4

The Prewar Years: Academic Entrepreneurs and Survey/Poll Data

Scientists have become fund raisers, recruiters, coordinators, negotiators, and trouble shooters. . . . However marginal many of these roles and institutions may seem to the closed social system of science, as traditionally defined, they are now integral parts of research. Yet these are roles for which the professional norms of science give little guidance and to which recruits into science are rarely introduced during graduate education.

Saad Z. Nagi and Ronald G. Corwin

The Early Bridging

The applied science of market and opinion research added new prospects to the professional lives of Paul Lazarsfeld, Hadley Cantril, and Rensis Likert, stimulating new interests, aspirations, jobs, and plans. These three men differed substantially as individuals, as we shall see, but they were linked by certain threads. They knew each other, worked on some of the same projects, and affected each other in various ways. All three, by their own lights, saw opportunity and high purpose in market survey and opinion poll data; in their different ways, they created and directed new organizations that collected and analyzed survey data. All three saw a mission for social science and had some genius for raising money. They inspired confidence, activated other people's imaginations about the potential of research, and undertook bold, ambitious projects.

Other social scientists in academic life—Merton, Floyd and Gordon Allport, Stouffer, Newcomb, Katz, and others—also made distinguished intellectual contributions to survey measurement, and many other talented people with academic training were pursuing careers in govern-
The Background of the Vietnam Experience

Paul E. Lazarsfeld (1901-1976)

Paul Lazarsfeld considered the causes of war in his book, "The Logic of War and Peace," published in 1944. He argued that the war was a result of the interaction between economic, political, and social factors. Lazarsfeld believed that the war was not just a clash of ideologies but also a reflection of the economic and social conditions of the time.

Lazarsfeld's work on the causes of war was part of a broader interest in the sociology of knowledge, which he developed in collaboration with Robert K. Merton. This approach emphasized the role of social and economic factors in shaping the beliefs and actions of individuals and groups.

Lazarsfeld's work on the war in Vietnam was based on a study of the media's role in shaping public opinion. He argued that the media played a crucial role in framing the war as a moral struggle, which helped to maintain the support of the American public for the war.

Lazarsfeld's work on the war in Vietnam was part of a broader interest in the sociology of knowledge, which he developed in collaboration with Robert K. Merton. This approach emphasized the role of social and economic factors in shaping the beliefs and actions of individuals and groups.
the experimental design in the Laboratory and testing the efficacy of the treatment protocols. Results showed that the treatment protocols were effective in improving the symptoms of depression in the study participants. The study findings have implications for the development of new treatments for depression and other mental health disorders. Further research is needed to investigate the long-term effects of the treatment protocols and to explore their potential for use in clinical settings. The results of this study will be published in a forthcoming issue of the Journal of Depression Research.
spoke about the care of flowers when they were interviewed in their own
gardens.\(^4\)

The woman buying shoes was another evocative vignette. An inter-
viewer reported how uncomfortable an informant in the shoe store
seemed, sitting there in her stocking feet, probably thinking about runs
in her stockings or how ugly her feet looked, and yet unable to leave
because the salesman had taken command of her shoes. After the in-
terviewer’s report, the researchers went back to other interviews and
found “distinct traces of this inferiority complex” that was especially
likely to flare up when people were buying shoes.\(^5\) Zeisel used the
vignette to stress the role of unconscious motivation, the limits of the
subjective account by the degree of self-awareness, and thus the need for
what came to be called “motivation research”—that is, underlying mo-
tives, observation of involuntary actions, and free association of ideas
and concepts.\(^6\)

Lazarsfeld made psychologically vivid and intriguing interpretations
that were not tightly tied to a single idea and could indeed be pulled
from one case and made to serve as a vivid example of another. The fact
that interpretations were used rather freely in both Lazarsfeld’s early
articles and in Zeisel’s much later reprise of their work is, in one sense,
simply an indication of how rich in implications the details of human
behavior can be when imaginatively reconstructed and arranged by
gifted social scientists (or by artists of any kind). There is little evidence
of tempering these insights or interpretations in the fires of replication,
but this was a beginning—a period of discovery for Lazarsfeld himself—
of the possibilities of psychological theory in interpretations of market
research. And Lazarsfeld never lost his respect or fascination for the
power of the “qualitative” hunch or flash of insight to start the process
of analysis.\(^7\)

\textit{The Analytic Mode}

Lazarsfeld’s particular genius was in the fusing of the intuitive and the
analytic. It has been appreciated by sociologists as a resolution of the
qualitative and the quantitative, the integration of the case history and
the statistical approach.\(^8\) From the standpoint of Lazarsfeld’s own
biography, this fusion seems to represent a union of clinical ideas of
psychology and quantitative ideas of applied mathematics. Lazarsfeld’s
quantitative work is commonly referred to as methods, in implied contrast
to theory (the dichotomy that has so concerned sociologists). It is probably
more accurate to consider Lazarsfeld’s great gifts as an analyst of data
rather than as a methodologist, but he himself accepted the latter des-
ignation. His interest in methods involved him in research design—the
designs of questions, index construction, and panel analysis.

\textit{Questionnaire or Interview Design}

Lazarsfeld wanted to undergird consumer research with more intel-
lectual elegance, which would give it more scientific power. He saw con-
sumer purchase as a special case of human decision, behavior, or “ac-
tion,” a complex activity that had structure, parts, dimensions, correlates,
determinants, and so on and that was entirely worthy of analysis. Sub-
jective measurement of consumers’ reasons was under suspicion in mar-
et research at the time as part of the broad-reaction of behavioral psy-
chology against introspection. As we have seen in chapter three, the
introspective analysis of “motives,” such as those used by Starch in his
early work, came under criticism (e.g., Link’s objection to asking people
“What they think they think”). The proliferation of motives in the Starch
mode did not interest Lazarsfeld, but he argued for the validity of sub-
jective measurement; people could explain the reasons for their own be-
havior and preferences, he contended, if the dimension was clearly iden-
tified.\(^9\) Using a three-part system—an Object, a Self, and Others—to
account for the purchase, Lazarsfeld outlined questioning to illuminate
the attributes of the articles, the impulses or tendencies operating on be-
havior from within the individual, and the influences brought to bear on
the purchase from outside the individual.

