Proceedings of the American Association for Public Opinion Research

At the Fourth Annual Conference on Public Opinion Research, Ithaca, New York, June 19-22, 1949

(Edited By Robert T. Bower)

FOREWORD

The year 1948-49 was a crucial one in the brief history of public opinion research. It is still far too early to estimate what may be the effects of the pollsters' miscalculations in the Presidential election predictions in the Fall of 1948. Perhaps from the perspective of 1958 one may look back and trace significant changes and important developments dating from this milestone in time. The meetings of the American Association for Public Opinion Research at Cornell University were the first full gathering of the professionals in the field of opinion research after the November setback. The most salient feature of the Ithaca meetings was the obvious determination of the conferees gathered there to leave nothing undone which could lead to greater perfection, greater reliability, more meaningful interpretations of the fruits of their research labors. The condensation below of the proceedings of their conference documents this serious determination, and in a variety of ways points up the directions in which the academicians on one side and the commercial practitioners on the other must continue to work together for the common development of the profession.

ELMO C. WILSON
President, American Association for Public Opinion Research, 1948-49

Editorial Note: The following reports represent a rather severe condensation, necessitated by budgetary considerations, of the full proceedings of the conference. All remarks have been compressed considerably, and it has unfortunately been necessary to leave out altogether many valuable comments from the floor. Thanks are due to the volunteer reporters who kept a record of the various discussions and in many cases wrote up the reports, and to the many others who assisted in the preparation of this record. Whenever possible, participants have been asked to check over the remarks attributed to them, but this has not proved feasible in a few cases. The editors regret any inaccuracies which may have crept in.

OPENING SESSION OF THE CONFERENCE

(Sunday, June 19, 1949)

Chairman: Elmo C. Wilson, President of AAPOR

Welcome to Members of AAPOR: President Edmund E. Day, Cornell University

Address: "Public Opinion Research and the Science of Politics." Stuart A. Rice, Assistant Director of the U.S. Bureau of the Budget and President of the International Statistical Institute.

THE CHAIRMAN: After exhaustive research, I find that the president of a professional association has two tasks at the annual meeting. He should congratulate the profession on the accomplishments of the past year, and he should sound a keynote.

The first function is somewhat difficult to perform, since there is no denying that the professional orchestra struck a discord in November of 1948. In spite of the fact that most instruments played the wrong note and mutual recriminations could be heard among the musicians, however, the vitality of the music proved to be too strong to be written off by unsophisticated critics. Public opinion research suffered a set-back, but by no means an eclipse.

As a keynote, I would suggest that we must broaden our horizons. Those engaged in commercial research should not brush off the criticisms of critics who have never met a payroll. But on the other hand, those who have never met a payroll should recognize that factors of cost exist, and also that their commercial colleagues must often take a stand one way or the other, and cannot merely present factors on both sides of an issue. Through discussions such as those we are starting now, we must agree on a body of essential principles and purposes. But our standards must be self-imposed—they can be elaborated and enforced only from within the profession.

Several suggestions have been made during the year looking toward expanding the functions of this association. Of these, I would like to urge three in particular for your consideration. First is the question whether we should not establish a review board on standards of opinion research. Second is a proposal that we form a Research and Development Committee to assist in the technical development of the field. Third, it has been suggested that we may profit by the establishment of sub-groups on a local or regional basis, and that our charter should be amended to permit this.

I shall not welcome you here formally, since President Day will do that for Cornell University, but I hope that all of you who have come to Ithaca will find the conference well worth while and will return with new ideas and stimuli which will be useful in your work during the coming year.

PRESIDENT DAY: Mr. Chairman and members of the conference:

One of the standard duties of a college or university president is to welcome important groups of visitors to the campus. My first official duty when I came to Cornell was to do just this, and it now looks as if my last official duty before I retire from the presidency at the end of this month might take the same form.

Many of the messages of greeting and welcome which I have had to extend on behalf of the University over the past twelve years have been more or less pro forma. They have been strictly official. However, I do not feel that way about this one. I have a strong interest of my own in the problems with which this particular Association is concerned. Hence I am led not only to welcome you to the Cornell campus, which I do most heartily, but to pose certain questions which seem to me as an outsider to merit your Association's serious attention.

First I am led to wonder how you propose to improve the important new tool which is basic to your work. Already this tool has shown itself to be one of the most important instruments in the whole professional kit of the social scientist. Quite obviously, however, it is not yet fully perfected. Every effort should be made to improve it just as rapidly as possible. How is this to be done?

Even with the tool in its present state of imperfection, certain questions loom large with respect to its use. What special functions can the tool be made to serve in political science and practical politics, in sociology and social work, in economics and business administration; in fact, in the whole range of social science, pure and applied? On what particular problems of American life should it be brought first to bear? What effect would an accurate voting poll have on American political life? Clearly enough, questions of this sort have far-reaching implications.

Finally it is important to deal with the fact that this Association operates in a field in which academic interests and commercial functions overlap. It is important to determine as far as possible what relationships should exist between the two. I am myself convinced that universities should not in general be in business. Nevertheless, they carry on work which inevitably involves them in matters of concern to business. Although it may be unwise to undertake to draw a sharp line between academic and commercial operations, there would seem to be every reason why some differentiation of functions should be recognized and respected. Cooperation to mutual advantage is certainly possible. That does not make it any the less important to identify the basis upon which the two can most wisely develop programs of give and take.

These are just samples of the problems with which I should expect your meetings here to be concerned. They all seem to me to be problems of great interest and major importance. Needless to say, I trust your sessions on this campus will prove both profitable and enjoyable. I can assure you all that the University deems it a privilege to have you here. Once more I bid you a hearty welcome.

Stuart A. Rice: My remarks this evening might appropriately be called the unfinished business of Rip Van Winkle. I find myself returning after twenty years to a consideration of the science of politics. In the late 'twenties this science represented aspiration rather more than realization. It was nevertheless in good standing, having attracted the enthusiasm of a substantial coterie drawn from political science, sociology, psychology, and social psychology; and it had begun to make substantial contributions based upon a new orientation to political affairs.

This new orientation demanded that conclusions respecting Government and politics rest upon empirical data. The resulting studies were often behavioristic and statistical in method. As early as 1901 A. Lawrence Lowell of Harvard University published tabulations of roll-call votes in American legislatures as the factual evidences upon which he based his analysis of "The Influence of Party upon Legislation in England and America." A little more than two decades later (still speaking of the decade of the 'twenties) statistical or semi-statistical studies by Floyd and Gordon Allport, Herman Beyle, Ernest Burgess, Harold Gosnell, Arthur Holcombe, Charles Merriam, Arthur MacMahon, William Ogburn, Stuart Rice, Claude Robinson, Dorothy Thomas, L. L. Thurstone, Julian Woodward, Malcolm Willey, and numerous others were supplying data from which the conceptions of a science of politics were being developed. Still later scientific sampling began to make its revolutionary contributions to survey techniques, permitting the rescue of public opinion polling from the disrepute into which it had been led by the *Literary Digest.*

I am not quite sure what has been going on in it since I left this field. One thing I wonder, however, is whether the quantitative methods developed two decades ago have not been too quickly seized upon and used for utilitarian purposes, at the expense of basic social science. I am questioning whether there is not a cumulative deficiency in our basic knowledge of the nature and characteristics of public opinion and social attitudes, relative to the superstructure erected upon it.

I will suggest by way of illustration some of the types of basic problems that I have in mind. For the sake of simplicity let us think of them in relation to leading national issues such as aid to Europe or a presidential election.

The first type of problem concerns the classification of opinions. Two categories of For and Against conceal an infinite variety of individual differences in the opinion structures of those included in each category. Intensity, for instance, and not the mere balance of individual opinions counted arithmetically, would appear to be of particular significance in political behavior under totalitarian regimes. But more convincing perhaps, is the consideration that when we leave elections and public issues to enter some other areas of opinion dynamics, the preponderance of otherwise unweighted individual preferences may lose much of its significance. Another spectrum of opinion might be associated with capacity to act upon the opinion held; for example, upon purchasing power. Should not preferences for a Cadillac in a poll on Park Avenue be given some kind of upward weighting as compared with preferences expressed in votes cast on Second Avenue? And still other significant criteria might pertain to certainty of opinion and to knowledge of the subject of the poll. Opinions pertaining to tractors, for example, might find the Park Avenue voters entitled to fractional rather than plural weighting.

The second problem to which I invite your attention is that of the characteristic form of the statistical distribution of indexes of opinion classified by intensity. It seems to me a reasonable hypothesis that if the voters were distributed in accordance with the intensity of their opinions about the two candidates in an election, they would tend to fall into the familiar bell-shaped pattern of the normal frequency distribution, or some approximation thereof. At least those with the extreme opinions in both parties would be somewhat less numerous than the larger groups of more moderate opinions at the center of the distribution. It is in this zone of indifference, at the point where the two classes meet, that the voters may most easily be induced to shift from one main candidate to the other. Here the principal struggle for votes occurs. Opinions initially falling outside the zone of indifference may be shifted considerably along the opinion scale under campaign stimulation, but still insufficiently to cross the class limits. On the other hand, relatively slight opinion shifts might throw the preponderance across these limits. This may have occurred in the 1948 election. As a corollary of this general hypothesis, neither the public characters of the respective candidates nor their positions upon important issues are likely to diverge greatly.

Although two frequency distributions may be equally normal and bell-shaped, they may differ greatly in their dispersion, that is, in their central tendencies. The amount of attention we should pay to the central portion of the spectra will vary.

This second problem leads us to the third which I should like to mention. What are the characteristics of stability and instability, including variability in time and under differential stimuli (such as campaign arguments) of that part of the intensity distribution falling within what I have called the central zone of indifference?

And now for the Postlude and Conclusion: Having carefully written all that precedes, re-read and pondered it, I have the uneasy feeling that at least some of the things I call for must already have been done. In that case, I would still feel that specialists in public opinion research should reappraise their past efforts, seek to discover and correct the weakness in their methods and strive for renewed relationships of public confidence. Equally, I am sure, your own long-run interests call for a redistribution of efforts, in which a greater part than hitherto will be devoted to the erection of foundations for a science of politics.

HOW CAN WE STUDY THE FORMATION OF PUBLIC OPINION?

(Monday, June 20, 1949)

Chairman: Rensis Likert, University of Michigan

Participants: David Riesman, Yale University; M. Brewster Smith, Harvard University; Robert Kahn, University of Michigan; Helen Dinerman, International Public Opinion Research, Inc.

DAVID RIESMAN:* In the course of a larger project at Yale, we had occasion to interview a number of seniors in a

* *Ed. Note*: Since this paper consisted largely of case material, it proved unusually difficult to condense it. The sections presented here are designed to provide examples of the speaker's approach, *rather than to reproduce his reasoning.*

Trade High School in Bridgeport, Connecticut. I would like to present a few illustrative observations from the interviews with two of these boys, since they represent an effort to illuminate characterological differences among various responses and to interpret this data in a manner which might be relevant in understanding political behavior.

Joseph Pizzeri, son of Italian immigrants, works after school as a gas-station attendant. An average student, he is very successful in extra-curricular affairs.

We are inclined to infer from the interview as a whole that Pizzeri has an oral-receptive character structure. We do not mean by this, however, that Pizzeri has regressed to or remained fixated at the oral stage of development; psychoanalytic characterology of this sort has little to say about such people in terms usable for an historical typology. Rather, we propose to use the term "oral" in its symbolic and phenomenological meaning, in much the same way as E. H. Erikson did in his study of the Yurok.

We cite some instances from the interview. Speaking of children, Pizzeri says, "I've had everything I've asked for. If my child asks me for anything, I'll get it for him." What would he do if he had only six months to live and could do as he pleased? "I'd try to get the most of everything out of those six months. I'd spend all the money I had to get it all." Asked why the union is on his side, he says the union helps you to "collect something—compensation"—selecting a theme which involves his being given something, his being "fed." He views life in terms of give-and-take relationships and derives a great deal of satisfaction from them. His most awe-inspiring experience was "when I knew I was going to get a bicycle. . . . Now I want a car—that would *over*-inspire me" (emphasis his). He would "like to see his daughter as a nurse." He says, "I always like to help other people" and he has, "always helped out in community work."

We may infer that Pizzeri experiences the world, as he does the family, as a source of pleasant things. While he does not sit around and wait for them, neither does he think of seizing them by aggressive efforts when they are withheld: in comparison with my other example, Janek, the absence of any "biting" remarks in his interview is striking. Since he is not insecure in his chosen path, he does not feel the need to force his attitude on others. He denies that he would differ in any way, in raising his own children, from what his parents did with him; "I've had everything I've asked for. If my child asks me for anything, I'll get it for him." Pizzeri makes no demands: on the contrary, his child is only to be given what he wants.

