authorized, I hope to deposit transcripts of them in the archive being created by the University of Chicago and the American Association for Public Opinion Research. They are accounts provided by scientists of fine mind and superb training who are deeply interested in their work, alive enough to good professional gossip and intrigue, rich in their appreciation of others' research, and sometimes quite modest about their own. As Harry Levin recently observed, "There is an inevitable compulsion to make a lifetime of research appear orderly, as though one interest led naturally to another"—and that tendency may be even stronger in individuals' accounts of their research organizations. But most of my informants tried to resist the compulsion, too, as they gave some room to accident, error, incongruity, and comedy as well as to the sober-sided pursuit of orderly plans. Most would surely be delighted by a foreign scholar's recent struggle to express his appreciation for empirical research: "Why, I have learned that it is the very basement of social science!"
The quality of my own interviewing was inevitably variable. I interviewed some people too early, before I had read certain key writings or before I had defined certain parts of the study well enough to hone my questions down to the most cogent ones. In some cases, nevertheless, I managed to bring the right questions to the right person, and in all cases I learned a great deal about survey research from my informants, all of whom were gracious and helpful. I have listed them in the Acknowledgements.

One of my major objectives in personal interviewing was to try to scale the walls of the research organization I knew best, the Survey Research Center/Institute for Social Research at the University of Michigan, where I have been affiliated for the last ten years in various modest research and writing roles. I was intent on trying to absorb some of the local culture of the other founding survey organizations, notably the Bureau of Applied Social Research and the National Opinion Research Center. Whether I learned enough to escape the local culture and attain a more general perspective on the field is an evaluation that I myself cannot make.
The Academic Establishment of Survey Research: A Summary and Evaluation

What assumptions did sociology’s reformers [of the 1930s and 1940s] make about the nature of true science? However brilliant, the individual scholar could accomplish little. Scientific progress was made in “knowledge factories,” staffed not by intellectuals but by technicians, who need not be persons of considerable ability, for they employed precise, easily mastered techniques to fulfill their assigned tasks.

*Henrika Kuklick*

Survey analysis is much akin to artistic creation. There are so many questions which might be asked, so many correlations which can be run, so many ways in which the findings can be organized. . . . Beyond his technical responsibility for guaranteeing accuracy and honest statistical calculations, the real job of the study director is to select and integrate . . . to simplify but not gloss over, to be cautious without pettifoggery, to synthesize without distorting the facts, to interpret but not project his prejudices on the data. These, I submit, are ultimately aesthetic decisions, and the process of making these decisions is much like aesthetic creation.

*James A. Davis*

The Postwar Period: 1945–1960

*The End of an Era*

The year 1960 provided something of a watershed. Survey research organizations had been established in universities. There was new recognition for early leadership, some changing of the guard, new activity of a younger generation. And certain revolutionary changes had not happened.

In 1960, Lazarsfeld became president-elect of the American Sociolog-
ical Association. In 1956, Herbert Blumer had devoted his presidential address to a critique of "variable analysis," meaning Lazarsfeld's work very particularly.¹ Four years later, on his third try for the office, Lazarsfeld won his own place in the association hierarchy.² In 1959, the American Sociological Association approved the formation of a section on methodology. It was not a section on survey methodology, but survey researchers such as Patricia Kendall, Leslie Kish, Peter Rossi, and Shirley Star were prominent in its council.³ In 1959, Rensis Likert served as president of the American Statistical Association. This honor did not have quite the same symbolic meaning that a presidency of the American Psychological Association would have had—with the new prestige in that discipline for social psychology and subjective measures—but it was welcome recognition from statisticians, and ISR enjoyed its first presidency of a professional association.

By 1960, a changing of the guard was in progress. In that year, Clyde Hart retired and Samuel Stouffer died. Thus, with Harry Field's early death in 1946, three important figures of the wartime cohort of survey researchers were gone. The interest of certain others had waned. Stouffer had not been as active in survey research after the war as he had been during it, in any case (though he remained active in AAPOR), and Cantril and Lazarsfeld no longer had the same involvement in the survey field.

After the war, Cantril's organization, OPOR, had shrunk into a small office operation devoted to creating an archive for poll data, with no field capacity to collect new data. In 1955, it was discontinued, as Cantril resigned from Princeton to accept a lifetime research grant from Rockefeller for policy research. He continued to work on surveys—*The Politics of Despair* (1958), for example, analyzed the Communist voter from data collected by Gallup affiliates in France and Italy, and *The Pattern of Human Concerns* (1965) brought together data on human aspirations from a number of countries—but these works did not feature much survey data analysis and, with the exception of the "self-anchoring" scale, did not bring any new methodology or analysis. Cantril was the single most prolific contributor to *POQ* from the journal's beginning in 1937 until 1967 (the year in which he died), but these articles, with one exception, were all published before 1950. Thereafter, other interests, such as problems in perception ("transactional analysis"), claimed much of Cantril's attention in the 1950s and 1960s. His greatest influence on survey research was felt during the 1940s.⁴
Lazarsfeld’s impact on the formation of American survey research was more central and long-lasting, as he remained directly influential from the mid-1930s to the late 1950s, some twenty to twenty-five years, and indirectly beyond that period. Though he lived until 1976, his last major book in survey research was *The Academic Mind*, published in 1958; after 1960 he did not publish any more articles in *POQ*. Berelson noted in 1955 that Stouffer and Lazarsfeld had both effectively left public opinion work, an exodus he suspected was a response to the low prestige of the field. That may well be, though in the case of Stouffer we are left with merest speculation. In Lazarsfeld’s case, it is clear that he chafed at the low prestige of quantitative sociology and tried to change it. His interest in the history of quantification in social science was in part an effort to endow it with some of the commanding prestige that theory had in the history of sociology. Lazarsfeld had catholic interests in science and culture, however, and great capacities, and he may simply have moved on to some of the other things on his mind. The best truth of the matter may be in Jahoda’s borrowing of Isaiah Berlin’s bestiary: for whatever reasons, Lazarsfeld’s many interests made him a fox, fascinated by many things, rather than a hedgehog, working on one big thing (Talcott Parsons, for example, was considered a hedgehog). Merton did not proceed into much further survey work after his explorations into housing and medical sociology, the bulk of which were conducted in the 1940s and 1950s.

By 1960, many in the generation of social scientists that had worked on surveys during the war (e.g., Field, Hart, Stouffer, Cantril, Lazarsfeld, Merton, Likert, and Cartwright) were no longer active in survey work, through death or through the ascendance of other interests or responsibilities. Likert continued as director of ISR, but his intellectual work centered on organizational theory and research. Cartwright, director of the Research Center for Group Dynamics, did not continue survey work after the war.

Of that wartime generation, only Hyman and Sheatsley of NORC and Campbell, Katona, Katz, Cannell, and Kish of SRC pushed on in full pursuit of careers in survey research in the 1960s and 1970s, in collaboration with younger-generation scholars. Campbell undertook election studies with the collaboration of Cooper, Gurin, Miller, Converse, and Stokes. Katona continued work in psychological economics with Mueller, Klein, Lansing, and Morgan. These research programs, especially, gave to SRC/ISR the ascendancy in survey research in the 1960s and 1970s.
that the Bureau of Applied Social Research had had in the 1940s and 1950s. Lazarsfeld's influence on survey research continued through the work of some of his heirs at the Bureau, such as Barton, McPhee, and Glaser, and of some who had gone out from the Bureau, especially Coleman, Rossi, Lipset, and Glock. In 1960, Rossi became NORC's director. Glock and Lipset were now both at the University of California, Berkeley, where Glock had become director in 1958 of a new Survey Research Center—the name reflecting the influence of the Michigan group, but Glock himself reflecting the more direct influence of the Bureau.

The end of this era saw a golden yield of books from all three organizations based on primary or secondary analyses of survey data. From the Bureau, there were Merton, Reader, and Kendall, The Student Physician (1957); Lazarsfeld and Thielens, The Academic Mind (1958); and, all in 1960, Lipset, Political Man; Hyman, Political Socialization; Berelson, Graduate Education in the United States; and Klapper, The Effects of Mass Communication. At NORC, projects of Rossi, Coleman, and Davis which had been initiated in the late 1950s came to major publication in 1961, with Coleman et al.'s The Adolescent Society, Davis's Great Books and Small Groups, and a variety of articles by Rossi. For ISR, 1960 was a vintage year, with Gurin, Veroff, and Feld, Americans View Their Mental Health; Katona, The Powerful Consumer; and Campbell et al., The American Voter. And journal articles based on surveys had begun to flourish in all three organizations.