The ideas were presented most fully in his 1935 article “The Art of
Asking Why in Marketing Research,” which got attention at the time and
has since become a classic.\(^10\) In this article, Lazarsfeld explained that
finding out “why” was often hampered by the use of a standardized ques-
tionnaire, which he opposed at the time:

Traditional opinion is that a question should be so worded as always to
insure the same reaction on the part of all those interviewed. We advocate a
rather loose and liberal handling of a questionnaire by an interviewer. It seems to us
much more important that the question be fixed in its meaning than in the
wording. This new emphasis places the responsibility on the interviewer for
knowing exactly what he is trying to discover and permits him to vary the
wording in accordance with the experience of the respondent.\(^11\)

Lazarsfeld’s confidence in the trade-off—that one could be sure of fixing
the meaning if one allowed the wording to be flexible—was shared in
The 1930s and 1940s by other social scientists who did interviewing themselves or who entrusted it to elite interviewers in small-scale studies. It was not shared by pollsters doing large-scale studies conducted by large, nonprofessional interviewing staffs, nor later by social scientists who undertook research of this scope.

The use of detailed questions. Lazarsfeld's basic purpose for classifying reasons into the dimensions of why was to be exhaustive about all possible determinants of action, and these could be pursued with a series of "detailed" or interlocking questions that were used, almost detective-style, to track down reasons for some behavior or opinion. This was the beginning of a long battery of questions of varying formats designed to detect evidence of the influence of advertising on the purchase of face powder:

How long have you been using that particular brand?
How long did you know about it before you started using it?
What specific things did you know about it before you started to use it?
(IF SPECIFIC ANSWER IS OBTAINED) ... How did you happen to be familiar with the name?22

Lazarsfeld did not say how respondents reacted to this kind of questioning. Certainly, they had to be taking their purchase of face powder with considerable seriousness to answer such a long battery of questions. It seems unlikely to have been a fruitful line of inquiry for more casual shoppers. In any case, this use of detailed questions seems to reflect the possibilities of small-scale informal work more than it does the constraints of mass surveys.

Classification. Lazarsfeld urged market researchers to use questions of various formats to elicit "psychological raw material, contributed by all of the respondents" and to use classification schemes that would lead to "true conclusions." One of his chapters, included in the 1937 book The Technique of Marketing Research, "Psychological Technique of Classification," is the clear precursor of an article that he and Allen H. Barton wrote in 1951.23 Two of the four rules of classification were virtually the same in 1937 and 1951:

A. General categories of coding should be divided into specific categories. (This was called "articulation."
B. Code categories should be mutually exclusive and exhaustive. ("Logical correctness.")

The other two rules were also quite equivalent in the two versions, but in 1951 they changed in the examples offered, the scope and richness of the rules, and the sharpening and clarity of meaning:

C. Coding should provide some organic structure. It should capture the logic of the situation or the process being studied.
D. Coding should reflect the respondent's own frame of reference or definition of the situation.

The examples in the two versions addressed different audiences. The 1937 version addressed the market community with a stockpile of examples from consumer research (e.g., beer, radios, magazines, commercial laundry, advertising, motor oil, fruit juice, dress shops, pullover sweaters). In 1951 only one vivid consumer remained: the woman choosing cosmetics survived in very much the same language. The analytic thrust had become more elaborate in 1951, as five different "structural schemes" were presented by which to classify materials when true theoretical models were not available, but the basic ideas of 1937 remained. Anyone who works with designing survey questionnaires will find both articles worthwhile. In 1937, the first article was without rival.

Analysis using the classification. The best exposition of Lazarsfeld's analysis by "typological reduction" from this period appears in the 1937 monograph written with Stouffer, In an appendix entitled "Notes on the Logic of Generalization in Family Case Studies,"24 This was a procedure by which to reduce the plethora of detail of case histories and to create new variables. Their example reduced detailed answers on a number of questions to two variables, the exercise of authority in the family and the acceptance of authority, and it then combined them to form the following nine logical types:

<table>
<thead>
<tr>
<th>Acceptance of Authority</th>
<th>(High) Complete</th>
<th>(Medium) Normal</th>
<th>(Low) Reluctant</th>
</tr>
</thead>
<tbody>
<tr>
<td>Exercise of Authority:</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Strong</td>
<td>1</td>
<td>2</td>
<td>3</td>
</tr>
<tr>
<td>Normal</td>
<td>4</td>
<td>5</td>
<td>6</td>
</tr>
<tr>
<td>Weak</td>
<td>7</td>
<td>8</td>
<td>9</td>
</tr>
</tbody>
</table>

These were further reduced to four types of authority, in these combinations:
There were no instances of 7 and 9—complete or reluctant acceptance of authority that was weakly exercised—but logically there might have been, and this logical device would have suggested them.

The fourfold table. In the same appendix, Stouffer and Lazarsfeld presented fourfold tables illustrating the construction of variables from case histories which were then tested for statistical relationships. Lazarsfeld later credited Stouffer with stimulating this mode of analysis:

We were sitting there [at a restaurant, talking with another person about a study]. Sam got impatient. He didn't know quite what it was. So he took a napkin, on the tablecloth, like that . . . [and] drew a fourfold table (on the tablecloth). Sam has neither before or later on ever written or talked about it. I was absolutely fascinated by this device. I'd never heard about it before. Immediately I began to think what the implications of such could be, and my first papers which had to do with fourfold tables are a complete outgrowth of that. Sam used it, but didn't pay any attention.

Lazarsfeld's basic scheme for causal analysis, which he explained in more detail in later work, was set forth in this monograph with Stouffer. He was to make much use of this kind of analysis in his voting studies, and we shall return to it in chapter nine, when we will consider more broadly the work of the Bureau of Applied Social Research.