We turn now, in more detail, to an examination of Pizzeri's relationship to authority. As to why he does not listen to quiz programs: "I don't know, they just don't interest me. I should listen to them in a way—there are points I should hear, I suppose." Asked whether he favors compulsory sports he says: "Yes. Because, for one thing, if a fellow's small and needs to be built up, he'll do it in school. I know—I've seen it. And if a fellow's big, it'll take him down too."

On going steady or seeing more than one girl: "No, I see more than one. I've got just one in mind now. But I'm too young. I'll go steady later.

On who is more likely to be right on

questions of war and peace: "The men in the State Department. (Why?) They know more about it than we do. We just get what we know about it out of the papers."
On who runs the country: "The government runs the country. It's up to the people to abide by the government."

How do we interpret this set of answers? The first (quiz programs), with its *shoulds*, indicates to us that he does not question the voices from outside, as he interprets them. While the quiz programs are above his educational level in that they demand at least the atomistic knowledge of the academic high school to give any pleasure, he does not defend his own self and interest but immediately and placatingly says he "should."

The same submissive bent appears in Pizzeri's answer that "I'm too young (to go steady). I'll go steady later"—compare the next example, Janek's "I keep away from them . . . I sure keep away."

The next two answers quoted indicate his obedience to governmental authority. In answering, as many do, that the State Department is more likely to be right on questions of war and peace than the man in the street, Pizzeri gives no hint of bitterness that this should be so, or antagonism against the presumed secrecy which is the cause of it. In the next answer quoted, to the question who runs the country, we again see the pattern, as in the question as to whether he goes steady, of saying more than the question asks for, as if to make sure he knows the rule: "It's up to the people," he says, "to abide by the government"; he rejects the possibility that the government should abide by the people.

The interview suggests the possibility that Pizzeri has so successfully internalized an *ideology* of acceptance to authority that no problem of *characterological* submissiveness arises. To "abide by the government" and to "do what parents tell you"—these may not *feel* to Pizzeri like submission, since they may be felt as part of the unquestioned order of the universe. Pizzeri does not display any compensatory superior and authoritarian traits. Though the interviewer reports that he is sports editor of the School's magazine and baseball captain, he does not once mention these honors in the interview, let alone boast about them. Nowhere can we detect resentment or envy. He would like to see his son become an automobile mechanic, not striving for superiority even for his son; nor does he want to "discipline" or "train" his children, as do some of our other interviewees; nor do we find statements symbolic of the desire to fly or to climb, "to be on top" or "to look down."

The interview with Leo Janek suggests an entirely different type of personality, one that we might term oral-sadistic.

Janek's parents were born in Poland; he in Bridgeport. He intends to become a tool maker, but at present holds no outside job. We rely heavily on the very limitations of the interview—in this case, the lack of "rapport"—for furnishing us with clues to Janek's character structure. The interviewer seems to have been at least as bothered by Janek as vice-versa; young, good-looking, and undeniably middle-class, she gives us (in the Personality Sketch) a long report on the encounter.

"Janek," she states, "gave me more trouble than any other respondent. . . . He was fresh and smart-alecky and restrainedly defiant. His reaction to my being a woman was to assume a very tough, sophisticated line—the most obvious kind of flirtation—which naively bogus manner he tried to reinforce with cynical smirk and insinuating cocked eyebrow. . . . When we were finally finished and he was leaving the room, he swept a mock bow and said, "It's been a pleasure knowing you."

The interview situation is a difficult one, for in his own sphere of operation a woman is the only person to whom he is allowed to feel superior, and here is a woman placed above him as an authority. Asked whether he goes steady, sees a number of girls, or keeps away from them, Janek says: "I keep away from them, What I mean is—I sure keep away. (You make that sound very definite.) Well, it ain't that bad. You just ain't seen me in action" (said with pseudo toughness). Lest his contempt be interpreted as insecurity he goes on to boast about his masculinity.

The interview not only touches Janek directly in his vulnerable role of masculinity, but also indirectly where it is felt as discrediting his intellectual capacity. Masculinity plays such a role for Janek just because it is the principal symbol of status within his reach. He tells us, not without contempt, that he wishes a future daughter's career to "be a good housewife. That's all"; then, asked whether men or women have an easier time, he says: "I think women has an easier time; men has a lot of responsibility, a lot of worries on his hands." Unlike many of our interviewees, Janek does not believe in equality of career-choice for women; and he seems—while men often think women have a harder time—to envy their "freedom" from competition for status, while at the same time taking certain pride in his masculine role with its "worries."

Bitterness about his status-role comes out clearly in other answers. Asked how he would raise his children, he says, "It depends . . . on what you want" and adds, "Well, I don't want them to be like me." He would differ from his parents—towards whom he manifests no resentment—by making " 'em have a better education;" he would want his son to "be an engineer." Obviously, he is well aware of what the route to higher status is—and his answer is in striking contrast to Pizzeri's wish to have *his* son become an auto mechanic like himself.

Janek is quick to discover favoritism in school, quick to reassure the interviewer that his standing as a student does not depend on his accomplishments: his longest answer is to the question whether he thinks his school is democratic:

"What do you mean by that? There's a lot of favoritism. If teachers don't like you they give it to you—know what I mean? They give it to you. They ain't no instructors here. (What do you mean?) They don't know nothin' about it. They ain't no instructors. Dwyer knows what I'm talking about. There's lots of complaints. He don't do nothing. . . ."

Usually he replies by monosyllables; here he repeats himself, as if to convince both the interviewer and himself.

What is the psychological meaning of the resentment against authority which is manifested, both in the attitude towards the interviewer and in this comment on the School authorities? In both cases it would seem that his resentment spends itself very largely in self-justification—and in self-deprecation where this is not possible—both self attitudes being rooted in a feeling of personal inadequacy.

At the moment, the School is the nearest object for his hostility to authority. Asked whether he favors compulsory sports, he says:

"Yes. 'Cause it's interesting. Kids go to school more just to play sports. Otherwise they wouldn't go. Only reason I'm here. Why else?"

M. Brewster Smith: One promising approach to the study of opinion dynamics looks for determinants of opinion formation in the sphere of personality. Traditional academic attempts to relate opinions and personality have been largely sterile because they have often been rooted in too simple a view of personality, ignoring motivation and dynamic organization, and in too narrow a conception of attitudes, fostered by a premature attempt to quantify that has left qualitative distinctions of cognitive and affective content and organization out of the picture.

A group of us at Harvard, including Jerome Bruner, Robert White, and myself, are now preparing for publication an exploratory study that had as one of its main goals to identify and formulate the ways in which a person's opinions contribute to his adjustment—what may be called the *adjustive determinants* of opinion. Our results in this respect come largely from an intensive investigation of the personalities and attitudes toward Russia of ten widely differing adult men, though some of the findings from the intensive study were checked in a broader sampling survey.

While each individual's pattern was unique, we were able to distinguish three broad classes of adjustive determinants: First, *"reality" adaptation*, including the anticipation of harms or benefits to the self or to *values* held by the self, and, more generally, the structuring of experience into a meaningful world in which adjustment is possible. Second, *social adaptation*, leading to conformity or non-conformity according to the social needs of the individual. Third, *ego defense*, including ways of coping with inacceptable needs, warding off the inner consequences of threatening experience, and drawing (as through identification) on sources of ego support.

Serious problems beset attempts to carry this sort of approach beyond the intensive case study. On the one hand, our cases show that it is futile to expect any particular sequence of events in a personal history to lead to a single outcome in opinions. It is equally simplistic, on the other, to expect similar one-to-one correlations between particular adjustive determinants and opinion characteristics.

What, then, can we hope to do? Most ambitiously, we can try to assess the extent to which public opinion on a given topic can be ascribed to factors of reality adaptation, social adaptation, or ego defense. Even a crude assessment of this sort would have broad implications for the political significance of opinion and the prediction of opinion change.

More modestly, we found one respect in which the correspondences clinically discernible in the individual case could be generalized: the bearing of the individual's central values on the nature and structure of his opinions about Russia, as organizing factors and standards of judgment. Since personal values tend to be held in common by members of a social group, the study of the dominant values in particular groups can tell much about how a given object will be conceived, what will be salient in opinions about it, and, presumably, what are some of the important conditions that should result in opinion change.

ROBERT KAHN: In mid-October of 1948, the Survey Research Center conducted one of a series of nation-wide studies on public attitudes regarding American foreign policy. At the end of each interview, the following two questions about the approaching presidential election were asked of 736 persons, selected on a probability basis. 1. In the presidential elections next month, are you almost certain to vote, uncertain, or won't you vote? 2. (If certain or uncertain) Do you plan to vote Republican, Democratic, or something else?

The responses to these questions can be summarized as follows: About one-third of the people who were planning or likely to vote in the presidential election said they would vote Democratic. About an equal number said they would vote Republican. More than 20 per cent were undecided which party they would vote for, and the remainder expressed a preference for one of the minority parties.

After the election, it was decided to conduct a second interview with the pre-election sample, in order to discover to what extent voting behavior differed from pre-election statements of voting intentions. It was also hoped that this twin survey of voting intentions and behavior might serve as a small-scale forerunner of more substantial studies of similar design which might be undertaken in subsequent elections. Ninety-five per cent of the persons contacted the first time were re-interviewed.

Somewhat less than 30 per cent of the people interviewed before the election said they were almost certain to vote or might vote and that they were planning to support the Democratic party. About two-thirds of this group reported voting for Truman after the election, as they intended to do. The number who switched from Truman to Dewey was small—about 5 per cent of the "intended" Democrats. Truman's greatest losses resulted from people who intended to vote Democratic but did not vote at all. One out of four of the people who planned to vote Democratic did not go to the polls on election day.

Among the people who planned to vote Republican, on the other hand, 71 per cent reported actually voting for Dewey, a larger proportion of voters than Truman received from his intenders. However, 14 per cent of the intended Republicans reported switching to Truman—three times as many as the intended Democrats who switched to Dewey.

One of the most important factors in the Democratic success last November is shown in the voting behavior of those people who were undecided prior to the election. After the election, almost 70 per cent of this group said they had voted and 40 per cent said they had voted for Truman. Less than half as many reported voting for Dewey. In all, 15 per cent said they would vote for one party, and reported voting for another. Several explanations may be advanced for this discrepancy: (1) the wording of the questions; (2) the possibility of an incorrect recording at the time of the interview; (3) errors in re-interviewing (it is possible that in some cases the second interview was not taken with the same person who had been interviewed originally); (4) *fait accompli* effect (it has been postulated that, in their wish to identify themselves with the winner, respondents in surveys of this type would tend to say that they voted for the successful candidate); (5) shifts of preference—the possibility that during the last month of the campaign genuine changes occurred in the preference of individual voters. There is evidence in the interviews of the November survey that some, though not all, of the discrepant cases were the result of late campaign shifts of this kind.

We also investigated the time of voting decision. People who reached their voting decisions before the convention were about equally decided between the two major parties. About one-third of all voters belong in this group. Among these people who made up their minds at the time of the convention, Dewey received a majority of the votes. More than one-quarter of all voters said they decided at convention time.

The campaign period from July through mid-October saw some increase in Democratic strength. There were five Truman votes decided during this time for every four Dewey decisions. From this evidence it would appear that the two major parties were almost equally matched when the last two weeks of the campaign began.

According to the post-election study, the last two weeks of the campaign gave Truman 17 per cent of his total vote and insured a Democratic victory. Among the people who reported making their vote decisions during the last two weeks before the election, there were four Democratic votes cast for each Republican vote. Truman received the support of 73 per cent of these last-minute voters; only 19 per cent voted for Dewey.

HELEN DINERMAN: In the study of opinion formation students have become concerned with the problem of the people under cross-pressures. By cross-pressures we mean those attitudinal tendencies which pull people simultaneously in opposite directions. For example, in the election situation of 1948 there were some people who intended to vote Republican but did not like Dewey's personality. And there were some people who intended to vote Democratic but thought that Truman was a relatively weak candidate. For the purpose of describing the development of opinion as well as that of forecasting election results, it is important to establish some generalizations about the behavior of cross-pressured people.

Since we are unable to conduct controlled experiments on this problem, the most appropriate method of securing data for our purposes is the panel technique involving repeated interviews with a single sample of respondents. In this brief report we shall deal only with three sets of data collected in the 1948 Election Study in Elmira: (1) the respondents' attitudes on the two issues of labor unions and price control; (2) the respondents' images of Dewey and of Truman; and (3) the respondents' vote intentions. All of these data are available for each respondent at two different points in time.