This published research was largely the fruit of the precomputer era. Data analysis was crafted with paper and pencil, desk calculators, the counter sort of Hollerith cards, and the IBM 101, which could be programmed by wiring a simple board to carry out various cross-tabulations. This technology was the most advanced equipment in the "machine rooms" of all three major organizations during most of these early years. Correlation, regression, analysis of variance, and factor analysis were only rarely carried out on the desk calculator. Giant computers, which began to be installed in major universities in the late 1940s and early 1950s, did not accommodate analytic routines for social science data until the late 1950s and early 1960s. A few intrepid social scientists, well counseled by their statisticians, set upon the giant central computers (such as the IBM 650) to wrest from them a few key analyses, such as some multiple regressions, but computer technology was still forbidding and inhospitable for social science use. The books and articles these researchers published around 1960 were, indeed, the last of the handcrafted wor
What Was Coming

Three new features were to come in the future. First, there would be machine technology: the computer models that would revolutionize the kinds of analysis that could be performed on survey data, as well as the speed. Prodigious new programs would be devised that could "ransack" data for relationships and test elaborate causal models. Computer technology, for all its power, had the major defect in the 1960s and 1970s of intruding between the analyst and the data. Analysts delivered their "batch" to the queue at a computing center, and if all went well they got their output a few hours or a day later. This put a crimp in the style of those artists in data analysis who liked to work with data as Stouffer did, turning at once to the counter-sorter to try out an idea or resolve an argument, intent upon the immediate detective work of survey analysis. In the 1980s, the hands-on approach to analysis provided by the old counter-sorters would become possible once again with the advent of small terminals and then microcomputers. Three new eras in computer technology were actually in prospect: the giant computer, or mainframe, as it is now called; the microcomputers; and direct data entry, in which telephone interviewers were equipped to record survey responses instantaneously into the computer.

New kinds of archives were coming, a second innovation in data storage and data diffusion made practical by machine-readable data. In 1951, Cantril and Strunk produced from the OPOR archive of poll data in the old style a large bound volume of marginal percentages, question by question and poll by poll. The new archives would be able to disseminate the data for whole studies in the form of magnetic tapes and disks that could be used for secondary analysis. Elmo Roper started the first archive in 1946 at Williams College, in honor of a son who had been killed in the war. (This resource has since been divided between Yale University and the University of Connecticut at Storrs.)

In 1961, after germination and discussion in the late 1950s, an Inter-university Consortium for Political and Social Research (ICPSR) was established at ISR. Within a few years, the ICPSR archive would provide machine-readable data to hundreds of subscribing universities and colleges and would have its own computing installation obtained with NSF funds.\textsuperscript{12} NORC would establish another unique data archive in the early 1970s, in the General Social Survey (GSS), an idea that had circulated in various forms at NORC since the late 1950s. The GSS was an annual
survey designed to provide data replication and diffusion to a subscribing community of social scientists. With the consultation of some fifty social scientists, GSS designed a national questionnaire that replicated many questions of the past—from NORC’s own studies, from Stouffer’s work, from SRC/ISR, from other academic research, and from the polls. (Gallup was in fact the single most important source of questions.) Together, the archives of ICPSR and GSS would provide a treasure house of survey data for students and scholars and a source of income for the parent organizations.

A third change was coming in research organization. Academic survey research centers mushroomed in universities. As of the early 1980s, about fifty university organizations had listed themselves as “academic survey research organizations” in Survey Research, a newsletter edited by Mary Spaeth at the University of Illinois. The directory is not complete because of nonresponse from various organizations, but the data at hand show that some seventeen new organizations were founded in the 1960s, another sixteen in the 1970s, and eight more in the 1980s, for a total of 55 extant in 1983, and 58 in 1985. Starting in the 1970s, survey organizations of a quasi-academic character also developed, the “not-for-profits” built on something of an academic model but operating outside the university (for example, Westat in the Washington, D.C., area).

With the expansion of scientific organizations, the entrepreneurial role that emerged in this early period would acquire new importance and complexity. In the late 1970s and 1980s, as federal funds for research shrank, the research entrepreneurs of social science would organize for informal and even formal lobbying of Congress and would seek harmony for the “applied” purposes of business/industry and the “basic” research of university science, just as had been done in the 1940s and 1950s. In these later years, the research entrepreneurs would be anything but “marginals” trying to span the cultures and contradictions of science and government/business. In the realms of highest scientific eminence, they would become scientific “statesmen” (and a few women) charting broad areas of scientific policy and politics—mainline.

These coming developments in “big” social science—the dazzling new capacities of computer technology, the growth of data archives, and the complex politics and economics of research administration to build and sustain these organizations—would be particularly visible in survey research. It was already “big science” for its time in the 1950s, and it would loom larger and spread in new institutional forms. The complexities of the more recent years are beyond our scope here, except in these most
general terms that we have just been considering. The continuity is nevertheless clear, for in these earlier years survey researchers achieved consolidation as legitimate research organizations in social science, and they also achieved some diffusion of survey methods to the traditional disciplines. We shall review both briefly.

Consolidation

The Local Cultures

Geology is a parochial science: rocks collected in familiar terrain always seem more important than rocks collected elsewhere by someone else.\textsuperscript{17}

Just like geologists, the survey organizations especially valued their local rocks. Their members generally had an esprit, a loyalty, and a strong sense of the history and impact of their own organizations. In this sense, they rather resembled scientists in industry, likely to feel more identification with and loyalty to their company than to their discipline, in contrast to their counterparts in the universities, who were more likely to identify more strongly with their discipline.\textsuperscript{18} Or perhaps it is more accurate to say that the emerging survey professionals identified with both. They continued their disciplinary and departmental orientation as sociologists, social psychologists, economists, and so on, but their day-to-day identification with their research organization was also strong.

Citation patterns, for example, reflected the intellectual culture of the given organizations. ISR produced its manual in 1953, Research Methods in the Behavioral Sciences, edited by Leon Festinger and Daniel Katz and written by nineteen authors, almost all of whom were at the University of Michigan, with more than half at ISR. Of the 458 publications cited in the bibliographies of the book, 25 percent were by ISR authors. Bureau authors accounted for only 3 percent of the citations—not invisible, but not much of a presence, either.

In the Bureau's 1955 volume, The Language of Social Research, edited by Lazarsfeld and Rosenberg, the authorship was more diverse: of the sixty-four chapters, fewer than half (44%) were written by Bureau authors. Neither did Bureau authors entirely dominate in the much shorter bibliography: only 10 percent of the eighty-five works recommended for further reading were by current Bureau writers. But the recent Michigan volume by Festinger-Katz was cited only once (a chapter written by Cart-
wright on the coding of qualitative materials). In the Bureau book, ISR was not entirely invisible, but it was not much of a presence, either.¹⁹

Were there plots to ignore the competition? That seems unlikely. There was much to separate these organizations without introducing a conspiracy theory—notably, the distance of geography, the fences of disciplines, and the internal concerns of organizations. The Bureau was the empirical arm of the Columbia Department of Sociology, and while it had interdisciplinary ties, these tended to be temporary and project based, linked to Lazarsfeld himself more than to ongoing programs.²⁰ SRC/ISR had only one staff member (Leslie Kish) on a tenured appointment in the sociology department during this period. While certain classes were cross-listed in sociology, SRC’s teaching links were basically with psychology, economics, political science, business. Not surprisingly, the editors of each book looked to their own. The volumes produced in the two centers also had quite different purposes, and the editors wanted to show what their organizations had learned and would gladly teach. The visibility of the local “rocks,” however, was an interesting comment on the state of the field. The communication and influence were probably still more local and organizational than professional, national, and interdisciplinary. Not surprisingly, there was some muted competition as well—for money, place, priority.

A MEASURE OF COMPETITION

SRC and NORC both vied for NIH support of a study of mental health, and NORC got it. NORC also got the interviewing project of SSRC. SRC and the Bureau came into some competition in their voting studies. SRC started election work in earnest on the presidential election of 1952, when the Bureau was analyzing data from the congressional election of 1950. SRC continued to capitalize on its national capacity, in election studies of 1954, 1956, 1958, and 1960, using both cross-section and panel design.²¹ The Bureau did not gather data on American elections after 1950, for reasons that are unclear, but SRC’s investment was perhaps a contributing factor.

Among all three organizations, there was a mild current of competition for prestige and priority. To a certain extent, this was handled by the organizations’ staking out of different claims in this period. NORC defined itself as the first academic center in public opinion research (see chapter ten), and SRC/ISR laid claim to being the first interdisciplinary center in survey research. The Bureau saw itself as a “general social re-
search center and training laboratory," second in time only to Odum’s Institute for Research in Social Science, and the first academic "applied" social research laboratory.\textsuperscript{22}

Members in each organization felt their differences from the others. For example, there was sentiment in the Bureau that both NORC and SRC were too “poll-like” and descriptive, without much analytic flair. Certain figures in NORC and SRC felt the same way—that the other organization was too close to polling or to "assembly-line research." In later years, at least, there was some sentiment at SRC/ISR that Lazarsfeld was brilliant as an intellect but chaotic as a director. At NORC, there was some feeling that SRC disdained the contract work that others did—just not its contract work.\textsuperscript{23} The invidious comparisons were mild-mannered, however, and did not preclude cooperation across organizations—more between the Bureau and NORC, especially through AAPOR, than between SRC/ISR and either of the other two. According to some brief reflections recently published by Pasanella, Lazarsfeld came to regret the competitiveness that developed among these organizations, especially SRC/ISR and the Bureau.\textsuperscript{24} With separate groups striving to build viable organizations; creating their own histories, loyalties, and morale; vying for scarce resources in money and prestige—how could it have been otherwise?