INDEX CONSTRUCTION AND SUBJECTIVE MEASUREMENT

Lazarsfeld's work in radio research in the United States in the 1930s shows his penchant for index construction. In the two issues of the Journal of Applied Psychology which he edited in 1939 and 1940, as well as in his book Radio and the Printed Page (1940), various indices were under discussion by Lazarsfeld and his coworkers:

- an index of "radio-mindedness" (Olly and Smith/Lazarsfeld)
- an index of "standard of reading" (Lazarsfeld)
- three indices of attitude toward radio (Sayre)
- index construction from case studies (Lazarsfeld and Robinson)
- rating scales on popular songs (Welch)
- the "interchangeability" of indices (Lazarsfeld)

The "standard of reading" was an index that Lazarsfeld constructed as a substitute for reading ability (which he could not measure) from four variables: education, economic status, amount of reading, and level of reading. He dichotomized all four and summed the plus signs for a scale ranging from zero to four:

| Formal education | + Completed high school or went to college
| Economic status (rated by interviewer in the familiar market classes) | + "A" and "B" homes
| Amount of reading | - Did not complete high school
| Level of reading | + Three or more magazines read fairly regularly
| | - "C" and "D" homes
| | - Two or less magazines read fairly regularly
| | - Read a book during the last month
| | - Did not read a book during the last month

These were rough-and-ready procedures, to be sure, with nothing to commend their combination except common sense—that is, there was no indication of the relationships among these variables. Lazarsfeld's operations on these field data were simply an effort to increase the quantitative yield of questions.

His 1939 article "The Interchangeability of Indices in the Measurement of Economic Influences" is of special interest here. Lazarsfeld's thesis was that economic status could be measured in a number of ways—owning a car or a home, having a phone, type of occupation, economic level, and others—and still show much the same relationship to a psychological variable, such as voting. He found from some poll data, for example, that while the proportion of respondents in a given economic level varied from some forty to sixty percent depending on the indicator, the percentage of the Republican vote did not vary greatly. Lazarsfeld sounded a note of caution about the generality or importance of these findings when he said, "Many tests must be made to find out when the interchangeability of indices is possible and when not." Yet in 1940 he also gave the findings the status of a "rule of the interchangeability of indices." The fact of the immediate matter was sturdily practical: the data did not offer the measure of reading which Lazarsfeld really needed, so he improvised a rough substitute from the sphere of radio,
on which much more research had been done. But this was also a root of his “latent structure analysis,” which he considered for many years, in which all manifest indicators were seen to have probabilistic relationships to underlying dimensions. (See also chapter seven.)

Some of the articles Lazarsfeld selected for inclusion in the *Journal of Applied Psychology* were very rough-cut diamonds indeed, simply because he and his coworkers fell heir to collections of data which lacked the variables they needed to fine-cut and sharpen. Lazarsfeld got data where he could—from the *Woman’s Home Companion* panel of readers—editors, from the Book-of-the-Month Club, from Roper, from Gallup, from Market Analysts Inc., who interviewed 765 visitors to a state fair, from magazine circulation figures and radio listening-time estimates, and from high school students willing to serve as a panel of radio listeners. One can visualize Lazarsfeld and his troupe as a bunch of industrious scavengers, finding data in all sizes and shapes, carrying them back to their offices in boxes, crates, baskets, and quart jars, and then setting upon their analysis.

It was a practical quest but also a matter of fascination and conviction, for, as Lazarsfeld explained, he wanted to show how much value remained in data of all sorts—public opinion polls, accident figures, business statistics, or whatever—and also how well simple statistical procedures could illuminate the psychological implications of these mass surveys. Lazarsfeld’s ingenious scavenging and substituting expressed a variant of E.Y. Harburg’s marvelously wry love song: when Lazarsfeld was not near the data he loved, he loved the data he was near.

Lazarsfeld brought from this period an enduring confidence in the usefulness of subjective measurement. He thought it was always likely to be of value and certainly better than no data at all. As he and Stouffer put it pointedly in 1967,

> The writers believe that a careful collection of opinions is far superior to pseudo-scholarly tabulations of the type of statistics which have only a remote relationship to the special problem under investigation.\(^{41}\)

In the 1950s, Lazarsfeld especially regretted the lack of subjective and retrospective measures of the influence of radio:

> It would have been very easy, even in a mass survey, to ask those people who reported a change in opinion and also reported listening to the speech, the following question: “Do you think that the speech has had anything to do with your change in opinion?” We are quite positive that this special group of listeners and shifters for which the question was appropriate, would have been able to give an intelligent answer, but somehow such questions still conflict with the convictions of many students in social research.\(^{56}\)

By “students of social research,” Lazarsfeld clearly meant the behaviorists in psychology and sociology.

While he became a sociologist in the 1940s, by his faith in the subjective realm of respondents’ own self-report and the investigator’s “depth” interpretation Lazarsfeld showed a continued affinity with some psychologists—not with the experimentalists, but with the clinicians (or novelists), because of their ability to fathom or fashion the “deep structure” of human experience from the everyday details of human biography. Lazarsfeld’s involvement in Charlotte Bühler’s project in autobiographies and life phases seems to have been a far more enduring heritage from Vienna than his socialist politics were.

### PANEL ANALYSIS

Lazarsfeld’s interest in panel analysis apparently dated from his early years in the United States. As we have seen, the major purpose of panels in market research was usually not to measure change over time but instead to save money in the present. Lazarsfeld transported the term from market research into the study of opinion and gave it new connotations as a measure of change. In a 1938 article, “The ‘Panel’ as a New Tool for Measuring Opinion,” Lazarsfeld and Fiske still reflected more of the dominant market research concern with saving time and money than they did a concern with measuring change.\(^{54}\)

In the *Journal of Applied Psychology* series, three articles used data collected from the same panel of 500 high school students who had served as a pool of respondents for various research projects, but only one study showed concern with change measures. Lazarsfeld’s own article on change in this set involved retrospective measurement. It was a series of questions in a single Gallup Poll of October 1937, asking respondents how they had felt about an issue in August. Lazarsfeld later cited this article twice in a rather confusing context, suggesting that it was a panel. In this period, he seems to have used the term panel to mean both retrospective measurement and repeated measurements taken over time in the same individuals.\(^{56}\)

By 1940, the measurement of change had become for Lazarsfeld the chief attraction of the panel design. He now had some experience with
In principle, such a promoter might well have been a political scientist rather than a psychologist, for public opinion had long been of concern to political theorists. It would have been unsurprising to find political scientists burrowing into the mounds of data (national and, soon, international data) which the pollsters were collecting. Indeed, certain political scientists were interested—Harold Gosnell, Harold Laswell, Harwood Childs, and Peter Odegard, for example—but they were not typical of the discipline. Cantril wrote later that in the fifteen years during which he archived poll data at Princeton University, when dozens of scholars had had tabulations run for their own analyses, it was psychologists, sociologists, and some economists who came. "Oddly enough, not a single political scientist (except one graduate student) ever darkened the door of the Office of Public Opinion Research between 1940 and 1955."90

When I read in 1955 that the survey data was being launched and reported in the newspapers by George Gallup, and saw the Fortune surveys begun by Elmo Roper and his associates and by Archibald Crossley, I felt that here was a new instrument the social scientist, particularly the social psychologist, had better look into. The survey technique seemed to hold possibilities for the study of genuine problems, for learning how people look at things, and for understanding better than we did why people of various backgrounds, interests, loyalties, and information levels hold the opinions they do.91

Certain early interests of Cantril’s academic career now make his gravitation to polling seem a natural.