Most respondents, of course, hold a consistent position. That is, Republicans tended to be against price control and to feel that labor unions should be curbed. However, there are always deviant cases.

That is, there were people who intended to vote for Dewey even though they favored price controls. An essential procedure for analysis is to discover what attitude such cross-pressured people hold at later periods in the campaign.

First, we found that cross-pressured people change more than others. They change in the direction of consistency. For example, Republicans with an unfavorable image of Dewey and Democrats with a favorable image of him were more likely to change either their image of Dewey or their vote intention than were people with consistent attitudinal patterns. Psychologically, we suppose that this represents a kind of striving for unity of the personality.

Secondly, when there was conflict between vote intention, on the one hand, and either issues or images of candidates on the other hand, the former was the more stable, basic attitude. When change occurs, it is least frequently vote intention which changes.

Thirdly, we compared the stability of attitudes on the two issues and found that attitudes toward labor unions were more stable and resistant to change than attitudes toward price control. In general, this corresponds to some of our other findings to the effect that the more class-related the issue, the more stable it is.

Finally, we were able to study the comparative stability of the images of the two candidates. We found that the image of Truman was more susceptible to change than the image of Dewey. This was not simply a reflection of the objective situation in which Truman was frequently viewed as changing from an inept and incompetent man to a vigorous leader waging a strong campaign against great odds, because changes in both the favorable and unfavorable directions were greater than any changes involving Dewey. Instead, the explanation may be found in the content of the stereotypes people held of the candidates. The central criticism of Truman dealt with his ineptitude and fumbling. The central criticism of Dewey involved his coldness and ambition. Psychologists have reported that in the formation of personality judgments the cold-warm aspect of personality is a crucial one.* Once a person has decided someone is cold, that impression colors everything else he learns about him. Thinking of a person as unintelligent, on the other hand, permits the incorporation of new qualities and allows for more room in changing impressions of the total personality.

JACK ELINSON (Office of the Secretary of Defense): Mr. Smith pointed out that he found no one-to-one relationship between personality factors and the opinions held. Did he make any analysis of the relation between personality factors and the intensity with which opinions are held?

M. BREWSTER SMITH: We did find after getting the value pattern of the person that an individual who conceived of himself as a person of broad interests was strikingly more likely to have intense feelings about Russia. We found some correlation between personality and opinion when we tried dividing people according to whether they tend to blame themselves or others when things go wrong; but we are not sure

* Asch, Solomon, "Forming Impressions of Personality," *Journal of Abnormal Psychology*, Vol. 41, 1946. I was referred to this article by Nathan Glazer.

whether we can control the other factors involved.

Daniel Katz: (University of Michigan): In relation to this very question, isn't it necessary, if we are going to find correlations between deep personality needs and opinions, to get a more general set of concepts for our opinion and attitudinal material. Some of the earlier work regarding radicalism and conservatism was an effort in this direction, for example, the Allport—Hartman study on the motivation of atypical opinion. Secondly, I should like to ask about the matter of needs leading to social adaptation. How was that broken down into specific dimensions?

M. Brewster Smith: On your first point, we have been trying to map out such a descriptive scheme to bring in factors in more intelligible relationships than appeared in the conventional radicalism-conservatism studies.

In regard to the second point, we did try to distinguish between several factors leading to conformity, and more personal determinants of opinions. Aside from direct motivation to conform, a second kind of relationship which we thought resulted in the effect of conformity was this matter of different distribution of dominant values among groups.

Raymond Franzen: I would like to ask a question regarding Mr. Riesman's paper. When looking for relationships among various types of attitudes, the universe of experience within which the attitudinal correlations are found is not defined. It would seem to me you could get entirely different relationships, practically anything you wish. For instance, in testing health attitudes, I can get quite different results when talking to doctors than when talking to teachers or the general public. Will you define your universe of experience in seeking these correlations in order to see what they mean?

David Riesman: Your question enters into the interpretation of the interview. If you do not know the particular milieu of the interviewee you cannot answer the question about intensity because you do not know what is expected in vocabulary or intensity within this group. Is violence expressed by a student toward his school, or conformity, aggression, etc., within or beyond the lingo of the school? How does it look to this person and to others in the school? You have to know before you begin the work of interpretation what is the mode of discourse in the particular group.

Laurence Benson (American Institute of Public Opinion): In connection with Mr. Kahn's paper, I remember reading an article in which the Survey Research Center analyzed why the leading polls failed. Two main thoughts were brought out in that analysis: (1) that the three national polls had used a quota sample instead of probability sample; and (2) that the polls would have been better off if they had used non-directive questioning. I would like to know, in view of the evidence you have presented this morning, whether the Center has changed its opinions about those two main reasons for the polls' failure and if not, how can they be reconciled with this paper?

Robert Kahn: I think the findings I have presented today are not inconsistent with those statements. One of the serious limitations of the study which I have just described is the fact that we used in the pre-election study only two very brief questions in which the whole situation was highly structured. Moreover, the pre-election findings, as we see them, had small predictive value, and as a matter of fact showed that there was a slight leaning in favor of the Republican majority in mid-October. The particular value of the study was that it gave us the opportunity to do a panel type of analysis with pre- and post-election observation.

PAUL LAZARSFELD (Columbia University: Mr. Kahn says that his panel technique can contribute to a knowledge of political behavior and understanding. It would be the case only if you didn't isolate studies as much as you have today. Since 1940 there have been tables available exactly like the ones you produced. The real problem is how much is due to the specific election, and how much is due to general psychological and social conditions. For instance, it has always been felt that the people who intend to vote Democratic are more delinquent at the polls than those who intend to vote Republican. This is a general social characteristic of the American scene, which is due to the lower income and education of the Democrats. Was this the case in 1948 or has the general rise in education removed the difference? Another point you missed is this: you did not isolate the people who said they didn't intend to vote but voted just the same, and inquire what effect their voting had on the outcome.

ROBERT KAHN: I don't know whether all those points need be replied to or not. I certainly agree that this kind of study gets its greatest value through comparison with other research. I will supply a couple of additional facts. We were lacking comparisons on any close basis with previous elections. On the point of behavior of people who didn't intend to vote but did so anyway; what our post-election study showed is that of this group, which represents about 100 people out of 700, 85 per cent did not in fact vote; the remaining 15 per cent voted two to one for Truman. It would be interesting to track down the local situation to explain this behavior on a case study basis.

RAYMOND FRANZEN: I want to correct a statement that Mr. Lazarsfeld made. He said we have always known that turn-out was in favor of the Democratic party. As a matter of fact, turn-out has changed from '36 to '40 and '40 to '44. It now has no effect whatever on Democratic or Republican advantage. In one study we found no relationship whatever between Republican and Democratic turn-out, county by county, and outcome of election.

PAUL LAZARSFELD: I did not refer to the effect of turn-out on the final outcome of the election. There are two things involved: (1) the Democrats are more apt to be delinquent than the Republicans—they don't carry out their voting intentions so consistently; (2) the don't-knows are more likely to go Democratic than Republican. These two factors may cancel one another out.

JANE SHEPHERD (Washington Surveys): I have one piece of evidence which will tend to answer Mr. Lazarsfeld's first point. We did very much the same sort of a study at the Washington *Post* in 1946 when we investigated reasons for failure to predict the turn-out and division of the vote. This was with respect to a plebiscite on home rule. We obtained results similar to those reported by Mr. Kahn. We were able in our poll to predict the division of the vote exactly, but our results on voting behavior were greatly inflated. We explored very much the same hypotheses Mr. Kahn had.

JOE BELDEN (Texas Poll): Those of us who are in the field, in the predicting business, and have to take the burden for the headlines, are extremely interested in these experiments. The important question to me now is: how much will these studies help us who have to make the actual predictions? Have we increased our knowledge?

HELEN DINERMAN: The kinds of things Mr. Franzen and Mr. Lazarsfeld were just discussing do help in that they tell you the kinds of shifts that will take place. They don't help you, however, in actually weighting your results, and such help I feel will not be forthcoming for a long time.

THE APPLICATION OF OPINION RESEARCH TO PROBLEMS OF HIGHER EDUCATION

(Monday, June 20, 1949)

Chairman: John W. Riley, Jr., Rutgers University

Participants: Julian L. Woodward, Elmo Roper; Frederick F. Stephan, Princeton University; H. H. Remmers, Purdue University

THE CHAIRMAN: Opinion research can contribute to higher education on two fronts: it can assist in measuring the impact of education on the student, and and it can help frame public relations programs for institutions of higher education. Since teaching is one of the most protected professions, its public relations are too often neglected.

Our three speakers this morning will discuss several aspects of the central problem. Mr. Woodward has had experience with the practical application of opinion research to the needs of educational institutions; Mr. Stephan is engaged in a five-year project of evaluating the effect of higher education at Princeton, and Mr. Remmers has been known for many years for his work on the teacher-student relationship in higher education.

JULIAN WOODWARD: The question I should like to ask is this: What have the market and opinion research people to contribute in solving the problems that face college educators? I would like to try and answer it by drawing a not-too-accurate but still useful analogy between higher education and business. We might regard a college first as a business enterprise selling a service to the public and then as a factory that turns out a product called "The College Graduate."

Now, as an institution selling a service, a university has a market problem just as do banks or railroads or insurance companies. Who will pay how much for what type of college education is something universities need to know. The fact that there is in general a seller's market for matriculations is pretty well known from the number of students who apply to registrars. But education may in the future find it necessary to discover both the demand at the going price and potential demand at different prices. The need for opinion research here is apparent.

Of course, only a very few of the colleges are proprietary institutions and to regard the rest of them as straight business enterprises is obviously incorrect. They are institutions heavily "affected with the public interest" and are usually granted subsidies in the form of tax exemptions, as well as outright grants from the government, and all of them are the recipients of gifts. Colleges can therefore make use of public relations research.

In the *Fortune* Survey on College Education, an attempt was made to study the broad field of the public relations of higher education. Using a national sample of 6,000 Americans over age 25, we tried to find out not only whether people thought college education was desirable for their children, but also what kind of higher education was most desirable. What should college do for those who go there? How should it change young people? In what ways should it equip them for life?

In the long run, the public relations of higher education will be dependent on the product turned out. Let us now turn to the second part of the analogy between college and business, and view the higher education institutions as a factory. Businesses are constantly conducting research to test the performance of their products so they can improve them and keep abreast of competition. The colleges, however, have done very little in the way of product testing, beyond the course examinations that they require for a degree. What substantiated claims can be made for the college product? What will it do? Does its performance justify the price that has to be paid for it?

There is beginning to be some research to answer these questions. The University of Minnesota, Princeton, Syracuse, Pennsylvania, and Claremont are institutions where I happen to know that studies of graduates have either been made or are contemplated. But so far all the research that I know of has been done without a control group of people who did not go to college. What we need is a matched sample study comparing college graduates with graduates of the "school of hard knocks."

Colleges will sooner or later have to be able to show concretely just what they do for students. They are also going to become increasingly concerned with the way in which the public appraises what it thinks they do. In both these matters the techniques of market and opinion research will be found useful.

FREDERICK F. STEPHAN: Princeton is engaged in a long-term study aimed at increasing our knowledge about the effectiveness of liberal education in achieving its generally recognized objectives. Professional and vocational aspects of higher education will not be emphasized, but even excluding these very important phases of higher education the Study has a tremendous field within which to select important problems for study. It will be conducted primarily as a study of undergraduate education at Princeton. While the results will not be typical of all colleges and universities in a number of respects, it is hoped that they will be applicable with suitable adjustments to other institutions.

Essentially what we are trying to find out is: (1) What are the principal factors—formal and informal—that make major contributions to the development of a student during his college education? (2) To what extent can these contributing elements be observed, measured, and their effects predicted? (3) How effectively is Princeton achieving its objectives as an institution of higher education in the development of its students?

Because of the complexity of the problem and the number of variables that are involved, the research program has been developing gradually. It has not been necessary to get it under way as rapidly as a program concerned with urgent practical problems. A great many smaller experimental studies have been conducted and an extensive statistical analysis of student records is well along toward completion. From the standpoint of opinion research this work may not be as interesting as some other projects that have not yet been launched. For example, it is desirable to know the purposes and expectations of the students who come to Princeton, and of their parents, in order to provide a basis for a study of their adjustment to college life. As is well known, their adjustment as well as their satisfaction or dissatisfaction with their new environment will depend very largely on their expectations and motivations. Hence, we are spending most of this summer on the intensive preliminary exploration of the problems of analyzing and observing the expectations and motivations of students. In this we will be assisted by three graduate students in psychology who have just graduated from Princeton and are in a unique position to play a dual role as researchers and subjects.