THE ANCESTRY AND THE INFLUENCE

Survey research still has a local culture. Within the ranks of the academic research organizations that started early, lasted long, and specialized in surveys—notably the three that are of special interest to us here—the past tends to be reconstructed in ways that emphasize the particular originality and priority of that group. As one talks to people in each organizational setting, one has a sense that “survey research”—or, at least a special aspect of it—was indeed invented here.

In the literature, there are some quite different accounts. Babbie, a sociologist in the Bureau tradition, considers that Stouffer and Lazarsfeld “must be regarded as the pioneers of survey research as we know it today” and that the Bureau must be seen as the first in the “development of the permanent research center supporting survey methods.”\textsuperscript{25} Barton and Glock focus on Lazarsfeld. Barton traces Lazarsfeld’s career as the inventor of the applied academic research center. Glock sees him as the founder of survey research methodology—or, more properly, survey analysis—as well as organizational innovator.\textsuperscript{26} Aubrey McKennell, how-
ever, in a historical article contrasting the academic development of sur-
vey research in Britain and America, sees ISR and NORC as “where it
all began.” With still another view, Edward A. Shils sees these organiza-
tions—at least the Bureau and ISR—as having been rather independent
developments. In all of these accounts, the case is essentially assumed
rather than argued, so it is difficult to know what are considered to be
“the facts” on which these different interpretations are based.

Two kinds of distinctions would seem to be useful for understanding
these differing accounts. The first is the distinction between survey meth-
odology and survey organizations. In this dichotomy, Lazarsfeld’s con-
tribution to survey research seems to be greater in the development of the
instrument than in the development of the survey organization itself.
Lazarsfeld was surely the first great practitioner of survey research meth-
odology, as he enlarged the analytic scope and power of the survey/poll
technique being used in market research and commercial polling. What
Lazarsfeld wanted for the Bureau itself, however, as an organization, was
really much grander than for it to be simply a survey research center.
He saw the Bureau as a laboratory in social science, organized for scholar-
ship, research, and training, not just as a research center organized
around the conduct of surveys. There was a time, as we have seen, when
he wanted to equip the Bureau with a national field staff and tried to
acquire NORC for that purpose (see chapter ten). But this appears to
have been a brief aspiration. Had he successfully obtained or organized
permanent staffs for sampling, fieldwork, coding, and data manage-
ment, it would surely have cramped his “foxy” style, for it would have
been hard to sustain a survey research center without becoming a survey
“hedgehog.”

The very technical capacities at issue here comprise the organizational
model for academic survey research, and this is surely what McKennell
referred to when he credited NORC and ISR with a pioneering role:
university-based organizations with the ongoing technical capacity for
the conduct of surveys. The Bureau improvised the academic conduct
of survey research, from study to study; NORC and later SRC under-
took the ongoing, institutional conduct of surveys in the academic set-
ting. The Bureau’s innovative role is clearest in methodology; the innova-
tion of the other two organizations is clearest in the institutional
organization of surveys.

The second distinction is between priority and influence. Whatever the
founding date of the Bureau is considered to be—1937, when the Can-
tril-Stanton-Lazarsfeld radio research project was funded by the Rock-
efeller Foundation, or 1940, when Lazarsfeld brought the Office of Radio Research under the administrative umbrella of Columbia University—it was clearly prior in time to NORC's founding at Denver in 1941 and SRC's at Michigan in 1946. Whether the Bureau was a formative example for NORC and SRC bears on the other matter, that of influence.

In recent histories of their organizations, neither NORC nor SRC/ISR offers Lazarsfeld or the Bureau as a model of their practice or a formative influence in their founding. In Sheatsley's account of NORC, he cites Lazarsfeld as one of several academics who were supportive and helpful to Field in his plan to found NORC as an academic center, but the idea itself is seen as evolving out of Field's own experience and aspirations for commercial polling, not as a response to Lazarsfeld's work in radio research with Stanton and Cantril. Cannell and Kahn trace the origins of ISR to Likert's survey organization in the federal government, starting in the Department of Agriculture in 1939 and expanding during World War II. Lazarsfeld does not figure in their account as a formative influence on the founding of SRC/ISR.

Apart from these organizations' own accounts of their origins, however, it seems clear that the taproot of both NORC and SRC goes first to the commercial polls. Each organization was designed to be a “full service” facility for the conduct of public opinion research. In their beginnings, they seemed to have been motivated less by following the example of Lazarsfeld than by improving on the example of Gallup—conducting polls in the public and scientific interest, with more precision, more scope, and more complexity in their coverage. Over time, in the academic setting, each took on broader mandates for social research as well.

Was Lazarsfeld's organization their model for becoming academic organizations? In Field's case, the timing seems to have been a little tight. Lazarsfeld's research project with Stanton and Cantril from 1937-39 had academic connections, but mostly on paper; its incarnation as a center with primary university affiliation started at Columbia in 1940. By that time Field's activities to create a nonprofit poll were already under way, with his People's Research Corporation, begun in 1939. Did Field proceed from that to the idea of a university-based poll because of Lazarsfeld's example? This cannot be ruled out, but there is no direct evidence for it in the surviving record of Field's own deliberations and consultations with pollsters and social scientists. It may nevertheless be the case that the Office of Radio Research in 1940 constituted for Field a general
example of feasibility—evidence that a research organization that did polling could be mounted in an academic setting—but this is neither self-evident nor clear from the record.

By 1946, when Likert and his group went to the University of Michigan, the examples of both NORC and the Bureau must have been more vivid and influential. Lazarsfeld's academic organization had become very visible, it had produced major published work, and Lazarsfeld himself had been in direct contact with Likert and his colleagues. The ORR/Bureau must surely have been a formative example as an academic research organization.

It seems a fair claim indeed that the Bureau was the first of a new generation of academic organizations engaged in empirical social research. As we have noted, it was original in three respects. First, it was constituted as a standing research organization, unlike the great range of research projects funded by the Local Community Research Committee at the University of Chicago in the 1920s. Second, it was original in commissioning or collecting primary survey data (unlike Odum's IRSS, which relied on published Census Bureau data and smaller-scale empirical investigations) and in making use of precollected poll and market research data in secondary analysis. Third, it was applied—it relied on contracts as well as grants.

In the fusion of these three features, the Bureau has clear priority to being the first academic social research organization that used and developed the instrument—the method—of survey research. Lazarsfeld's creative use and development of survey designs surely had great intellectual influence on scholars in both NORC and SRC. But in their origins, the two organizations do not seem to have been an intellectual or organizational spin-off of the Bureau, because they were modeled so closely on the polls. In the 1940s and 1950s, in fact, the two organizations were regarded by Lazarsfeld and others at the Bureau as a rather negative reference point, as too close to the practice of the polls, too mechanical and descriptive. Lazarsfeld's paternity of the organizations as survey research centers seems to have been a recent one, of adoption.

A New Discipline?

Could it be said that by this point, circa 1960, these new organizations had crystallized a new discipline? Probably not. Survey research was a complex research instrument that could be put into use only with the coordination of hundreds of people linked in an elaborate division of
labor and with the expenditure, often, of hundreds of thousands of dollars. So, while it was no garden-variety tool, it was still something less than a discipline, for academic survey practitioners kept one foot in their traditional disciplinary associations and departments as sociologists, psychologists, and the like. They kept the other foot in their research organizations, where the practice of their trade made them different from their disciplinary colleagues by opening up certain kinds of problems (and not others) and by facilitating certain kinds of thinking (and not others). But this activity did not organize them intellectually or socially as a distinctly new discipline.

Survey research nevertheless was consolidated as a method of research, and its influence was diffused into social science disciplines. In these few postwar years, the new survey researchers made significant changes in the four criteria we considered in chapter seven: theory, methods, good examples, and social organization. We have already seen changes in methods and “good examples” of the practice, in the foregoing chapters on the three major research organizations. To see the developments in theory and social organization, we need to take a broader view across organizations and disciplines.

**Interstitial Theory of Attitudes**

Survey researchers did not develop a unique core of theory which entirely distinguished their endeavor from that of their mainline disciplines; rather, survey researchers emerged from one discipline and encroached upon another, weaving together some conceptual strands from each one, creating new subdisciplinary specialties supported by quantitative analysis. Lazarsfeld, for instance, worked on surveys of radio audiences, endowing them with new analytic power, and he opened up new interest among social scientists in “communication research.” The “two-step flow of influence” was a conceptual link between the influence of the media, individuals, and small groups.\(^{30}\) The early voting work of the Lazarsfeld group came to be labeled “political sociology”—honoring the fact, aptly enough, that sociologists had been turning their attention to political attitudes and behavior. The implicit theory was, of course, that the voting behavior of individuals could not be understood without reference to their attitudes, and attitudes could not be understood without reference to their formation and transmission in group structures and membership. The Campbell group at SRC took the concept of group identification from the fields of personality and social psychology and
wove it into the study of voting behavior as "party identification." "Psychological economics" (or economic behavior) wove the borders of the two disciplines, positing that consumer attitudes could be one of the leading indicators of business cycles.