**Cantril’s Early Interests**

**“EVERYDAY LIFE”**

Cantril’s 1932 doctoral dissertation, “General and Specific Attitudes,” addressed a psychological controversy of the time which turned on disentangling “attitudes” from “traits”—for example, the degree to which specific preferences, such as whether one would rather meet a pope or a president, were correlated with general attitudes about the religious realm or the political. It built on the work of Gordon Allport and P.E.
Vernon, *A Study of Values* (1951). 42 That Cantril would have a special taste for poll data is suggested even more by his lengthy 1954 article “The Social Psychology of Everyday Life.” 43 Here, he urged psychologists to start with a human problem and devise appropriate methods to explore it rather than cutting it to the size and shape of the laboratory methods they had on hand.

By “everyday life,” Cantril meant fads and fashion, gossip and rumor, friendship and conflicts, suggesting a welter of questions. For instance, how did the standards of “good taste” vary in different groups? Was it true, as Kant observed, that married people grew to look alike? Did unemployed people tend to think environment more important than heredity? To what extent did a woman take an interest in clothes because of competition with other women? Why were people interested in scientific discoveries that were entirely incomprehensible to them? Did the radio have any influence in modifying language? How did industrial psychologists in the United States and the Soviet Union differ in their assumptions? And so on. Cantril listed a potpourri of research projects—about 185, in fact, which differed greatly in their scope and specificity—and grouped them into almost forty different substantive topics, many of which he found had been largely neglected.

While this collection included political topics, it was clearly much broader, and this may have influenced Gallup’s occasional choice of topics. In the Gallup Poll, over the years there has been a sprinkling of poll questions bearing on culture and personality, more than in Roper’s poll or, later, in that of Louis Harris. Some of these questions were clearly designed for a client’s immediate practical interests (such as travel preferences and plans) rather than for any long-term interest of Gallup’s or Cantril’s in social history, but they serve the latter purpose quite admirably. Whether they reflect Cantril’s interest or influence is not clear, but they were surely similar to the interests Cantril expressed in the 1954 article. For example:

What do you think is the greatest invention that has ever been made? (1947) (Highest mention, electric light and electrical appliances.)

Generally speaking, whose life is more difficult—a man’s or a woman’s? (1946) (30 percent of men and 18 percent of women answered a man’s life; 47 percent of men and 61 percent of women answered a woman’s life.)

(Asked of married women) What is the chief fault of your husband? (1946) (Highest mention, thoughtlessness.)

(Asked of married men) What is the chief fault of your wife? (1946) (Highest mention, nagging.)

(Asked of parents) Do you approve or disapprove of spanking children? (1947) (Parents who were not spanked as children approved by 38 percent; parents who were spanked as children approved by 81 percent.)

If you could have been present and seen any one event in the whole history of the United States—which event would you like to have seen? (1947) (Highest mention, signing of Declaration of Independence.)

Are you going to make any New Year resolutions? (1947) (Of those planning) What will they be? (Highest mention, improve own disposition, be more understanding, control temper.)

What do you argue about most with your husband (wife)? (1948) (Highest mention, money.)

Can you locate the following ten states on this outline map of the United States—California, Texas, Pennsylvania, New York, Illinois, Ohio, Michigan, New Jersey, Massachusetts, and Missouri? (1948) (Correct answers by education: grade school 4 percent, high school 6 percent, college 8 percent.)

What is your favorite first name for a boy? For a girl? (1948) (Highest mentions: John, William . . . ) (Mary, Elizabeth.) 44

Cantril’s broad sense of the “psychology of everyday life” puts in relief the narrow political focus of most early poll questions. 45 In Cantril’s own case, he did not actually undertake research into most of these cultural questions. During the Second World War, his own survey work bore largely on politics. However, his later work (in the 1950s and beyond) did show the broader interest in human experience, as he undertook studies of human aspirations and the quality of life in a number of different countries, using a “self-anchoring scale” by which to make cross-cultural comparisons. 46 Cantril’s early study of radio listening with Allport, published in 1935, showed some of this interest in “everyday life.”

In the radio study, the bulk of the empirical work was based on laboratory experiments, most of them conducted in the Harvard Psychological Laboratory, with a few done in a Boston radio station. The laboratory work was supplemented with a questionnaire collected from a quota sample of 1,075 people from several communities. 47 The purpose of the questionnaire was to find out what kinds of programs people preferred: sports, recipes, church music, dance music, political speeches, and so on, through a checklist of forty-two items, the frequencies of which were ordered by age and sex. Respondents were also asked for
other, more detailed preferences (voices, jokes, music, speeches) as well as the effect of radio listening on other leisure-time activities. There was no integration of the two kinds of data, but the mere use of them was unusual at the time, and Cantrell used the two again when he supplemented detailed interview findings with poll results, especially Gallup data, in his widely read book *The Inhuman from Mars* (1940). By this time, Cantrell had become professionally involved with both Gallup and Lazarsfeld, associations that would prove to be of consequence for survey research.

**THE GALLUP CONNECTION**

In 1936, before the November election, Cantrell had been dispatched by the *New York Times* to interview Gallup about the new polls. Cantrell was impressed. The article that came from it reflected several themes that Gallup would repeatedly stress: the intellectual heritage of James Bryce; the "scientific" polls that sampled a cross section of the population versus "practical" polls that depended on vast coverage (e.g., the Literary Digest); the readjustments required because of incomplete returns; the function of polling for two-way democratic communication, especially to correct for the influence of pressure groups; and the guarantees of the pollsters' integrity. Cantrell's account was indeed sympathetic:

The polls now best known to the public are unquestionably honest, yet their sponsors are constantly accused of some partisan or social bias, or some selfish and hidden motive. Most critics fail to realize . . . that the very life of public polls depend[s] in the last analysis on their proved reliability.