In certain respects Princeton is a very fortunate location for a study of opinion. Almost all the students live on the campus and are hence conveniently located for interviewing and observation. They are accustomed to filling out forms and answering questions. A number of opinion polls have been conducted by the students themselves.

From the standpoint of research methodology there are a number of formidable problems. Since we are interested in lasting effects and long-term trends, rather than in the cross-section picture, we must make repeated observations on the same individuals over a period of years. Hence, we will be conducting a panel operation of much greater complexity and difficulty than most of those that are employed in opinion research. It is quite difficult to decide with assurance at the beginning of the study just what questions will be most significant in the final analysis. When the study is nearing completion it will not be possible to go back and ask the questions that should have been asked had we known more definitely what the answers were going to be. The problems of memory and distortion will be especially important in our attempts to get information from alumni or even from upper-class students about their experiences as underclassmen.

We will in addition face all of the usual problems of getting behind superficial, stereotyped answers to obtain adequate information about the deeper psychological processes that can be observed and measured only by skillful interviewing and experimental observation. The techniques of public opinion research that are presently available will be useful, but it is probable that we will have to do a good deal of work on developing new or improved tools as we go along. We hope that out of our studies there may come some results that will be of value in the broader field of opinion research.

H. H. REMMERS: In a recent article[1] four areas of the use of opinion and market research are discussed: (1) the "educational market"; (2) impact upon students; (3) public relations; and (4) potential public support. I wish to add a fifth and a sixth category—morale of the institution, particularly staff morale, and staff evaluation. I shall describe briefly a few examples of applications of attitude research in these last two areas.

Staff evaluation is exemplified in the recent Staff Evaluation Project in Indiana colleges and universities which the Indiana Conference on Higher Education, composed of presidents or their delegates of Indiana colleges and universities, asked me to design and implement. Since a report will appear in an early number of *School and Society*, suffice it to say here that 14 institutions participated in the use of one, two, or all three of the measuring devices employed. These were: (1) *The Purdue Rating Scale for Instructors* by means of which 420 teachers in ten institutions received 25,176 ratings; (2) *The Purdue Rating Scale for Administrators* on which 55 administrators in 9 institutions received 823 ratings; and (3) *How to Teach and Learn in College*, a 162-item test on the psychology of teaching, on which 324 teachers in 12 institutions were tested. All three instruments yielded high reliability estimates.

The Purdue Rating Scale for Administrators yielded a correlation of +.82 with staff morale as judged by the staff, and three relatively independent factors which we labeled *fairness to subordinates, administrative achievement* and *democratic orientation.*

In an investigation of staff evaluation practices at an institution with which I am well acquainted, the teaching staff, department heads and deans were asked: "Do heads and deans have adequate information on which to base recommendations for promotions and salaries?" Seventy-five per cent of the staff said "No, they don't," while 25 per cent of the administrators said "Yes, they do."

Citizenship attitudes of about 10,000 college graduates studied by *Time* magazine were reported by Professor Robert Pace, speaking before the American Psychological Association in 1948, as being better for those who described their education as general, and less good for those who said their education had been professional, technical, or vocational. The same attitude questions (a few slightly modified) were administered to some 12,000 high school pupils in the Purdue Opinion Panel and related to the fathers' education where this was of college level. The children of fathers with a general college education have significantly better citizenship attitudes than do those whose fathers' education was technical or professional. Here,

[1] Woodward, Julian, "The Use of Public Opinion and Market Research Techniques in Education," *The Educational Record*, Vol. XXX, No. 2, April, 1949, pp. 186-196.

then, are data strongly suggesting the impact of higher education carried over to the second generation via the family culture.

Attitudes—the way people feel—are crucial in determining behavior. It follows that their role in the total educational process must be a major concern of the social scientist as well as the public.

THE CHAIRMAN: At Rutgers we have studied the relationship between what students expected in teaching and what they were actually getting. The students were given an opportunity to rank ten characteristics in order of importance in making for *good* teaching and then each teacher selected for appraisal was rated on the same attributes. The evaluations on each of the items for instructors were combined and on the basis of this combined evaluation, a rank order array of the attributes was derived in which the topmost item represented the attribute on which instructors collectively rated most highly. Thus, the ten attributes were arranged into two rank order scales, one representing "Ideal" and one "Actual" teaching traits. By comparison of the two scales, indication can be had of the similarities and divergencies between what the student wants and what he thinks he is getting.

Generally speaking, the rank-order comparisons indicated a closer conformity between the derived attributes and the discovered ones among liberal arts than among natural science or social science instructors. While the interpretations cannot be made on a statistically conclusive basis, they do suggest the following conclusions which summarize current student reaction to the specific faculty under consideration: (1) students want a high level of scholarly competence in their instructors and they are finding it; (2) students place high value upon that type of teaching which stimulates individual thought, and it is on this score that the faculty fails most strikingly to meet student demand; (3) students want teachers who have the ability to offer adequate explanations of their subject matter, and with the exception of natural science teachers, the faculty approximates student expectations. In the natural science field, however, the discrepancy was found to be striking and possibly serious.

HUGH J. PARRY (Opinion Research Center): The Opinion Research Center at Denver has made numerous surveys on educational questions and our chief problem has been with the school administrations. Even if administrative officials are not willing to act on the basis of a survey, I wonder if there is any way to persuade them at least to circulate the results. For instance, we made a study of academic problems in one university, and turned in a report of 350,000 words, but the professors concerned never were allowed to see these results.

C. ROBERT PACE (Syracuse University): One way to persuade administration and faculty members to do something about survey results is to have them take part in making the survey. If an outside agency is called in, then the investigators who do the actual work learn the most about the problems involved and leave, taking much of their knowledge with them. Participation in surveys can be an important device for self-education.

GEORGE GALLUP (American Institute of Public Opinion): I would like to comment on Mr. Woodward's remarks drawing an analogy between product testing and evaluating the results of education. Professional pollers are continually disturbed to find that college graduates know so little about government, the North Atlantic Pact, and other of the most important issues of the day. Many of the graduates appear to be quite inarticulate and unable to think logically. I wonder, therefore, whether it would not be possible to evaluate the work a university is doing by testing its students who have been out a few years. One could determine, for instance, the extent of their participation in community affairs, whether they vote regularly, whether they read serious books, and so on. These questions should, of course, be adapted to the course of study in which the student engaged while at college.

DAVID WALLACE (Ford International): In view of the range of innate differences among individuals, it might be extremely difficult to measure the effectiveness of an education in the manner Mr. Gallup has suggested. You will certainly find differences in intellectual interest among persons but you can't automatically attribute these to education. Chances are that the superior being in later life always has been a superior being, while he was exposed to education and long before.

GEORGE GALLUP: Mr. Wallace is certainly correct in saying that measurement of this type would be a most difficult job; but some standards of performance could be set up, possibly through the use of a control group. Standards of this type would help educators to combat the all too prevalent student conception that all they have to do is pass each individual course—that education ends when one leaves college. I might even suggest, somewhat facetiously, that colleges wait to award their degrees until the student has been out a few years and has shown what he can do with his education.

FREDERICK F. STEPHAN: You might also delay the instructor's salary check until it has been determined whether his teaching has taken effect.

H. H. REMMERS: I agree with Mr. Wallace that individual differences are important, but on the other hand, the Newcombs' study at Bennington showed that the type of education a student received *did* have a measurable effect.

JULIAN L. WOODWARD: One difficulty in all this is that extra-curricular activities are generally conceded to be an extremely important part of the student's college education; yet they are largely unsupervised and unintegrated into the college curriculum. Distinguishing between their effects and the effects of the formal curriculum would offer serious problems.

CURTIS D. MACDOUGALL: Certainly the cultural group in which a man moves after college is more important in determining his behavior than the type of education he has received. Even the best intentions can be submerged by group pressures and economic struggle.

G. K. SMITH (U.S. Office of Education): In studying the effects of progressive school education, the U.S. Office of Education has matched the college records of students who attended progressive schools with those who have attended other schools. Couldn't an analogous method be applied after the student has been out of college a few years?

THE CHAIRMAN: Certain types of generalizations regarding later performance can be made on the basis of college records. In a recent study we classified the careers of 10,000 individuals on the basis of field of concentration in college. We found that students who majored in the social sciences are less likely to achieve professional status than those who had majored in the natural sciences, and also that the social scientists tend to earn less, regardless of the specific occupation which they enter.

BRYAND CUSHING (Boston University): Perhaps we should go back farther than college in studying the effects of education and observe the individual in primary and secondary school. If an adequate analysis of a student's talents is made before he goes to college, it is quite possible that the college would be able to do much more for him.

ELMO ROPER: I would like to refer back to Mr. Remmers' remark that the results of his tests of faculty members were not made available to college administrators. To imply that the administrators cannot be entrusted with this information is a serious indictment of our teaching system. The results of these tests should be of considerable value in deciding on such administrative matters as promotions and assignments.

H. H. REMMERS: We have been afraid that administrators would apply our results too mechanically. On the other hand, the professors concerned are usually extremely conscientious in their desire to improve their performance, and consequently take our findings very seriously. I should personally be willing to give the results to administrators if faculties, including administrators, operated under the referendum and recall or, as is now the case in some institutions, under a rotating chairmanship.

FREDERICK F. STEPHAN: This is one of the cases where it may be dangerous to entrust a tool to someone without telling him how to use it. We have been so concerned with technical problems of measurement that we often have failed to explain to the consumer how to interpret the results.

THE CHAIRMAN: Our time is now up. I believe you will all agree with me, however, that it is encouraging to find that so much consideration is being given to the application of opinion research to problems of higher education, and that so much work has already been done in this field.

NEW PERSPECTIVES ON THE PROBLEM OF INTERVIEWING

(Monday, June 20, 1949)

Chairman: Herbert Hyman, National Opinion Research Center.
Participants: Samuel H. Flowerman, American Jewish Committee; J. Stevens Stock, Opinion Research Corporation; Cornelius DuBois, Cornelius DuBois and Co., Inc.

THE CHAIRMAN: This session is concerned with problems of interviewing. We have three speakers who are going to present rather sharply contrasted approaches to the problem. We are also going to have the benefit of two demonstrations. As well, as I look around the audience, I note a number of individuals who are working on similar problems and I hope that they will present their progress reports during the general discussion following the prepared papers. Our first speaker is Mr. Samuel Flowerman who will present a most interesting experimental approach to the problem of interviewing, that of direct observation of the interview with the aid of mechanical recording devices, and the like. Our second speaker will present to us a sort of magic whereby, without the aid of such costly observational methods, one can nevertheless infer the effect of the interviewer on the survey data. Thirdly, Mr. Cornelius DuBois will present a paper describing a radically different interviewing procedure.

SAMUEL H. FLOWERMAN: The present investigation grew out of our need to explain shifts or changes in response to attitude questions administered in two waves of a panel study conducted in Baltimore. In addition to such obvious shift-causing factors as second-wave interviews with the wrong respondents and ambiguity inherent in the questions, we were disturbed by our lack of knowledge of what actually happened in the interview itself which might account for shifts. This study was initiated in an attempt to learn about interviewer behavior as it affects the interview situation, and therefore affects reliability.[1]

This is a preliminary report of a pilot study which is still in the process of analysis. We are especially concerned here with the behavior of a "typical" interviewer, who is assigned to conduct an interview designed to uncover the respondent's attitudes towards Jews, Negroes, and certain aspects of Authoritarianism.[2] This preliminary report is based upon thirty-six interviews administered by fifteen interviewers who were recruited, hired, trained and supervised in the same manner as were the interviewers used in the original panel study of attitudes.

It was planned that each interviewer would conduct twenty interviews. Three

[1] The present study was undertaken with the assistance of Grete Haberman, Herbert Hyman, Patricia Kendall, Dean Manheimer, Myra Schuss, Naomi Stewart, and Faye Stollman, by the Department of Scientific Research of the American Jewish Committee and in cooperation with the National Opinion Research Center.

[2] Instruments to measure authoritarianism were developed in the Berkeley Public Opinion Study in California. See the forthcoming book, *The Authoritarian Personality*, by T. W. Adorno, Else Frenkel-Brunswik, Daniel Levinson, and R. Nevitt Sanford, to be published by Harper and Brothers on or about January 1, 1950.

of these (the first, tenth, and twentieth interviews) were with coached respondents, at specific addresses, where the interview was recorded using a concealed microphone attached to a WireWay wire recorder and attended by one of the experimenters in another room.