The specialists aspired to integrating the study of attitudes and behavior and called their work "political behavior," "economic behavior," and "organizational behavior." While certain studies in behavior were appropriately named, such as Kinsey's study of self-reported sexual behavior, in much the greater part survey researchers were intent upon attitudes as independent, intervening, or dependent variables, primarily because their instruments were a better fit for attitudes—a short self-report by interview or questionnaire. There was some use of behavioral measures, which complemented the special techniques of survey research. In studies of organizations, for example, measures of physical productivity were gathered from records, and measures of group process and morale were gathered by observation. But these studies did not develop any special techniques that were fine-tuned for the study of the behavior of individuals or of group process beyond the techniques of small-group dynamics.

In this era, whatever the aspirations of psychologists and political scientists to illuminate the behavior of interest in their disciplinary realm or of sociologists to cast new light on behavior within the context of social structure and group process, all of these disciplines were doing a better job of gathering and analyzing attitudes. By 1960, survey research had not revolutionized the theory of attitude change or development, but it had stamped the major social science disciplines with the study of opinions and attitudes and had established some interdisciplinary organizations of its own.

Social Organization

THE IMPERFECT, DURABLE UNION

AAPOR, officially constituted in 1947, was a successful effort to span the academic/commercial divide of survey research. The bridging had its trials. First, there was difficulty in building a constitution in 1947. Then, by the next year, the misforecast of the polls resonated the same conflicts between academic and commercial survey researchers and threatened all of opinion research. In that emergency, AAPOR itself was sore beset because it had not yet constructed a durable coalition, but those who
were interested in forging that alliance tried to help and used SSRC to good advantage. Certain academics went into immediate action to review the pollsters' problems, and the pollsters cooperated in making their records available. In time, the tensions of 1948–49 subsided and the polls survived and went on—and AAPOR, along with *PoQ*, continued to provide professional forums for discussion and research for both commercial and academic wings.

AAPOR's difficult constitutional debate of 1947 was on professional standards—"the rock on which the AAPOR craft almost foundered before it was well out of the way," as Hart and Cahalan described it in their 1957 review of AAPOR's history. The issue was whether AAPOR should have the power to expel wrongdoers, and this in turn depended on the definitional matter of whether AAPOR was to be considered an organization solely of individuals or also one of organizations that could be expelled for unethical practices. The 1947 debate was not perfectly polarized between academic versus commercial attitudes, but the sides were visible—more of the vocal academics pushed for specific rules and tough enforcement, while more of the vocal commercials argued for general principles and the encouragement of cooperation.

The debate was not always that polarized. In 1948, for example, academic Frederick Stephan argued that sound practice would develop not through formal statements but through informal processes that stimulated consensus on striving for higher standards. He cautioned that formal standards at this point, when AAPOR had so little experience, might actually shield "shady characters" who might advertise conforming to the letter of the law, but actually "engage in slipshod research." And he noted that AAPOR had company in these problems: the American Psychological Association had been struggling with the issue of standards and enforcement for some time. An accommodation was reached with the creation of the Committee of Standards, whose mission was "to contribute to the elevation of professional standards"—without power of enforcement.

AAPOR has hardly been alone in struggling with professional standards—it was the 1950s and beyond before either sociologists or psychologists managed to create a code—or in laboring to create a common culture and a shared past across a heterogeneous membership. But the 1948 failure of the pollsters put another strain on the coalition. AAPOR leadership acted quickly to try to help. Hart, in particular, went into immediate action to organize a scientific review of the polls' procedures. Arrangements were made right away with SSRC, which ap-
pointed the Committee on Analysis of Preelection Polls and Forecasts, to be led by academics S.S. Wilks and Stephan. Foundation grants were obtained from Carnegie of New York and from Rockefeller, and a technical staff was put to work. Just after Christmas, the committee released a preliminary report (which was followed in 1949 by a full book). Haste was of the essence, as Pendleton Herring of SSRC explained in his foreword to the book:

Appointment of the committee rested upon the judgment that extended controversy regarding the pre-election polls among lay and professional groups might have extensive and unjustified repercussions upon all types of opinion and attitude studies and perhaps upon social science generally. . . . Quick action seemed necessary after the election for several reasons. An authoritative factual inquiry was needed to terminate the growing controversy or to focus discussion upon specific issues at the earliest possible moment.55

Academics and commercials alike were running scared. Campbell, who was not ordinarily given to purple prose, reviewed the SSRC book in language that conveyed some of that emotion and fear:

The detailed post-mortem which this volume presents needs to be seen in some historical perspective as it represents the dénouement in as lurid a melodrama as American social science has ever seen.

Rising phoenix-like from the ashes of the Literary Digest, the pollsters burst on the national scene in 1936 with a dramatic entry that has few equals. From their original success in predicting the 1936 election, they went on for ten years from one triumph to the next. Their names became household words, not only in this country but abroad. They became in some sense the vanguard of the infant social sciences and were seen by some as proof that the study of society, like the study of less diffuse and more controllable phenomena, could also be scientific. Then, finally, flushed with success and too over-confident to heed the portents of disaster, they plunged down together to public humiliation and ridicule. Now, with the melancholy chorus of the SSRC committee to explain the tragedy, the drama is complete.56

AAPOR endorsed research into the matter, trying to stem the controversy. In September of 1948, POQ had become the official organ of AAPOR while remaining under the editorial sponsorship of Princeton University, and in 1949 it did not dwell on the fateful forecast. It would not, in fact, have been entirely difficult to skim the four issues of POQ in 1949 and remain largely innocent of the news that there had even been a polling “debacle” in 1948. There was one lead article that year
which analyzed the reactions of a sample of newspaper editors to the polls' failure, "Election Polling Forecasts and Public Images of Social Sciences: A Case Study in the Shaping of Opinion among a Strategic Public," by Merton and Hatt. There was another by Alfred M. Lee, a polemic piece that denounced those academics who rose to defend the polls, probably referring to Cantril and Meier and perhaps also to Hart's organizing efforts. These were the only full-length POQ articles in 1949 that dealt centrally with the pollsters' misforecast.

This was a vivid contrast to IJOAR, the other leading journal of opinion research, which had just started in 1947 and was publishing many interesting reports on survey methods. In the wake of the election, that journal launched a series entitled, "The Opinion Polls and the 1948 U.S. Presidential Election: A Symposium," which coursed on for five issues, forty-three articles strong: Cantril's was the lead article, a judicious consideration of probable sources of error which should be the subjects of research but also a support of the polls against the "malicious glee" of their fellow journalists:

Newspaper editors and reporters along with radio commentators have finally been able to make the pollsters their whipping boy. Their use of the pollsters as the scapegoat for their own unanimously wrong predictions shows the extent to which they themselves had begun to rely on polls even though many of them had always been irritated by the intrusion of scientific forecasting into a field of prediction traditionally regarded by them as their own special province.

At the other extreme in this series, Krech invoked the pollsters' failure as a "grand opportunity" for social scientists to get shut of "the notorious pollsters." It was also in this series that Gallup asked that there be standards for critics; that Roper called the issue of sampling a red herring; that Bernays argued that pollsters should not be allowed to practice without a license; and that market researcher Coutant tried to distinguish carefully between market research and polling. It could hardly be said that IJOAR failed to confront the hot topic of opinion/attitude research in 1948. AAPOR/POQ leadership may well have felt that IJOAR was covering the issue well enough and that they were making their contribution by supporting the SSRC study. It also seems likely that AAPOR leadership felt that further deliberations on the pollsters' failure threatened the association.

Protecting the coalition had advantages for academics as well as for commercial researchers. Notwithstanding Lee's view that scientists had
nothing to fear—that indeed "social science gains from every such situation as the November affair"—social scientists in opinion research did have something to lose from a breakup of AAPOR: the association itself and especially the avenue for publication in *POQ*. The latter became the more important because *IJOAR*, the main alternative, ceased publication at the end of 1951.42

**THE DIVISION OF LABOR**

By agreement, from the beginning AAPOR alternated its presidency between academics and commercials. Hart of NORC was followed by Elmo Wilson of CBS, and the succession proceeded in orderly fashion, alternating academic Lazarsfeld, commercial Woodward of Roper, Inc., Berelson of the University of Chicago, Crossley, Stouffer of Harvard, Gallup, and later Hyman of NORC and the Bureau.43 AAPOR also seems to have kept its meeting participation carefully apportioned. From 1946 through 1960, the number of participants in the AAPOR meetings grew, but the share of participation stayed in delicate oscillation between its two major sectors (table 4).

Information about AAPOR membership over the years is more fragmentary. What is easily accessible suggests that the commercial sector was carrying more of the membership, and thus more of the financing, of the association, as shown in table 5. Academics, for their part, were writing more of the *POQ* articles. Over this period, as another set of numbers shows, academics gradually took over more of the pages of *POQ*. In the graph in figure 2, the decline of governmental representation reflects the end of the war and the return of many academics to universities. Over and above that adjustment in the 1940s, academics increasingly showed more authorship in the journal than the business sector did.