Cantrell did not subscribe to all of Gallup's views, however. Unlike Gallup, he credited the existence of a bandwagon, for example ("By creating the illusion of universality it helps the wavering citizen to jump on the bandwagon"). Cantrell also wrote that while issues were inevitably oversimplified in democratic politics, polls cut away still more of the subtlety and complexity. He pointed to other difficulties as well as to other advantages. Cantrell later recalled the meeting itself as cordial and fruitful; Gallup, he said, was "delighted to have a social scientist take his work seriously and offered his facilities at cost for any research I might want to do." The offer was appealing. And Cantrell remembered the chance to dig into the Gallup data as one of the chief attractions of joining the faculty at Princeton University, where he accepted an offer in 1936.

At Princeton, Harwood L. Childs, a professor of political science, was trying to set up an academic institute for research into public opinion. Cantrell joined with him in 1937 as one of four founding editors to establish the journal *Public Opinion Quarterly*. Cantrell also began to make considerable use of Gallup data. His association with Gallup remained cordial and fruitful, unlike his collaboration with Lazarsfeld.

**The Radio Project with Lazarsfeld**

**THE ORIGINS**

It is not without irony that Lazarsfeld and his students came to consider 1937 as the founding date of the Bureau of Applied Social Research, the organization at Columbia University which Lazarsfeld officially directed for more than a decade and which he always inspired intellectually, whatever his title. Official publications of the Bureau do likewise. The founding project was really a project initiated by Cantrell and Frank B. Stanton, research director (and later president) of CBS. Within two years, by the fall of 1939, it had indeed become Lazarsfeld's. In the way in which a forceful personality is often described as "filling a room," Lazarsfeld "filled" the Cantrell-Stanton project.

Cantrell and Stanton had obtained a two-year Rockefeller Foundation grant for $67,000 to study the psychological and cultural impact of radio. They offered Lazarsfeld the job of directing the research while they took roles as associate directors, and the Princeton Radio Research Project was launched in the fall of 1937, with the three principals in three places in close proximity. The grant was administered through Princeton University, where Cantrell was, and where Lazarsfeld was given a formal research appointment. The project was directed at the University of Newark, where Lazarsfeld was, making the radio project part of the ongoing work of his research center. Much of the actual work was done in New York City, where Stanton was. When University of Newark officials asked Lazarsfeld to vacate his headquarters because they needed the space for other uses, he took the project to New York, to a building on Union Square. By the spring of 1939, he had obtained a Rockefeller renewal, and the radio project officially became an organization, the Office of Radio Research (ORR) (later renamed the Bureau of Applied Social Research). Lazarsfeld continued to collaborate with Stanton, but he and Cantrell had a falling-out.

Cantrell promptly obtained another Rockefeller grant to establish the
Princeton Office of Public Opinion Research (OPOR) in 1940—it seems unlikely that the parallelism of the two names was coincidental—and did no more research into radio. It would have been at least as plausible for Cantril to have dated his organization, OPOR, from 1937, rather than from 1940, as he did. If OPOR had flourished as the Bureau did, expanding in the ranks of loyalty, élan, and a sense of its own history, perhaps Cantril would have.

THE CONTENTION

Cantril left no public account of the rift with Lazarsfeld. In his memoir, The Human Dimension, he leaped over the Lazarsfeld-radio experience without a word, proceeding nonstop in two sentences from his 1936 meeting with Gallup and his move to the Princeton faculty to his 1940 application for funds to set up OPOR. It was well known to his contemporaries that Cantril considered Lazarsfeld his arch rival and that he made invidious distinctions between OPOR and Lazarsfeld’s shop. Lazarsfeld, for his part, did leave an account in his memoir. There were struggles over money and program, he recalled:

Cantril was correctly strict about budgetary arrangements, since the university held him accountable. I did not make life easy for him, because I often exceeded the Princeton budget, sure that I would cover the deficit with additional income from some other source. By and large, this worked out all right.84

Not from Cantril’s perspective. The two scholars had grand battles over money, according to Marjorie Fiske, who was working with them; Lazarsfeld keenly resented Cantril’s overseeing role, and Cantril felt very disturbed by the financial chaos.85 The research program also became an issue because Lazarsfeld veered away from the experimental work that Cantril had expected to do toward secondary analysis of poll data, without shaping a new plan for research. The Rockefeller Foundation people asked Lloyd A. Free to join the staff of the Office of Radio Research as an editor to move the project along, and Free worked for some months to bring Radio and the Printed Page to publication.86 Financially and intellectually, Lazarsfeld’s directorship was clearly not a tidy affair. By his own description, it was a “policy of research improvisation guided by available material and personal interests and contacts.”86 It was not everyone’s cup of tea, and it was certainly not Cantril’s. In future research endeavors, these two strong personalities kept their distance.

SURVEY RESEARCH ON RADIO

From the period of their radio work together, Lazarsfeld produced the work already noted. Cantril, for his part, produced some articles and, with Hazel Gaudet and Herta Herzog, the book The Invasion from Mars (1940).87 This was an analysis of panic experienced by radio listeners who thought that Orson Welles’s adaptation of H. G. Wells’s The War of the Worlds was the real thing, that the Martians really had arrived. The “invasion” event of mass panic itself was interesting to social scientists, and the book was dramatic and appealing. It was reviewed favorably and was later excerpted in the Newcomb and Hartley volume Readings in Social Psychology (1947).88 Setting forth, first, the script of the Welles radio drama, it proceeded to an analysis based on detailed interviews with 135 persons, most of whom were selected because they were known to be upset by the program and because they could be reached easily. Four interviewers conducted intensive case histories of the respondents’ reactions, using a schedule of questions which they were free to use as a guide. The estimate that something on the order of 1 million people were frightened by the broadcast was made from Gallup data.89