Each of the fifteen interviewers conducted two or three recorded interviews with "coached" respondents (professional actors, social scientists, graduate students, etc.) who simulated various interview situations: e.g., one professional actor played the role of a low income, low educated bigot; one social scientist was unable to give categorical, unqualified responses; etc. The remainder of the interviews were conducted with uncoached respondents (selected by judgment sampling methods according to age, sex, and socio-economic status within given city blocks) for the purpose of giving the interviewer additional experience with the schedule, as well as ascertaining consistency of interviewer behavior over a period of time.

For each interviewer the following data were collected: a detailed application blank; interviewer's responses on an information inventory which included diagnostic personality items; supervisor's impressions obtained during contacts with the interviewer; wire recordings of interviews with "coached" respondents and the interview schedules for those interviews; "coached" respondents' evaluations of the interviewer's performance; interviewer's comments on the interview situation; and the interview schedules for unrecorded interviews.

The method of analysis involved a constant interplay of quantitative and qualitative data. All data available concerning interviewers were analyzed with particular regard to each interviewer's perception of his role in the interview situation. The quantitative data were based upon recorded and unrecorded interview material. Recorded interviews, for example, were coded for various factors involved in interviewer performance, such as the nature and extent of interviewer's errors, possible bias or distortion of any kind, etc. The unrecorded interviews were examined for consistency of response within each interview.

The study has obvious limitations and can be regarded as only a pilot study in this area. We should like to use interviewers and respondents of known bias, previously determined; we should like to obtain recordings on all of the interviews, including those with uncoached respondents; and obviously we should like, if possible, to have the same respondents seen by all the interviewers.

We believe that this study has important implications for panel techniques. When the interview is regarded as a social (group) situation in which such variables as group pressures, personality characteristics, etc., doubtless operate, then it is reasonable to expect that the interview process itself must be carefully examined, rather than the mere content of questions and responses.

J. Stevens Stock:[1] Interviewing is often a major source of survey error, both in opinion and factual data. Consistent interviewer bias—that is, bias which is constant for all interviewers—can be measured only by specially designed validity studies. However, a measure of the *statistical* error associated with varia-

[1] The paper on which this summary is based was prepared by J. Stevens Stock and Joseph R. Hochstim, and was read by Mr. Stock.

tions from interviewer to interviewer can usually be computed in the same way sampling error is computed—by applying analysis of variance techniques to survey data.

The data for each sampling unit (for example, each block in block sampling) are tabulated separately for each interviewer. By a series of simultaneous linear equations relating the means of squares, the total variance is separated into its component parts—interviewer variance and the one or more sources of sampling variance.

Thus, in simple random sampling, we compute two figures: the mean square among interviewers and the average mean square among respondents by interviewers. The degree to which the mean square among interviewers exceeds the mean square among respondents is a measure of the magnitude of interviewer variance.

In block sampling, where respondents are subsampled within blocks, three mean squares must be computed: (1) the mean square among interviewers; (2) the average mean square among blocks within interviewers; and (3) the average mean square among respondents within blocks. From these three figures the separate variances among interviewers, blocks, and respondents may be computed. This method applies only to cases where the general areas assigned to interviewers overlap. In most practical studies of single cities or counties, this condition is approximately met and can be tested statistically.

Interviewer variance thus computed is a combination of two sources of variability: (1) *interviewer selectivity* in choosing respondents where strict preselective sampling methods are not used; and (2) the *interviewer effect*, which includes all the phenomena that happen in the interview, such as the effects of dress, manner, and mien of the interviewer and the speed and emphasis with which he reads the questions.

Very frequently interviewer variance accounts for a sizable proportion of the total statistical error. In fact, it is not uncommon for the interviewer variance to be several times the size of the sampling variance.

The interviewers' contribution to the total error of the sample can be reduced in three ways: (1) variability among interviewers can be reduced by more careful training and supervision towards more uniform methods; (2) freedom of the interviewers in the selection of respondents can be restricted; and (3) the number of interviewers may be increased.

Because of the sources of interviewer variation, we have two tentative general hypotheses the over-all effect of which can be computed by this method, and which are pretty well borne out by the analyses thus far: (1) the more restricted the sampling design on interviewers' judgment in the selection of respondents, the lower the variability will be; (2) objective factual questions are less subject to interviewer variability than opinion and information questions. Interviewer judgment or rating questions are most subject to interviewer variability.

The appropriate mathematics and computing methods will soon be published.

CORNELIUS DUBOIS: (*Mr. DuBois described a new interviewing method based on having respondents sort a specially-prepared deck of cards into* *various categories. This method is described in the article entitled "The Card-Sorting or Psychophysical Interview," which appears elsewhere in this issue. The article is based on his remarks at this session.*)

CLYDE HART (National Opinion Research Center): I am interested in Mr. DuBois' statement concerning the comparative costs of interviewing. Why should the costs of using the kits be higher in some circumstances? Does cost include more than the interviewer payroll? How about the length of the interviews—are they not cut down?

CORNELIUS DUBOIS: The cost of using the kits is excessive for small surveys with respect to the pre-test cost and the cost of printing and binding the kits. The interviews run some 30 or 40 minutes, so therefore this is not a factor in the over-all cost. Instead of cutting down the length of the interview, we take advantage of the respondent's interest to get more information from him.

HERBERT STEMBER (National Opinion Research Center): I have noticed that Mr. DuBois refers only to the use of visual symbols in his work. Do you plan to do any work on the comparable reliability of oral and visual symbols?

CORNELIUS DUBOIS: This may be a weakness of the brand consciousness quiz used along with these kits. We have attempted to use audio-symbols, such as those used in radio, but we have not projected them, only printed the words. However, the words don't "sound" as they do on the air, and we found they did not have the same impact on the test-group.

WILLIAM REYNOLDS (National Broadcasting Company): I should like to ask Mr. Stock a few questions: What is the magnitude of interviewer variance as against sampling variance? How do interviewer variances increase or decrease with relation to the number of interviews? If you get a large number of interviews the variance should be decreased. If it is reduced enough is it really important to compute it, because do we not then have an unbiased estimate of the questions?

J. STEVENS STOCK: It is certain then that with two interviews there is no chance for bias to cancel out. But even in a larger number of interviews it makes a sizeable contribution, even under probability sampling.

ROBERT WILLIAMS (Elmo Roper): I'd like to ask Mr. DuBois what difficulties he runs into. What percentage of the original designed sample do you fail to achieve?

CORNELIUS DUBOIS: It depends largely on the particular place. In two towns in Texas, for example, we failed by 95 per cent. Eventually we had to drop them from the sample because the population was mainly Mexican and did not read or speak English. Usually we complete 90 per cent of the assignments, or better.

DAVID RIESMAN (Yale University): I should like to ask Mr. Flowerman whether there is any correlation between interviewer types and a tendency of respondent to give authoritarian or non-authoritarian answers.

SAMUEL H. FLOWERMAN: There are two items of evidence. First, in general surveys the authoritarian items turn out to be the best single predictors of anti-Semitism. Second, there is evidence to support your question about the stability of these items; it seems that interviewers can do something to shake the stability. When respondent "L" was interviewed (tough, active) and was asked sex questions, he first laughed, but later, when the interviewer continued in a somber, prudish, humorless fashion to repeat the question, the question was answered in "moralistic" terms, following the persistent moralistic pattern of the interviewer. There seems to be some kind of self-selective process which directs certain people into interviewing jobs. "L's" interviewer makes few of the usual overt errors, but his dogged, moralistic behavior appears to affect the respondent's replies.

Leo Shapiro: Which groups are more readily affected by the bias of the interviewer?

Samuel H. Flowerman: There is some evidence that respondents in the upper-educational brackets are least susceptible to shifts. The lower-educated groups shift a good deal and seem more susceptible to shifts for a variety of reasons, such as mood, saliency, etc.

The Chairman: There is time left for a few progress reports. We might start off by hearing from Joe Belden of the Texas Poll.

Joe Belden (Texas Poll): We have experimented with a notebook type of questionnaire. The respondent is handed a loose-leaf notebook; thus his attention is centered on something and he is less likely to get away from us. The interviewer then turns to the first page; it contains only one question, which the interviewer *reads aloud,* just as in any other interview, while the respondent *reads the question to himself.* The respondent, in other words, is given the question in two ways: audibly and visibly.

Then the interviewer turns the next page, and the next question is posed. There is one page for each question. Indicators may be used to show what page to turn to if the answer requires skipping certain questions. Each question-page has a tab with the question number. Any multiple-choice answer lists that are to be presented to the respondent may be made to appear right under the question on the page, eliminating "card" questions.

The materials for this notebook can be mimeographed, saving money. Printing, of course, adds to readability.

We think the method improves interviewing in that there is little chance that the respondent will fail to understand the question because the interviewer read it improperly. And there is little chance of skipping questions.

Paul Sheatsley (National Opinion Research Center): NORC is currently engaged in an analysis of the characteristics and performance of all interviewers who have been recruited and trained for its regular staff during the eight years since the organization was established. This involves coding, for each of approximately 1,200 part-time interviewers, such factual characteristics as age, marital status, amount and type of education, previous work experience, past interviewing experience, source of application, etc.—and then correlating these data with such performance factors as length of time on staff, number of surveys completed, number of refusals, reason left staff, and average rating. Ratings are based on the regular NORC office analysis of returned interviews, and permit the coding of each interviewer as average, above-average, or below-average in terms of each of the three main aspects of the interviewer's job: (1) rapport and ability to obtain complete, relevant replies to open questions; (2) ability to fill out the questionnaire without error or omission; and (3) ability to follow sampling instructions and to achieve a representative cross-section.

It must be realized that research agencies are necessarily restricted in the type of interviewers available to them, so long as the work is only of part-time nature and is not highly paid. Thus it becomes important to analyze the characteristics of the available universe of interviewers and to relate these characteristics to performance. It is believed that the 1,200 NORC interviewers used during the past eight years represent a fair cross-section of those engaged in the field, and analysis of our data should answer such questions as: Is a college education necessary to successful interviewing? Are professional interviewers who work for many agencies superior to those who work only for one?, etc.

We might note that comparable data is being made available to us, through the cooperation of the Program Surveys Division of the Bureau of Agricultural Economics, on the characteristics and performance of their staff of full-time interviewers who were retained by the agency during the war years. Mr. Ruby of the American Institute of Public Opinion has furnished us with data on the characteristics of the current staff of Gallup Poll interviewers, and we invite other agencies who hire, train and supervise interviewers, either nationally or regionally, to contribute whatever comparable information they may have regarding their own field staffs.

HERBERT STEMBER: A great deal of the research on interviewer effect has concerned itself with measuring effect under conditions of interviewee variation or respondent variation. A third variable—the situation itself—is the subject of a series of experiments under the SSRC "interviewer effect" study being carried out by NORC.

Several experiments are being conducted through use of a "split ballot" to determine how effect operates when the formal requirements of the situation are differently defined.

HARRY L. SMITH (National Opinion Research Center): This is a progress report on still another aspect of the NORC study of interviewer effect. The principal hypothesis of this experiment is that interviewers tend to record responses made during the course of an interview in terms of the stereotype they have of the respondent. To test this hypothesis, two dummy interviews were prepared and recorded in which strong stereotypes were built up, one of a well-informed, intelligent interventionist in international affairs, the other an ill-informed unintelligent isolationist. Certain responses during the course of the dummy interviews deviate from the stereotypes. The test of the hypothesis is whether these responses, both free answer and pre-coded types, are recorded or coded as survey instructions and specifications require, or in terms of previously formed stereotypes. Experimental sessions have been held with these recorded, dummy interviews using both professional interviewers and students as subjects recording the responses. Preliminary tabulations of their work indicate that this process of stereotyping does to some extent influence the way responses are recorded or coded.

CONSTRUCTIVE STEPS IN THE MEASUREMENT OF POLITICAL OPINION

(Monday, June 20, 1949)

Chairman: Bernard Berelson, University of Chicago.

Participants: Archibald Crossley, Crossley, Inc.; George Gallup, American Institute of Public Opinion; Elmo Roper.

THE CHAIRMAN: For seven months and eighteen days we have waited for this day. The speakers, I am sure, have been waiting also. Journalists and lecturers at women's clubs have told us what happened on November 2, but now the speakers of the evening have an opportunity to tell their peers. It would be ludicrous to introduce the speakers to this audience.