**THE LEADERSHIP**

AAPOR remained a small outfit of several hundred members, very junior to the major disciplinary groups, which numbered in the many thousands, and its journal was comparatively low in prestige. The various editors over the early years had a good deal of freedom to publish what they thought best, without much refereeing.45 The association and the journal nevertheless offered an intellectual forum and a point of consolidation for survey researchers which was not available in their original disciplinary associations.
TABLE 4
AAPOR Participation: AAPOR Programs 1946–1960

<table>
<thead>
<tr>
<th>Year</th>
<th>Academic</th>
<th>Commercial</th>
<th>Other*</th>
<th>Total</th>
</tr>
</thead>
<tbody>
<tr>
<td>1946</td>
<td>13</td>
<td>24</td>
<td>7</td>
<td>44</td>
</tr>
<tr>
<td>1947</td>
<td>19</td>
<td>16</td>
<td>6</td>
<td>41</td>
</tr>
<tr>
<td>1948</td>
<td>18</td>
<td>29</td>
<td>9</td>
<td>56</td>
</tr>
<tr>
<td>1949</td>
<td>25</td>
<td>17</td>
<td>6</td>
<td>48</td>
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<tr>
<td>1950</td>
<td>17</td>
<td>23</td>
<td>11</td>
<td>51</td>
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<tr>
<td>1951</td>
<td>28</td>
<td>20</td>
<td>19</td>
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<td>1952</td>
<td>19</td>
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<td>1953</td>
<td>30</td>
<td>24</td>
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<tr>
<td>1954</td>
<td>26</td>
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<tr>
<td>1955</td>
<td>36</td>
<td>44</td>
<td>8</td>
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<td>1956</td>
<td>60</td>
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<td>11</td>
<td>126</td>
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<td>1957</td>
<td>43</td>
<td>37</td>
<td>14</td>
<td>94</td>
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<td>1958</td>
<td>47</td>
<td>44</td>
<td>9</td>
<td>100</td>
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<tr>
<td>1959</td>
<td>33</td>
<td>31</td>
<td>8</td>
<td>72</td>
</tr>
<tr>
<td>1960</td>
<td>39</td>
<td>43</td>
<td>19</td>
<td>101</td>
</tr>
<tr>
<td>Total</td>
<td>453</td>
<td>448</td>
<td>153</td>
<td>1,054</td>
</tr>
</tbody>
</table>

*a "Participants" are panelists or presenters of papers. Most of these counts are taken from the printed programs; they do not reflect such last-minute changes as there may have been in the participation of panel audiences. For 1950, 1951, and 1952, the data were taken from Proceedings.

* Governmental workers are the largest part of this category. It also includes those coded as Other and Affiliation Unclear.

TABLE 5
AAPOR Membership: Selected Years

<table>
<thead>
<tr>
<th></th>
<th>Conference registration 1947</th>
<th>Membership directory 1956</th>
<th>Response to mail questionnaire 1959a</th>
</tr>
</thead>
<tbody>
<tr>
<td>Percent:</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Commercial</td>
<td>46</td>
<td>55</td>
<td>61</td>
</tr>
<tr>
<td>Academic*</td>
<td>39</td>
<td>28</td>
<td>33</td>
</tr>
<tr>
<td>Government</td>
<td>10</td>
<td>9</td>
<td>6</td>
</tr>
<tr>
<td>Other</td>
<td>5</td>
<td>8</td>
<td>—</td>
</tr>
<tr>
<td></td>
<td>100</td>
<td>100</td>
<td>100</td>
</tr>
<tr>
<td>N</td>
<td>(194)</td>
<td>(380)</td>
<td>(329)</td>
</tr>
</tbody>
</table>

*a A response rate of 60 percent: 329 of 545 responded.
* Academic includes nonprofit organizations.
The leadership for AAPOR and *POQ* came from both wings. From the commercial side, the AAPOR regulars were Elmo Wilson, J. Stevens Stock, Gallup, Roper, and Louis Harris, all of whom appeared on half or more of the annual programs. The list of academic regulars featured the Bureau and NORC, especially: Lazarsfeld, Berelson, and Zeisel, with Bureau roots; Hart, Sheatsley, Hyman, and Cahalan, of NORC stock; and Stephan, Stouffer, Dodd, and John Riley, from other places—all appeared on half or more of the AAPOR programs. There was a little overlap between being on the AAPOR program and being published in *POQ*. A few of the conference mainstays were also frequent publishers in *POQ*, notably Roper, Gallup, Lazarsfeld, Sheatsley, Hyman, Dodd, Wiebe, and Davison. There was rather more specialization. Cantril, for instance, was the single most prolific author in *POQ*, but he faded early from the programs of AAPOR meetings. Stouffer's situation was the reverse: he published only one article in *POQ*, but he was on every program from 1953 through 1960.

**Diffusion and Expansion**

The practice of American survey research spread from 1940 to 1960 along four routes. First, it was *taught*, through on-the-job training, courses, and texts. Second, surveys were *used* more frequently, and publications based on surveys gained greater visibility in the social science literature. Third, surveys spread into *new subject matter*, and fourth, they expanded in *cross-cultural* scope.

**On Teaching the Trade**

Survey organizations provided some on-the-job training just in the course of getting the work out. There were jobs in coding, machine work

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*Fig. 2. Public Opinion Quarterly Articles, Thirteen Volumes, 1937–1961, Proportion Written by Authors of Academic and Business Affiliation*

*Note:* The two categories do not sum to 100%. The remaining proportion not shown includes authors with governmental affiliation, independent writers, those with double affiliation, and affiliation NA. The only substantial group in that miscellany is the government group during the war years, when their contribution in 1941–1945 ranged from 12 to 24%. The coding unit is the article, not the author. In the case of multiple authors, the first author's affiliation is coded. Authors of news notes and editors of journal departments are not coded.
and data analysis, and some in interviewing. These jobs provided an avenue for professional recruitment of students as well as support for graduate students who were already training in the field and doing dissertations. The Bureau was the most active teaching "shop": it had a training mission, it employed students, and Lazarsfeld's own teaching was exciting in this on-the-job context.\textsuperscript{47} NORC and SRC provided many jobs for students, too, and each developed a survey practicum. NORC's Community Survey of Denver, organized as a graduate class in 1946, was the first. (This was not transplanted to Chicago-NORC after the Denver branch closed in 1949.) In 1951, at Michigan, the formation of the graduate practicum, the Detroit Area Study, took the pressure off SRC itself for developing a training program. And there were others: in 1947, Dodd organized the Washington Public Opinion Laboratory at the two major state universities at Seattle and Pullman, which integrated survey work with graduate training up through the Ph.D. level.\textsuperscript{48} There were survey organizations or frequent surveys conducted at Washington University (Theodore F. Lentz), the University of Minnesota (R.O. NaFziger), the University of Miami (Ross C. Beiler), Iowa State University (Raymond Jessen), Purdue University (H. H. Remmers), as well as many programs and classes.\textsuperscript{49}

SRC and NORC both developed summer institutes in survey methods. NORC organized two sustained summer programs, one in 1949 dealing with communication and another in 1956 dealing with health methods.\textsuperscript{50} SRC undertook a summer institute in survey methods for graduate credit in 1948, for the very practical purpose of getting summer salaries for SRC staff members, and it became annual. SRC's summer registration averaged around fifty in this period, diffusing SRC influence.\textsuperscript{51}

Texts in methods of survey research developed in the 1950s. Sociology texts of the 1920s and 1930s had set forth the "statistical" method, the "case study," the "historical" method, and so on, and the "survey" method represented the multifaceted and loosely defined social survey. In Young's \textit{Scientific Social Surveys and Research}, first published in 1939, one can see something of a turning point in that the book dealt briefly with sample surveys but at much greater length with social surveys. In 1942, Lundberg published a revision of his text \textit{Social Research}, with a new chapter on the questionnaire as one of the methods of sociological investigation. He, too, dealt with sampling in a general way, but he did not deal with surveys as a composite, complete method. Blankenship's book \textit{Consumer
and Opinion Research (1943) was too early to reflect wartime developments, especially area probability sampling.52

Parten's 1950 book Surveys, Polls, and Samples was the first text in contemporary survey research.53 A student of Chapin's, Parten had done survey work early in the 1930s using systematic samples. Her book remains a very ambitious and particularly useful historical document, combining "how to" instruction with a historical overview and a generous bibliography of nearly 1,150 items.

Other important texts appeared in the 1950s that gave survey techniques considerable space. The two-volume work Research Methods in Social Relations (1951), by Jahoda, Deutsch, and Cook, covered social research generally and a variety of techniques (field observation, small-group analysis, sociometry, content analysis of media, etc.), but much of the material bore on the techniques of survey research. (The chapter "Community Self-Surveys" evoked the old social survey movement in its focus on community participation and political action.)

Nineteen fifty-one also saw the publication of Payne's book The Art of Asking Questions, which summarized 100 guidelines for the writing of poll/survey questions. It has long been the single most informative book on the subject, reflecting the sophistication of Payne's experience in governmental and commercial work and some experimental evidence, aided by his sprightly writing.

The 1952 volume by Goode and Hatt, Methods in Social Research, represented a more general text that tried to sweep across all methods relevant to "the new sociology." Much of its content was especially relevant to survey research (although the term itself was used rarely, if at all). In 1953, ISR made its contribution to research methodology in the volume edited by Festinger and Katz (Research Methods in the Behavioral Sciences), a good half of which had special reference to survey research methods. In 1955, there were two strong offerings from the Bureau: Hyman's case studies in Survey Design and Analysis and the Lazarsfeld-Rosenberg volume The Language of Social Research. Both had rich implications for the sophisticated design, conduct, and analysis of surveys. In 1957, Kahn and Cannell of SRC/ISR published a major book on survey interviewing, The Dynamics of Interviewing, reflecting especially psychological theory and the practice of open-ended questioning.