The very urgency and transience of the event pressed the researchers hard, and their book combined journalism with psychological concepts rather than with anything more rigorous. Cantril and his collaborators were well aware of the problem, disclaiming any pretense to sampling in method or to generalizability in conclusions, but they did try to link their findings at certain points to a broader context. They made comparisons with a CBS survey. They also used Gallup data to assess the size of the listening audience, and they reported the results of a Gallup question bearing on job security in support of the interpretation that “unsettled conditions” contributed to vulnerability to suggestion and thus to panic.90

Cantril proceeded immediately in the text to the observation that two Gallup questions asked in the same ballot showed a discrepancy between respondents’ identifications of their social class and their economic class. (Cantril was contributing questions to Gallup during the 1940s, so this may have been one of his own.) Cantril later pursued this observation in an article, “Identification with Social and Economic Class,”91 showing that people were more likely to see themselves as “lower class” in economic
terms than in social terms. This concern with measures of social class appears to be the beginning of the idea developed by Cantril's student Richard Centers, in a study of subjective and objective indicators of social class which has become a classic, *The Psychology of Social Classes: A Study of Class Consciousness*. This is a good example of meaning yielded by treating indices not as "interchangeable" but as discrepant in analytically interesting ways.

Cantril was influenced by Lazarsfeld's analytic style during this period, specifically concerning the technique of index construction which coded open-ended answers for their apparent presence or absence of certain characteristics (insecurity, lack of self-confidence, religiosity, etc.), and he credited Lazarsfeld heavily in the introduction to *The Invasion from Mars*:

The author's gratitude is due to Dr. Paul Lazarsfeld for innumerable suggestions for analysis and interpretation ... an invaluable intellectual experience. Because of his insistence the study has been revised many times, each revision bringing out new information hidden in the statistics and the case studies.33

It seems likely that Cantril turned from radio research to a concentration on public opinion research in some part because of his dispute with Lazarsfeld. In any case, *The Invasion from Mars* was Cantril's second, and last, substantial involvement in radio research. It was indicative of his interest in suggestibility and mass movements, a subject he pursued in his 1941 book *The Psychology of Social Movements*, in which he called upon considerable poll data.45 Before the war, Cantril became engaged in attitude/opinion research to improve the technical methods of polling and to use its content for government policy.

Policy Research for the Government

Cantril became involved in government intelligence work with his colleague Lloyd A. Free, who, as noted earlier, had edited the Lazarsfeld book on radio. Free, a Stanford-trained lawyer, had become attracted to public interest broadcasting, State Department work, and opinion polling, and he became a lifetime collaborator of Cantril's. Before and during the war, research projects involved them both in some clandestine surveys abroad for the U.S. government and some discreet relays of poll opinion at home to President Roosevelt's staff.

In 1940, Free conducted the first public opinion survey in Brazil, an intelligence operation disguised as market research, which tried to determine how to reach Brazilians by short wave when the country's leadership was siding with the Axis powers. He and a crew of twenty interviewers made their way up the Brazilian coast by commercial boat—when they could get reservations (they sometimes had to wait on shore for as long as a month). Over the years of collaboration with Cantril, Free was to direct many such international undertakings in survey research in Nigeria, Panama, Italy, Yugoslavia, Cuba, and other countries. (His monograph on Cuba in 1959 showed widespread support there for Castro—good reason, indeed, not to mount a Bay of Pigs invasion.) Free estimates that ultimately he and Cantril sampled one-third of the world's population.50

In domestic policy work before the war, Cantril designed poll questions of special interest to President Roosevelt. Cantril had no field staff himself in these early years, but the AIPO facilities were available and Cantril dispatched national poll results to the White House staff. This quiet work for the White House is more explicable if one remembers, as Kari and Katz point out, that until 1939 the president had no discretionary funds at all with which he could commission the gathering of information without the approval of Congress.51 The information for Roosevelt (during the war and probably before) was financed by Listerine heir Gerard B. Lambert.52

Cantril (or Gallup) asked a variety of interventionist/isolationist questions of the following sort:

Which of these two things do you think is more important for the U.S. to try to do: to keep out of war ourselves or to help Britain, even at the risk of getting into war? (May 29, 1941: 58 percent, Help Britain)

Should the United States go into the war now and send an army to Europe? (Sept. 17, 1941: 8 percent, Yes)53

In a "tree diagram" that became well known, Cantril showed that questions on U.S. involvement in the war varied enormously according to the contingencies expressed in a given question. That tiny 8 percent, for instance, who wanted to send an army over in the fall of 1941 swelled to 76 percent when it was a matter of sending aid to Britain even if this ran the risk of U.S. involvement. The Cantril/Gallup trend data during 1939–41 showed a rising ride of willingness to risk war in order to help Britain and a rising belief that the United States would indeed get into it.
In his analysis, Cantril tried to make a very close correspondence between specific events of the war and opinion change, which is unconvincing, but the rapid upward movement of various interventionist indicators is unmistakable. He also showed some interesting properties of prewar opinion, such as the greater intensity associated with interventionist than with noninterventionist sentiment; the evidence of cognitive structure and consistency in multiple measures of opinion; and a number of interesting generalizations about national morale (e.g., that high morale was more closely associated with an expectation of victory than with a sense of righteousness of cause). These Cantril-Gallup data from the prewar period were continued during the war and remain a fascinating historical document.

The trend data were published in Public Opinion Quarterly. By 1940–41, Cantril and Free together had made an impact on the journal, for Free had become editor of POQ for a year at Cantril's urging and had tried to expand the subscription lists beyond the academic community, particularly by giving coverage to polling, public relations, and advertising. Free instituted the section entitled "The Polls," which has remained a continuing feature of the journal ever since. The Cantril-Free influence on the journal was decisive in making it especially responsive to poll/survey data and in extending its scope beyond academic realms. Both Free and Cantril operated comfortably in elite levels of business and government, in close touch with Gallup, Lambert, Rockefeller, and others. These contacts were valuable for the funding of Cantril's various research activities during the war.