ARCHIBALD M. CROSSLEY: As the first constructive step in future measurement of political opinion, I suggest that we stick to presentation of the facts and stop speculating, no matter how many editors and others ask us for forecasts. We know something now about these facts after seven months of research by many people, and what we know, while far from conclusive in many instances, may spoil some of the fun of the poll-damners.

First it is now pretty well believed, I think, that Dewey was fairly certainly in the lead at some point in October, and probably in late October, and that a considerable change of interest took place at the end of the campaign, after final polls were taken. Second, we are reasonably certain that no poll, no matter how good, could actually have shown Truman to be a sure winner. The best that could have been done would have been an indication of a close situation. These two points do not make angels out of the poll-takers, but they do suggest that now is a very good time to make a realistic appraisal of the extent and nature of fault and more particularly of what is indicated for the future.

One of the questions I am asked very often is: "If you had an exact duplicate of the 1948 situation what would you do now in the light of experience?" If we were embroiled in such a situation, the following are some of the things we would do differently.

First, and most important of all, we would make no categorical prediction even the night before Election Day. I say that now for two reasons. In the first place, in a number of recent elections many electoral votes have been determined by a margin too small for any sampling system to call properly except by luck. Secondly, entirely aside from the accuracy of sampling, the turnout variations and late shifts are at present predictable only within wide ranges.

Next, if we had the same job on our hands again, we would most certainly carry it up to the last possible minute. Our previous experience had all been in Roosevelt years in which there was little late shifting, and in which there was fairly heavy voting. So it seemed likely that a campaign would end about as it began. We took a chance on that assumption.

Next, and very important, we would devote every resource at our command to the study of indicators of probable turnout. In 1948 the ratio of Democrats to Republicans who actually voted may have been nearly the same ratio as that of Democrats to Republicans who stayed home. We knew Truman interest was rising and Dewey interest was not, but three weeks before the election our filter questions indicated relatively more Truman stay-at-homes than the Dewey stay-at-homes.

The greatest lesson of the polls is the need for the study of human behavior. That is the weakest link today in all research, whether in the measure of political opinion, or in any other opinion or marketing surveys. Even problems of interviewer bias did not greatly affect the poll results, and usually this is ironed out in any marketing or opinion survey with careful testing and training. But such testing and training are vital, and much more information on related problems is needed.

It's an ill wind that blows nobody good, and that goes for election polls. Our number one problem—our biggest error—came from forecasting. Our crystal balls didn't function. That is about the most succinct answer I can give to that wearisome question—"What went wrong with your poll?"

George Gallup: I am not going to make a speech but merely offer some observations on the contemporary scene in respect to the measurement of public opinion.

First, however, I would like to make some comments about the critics of polling. It seems to me that the most vitriolic and most vocal of the critics are the ones who know least about this field. Journalists and others have asked questions which indicate they have not the foggiest idea about polling techniques. By way of illustration I recall the statement of an editor who said that he was not going to rely on polls. "Instead," he remarked, "We are going out and talk to the people in all walks of life." Harry Truman, to cite a more illustrious critic, has stated: "I don't believe in polls." I would like to ask Mr. Truman what he means by this remark, for the Truman administration has made the greatest use of polls of any administration in history. The government calls its polls "surveys." Perhaps we, too, should call our polls "surveys."

Let us see just how good we are. Our over-all average error is about four percentage points. The past election proved again that we can be right within this margin. Our first job, of course, is to cut the four percentage margin to three —and then maybe in time to get it down to two percentage points. I doubt if we will ever get it below that.

One comment might be made concerning the Likert study. In the light of the study reported earlier here I am not sure that Mr. Likert should have rushed into print with his criticisms of our poll and others immediately after the election.

Finally, let me say what we are planning for the future. First, we are designing a national probability sample. We want to be able to make it possible to use more questions of the census-type. In this respect we are working on the problem of call-backs. We have studied 31,000 people to find out when they are home. Knowing this, we can better control time of interviewing, thus minimizing the number of call-backs. Secondly, we are setting up a panel across the country to enable us to report opinions on a twenty-four hour basis.

ELMO ROPER: Any study as to what might be done to improve election forecasting ought to begin with an examination of whether or not election forecasting should be done. I have stated repeatedly during the past eight years that I see no social benefit resulting from election forecasting as such. However, if this is a science, it must eventually prove itself in some predictive capacity; and while I am sure that election forecasting is not the best medium for it, it might very well be the most practical, as well as the most dramatic prediction application we now know. If Gallup and Crossley decide to go that way, more power to them! I may do one on an unpublished basis, showing the results to a few key people, or I might go even further and publish the results. But then again, I may not do one at all.

The most important thing I learned from the last election was that under certain circumstances, campaigns *do* change voters' minds, and we can't assume that campaigns will always be of equal strength—winning as many votes in one place as they lose in another. A way must be found to do surveys very close to election day. And it is to be hoped that a way can be found to minimize the rather large errors our own group has come to associate with telegraphic surveys as compared to the normal "tabulated in New York" studies.

The second improvement which is obviously needed is a better way of measuring the intensity of feeling, or calculating the probable turnout. In the midst of all the clamor about probability sample versus quota sample—and let me say here again that I believe in both methods—it has been rather blissfully overlooked that half the people in either a probability or a quota sample don't vote. What we really failed to find out last November was who would vote.

As a matter of fact, probably the most significant finding coming out of the election and the failure of the polls to predict it, is the fact that there was a changed voting pattern in 1948 as compared to 1944.

After a six and one-half month study of election returns ward by ward and county by county in seven of the large cities and in 112 industrial counties, we now realize that certain important and significant shifts in voting patterns took place in 1948.

We found in the big cities that the Democratic vote among the lower income groups was much more solid in 1948 than it had been in 1944. Boston was typical of this trend. Dividing its wards according to income we find that the poorest wards were 32.2 per cent Republican in 1944 and only 17.5 per cent in 1948. In the richest wards it was 57.4 per cent Republican in 1944 and 61.6 per cent Republican in 1948. Accompanying this trend was a marked increase in participation among the lower income groups, and a decrease in the upper income brackets. Confirming this trend, we found that the vote in the industrial counties of 10 states increased over 1944.

There is further evidence pointing to the possibility of a real trend toward the Democratic Party in the farm vote. We found that turnout in rural areas outside of the South did not increase. What did increase, however, was the Democratic vote. This fact might well have long-term implications. If this trend were to continue—if the farmers who take the trouble to vote were to continue to become more Democratic and if the lower income industrial vote were to continue to turn out in greater numbers—it would seem reasonable to expect even more convincing Democratic victories in the years ahead and an even harder predictive job in future years.

There are, of course, other things which we have learned. I now think that our old four-part attitude scale, however inadequate it may be as a measure of intensity, would, had it been used three days before the election, have come pretty close. But it might very well not have come close enough, and this means that predicting turnout remains our greatest unsolved problem, just as we have always known it did. In any future survey, we must account for turnout. This means better question-asking.

Another point which I have been concerned with is the "reverse coattail" vote. We have all the evidence we need to indicate that Mr. Truman picked up votes in Minnesota because of Senator Humphrey. We feel quite sure that a good many people who had a mild preference for Dewey in Illinois and a strong preference for Adlai Stevenson or Senator Douglas went along with the top of the Democratic ticket because of their strong preference for people below the top.

These facts raise the possibility that we have to do more than ask questions about the head of the ticket—we may need to know not only about the status of some local candidates, but also something about the political-economic orientation of the voters.

ANDREW BATO (*Fortune*): There is considerable gossip going around about some electoral reforms going through. I should like to ask this of Mr. Crossley: If these reforms should be legislated and incorporated into law, do you think that they will make forecasting more reliable?

ARCHIBALD CROSSLEY: Yes, electoral reforms would make forecasting easier. But I don't know how much easier it would be.

ANGUS CAMPBELL (Survey Research Center): In Mr. Likert's absence I should like to speak to Mr. Gallup's remarks. The criticisms made in the fall and those made four years ago as a result of the Congressional investigation are still appropriate. The study in question was based on a small sample. The findings, however, which would have made you more comfortable, were 50-50 except for 20 per cent undecided. These findings agree with those of Crossley. There are risks in opinion research which must be considered. There seems to be little doubt at this time there were errors in the sample. Another fact which cannot be refuted is that the undecided and the changers will alter the results of a survey.

DANIEL KATZ (Survey Research Center): Mr. Gallup has said Likert maintained that the reason the election prediction was wrong was because of probability sampling. The report published in the *U.S. News* mentioned this as only one of several reasons. The chief point which was made was that the psychological design of the questionnaires was faulty, and that there was a need to design experiments to study the undecided vote and the changing vote.

FREDERICK F. STEPHAN (Princeton University): It would be a mistake if we should leave this meeting thinking that we now know what went wrong. A search has been made of the available facts and many clues and suggestions about the causes of the errors have been examined. They reveal sources of error in every phase of polling but they do not tell us which had only a negligible effect and how much each of the others contributed to the total error. Hence we should state the facts clearly so that the public will not be misled to think that the weaknesses have been corrected and that there is no further danger of erroneous prediction. We should not take our guesses too seriously but should look for more data. We know more than we did before the election but until great areas are cleared up, we must say: "We don't know. The errors appear to be due to a combination of many discrepancies." Research methodology must be improved so that it not only reduces the errors but observes accurately how each part of the survey contributes to the total error. Until this is done we should suspend judgment about the relative importance of the various causes of the error that made the polls go wrong.

ELMO ROPER: I would like to assure Mr. Stephan that I have no feeling of complacency. On the subject of how best to do last-minute polls, I don't know. On the subject of predicting reverse coattail psychology, I don't know. On the subject of better predicting turnout, I don't know. On the better psychological design of questions, I don't know.

THE CHAIRMAN: I shall trump Mr. Stephan's ace. I don't think we'll ever know what happened.

NEW AREAS OF OPINION RESEARCH IN OTHER SOCIAL SCIENCES

(Tuesday, June 21, 1949)

Chairman: Col. Paul D. Guernsey, U.S. Army.

Participants: *Anthropology*: Rhoda Metraux, Research in Contemporary Cultures, Columbia University; *Political Science*: Clinton Rossiter, Cornell University; *Economics*: Franco Modigliani, University of Illinois; *International Relations*: Stuart C. Dodd, University of Washington.

RHODA METRAUX. In the last fifteen years, anthropologists have been borrowing techniques from other social sciences. This has been the trend particularly in the field of applied anthropology, where the growth of interdisciplinary research has been considerable. The borrowing, however, has not been a one-way but a two-way traffic. There are many concepts and skills anthropologists can bring to public opinion research. For example, the basic concept of culture which naturally applies to all societies can be fruitfully applied in public opinion work. The knowledge gained from a study of many societies can bring a cross-cultural perspective to the study of public opinion. As a tool of analysis, the structural functional approach should prove of value as well as the concept of culture patterning which directs the attention of the investigator to the structured regularities which run like themes through a culture.

The contributions of anthropology to public opinion research can be exemplified by the papers given by David Riesman and M. B. Smith which illustrate the sort of qualitative analysis anthropologists are skilled in.

Clinton L. Rossiter: I should like to talk today on the intimate and profitable relationship of Political Science and Public Opinion Research. Such a subject demands consideration first of the way in which each of the fields can be of service to the other.

The services of public opinion research to political science are considerable and welcome, and the political scientists should be the first to acknowledge them. Public opinion research can aid us immensely in dispelling the present great areas not so much of ignorance as of lack of information. In the field of political parties, for example, we need help in such matters as patterns of non-voting (educational, racial, religious, economic, social, party-adherence criteria), patterns of party allegiance (according to the same criteria), the significance of elections (through the post-election poll), and the tie-up of community leadership with political leadership. These are but a few of the areas in which we lack reliable information.

The most important thing political science can do for public opinion research is to present it with the actual tasks the latter is to perform. Public opinion research rarely formulates its own goals, but looks to other disciplines for direction and assistance. Political scientists know far better than any one else, particularly public opinion researchers, just what political areas require careful probing by trained analysts. Public opinion research makes a great mistake whenever it begins a poll of a political-social nature without first seeking help from political science. The latter can provide a stabilizing influence, refine techniques, fix proper ends, and above all remind public opinion research of its inherent limitations.