In 1958 there were two more books of importance. Stephan and McCarthy's Sampling Opinions appeared in that year, having been planned initially as one of the three main projects of the SSRC Com-
mittee on the Measurement of Opinion, Attitudes, and Consumer Wants. From England, another text appeared that was devoted exclusively to survey research, although it reflected the British social survey emphasis on "factual" measures: *Survey Methods in Social Investigation*, by Claude Moser of the London School of Economics. Texts in elementary statistics for social scientists appeared with growing frequency in this period: among others were Hagood and Price's 1951 revision of their 1942 text *Statistics for Sociologists*; Wallis and Roberts, *Statistics: A New Approach* (1956); and Blalock, *Social Statistics* (1960). Technical counsel for survey research began to flow into *POQ* and into the sociology journals.

**Diffusion to the Disciplines**

By 1949–50, survey research was very visible in the *Public Opinion Quarterly*: 43 percent of its articles in those two years were based on survey data. This is not to be wondered at, from one standpoint: if surveys had not resonated in *POQ* by this time, where would we have heard them? But in fact, this represented some real change. *POQ* was not a journal of survey research at the outset. It had been established for the study of public opinion and thus drew articles on public relations, advertising, propaganda and censorship, radio, film, the press, and public opinion generally, many of which had little or nothing to do with surveys. Of academic journals, *POQ* showed the strongest increase in the use of survey data in general.

In the major disciplines, survey research had the greatest impact on sociology. Economics showed increased use of surveys, most of it in government surveys. The impact on political science was a later phenomenon; psychology reflected a rather steady 10 percent of articles based on survey data. Table 6, showing this change over time, uses and extends Presser's recent data on the diffusion of survey research. I have included Presser's data for 1964–65 because they show the continuing growth in the use of surveys in the mid-1960s; in the 1979–80 data (not shown), that growth is shown to have leveled off in the journals of sociology and economics but to have continued upward in political science journals and in *POQ*.

**Subject Matter: The Luxuriant Fields of Self-Report**

Early poll-survey questioning now seems like a curious mixture of daring and caution. Gallup, for instance, asked opinion questions about vene-
TABLE 6

PERCENTAGE OF ARTICLES USING SURVEY DATA
BY FIELD AND YEAR

<table>
<thead>
<tr>
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<tbody>
<tr>
<td>Sociology</td>
<td>17.6</td>
<td>24.1</td>
<td>42.6</td>
<td>54.8</td>
</tr>
<tr>
<td>Political</td>
<td>2.6</td>
<td>2.6</td>
<td>10.5</td>
<td>19.4</td>
</tr>
<tr>
<td>Science</td>
<td>(113)</td>
<td>(114)</td>
<td>(152)</td>
<td>(160)</td>
</tr>
<tr>
<td>Economics</td>
<td>10.2</td>
<td>5.7</td>
<td>16.1</td>
<td>32.9</td>
</tr>
<tr>
<td>Social</td>
<td>20.5</td>
<td>22.0</td>
<td>17.0</td>
<td>14.6</td>
</tr>
<tr>
<td>Psychology</td>
<td>(54)</td>
<td>(59)</td>
<td>(229)</td>
<td>(239)</td>
</tr>
<tr>
<td>Public</td>
<td>27.5④</td>
<td>43.0</td>
<td>57.8</td>
<td>55.7</td>
</tr>
<tr>
<td>Opinion</td>
<td>(51)</td>
<td>(86)</td>
<td>(64)</td>
<td>(61)</td>
</tr>
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The coding rules for this table, which exclude symposia and research notes, happen to minimize the figure for POQ in these years because reports on poll data were introduced to the journal through its research note “Departments” and because a special issue was devoted to the polls. With the inclusion of these two types of articles, 85.7 percent of 126 articles were devoted to poll/survey data.

real disease and birth-control information in the 1930s, but he and other pollsters did not ask respondents their educational attainment until the 1940s. Pollsters generally thought that inquiry into education, occupation, and income was “sensitive” enough that they had to proceed cautiously. Well into the 1940s, investigators were getting only very gross demographic measures at best, sometimes asking interviewers to make estimates and sometimes avoiding the issue entirely. Survey professionals had to feel their way and learn what things people were willing to discuss and what they were not.

In the course of the 1935–60 years, investigators pushed at the borders of inquiry, and they learned that most people were willing to discuss almost anything. For good or ill, people were in general willing to express opinions and attitudes not only on innumerable impersonal topics but also on subjects very close to the bone of personal revelation, emotion, and intimacy. Whether they did so with candor and completeness was an
ongoing question, but survey research did indeed expand the realms of inquiry.

Not all of these topics were new to social science. Many had been touched on before at some point, in some study or another. But survey investigators dealt with new explorations of old topics as well as with new topics, not only in the more secluded environments of clinics, small groups, or classrooms but also in the mass population. The reporting of income, savings, and assets, for example, was a “sensitive” issue, especially at higher income levels. And it proved feasible for surveys. The study of racial attitudes, for another example, was not an entirely new preoccupation of social scientists—Bogardus and Park had studied Oriental-Caucasian relations in the mid-1920s (see chapter two)—but black-white attitudes had been touched on only lightly by the polls. Survey investigators embarked again on this subject and on the subject of anti-Semitism as well.

Large-scale surveys were used to explore attitudes toward marriage, mental illness, occupational prestige, political socialization, child-rearing, and national character. There was new survey work on attitudes in the context of groups and institutions, such as housing units, industrial organizations, volunteer groups, unions, the medical profession, college departments, the sick, the elderly, and mental hospital patients. Some of this work used self-report of attitudes in conjunction with other methods of observing behavior, but it was in the use of self-report of behavior that investigators pushed surveys further still into “sensitive” areas of inquiry.

The most dramatic of these areas was sexual behavior. The so-called Kinsey Reports on American males (1948) and American females (1953) did not use a standardized interviewing schedule. Like Lazarsfeld’s early market research work and some of Likert’s early questionnaires in Agriculture, the interviewer was allowed to vary the language in the judgment that only thus could meaning be standardized across very different groups. The topics of the interview were specified (some 300 to 500, depending on the individual’s kinds and ranges of sexual experience). But the language of the questioning was shaped to the particular sexual vernacular—of prison inmates, marriage counselors, farmers, housewives, headmasters, prostitutes, physicians, bootleggers, and so on. Kinsey, a zoologist at the University of Indiana, used an unorthodox and fascinating mix of methods to try to obtain individual candor and mass data. These were something of an amalgam of survey research and the field methods of anthropology.
Feeling that they could not get enough cooperation from a national sample of individuals, Kinsey and his colleagues tried to reach many different kinds of groups of varying class, age, ethnicity, occupation, intelligence, and region and to interview as many people as they could in that group. This was not unlike Bogardus's efforts in his mass study of social distance, but it was more ambitious. Kinsey and his associates got acquainted with members of the community. They tutored themselves in local customs and language, and they made friends with people who would vouch for the research to their friends and acquaintances.

The interviews were relatively short (from one to two hours) because it was found that beyond that length of time, fatigue diminished the quality of the information. The interviewing style assumed that all respondents had engaged in every kind of sexual activity; interviewers asked not whether respondents had engaged in homosexual practices, masturbation, extramarital intercourse, and so on but when they had done so for the first time. They elicited the straightest kind of talk and were free to contradict respondents when they sensed any falsity, but they also tried to convey in some wordless way (i.e., with a glance or a facial expression) that they understood and sympathized when the sexual history was a record of "hurt, of frustrations, of pain, of unsatisfied longings, of disappointments, of desperately tragic situations, and of complete catastrophe." They recorded data in the presence of the respondent in a quick check-off system, using a code that the six interviewers had memorized and which was never written down. Kinsey's standards for confidentiality could hardly have been more scrupulous. He and his small group of colleagues were also tireless. By the publication of the 1948 book, they had conducted more than 18,000 interviews. Kinsey and Wardell Pomeroy had each done 8,000.

As Kinsey et al. emphasized, human sexual behavior had inspired a mountain of graphic art and print—probably more thought, more talk, and more books than any other subject in the human repertoire, and goodly amounts of scientific inquiry, too. But there had been little systematic work of broad coverage that allowed statistical treatment. Kinsey located twenty-three studies published between 1915 and 1947, most of which had no pretensions to being a sample of any population, the largest set of which were based on college students. Kinsey's work was not a real sample, either, which he recognized full well and for which his work was duly criticized by social scientists, but it had gone way beyond the college classroom in its inquiry into very intimate human experience.
As a hotel manager said when he refused to allow Kinsey and his group to conduct any interviews on his premises, “I do not intend that anyone should have his mind undressed in my hotel.”