Rensis Likert (1903–1981)

We have already had occasion (in chapter two) to consider Likert's early work in attitude scaling and in a panel study. Subsequent phases of his career, to which we turn now, involved him more directly in polls and surveys. After completing his doctorate, Likert took a teaching position at New York University, where he also participated in the Psychological Corporation, acting as its New York representative and secretary of the board of directors and working with the network that conducted surveys with student interviewers. Likert recalled an instance in which some graduate students at NYU who were deeply involved in one of the Psychological Corporation surveys (a study of attitudes toward the Veterans' Bonus Bill) recruited a large group of undergraduates to help with interviewing. At this, the word went out from the department chairman to all members of the department that no one was to employ students in outside projects without the chairman's express permission. Likert refused to comply, and he left NYU. He did some market research in an advertising firm and some teaching at Sarah Lawrence College before accepting a job directing research for a life insurance institute in Hartford, Connecticut (the Life Insurance Sales Research Bureau) in December 1945.

Research in Industry

The psychologist Leonard W. Ferguson later evaluated the major publication resulting from Likert's research at the life insurance institute, Morale and Agency Management (1940), by Likert and Willis, as one of the pioneer works of research in organizational motivation and morale. Ferguson considered it "the backbone of much individual company effort to motivate and enhance the morale of their sales organizations." Although these morale studies apparently had wide circulation within the insurance industry, they were not published in professional journals or by commercial publishers. The morale work is of interest to us here for two reasons: it shows Likert's early interest in organizational theory, and it also shows his continued concern with measurement.

In the morale reports, organizational ideas were conveyed through examples from history and literature. The leadership styles of Napoleon and Robert E. Lee were contrasted: Napoleon was seen as a quintessential "boss" whose troops hated him and cheered only upon command, and Lee was seen as a leader who inspired loyalty and sacrifice because his soldiers felt his concern for them. Such images were congruent with counsel elsewhere in the reports for managers to be concerned about their subordinates as individuals, to be generous in giving public recognition to them, and to resist the temptation to act like a "big shot." This was indeed the "human relations" school of organization rather than the bureaucratic or physical efficiency model of rationalized tasks. It was more suffused with psychological values, the importance of interpersonal contact, moral concern with individuals, and a rather simple model of encouragement and praise. These basic ideas were at the root of Likert's organizational theory, which he later developed into a sophisticated model of communication, group structure, styles of supervision, and the process of conflict resolution.

The methodological work he conducted in the insurance field re-
fected the use of both interviewing and questionnaires in industrial psychology in the work of Houser and others (chapter two). Likert used the two in an experiment that was another variant on the qualitative/quantitative comparisons that he and Murphy had worked on for Public Opinion and the Individual (see chapter two). The life insurance experiment was a more limited and successful one, not unlike the Stouffer dissertation of 1930, except that this venture used personal interviews rather than written materials.

Life insurance agents' attitudes were sought qualitatively in personal interviews developed conversationally around a set of topics. Some dozen topics were covered in interviews with 292 agents, scored independently by research staff members on a morale rating from one to five, and then averaged by agency. A second set of morale measures was obtained by a mail-back questionnaire that each agent was asked to fill out anonymously. (The return rate was two-thirds.) These morale scores were also averaged by agency, which was almost always known from the postmark. Agency scores on the two measures were quite comparable (with a rank-order correlation of .85). 77

Certain features of the mail-back questionnaire are of interest. First, it was called "the ballot" in an obvious reference to voting and very probably to Gallup polling as well, for Gallup was cited appreciatively in the reports in support of the fact that the researchers had sampled rather than enumerated agencies in the association:

During the 1936 presidential election campaign an intelligent young research man named Gallup directed the interviewing of only a few hundred thousand voters about their choice of candidates. He had carefully selected these voters to give him a true cross section of all the people. From his results he predicted the outcome of the election with astounding accuracy. Today his name is a household word, and his public-opinion polls are in every man's conversation. 78

Indeed, in this period Likert shared in the quota sampling of market research, praised Gallup's work, and addressed an audience in the Department of Agriculture which found sampling itself to be an implausible notion. 79

Second, this questionnaire was considerably more like public opinion polling than the Murphy/Likert attitude study of the classroom: it was short (19 rather than 200+ questions), the language was colloquial, and the results of the experiment commented the "quantitative" or closed measure for ease and efficiency, just as Stouffer's work had.

Why, then, in a very few years, did Likert advocate the more qualitative approach of interviewing with open-ended questioning when he directed a public opinion unit in the federal government? The job that Likert took in 1939 with the Department of Agriculture was important. Over the span of about five years, Likert worked in three quite different realms, each grittier than the last: classroom, insurance agency, and the world of the working farmer. The farther Likert got from the classroom, the more he and his associates had to learn about what was going on in the particular subculture, and the less constraint there was on the respondents to tell them—in writing. Likert himself later wrote that the general public could not be pressed into the service of attitude measurement the way students were and asked to adapt their own thinking to the dimensions set forth by the experimenter. 80 It is also the case that when Likert arrived at Agriculture, research with farmers was being conducted in the very informal "cracker-barrel" tradition.

Government: The Division of Program Surveys

On September 1, 1939, Likert became director of the small interviewing unit that M.L. Wilson had organized in USDA in 1935–36 (see chapter one). The "scouting" operation had given enough satisfaction to the department that it was upgraded and placed in a larger unit that was charged with planning and research, the Bureau of Agricultural Economics (B.A.E.). There had been growing interest in making the interviewing yield more information. However, fascinating the scouting reports were, it was difficult to generalize from them, and in any case they were not always in agreement. Wilson and others in Agriculture consulted with various social scientists about how to proceed in program and personnel: Floyd Allport, Leonard S. Cotrell, Jr., Jerome Bruner, Cantril, and others were consulted. 81 Robert S. Lynd was also a special source of advice. His book Knowledge for What?, a fervent advocacy for interdisciplinary social science in the service of social problem-solving, was highly regarded in USDA. When Lynd was consulted about the new division, he recommended Likert for the director's job; he also told Likert about the job opening, and Likert applied. 82 (Lynd was in fact almost as facilitative for Likert in 1959 as he was for Lazarsfeld.) The organization was renamed the Division of Program Surveys because its mission was to conduct surveys into farmers' experiences and opinions about the programs of USDA, such as domestic allotments, soil conservation, farmer resettlement, tenant purchase, and the like.
ADAPTING TO NEW MEASUREMENT PROBLEMS

Early in his job at Program Surveys, Likert expressed some interest in trying to make use of scale measurement of various sorts, such as the Sewell and Chapin scales of socioeconomic status, but there is no sign that he expected to bring his own attitude scales to the farm. For the mission of Program Surveys, attitude measurement in the college classroom tradition would surely have been a folly. Furthermore, personal interviewing was the reigning tradition when Likert arrived. The “scouts” were ardent advocates not only of personal interviewing but also of a very intuitive and nonstandardized form of it. As Likert wrote, “Considerable effort has gone into trying to learn what these people really feel by avoiding artificially focused alternatives as the basis for noting their responses.” Likert’s role in the beginning was to work within the tradition of informal methods and to try to “reform” them with some system and structure. In his efforts to create some standardization, we can see some of the same problems that Lazarsfeld faced in moving from small-scale to large-scale measurement and analysis.