I would like to turn my attention now to an evaluation of the "good" and "bad" in opinion research, as a political scientist sees it. As far as the "good" goes, there is no need to demonstrate the basic importance for our type of polity of knowing the opinions of the people for whom it is supposed to exist. Lincoln proved this far more convincingly than any group of researchers could possibly do. The attempt to find public opinion, therefore, deserves and wins our quick sympathy. Among the specific advantages of public opinion research that we view with satisfaction are its discovery and delineation of areas of ignorance (which offer us a real challenge) and its splendid potentialities as an instrument for exposing the pretensions of our various interest groups, in other words, as a magnificent "debunker." Why doesn't the AAPOR form a *super*-pressure group to ride herd on the NAM, PAC, American Legion, and others?

The main general criticisms I have of public opinion research are: your natural tendency to claim far too much for your sub-discipline as a science and an oracle; your poor public relations (you, of all people!); your over-charged aspirations to be, like the rest of us, a "cult." Specifically, I would criticize the presidential and other election polls, which you should abandon forthwith; your failure to educate the public in the manner of handling your results; and your refusal to make available to political science the complete facts about your methodology.

Franco Modigliani: It is my intention to acquaint you briefly with the research project in which I am presently engaged. It is a project which may be decidedly regarded as economic research, yet there is hardly any doubt that if we are to come to grips with our problem, we will have to rely very heavily on methods of collection of information and of analysis which are being developed and employed in opinion and attitude research. The subject with which I am concerned will require the application of these methods to the empirical study of an important group of entrepreneurial decisions.

The general aim of the project, as clearly indicated by its title—"Businessman Expectations and Business Fluctuations"—is the study of the impact of such psychological data as expectations and plans of entrepreneurs on economic phenomena, especially economic fluctuations. More specifically, we are concentrating our attention for the moment on a study of decisions relating to investments by business firms, i.e. to expenditure on plant equipment and inventories. This intention is motivated by three closely interrelated considerations: (1) the strategic nature of investments; (2) our ignorance of the factors controlling them, or at any rate of their quantitative importance; and (3) the dominant role that expectations are likely to play in this type of decision.

The plan of investigation is largely organized about a crucial hypothesis: that investments in plant and equipment are typically subject to fairly detailed previous planning. We hope further to establish that investments actually made in a given interval of time are governed in a systematic and predictable fashion by initial plans, as well as by events outside of the firm's control which intervene in the period between planning and realization. Accordingly our investigation will develop along three main lines:

1. We want to study systematically the available information and gather additional information on the extent and character of business planning in general and of investment planning in particular.

2. We intend to analyze the relation between investment plans and investment realizations and try to establish and evaluate the factors affecting this relation. The main evidence for this type of study will be presented by the survey of investment plans which has been conducted for the past three years by the U.S. Department of Commerce and the Security Exchange Commission.

3. The third aspect of our investiga-

tion is the study of the factors governing the formation of the plan itself. The question we will be attempting to answer in this part of the study is essentially the following: On the basis of data currently available to individual firms composing an industry can we account for the present investment plans for the industry? Our plan is to analyze the readily available records in an attempt to reach some broad generalizations; but beyond this we feel it will be necessary to carry out a detailed analysis industry by industry, combining the study of records with the interview method.

In conclusion, economic theory has been giving more and more attention to expectations and other psychological elements in the process of decision making, but so far the treatment has been predominantly of a theoretical type, i.e., it has consisted mainly in deducing the consequences of alternative hypothesis about these expectations. But if this development is to be really useful the question must be faced as to whether these psychological magnitudes can be operationally defined and measured, and whether once measured they will effectively improve the economist's analysis. I am confident that with the help of the tools you have been developing and with the benefit of your experience, we shall be able to meet this challenge.

Stuart C. Dodd: My remarks this morning relate to the use of opinion research in international relations. The United Nations' Trusteeship Council inherits the problem of determining when a dependent area is fit for self-government. We developed eleven criteria of fitness for independence and these were adopted by the Mandates Commission of the League of Nations. More recently, we developed a scale of 350 indices for measuring these criteria both as realized and as goals. Many of these indices are best got from sample surveys or polls. Polling can thus help backward peoples to earn their independence without bloodshed.

The U.N. Commission on the Status of Women needs a scale to measure that status in any culture or period. Years ago we began building a composite scale of ninety indices. Each index was the percentage of men's status that their women possessed. The attitude scales and other indices of women's status require polling.

The Human Rights Commission has codified 30 rights or desiderata of nation-to-person relationships. They are now trying to implement these in a treaty enforceable by courts. But all rights depend at bottom on the world's attitudes. By measuring these attitudes, polls can help the world realize these human rights.

Our Washington Public Opinion Laboratory is undertaking a basic research on the measurement and theory of values—of which human rights, women's status, self-governing fitness, are but special subclasses. Values are measured by their exchange ratios—the amount of desideratum *A* a person or a group will give to get desideratum *B*. Exchange ratios are operationally defined by people giving dollars for economic goods, or giving hours of effort for their children, or giving lives for winning a war, or giving anything to get something material or symbolic in any of our dozen social institutions. Exchange ratios are pollable wherever people will choose among desiderata realistically presented. Thus our working hypothesis becomes: "polls can measure approximately the nations' values"—and eventually all international values.

UNESCO has a research project on "tensions affecting international understanding." Our Laboratory contributed a questionnaire for polling international tensions including two attitude scales. Our Washington State poll is being repeated in Norway and is planned for other countries.

The work of many other United Nations bodies require an international demoscope, or instrument to observe the world's population by sampling. ECOSOC's Sub-Commission on Sampling Statistics may develop this some day. Meantime, a private organization with semi-official backing seems most feasible. For international polls now exist among the ten Gallup institutes and IPOR's 22-nation network. The World Association for Public Opinion Research (WAPOR) is sponsoring the promotion of an international permanent poll or "International Barometer." UNESCO is being asked to adopt or sponsor this Barometer also. We are trying to finance it by annual subscriptions from governments and private bodies such as support the news services. Subscribers would nominate questions and get first release of findings before later publication.

The Barometer's civic purpose would be largely to service the U.N. Its scientific objective would be to develop a world demoscope for observing human behavior and relations universally. This transcends particular cultures. The Barometer could thus search and research for general laws of human relations. This would develop the social sciences as exact sciences which are increasingly able to perform the function of all science—to predict and control phenomena.

Ross Beiler (U. of Miami): It is important that political scientists and economists appreciate that they are studying particular aspects instead of "whole" phenomena and that they have been superficial in their approach to the behavior process. They have neglected to employ psychological hypotheses and techniques which have some empirical corroboration. They have traditionally studied responses and have failed to observe "scientifically" and classify systematically the relations of stimulus-attitude-response which would make possible *predictive* sciences of political or economic behavior. For this reason, I believe that public opinion research is basic to the creation of true political or economic science.

Shepard Jones (Department of State): I wonder if Mr. Beiler wishes to lump together all members of the Political Science Association. There are many within that organization who are very interested in public opinion research. I would like to emphasize that an important contribution to be made by political scientists to opinion research is in the formulation of research problems, in relation to the political process.

Clinton Rossiter: I think I can agree with what both Mr. Beiler and Mr. Jones have just said.

Alfred Watson (Curtis Publishing Company): I would like to ask Mr. Modigliani whether he has gone so far as to interview corporation executives?

Franco Modigliani: Yes, but so far only

on a sporadic case study basis. We are, however, planning to do it on a larger and more systematic basis in the near future. We are trying to gain information on the relevant factors in investment planning: technological (state of equipment); expectational (what one expects to be able to sell); financiability, availability of funds, etc. By aggregating the investment plans we hope to predict a future state of affairs and then see whether that state of affairs actually ensues.

JOSEPH A. S. KENAS: Contemporary economic theory employs certain fundamental assumptions which are primarily on a psychological level, e.g., the maximization of profit. Today, however in contradiction to this assumption, and despite an optimistic stock market, large firms are slicing their labor force. Why? Apparently security is the value rather than profit maximization. One of the prime values of the public opinion research tool is in probing for such psychological attitudes.

FRANCO MODIGLIANI: I should like to reply that economic theory, especially in its recent developments, fully recognizes the importance of the quest for security or "risk aversion." To oversimplify somewhat, we might say that in an uncertain world the outcome of every action may be thought of as a probability distribution with a certain "mathematical expectation" and a certain "dispersion." Economic decisions may then be reduced to the problem of choosing one from the set of alternative distributions accessible to the agent, the condition of the economic "game" being generally such that the agent can secure a greater "expected value" only at the cost of accepting a greater "dispersion," i.e. of sacrificing "security." The economist, however, does not assume that the exclusive goal of the agent's choice is either the maximization of the expected value of profits or of "security," but more generally assumes that the agent considers a greater expected value as a desirable thing and a greater dispersion, i.e., less "security," as a nuisance. It may well be, of course, that the relative weight assigned to "security" relative to the "expected value" of the gain is now generally larger than in the past.

TEACHING, TRAINING, AND PLACEMENT PROBLEMS

(Tuesday, June 21, 1949)

Chairman: Edward H. Suchman, Cornell University.

Participants: Dwight Chapman, Research and Development Board, U.S. Military Establishment; Dilman M. K. Smith, Opinion Research Corporation; Don Cahalan, University of Denver; Sherwood Dodge, Foote, Cone and Belding, Inc.

DWIGHT CHAPMAN: The subject of this round table seems to put special emphasis on the needs of commercial research organizations; but in the long run these needs will not be very different from the requirements of academic and governmental centers of public opinion research. It is at present true that it is commercially profitable to publish polls on issues, using techniques which contribute neither to the social sciences nor to the solution of any important social problems. And, on the other hand, much basic research in the social sciences is not yet ripe for practical application. Given time, however, it is reasonable to predict that the activities of commercial and non-commercial professional people in the field of public opinion will be as well related as are the activities of medical people in industrial and non-industrial clinics. In many respects, of course, this is already true.

When we talk about the kind of education and special training that will improve work in public opinion research, we have to project ten or fifteen years and more into the future of this field. I think of three trends that seem to me likely and important, and which therefore set some aims in the education of this sort of social scientist.

The first is that attitude surveys are supposed to be—and I believe they are—a technique for advancing social science, particularly social psychology. I think it would be still better to say that they are *potentially* tools for this, because I do not think we can point to many very impressive advances in the theoretical structure of social psychology that have been made possible by survey methods, or have been crucially tested by them. There are numerous examples of the solution of practical social problems in which attitude surveying has provided indispensable facts, but its use in developing and assessing really basic hypotheses about social behavior seems to me much more meager. This is to say that in the next decade or so, opinion research is going to have to demonstrate whether it really is a fruitful scientific tool or whether it is going to remain largely a technical tool for gathering the data which facilitate some forms of social engineering. If the latter case is finally true, the field will be populated largely with technicians—in sampling, questionnaire writing, and so forth—but the social scientists will progressively lose interest in it. If the former case is finally well demonstrated, it will be so only by intensive effort of professional people who are well grounded in the theoretical structure of the social sciences and who have a lively interest in advancing them. That consideration spells out one part of a useful education for the public opinion specialist.

A second trend that is foreseeable is that, like other techniques applicable to engineering, public opinion research is bound to become more and more integrated into what is known as operational research. Such research consists fundamentally in structuring research projects with the highest regard for the total situation to which they are pertinent, and in restructuring them as they, in turn, change this situation. It requires great skill in interpreting and analyzing administrative demands, in transforming these into scientific hypotheses, in interpreting scientific findings into the useful language of administration, in counseling as the results are applied, and in designing new projects to analyze the results of administrative application. Both the concepts of group dynamics and wisdom of much personal experience with live, urgent, administrative problems contribute to the kind of proficiency that will be more and more needed in research people. These considerations make it plain that training for our profession must include both the theoretical and the practical aspects of action research or operations analysis.

The third consideration is of a more special sort. It flows from the general fact that if, in the next ten or fifteen years, there are going to be notable advances in the techniques of opinion research, these will lie outside the field of sampling. They will be in the direction of solving the problems of what data to seek and how to elicit human responses which in fact furnish these data. Not only interviewing methods but questions of the relation of what is got by interviewing to the mainsprings of human activity are going to be the puzzling matters for exploration. It is quite evident that, just as two of our most progressive developments—open-ended interviewing and the use of projective questions—have come from clinical psychology, so the most likely advances are going to be made by those who fully appreciate the basic theories of personality dynamics and who are practised in a clinical approach to the individual. This is to say, then, that the roundly competent public opinion analyst must develop some of both the technical knowledge and the personal sensitivities of the good clinician.