Kinsey traveled farther into the “sensitive” areas of human experience than anyone else did in this period, but other surveys also pushed at the boundaries. Studies of self-reported drinking behavior were undertaken—Jellinek’s survey of Alcoholics Anonymous, for instance; “polls of drinking” conducted by the Washington Public Opinion Laboratory; and Riley and Marden’s surveys of alcohol users and social drinkers, for which NORC did the fieldwork. Reports of personal “worrying” behavior were gathered by Stouffer for his communism study. More intense inquiry into worries, fears of “nervous breakdown” and mental illness, and the incidence of seeking professional help for personal problems was done with a national sample in the Gurin, Veroff, and Feld study of mental health, along with self-reports of personal problems in marriage, feelings of inadequacy, and marital happiness and unhappiness.

It appears that surveys in this period did not explore mystical or religious experience or personal trauma such as the immediate impact of divorce, the experience of grief and death in the family, or the onset and course of terminal illness, but all of these experiences would be the subject of surveys in a later epoch. Psychologists began to use survey techniques in conjunction with indirect and projective measures to study personality in adult populations outside the classroom.

The Authoritarian Personality (1950), by Adorno, Frenkel-Brunswik, Levinson, and Sanford, one of the several books in the Studies in Prejudice series sponsored by the American Jewish Committee, was the source of the famous “F-scale” designed to measure the potentially Fascist personality. The other scales used in the book (on anti-Semitism, ethnocentrism, and political-economic conservatism) were also influential, but the F-scale had special impact and wide circulation. These items were among the large set comprising the F-scale:

Obedience and respect for authority are the most important virtues children should learn.

Young people sometimes get rebellious ideas, but as they grow up they ought to get over them and settle down.

Homosexuals are hardly better than criminals and ought to be severely punished.
Most of our social problems would be solved if we could somehow get rid of the immoral, crooked, and feebleminded people.

Human nature being what it is, there will always be war and conflict.\textsuperscript{65}

The book can be viewed now as a fine point of comparison to the Kinsey Reports, for both represent interesting variants on the survey method. \textit{The Authoritarian Personality} used the direct measures of survey questions and the indirect methods of “depth” interviews and thematic apperception tests of the clinical tradition; the Kinsey group relied on straightforward self-report of surveys, with variants on interviewing and coding practice. Both used members of groups rather than large-scale samples of individuals. Kinsey had much better scientific warrant for doing so, for the sensitive nature of his subject matter argued for the use of special measures to elicit the cooperation and confidence of his respondents. The Adorno group apparently used special groups for entirely practical reasons. Confronted with a scarcity of resources, they started their study with college students; as they expanded the study, they collected questionnaires from a variety of adult groups. But, overall, it was a far less heterogeneous set of groups than the Kinsey collection and used a variety of questionnaire forms. One hundred ten San Quentin State Prison inmates, 121 psychiatric clinic patients, and another group were given one form; 343 merchant marine officers and 106 veterans were given another; and a variety of groups identified by the authors as middle-class or working-class were given still another, and so on.

\textit{The Authoritarian Personality} had problems in sampling bias, along with problems in coding and analysis, which Hyman and Sheatsley explained in a long review article. The most striking defect of analysis was the failure to subject the hypothesis of an authoritarian \textit{personality} systematically to a thoroughgoing test of a rival idea, namely, that differences in formal education might account for much of the variation in expressed “authoritarianism.” Hyman and Sheatsley marshalled compelling illustrations from the Adorno protocols in support of the education hypothesis. They also adduced results from more than a dozen relevant attitudinal items drawn from national NORC surveys (five of which were abbreviated F-scale items) that showed strong differences by education.\textsuperscript{64}

Their critique was the work of sophisticated survey researchers who were experienced in survey sampling, questioning, coding, and analysis and who also could call upon cogent national survey data to argue their case. The critique did not argue against using projective tests in a survey
context—indeed, this was a perfectly appropriate and feasible expansion of survey techniques, which others were also using. It was, rather, an indication of the resources available by the mid-1950s—sophisticated researchers and a wealth of survey data on a national scope.

The Cross-Cultural Expansion

At the end of the 1950s, a new and ambitious international survey conducted in five nations went into the field; it was published in 1963 as *The Civic Culture*, by Almond and Verba. While the authors traced their intellectual roots back to Charles E. Merriam and University of Chicago political science, they cited both of the international studies we have discussed in chapter nine. Their study yielded data of greater richness than the Buchanan-Cantrell UNESCO report and greater comparability than the Bureau-Lerner studies.

The so-called Five-Nation Study by Almond and Verba went through two pretests and seven separate versions—the first interviews lasted no less than a whole day!—as the investigators progressively whittled their vast aspirations down to an instrument that could be administered with fair comparability in cross-sectional national samples of 1,000. (Wilson's International Research Associates did the fieldwork in England, Germany, Italy, and Mexico, and NORC did it in the United States.) The rich explorations of unstructured questions and elite interviewing were sacrificed in favor of less complete, more comparable measures. The questionnaire itself was 90 percent closed; on the open-ended questions, interviewers were leashed in:

It was necessary . . . to specify precisely the number of times an interviewer could probe for further information in an open-ended question, as well as the content of that probing. In this way, the extent to which a sophisticated interviewer could obtain a full response in a particular area through extensive follow-up questions was circumscribed, but the comparability of the results was increased.

The Almond/Verba study also tried to incorporate some “in-depth” materials from small “life-history” subsamples in each country (with Ns ranging from 45 to 135). From the cross-section samples, respondents were selected who had special interest as political types or as “deviant cases” in the Lazarsfeld tradition (e.g., poorly educated women who were politically active). The study nevertheless relied most heavily on the mass questionnaires. For national and international studies of this scope,
“mass” and “elite” techniques and goals were not wholly fused, in some fifty-fifty blend. Rather, the enduring compromise from this period favored the standardized and quantitative survey instrument that could be administered to large samples by large field staffs.

The Power and the Limits

The Historical Ascendance of the Subjective Realm

The survey data amassed by academics in the postwar period were dominantly subjective: beliefs, ideologies, preferences, feelings, and, especially, opinions and attitudes. As we have defined it from the outset of this book, survey research as a technology encompassed anything that could be stored and analyzed by the individual record, as reported by or observed of individuals—for example, “facts” of income, amounts of spending, space of residence, amounts of nutrition, reactions of physiology, states of disease or health—and much survey work in government in particular continued to gather data in these less subjective realms. In principle, in the same survey one could also gather data of the respondent’s self-report and the observational measures of the interviewer, as in the “schedules” with which the social survey movement had tried to systematize an investigator’s observations of a community. In practice, the role of the interviewer-as-observer dwindled to the making of a few estimates, such as the class level of the neighborhood, the condition of the house, and the general intelligence of the respondent.

In university-based survey research, self-report and subjective measures quite overshadowed the observation of “objective” conditions. This happened for two main reasons. In general, much of the survey work reporting on community conditions became institutionalized in realms of government. These surveys of “objective” conditions, which took on special visibility with Charles Booth’s study of London and which persisted in the American social survey movement, were absorbed by agencies of local, state, and federal government work, especially as the role of the federal government burgeoned during the New Deal. Furthermore, academic work was formatively influenced in this period by the preoccupations of opinion pollsters, social psychologists, and sociologists—people interested in attitudes. This was not an intrinsic limitation of the method but a historical one that made the scope of survey research in this period even narrower than subjective phenomena generally.
If other people had been the first captains of survey research, attitudes surely would not have been as ascendant and more data would have been collected on self-report of experience, behavior, and information. Historians and anthropologists of a certain stripe, for example, might well have tried to bring the self-report of individuals' life experiences to the fore, and we would now have a richer record of the human impact of wars, refugee flight, the Nazi experience, depressions, unemployment. Political economists might have been drawn to individuals' experience with industrial technology, the lateral spread and high-rising of cities, the impact of the bulldozer, the onset of computers, the rise of affluence, the endurance of poverty. If social biologists, anthropologists, or demographers had been in charge, survey research might have been put into first service for studies of culture and class, migration and assimilation, sex roles, generational strain, life phases, fertility, child-rearing, and aging. If educators had been dominant, we would presumably know much more about the kinds and distribution of public information, ignorance, understanding, and belief—not just an occasional question aimed at testing knowledge or interest in politics. Since 1960 surveys have been spreading into these areas. Still, the early period had its own richness, in its concentration on attitudes and some extensions into behavior. It demonstrated unequivocally that attitudes were indeed "facts" of life and that much so-called factual data had vivid and intractable attitudinal components. The limited content of surveys in this period also provided a powerful focus.

Survey research of these early years was critically shaped by social psychologists in psychology and sociology. More than other disciplinary groups, social psychologists were prepared to undertake large-scale surveys, for they had an interest and experience in subjective measurement which economists, for instance, did not share. They also had quantitative skills from their training in psychology which most political scientists still lacked. Without interested and prosperous clients, however, who had money to commission this expensive, broad-scale work, social psychologists would have continued to study attitudes in the low-cost populations of students and small groups.

For the kind of support that provided access to the broad population, the social scientists were indebted, indirectly, to the pollsters. It was the commercial pollsters, especially, who provided the most dramatic and persuasive example to prospective clients that it was feasible and affordable to sample opinions and attitudes of great economic and political importance, with results that could be generalized to vast populations. During World War II, the federal government was the greatest client of
them all, with urgent political and administrative needs to know about soldiers and civilians, their "morale," their support of the war and of the government. This federal use was a prime factor in steering survey research in the direction of opinions and attitudes.