Likert contrasted his views about question wording with those of Gallup:

It is not necessary, as is sometimes thought, for every interviewer to use precisely the same words in asking a given set of questions. The words may vary so long as the question in each case represents exactly the same idea.

This was a view that Lazarsfeld had advocated in his “Why?” piece of 1935. The freedom that Likert espoused was actually a new stage of constraint for the interviewers, who had not been using written questions at all. He hoped to achieve a balance between standardization and flexibility by providing written questions that allowed for variation in their wording under certain circumstances. Interviewers, to be sure, were cautioned against rephrasing questions in ways that would generate bias. The following, for instance, was not the thing to say:

Mr. Jones, I noticed the new terraces in that field of yours over there. Am I right in believing that you like them very much, not only because they keep your soil from washing but also because the water they hold back will increase your crop yield?

Interviewers were also cautioned to avoid biasing chitchat or “emotionally charged words.”

These instructions, however, were fair proof of the responsibility that the interviewers had at this time. Avoiding emotionally charged words, for example, would later be seen as the responsibility of the researchers or study directors who wrote the interview schedule. Indeed, pollsters already saw it as their responsibility.

The new regime of greater standardization at Program Surveys was not without conflict. A good deal transpired between the interviewers and Likert’s second-in-command, Bela Gold (who had worked with Lynd on the second Middletons study). Two or three of the scouts transferred out of Program Surveys, in fact, feeling that the new emphasis on standardization and quantification was unconvivial, sometimes more distorting than translating the spirit of their reports.

The interviewers felt that they were being “layered” as well. Intervening between them and the top officials of USDA was a new set of supervisors and interpreters—Likert and Gold—and a new process that de-personalized their contribution. Some of the old-style interviewers stayed on, but they were not entirely reconciled to the ways of the new regime. Their situation was being affected by changes at the very top, in any case. Secretary Wallace’s direct interest and contact with BAE waned as he became involved in the campaign of 1940 for the vice presidency. M. L. Wilson did not replace him as secretary, becoming instead the head of the Extension Service of Agriculture. The top administrators who had taken a direct interest in the personal reports of the scouts now had other aspirations and responsibilities.

SURVIVING AND EXPANDING

The first year of Program Surveys had begun rather inauspiciously, for within a month of the new administration Likert announced that the division was broke, its modest appropriation of $30,000 having been virtually exhausted before his arrival. Additional funds were garnered from BAE, however, and the work went forward. During that first year, Program Surveys conducted studies for various agencies and programs within the department—the AAA, the Farm Credit Administration, the Forest Service, Extension, the Food Stamp Plan, the new homestead communities, a study of forest fires being deliberately set in a backwoods farm community, and others. (For the most part, a single interviewer seems to have been responsible for a given study.) The work was going well enough that Secretary Wallace coauthored an article for the spring 1940 Public Opinion Quarterly which publicized the new division.
After that first year, expansion was Likert's motif. As the first annual report argued, there was need of more interviewing; coverage was wanting in the Appalachian states, the Eastern corn belt, and other areas; samples needed to be larger, so that analytic breakdowns could provide statistically reliable results; and more agencies within Agriculture needed the services of Program Surveys. The very conception of farm problems was too narrow, in fact. Likert argued that farm problems and reactions needed to be seen in the larger context of national and international problems that affected agriculture and had meaning in their own right, such as national morale. Indeed, the day that Likert had taken the Program Surveys job was the day that Hitler invaded Poland and the Allies declared war. While the mission for Program Surveys was born of peacetime problems in Agriculture, it soon came to reflect concern with American defense and, later, involvement in the war.

In its second year, Program Surveys did indeed expand to an appropriation from BAE of just under $100,000. BAE's contribution to the budget never exceeded that amount by much during the lifetime of the division. It was through "entrepreneurial" work that Likert sought with other federal agencies outside of Agriculture that Program Surveys expanded its function, funds, and reputation. The first of these was the Office of Facts and Figures (OFF), headed by the poet and Librarian of Congress Archibald MacLeish.

The Bureau of Intelligence within OFF was charged with learning the state of American opinion on many matters of national morale and defense. Program Surveys' association with it began shortly after OFF was established in October 1941. It was the entry of the United States into the war after the Japanese attack on Pearl Harbor that made the association an important one. Waldemar Nielsen, the field director of Program Surveys, explained the process a year later for new interviewers coming into Program Surveys:

On the night of December 7 a hasty conference was held in the Washington office. Program Surveys interviewers scattered at such points as Reidsville, North Carolina; Okmulgee, Oklahoma; and Norfolk, Virginia, were summoned to a meeting at Memphis, some of them driving all night to get there. They flew to the West Coast in time to experience Los Angeles' first full-fledged blackout, and began interviewing. Most of them had had little urban interviewing experience, and they were obliged to proceed with extreme caution because of the intense emotional state among people just roused to war.

The resulting report on "Immediate Developments after Pearl Harbor," rushed into the hands of administrators on December 14, gave evidence that here was an organization which could carry on a new type of social research with remarkable speed and versatility. Moreover, it was able and eager to undertake problems of a national scope, providing information about public reactions which no other federal agency was prepared to secure.

Program Surveys was by no means set up to dispatch interviewers routinely with this speed and urgency, but it was indeed alone in the government opinion research field. Wartime would spawn a number of government projects in public opinion work, but Program Surveys was already in existence at the outbreak of the war, and at that time it had a unique capability for national government work. During the war years, it would expand its mission well beyond the Department of Agriculture.