The Relentless Need for Big Money

Funding was, and continues to be, a vital factor in the origins and development of survey research, simply because by the standards of social science this research is such expensive work. This is not to say that the content of surveys is rigidly determined by economic interests. Even in very specific "applied" contract work, scientists are usually at pains to try to widen the research agenda of clients or sponsors to admit more of their own intellectual interests, in the practice wryly called "bootlegging." Furthermore, as science has gained in cultural prestige and political power, scientists have been more successful in garnering resources for their own intellectual pursuits. Rather, it is to say that the "entrepreneurial" efforts of survey researchers in this early period were not only the struggles of an infant industry but also reflected an enduring tension between social scientists who wanted to be free to list where Truth led them (or at least intellectual Beauty) and the people who paid for their work. The latter expected from social research some payoff, as they defined it, in practical benefit for some objective, as they sighted it. And funders still do.

As Stouffer reflected about these matters during World War II, it was necessary for social scientists to come out now and then and show what they could "do." But showing what they could (or can) do inevitably raises the question of doing what and for whom. The wartime work was performed in a spirit of patriotic national consensus, so this question was not an acute one. The question was muted, too, by the more general funding of foundation and governmental grants that asked no immediate practical payoff, and survey researchers in these early years were of course intensely interested in trying to get this more ample and generous funding. The question is nevertheless raised, as a chronic condition, by even the most disinterested and generous kind of support.

If social science is not required to come out now and then and show what it can do, as its practical benefits are defined in some fashion or another, there is an alternative justification: namely, that basic science is another facet of human intelligence and creativity, equivalent in value and meriting the kind of public support given to museums, orchestras, repertory theater, libraries, and other resources for a cultivated, intel-
lectual life. The contemporary attack on positivism by a new generation of historians and sociologists of science of the post-Kuhnian era would suggest that social scientists might well argue for support on such cultural rather than practical grounds. They do not tend to, however, and they probably do not because these cultural institutions are usually mendicants, struggling to stay alive. Rather, social scientists tend to argue that support of their basic research will bring benefits in application to the society as a whole—with world enough and time.

Survey research is not peculiar in needing money, but it vivifies the issues of basic/applied research simply because it usually requires so much of it. If survey research is not to be funded by the personal fortune of a Charles Booth, it is to be subsidized from the coffers of foundations or governments, or it must make its own way in work for business, industry, labor, political groups, and other large sectors. The applied/basic tension will not go away.

As an instrument of social science, survey research grew in power and precision in this early period and also showed some sophistication concerning the limits of its methods. It aroused vocal criticism in a series of complaints which we have already reviewed briefly in chapter eight; some of these complaints still flourish. The advent of survey research certainly leaves other empirical methods alive and well, with their own characteristic advantages and disadvantages: participant observation and community studies of sociology and anthropology, aggregate data analysis of macroeconomics and political science, laboratory experiment and clinical interviews of psychology, documentary studies of history, and so on. In principle, these various strategies can even be used together. In practice, however, it has become increasingly difficult to mix methods, because the use of any one of them has come to involve professional specialization of a high order, with vast educational preparation and ongoing intellectual commitment. And the various methodologies of social science are to a certain extent in competition with one another as they vie for scarce resources, striving to become attractive to government and foundation funding. Just because the practice of survey research is especially expensive for social science inquiry—even if for no other reason—it is especially likely to inspire criticism from other wings of social science.

*Of Time, Structure, and Self-Report*

Certain criticisms of survey research in this early period pointed then, and continue to point, in my view, to specific weaknesses or limits of
survey research that are also its peculiar strengths. The first feature is the constraint on time. A survey typically averages not much more than an hour of an individual's life, a limit that is dictated largely by respondents themselves. An hour or so of survey questioning is about as much as most people in the general population will concentrate on, enjoy, or put up with. Special populations will submit to much longer, more intensive, and more frequent interviews or observation because they have special desires or special pressures—for example, clinical populations, prisoners, college students, schoolchildren, and paid respondents. Such special groups can provide much more detailed information, "in depth," but their very specialness limits the generalizability of the data they provide. If one works with samples of the broad population—people who are much freer to refuse to cooperate—a researcher is largely constrained to the brief and necessarily rather superficial encounter. This is true almost regardless of the money available for the total size of the survey. The great weakness of brevity is the counterpart of its great strength, generalizability to people in general.

A second constraint is standardized questions. All three of the early organizations tried to enrich survey questions with inquiry into detail and "depth," using open-ended and qualitative material, profiting from interviewer insight and flexibility. NORC, however, relied more heavily on standardized wording from its beginning, out of the experience of its own polling tradition; in the course of the 1940s and 1950s the Bureau and SRC also converged in the practice of greater standardization. In using structured questionnaires, the survey researchers banked on two assumptions. First, they assumed that interviewers could and would deliver the same questions, whatever the variety of conditions they found. Second, and still more basic, they assumed that English-speaking respondents in the United States shared common culture enough that they could all be addressed in the same vocabulary: not, to be sure, an identical culture, education, or interpretation, but close enough that survey questions did not have to be "translated" or selected for the special cultures of region, class, or community.

There was some scientific warrant for these bets. Survey investigators could not insure that each respondent would receive the same meaning, but they aspired to send the same literal message, trying to word that message in such a way that it would communicate much the same meaning to the mass public without being insulting to elites. (Pretests were aimed especially at finding that language.) There was an even stronger practical warrant. Survey investigators could not allow a flexible and intuitive style when the data collection was out of their hands, conducted
by large groups of interviewers who were not professionally trained in the social sciences and who were paid modest wages. Standardized questions represented an instrument of administrative control, offering some hope that interviewers would not simply use their own (highly variable) judgment of what was appropriate in the given situation.

The highly stylized closed question that "puts people in little boxes" was a part of the same constraint posed by large-scale data collection for statistical analysis. If one is to do quantitative research of any sort, data must be put into boxes, and a limited number of them at that. All the rich material of an entirely open-ended interview, recorded verbatim, will either be put to no systematic analytic use at all or it will be put into boxes, by somebody—either analysts, who design codes for the open-ended material, or respondents, who try to find from among the boxes offered those that best fit their lives and souls. In either case, one hopes for artful, realistic, valid choices.

Are there alternatives? There are no simple ones, in any case. The most obvious remedies would tax enormously both the resources of survey organizations and the tolerance of respondents. What about using highly educated, intensively trained interviewers who would be paid at truly professional levels? This solution would cost a fortune. Or highly detailed, intensive questioning of respondents over several hours and repeated visits? This would likely be at a high cost in response rates; the willingness of respondents to provide data for social science is another special constraint on surveys.

A third factor is self-report, which also constrains surveys with certain limits and particular powers. The validity of survey reporting has been in vigorous question from the beginning, offering a rich and contentious history of its own which I have slighted here in favor of other topics. Do people mean what they say? Do they know what they mean? Can they say it? Do survey questions even give them a chance to speak plainly? The adequacy of both questions and answers has repeatedly been called into question. The validity of self-report has probably aroused more skepticism in surveys than it has in other areas of applied science. The practice of medicine, for instance, would be hobbled indeed if professionals could not put some credence in the data of self-report, although medical histories must rely on much retrospective measurement and are probably still gathered with little attention to even such principles of interviewing and question wording as survey research has developed. There are obviously differences, however, for medicine generally goes beyond self-report to physical examination of patients and the assays of the biochemical laboratory as primary data.
Survey researchers rarely go beyond self-report. Validity studies are not only expensive but amazingly complex as well. One can compare the "subjective" self-report of surveys to the "objective" data of official records, but most of the documents that one can consult to validate surveys are also constructed of self-report and thus are usually vulnerable to the same kinds of measurement error and bias that surveys themselves are prey to. In some situations, the survey information may be better than the records: samples are likely to be more accurate than censuses, and contemporary data management by computer can monitor error more successfully than the paper-and-pencil systems still governing much official record keeping of the past.

Even so, self-report has left a record of some ingloriousness. In the 1980s, the distribution of ignorance has come to seem quite awesome, as documented by surveys and as interpreted by social science elites. On issues of manifest importance to the culturally well educated or the politically sophisticated, a substantial fraction of the general public has scored low. For all the goodwill and pliability that many of the American public have shown in their willingness to be interviewed, there is much that social scientists want to learn about people which a substantial part of the mass public cannot teach—they don't know.

This datum itself is one we have learned from polls and surveys, and it illustrates one of their great strengths. With no other instrument can we gather data from the broad public in some semblance of their natural setting. And there is no other method of data collection and analysis that so successfully forces social scientists to contemplate the narrow limits of their own construction of social reality.

George Gallup was long sensitive to the democratic potential of polls: that polls at their best could permit mass publics to speak for themselves rather than being interpreted by political pressure groups or cultural elites who spoke for them. Academic survey researchers can also be forced by their data to consider a question that is a variant of Gallup's guarded, qualified populism, with respect not only to politics but to science as well: If The People do not know, are survey researchers asking the right questions?