DILMAN M. K. SMITH: It seems to me that college counselors who have an opportunity to advise young people on their interests in public opinion research must satisfy themselves on the individual's possession of these basic requirements: broad general background; high interest in business problems; curious-mindedness; an analytical approach; ability to reason from a set of figures; high order of general intelligence. Since these broad basic requirements point to well-roundedness in inherent qualifications and academic training, it is our firm conviction that specialization in the opinion research field should come at a graduate level. Undergraduate work should be broad in nature, with emphasis on the social sciences, economics, psychology, and perhaps the physical sciences.

Beyond the academic training however, we find it is difficult to discuss what characteristics will make for success in an opinion research organization such as ours. Like other organizations we have often made mistakes in hiring personnel. People who "look good" to us have frequently been unable to adapt themselves to commercial research pursuits. I should like to discuss a study the Opinion Research Corporation conducted to see if we could not increase our predictive powers concerning our employees and reduce the uncertainty of our hiring process.

For this study we called on the services of the Personnel Laboratory of Chicago, Dr. Leron N. Vernon, Director. The Personnel Laboratory gave tests to everyone on our staff. The tests covered a very wide range of types from the usual interest inventory and general intelligence to the Minnesota clerical and Stanford Scientific Aptitude tests. The staff members were told the purpose of the program and that they would have personal interviews with Dr. Vernon covering the results of their tests. Reactions were generally cooperative both before the tests and after the interviews. As a basis for setting our standards for future use, we made ratings of all of our technical staff, covering 7 factors as follows: (1) ability to develop a sound research plan; (2) administrative ability in field operations and use of office facilities; (3) knowledge of research operations; (4) analytical ability (excluding writing ability); (5) writing ability; (6) client contact ability; and (7) clerical accuracy.

The process by which the test scores were related to these ratings is briefly as follows. Over and above the tests, ratings on all seven factors were made by two executives on each staff member. These executive ratings were combined to give us a crude indication of the man's value to the organization. On the basis of these figures, all of our staff members were placed in rank order. This rank order was then correlated with scores on 23 different psychological tests. Results were fairly encouraging. Validity coefficients show much to be desired, but we are still working on the problem.

The ability measurement most closely related to our judgments on these men has to do with verbal ability, the ability to use the English language with precision and clarity. The tests from which scores on this point were considered were: general vocabulary test, English aptitude test, and technical vocabulary test in the social sciences. It was possible to set critical scores on intelligence tests and on personality inventories which differentiated between our better staff members and those we feel are weaker. The Allport-Vernon Scale of Values also gave us some usable indications. Men who turn out well with us make high scores on the economic and theoretical values, and low scores on the religious value.

We feel that results from the experiment so far have given us an illuminating glimpse of the way the thing *should* be done. The time when we intend to use it is before we have committed ourselves in considering a new employee. Correlation coefficients of around .50 show that while we do not have a perfect answer, we do have in psychological tests a useful tool. We expect to be guided by this type of expert assistance in future selections, and we expect our product to be a little better over the years as a result of it.

DON CAHALAN:* What kinds of trained research personnel do the universities aim to turn out? It is becoming clear that the preparation of all-around research directors must include a broad training in the social sciences. It is a mistake for universities to try to turn out mere technicians; that should be left to the trade schools and to survey research firms themselves.

Since time is short, I will confine myself primarily to describing the training program that was set up at the University of Denver three years ago. It drew from the NORC wartime experience, from academic practice in teaching social science, and from experience in training personnel in commercial and governmental research. The Center, affiliated with NORC, Chicago, offers a program consisting of practical survey work and courses within the Division of Social Sciences and the Department of Psychology.

We offer three main types of courses or training. One is the upper-division course in public opinion, which has successive quarters devoted to theory, tech-

* Director, Opinion Research Center, University of Denver. After July 1, Research Director, Attitude Research Section, 7700 Armed Forces I & E Group, APO No. 807, c/o Postmaster, New York City.

nology, and practice—the practice being effected through the plan of having the class help develop and administer a full-scale community survey of Denver each spring. Another course is the graduate seminar, primarily for graduate assistants, other advanced students in the social sciences, faculty members, and the Center staff; we call it Denver University's "clinic" for survey research.

The third program is the in-service training of graduate research assistants to fit them for positions as study directors upon graduation. In addition, we offer short-term field-work training sessions to lower-division social science students.

The chief emphasis in the formal courses is upon the whys and wherefores of social research, with just enough technology to acquaint the student with the range of pitfalls he needs practice in avoiding. We try to tie our courses in with other programs in psychology and the social sciences in order to avoid overlap, as (for example) in teaching statistical method. While texts are used (Albig, Cantril, and *Interviewing for NORC*), the bulk of the instruction is from current social research journals and the past and current experiences of the teachers. I set up the courses during 1946 and 1947, but since then Hugh J. Parry, Associate Director, has borne the main responsibility for the teaching and the continual revision of content.

The annual Community Survey has paid dividends in practical training for about thirty students yearly. The NORC sponsored the first one; the Anti-Defamation League the second; and the Rockefeller Foundation, NORC, and Denver have supported the third one, which has just been completed.

It appears sounder to try to do surveys that meet professional standards and provide useful findings than to limit the practical work to a series of hastily-conceived pilot studies that are filed away and forgotten.

Within the training program for study directors, all students (mostly graduate assistants) work on practically every phase of at least one complete Center survey, and are considered functioning staff members. At Denver this training is limited to M.A. candidates. This sets ceilings on what can be accomplished in a single year; but within the last three years eight students have completed the program, and three more will finish this summer. Of those eleven, eight theses are primarily methodological (effect of question variations, levels of information, readability of poll questions, deliberative vs. non-deliberative interviews), and three are primarily substantive (community satisfaction and its correlates, trends in opinion on international affairs). Most of the graduates are now working in positions on at least the assistant study-director level.

A word on placement: if AAPOR is going to be effective as a middleman between training centers and employers, ultimately it should have some secretariat arrangement whereby someone with wide acquaintance with commercial, governmental, and academic research will be paid to devote considerable time to placement. The right person could assist schools in gearing their training to the long-term needs for personnel, and could help place graduates not only in active research but also in administrative jobs where good training in social science research theory is valuable. If the social sciences are going to get out of their poor-relation position, AAPOR could help by concerning itself with placing people with research backgrounds in administrative posts where they can "bore from within" to establish survey research in its rightful place at the right hand of policy-making.

SHERWOOD DODGE: Considering business demands for research training, we must first study the nature of research in business. To do so, I should like to draw an analogy from the field of medicine.

Here we discover two types of mutually dependent operations: The field of the practitioner and the field of the laboratory scientist—the anatomists, the biochemists, physicists, physiologists, etc. The practitioners live in an infinitely varied world. From an expression of the symptoms, the practitioner constructs a hypothesis about the nature of the malady. He then is in a position to effect a cure.

The anatomist or the physicist, on the other hand, lives in a far more abstract world (by "abstract" I mean "things left out"). He sets up elaborate controls to test his abstractions. When he has finished his experiments, if he is successful, he is able to generalize about just one point in the complex processes of life.

The intelligent practitioner keeps abreast of these developments. They are important to diagnosis and therapy. His knowledge might be visualized as a series of points upon which he has specific information. But as he faces a problem from real life—a Mrs. Smith who says, "Doctor, my arm aches"—he has to treat the *whole* Mrs. Smith. And in order to do so, he must interpolate as best he can between the points.

At this point, the analogy with business breaks down. The medical practitioner is sufficiently trained in scientific method to be able to absorb the product of the laboratory. The business practitioner, for the most part, is not. For this reason, the capacity for industry to absorb the product of the social research laboratory depends, to a large degree, upon a middle class of employees who can bring the two into a closer working relationship.

This the middle man accomplishes in two ways. To fall back upon our analogy, he must deal with business problems "in the whole" very much the same way the doctor must treat Mrs. Smith as a whole person. In his relationships with operating executives, he must keep them continually exposed to the advantages of the experimental point of view. In his dealings with the laboratory men, he must be familiar enough with experimental design so that the end product of the experiment is readily translated into business operations. This is far more than presentation writing. More important, it is the job of relating the structure of the experimental design to the structure of the problems that exist in the real world.

I conclude that, therefore, there is a need for more training in over-all business problems; more training in how to plan; more training in how to relate the structure of experimental design to the structure of the real world of business problems.

LESTER GUEST (Pennsylvania State College): I would like to comment on Mr. Dodge's point that colleges and universities are lax in respect to training for research on practical problems. One trouble is that in college accuracy is required, and the student—under pressure to produce clean-cut research—chooses neat data with relatively little significance. We need a change in approach in academic training.

JOHN RILEY (Rutgers University): I would like to say a few words in defense of the academic profession. We wouldn't be here today without the development of the concepts of dynamic psychology, learning theory, and very substantial experience in the whole field of experimental method. In the social sciences there is certainly something more than the application of native intelligence and the naïve aping of the natural sciences. I would take sharp exception to some remarks on what we should be turning out, and need only cite the recent two volumes of the I & E studies as examples. In line with what Mr. Chapman says, there is a very substantial body of practical research experience and theoretical knowledge at our disposal.

SHERWOOD DODGE: I disagree in one respect. The biggest limiting factor in the application of Mr. Riley's point of view is the inability of industry to absorb this experience and theoretical knowledge.

RAYMOND FRANZEN: My point is probably implicit in some of what has been said, but we still seem to be suffering from academic specialization. Economics is kept too far from psychology. Statistics is taught as economic statistics, psychological statistics, or sociological statistics, while what we really want is statistics that will apply to a general social science.

FRANK LANG (American Statistical Association): In the past few years the New York Chapter of the American Statistical Association has had a placement committee of which I am the chairman, and I can bear out what Mr. Franzen has said. What firms apparently want is not extreme specialization but people with broad methodological training. The ASA hasn't funds to carry out a really extensive placement program. With the cooperation of public opinion groups, psychology groups, and so forth, perhaps a decent program could be established. We would be glad to arrange for exchange of jobs.

The colleges are getting more and more requests for people with graduate training in applied statistical work.

JOSEPH C. BEVIS: The discussion would be more enlightening if you could tell us how many of the trained men go into the field and how many stay there.

FRANK LANG: There has been an unfavorable trend in the last two years. The public opinion and market research fields are now crowded. These fields have become a "fad" with the eastern colleges, particularly the women's colleges, so that there are several dozen applicants for every possible job.

DON CAHALAN: Our program is only three years old, but I can tell you about our eight graduates. Two are assistant study directors with the federal government. One is assistant study director, and another is statistician with a quasi-academic research corporation. One has left the field and two are doing advanced graduate work.

REGIONAL POLLS—THEIR CONTRIBUTION TO THE KNOWLEDGE OF POLITICAL BEHAVIOR

(Tuesday, June 21, 1949)

Chairman: Paul F. Lazarsfeld, Columbia University.

Participants: Ross Beiler, University of Miami; Henry J. Kroeger, The Iowa Poll; Joe Belden, The Texas Poll; Robert T. Bower, Columbia University; Stuart C. Dodd, University of Washington.

THE CHAIRMAN: Regional polls can make an unique contribution to our understanding of political behavior. It is true that on a national level we can spend more time and money in framing questions and designing samples, and can correlate answers with income and other personal data, but we are unable to concentrate our attention on a specific area. On a local basis, on the other hand, we can add to our survey results some information about the strength of local political organizations, local issues, and other factors which ordinarily cannot be taken into account in a national poll. For example, Mr. Roper spoke last night about a "reverse coat-tail" effect, which seems to have affected the vote in some states during the last presidential election, but national surveys can tell us very little about the mechanics of this. Our speakers this afternoon are specialists in local and regional polling, and will be able to tell us more about the peculiar advantages and problems of this type of opinion research.

ROSS BEILER: Polling in the Miami area has been in a special type of situation that I hope may become common. Our samples so far have been restricted to an urban population of 400,000. Interviewing has been done by students in my public opinion classes, a few of whom have provided clerical assistance.

There are at least two major ways in which regional and local polls can be of value. One is in establishing the unique regional and local patterns of stimulus and response (both verbal and "effective"). The other is in contributing data to both methodological and substantive investigations on a more complete scale than the national polling services will often be able to do, and with more refined interpretation of results. I have time only to mention a few examples of the first function, taken from a poll of six weeks ago.

One of our objects this spring was to explore the relationship between public opinion and newspaper "slant" in Miami. We used issues on which the leading newspaper (daily circulation—175,000) had attempted persistently to convey its views. We are not yet organized to make actual content analyses in connection with such studies, but we found such an inverse relationship between the intensity of newspaper opinion and the popularity of that opinion with the public as to strengthen the impression gained by interviewers that such newspaper campaigns are resented, consciously or unconsciously, by the people of Greater Miami. (Both of Miami's dailies are "northern owned" and the *Herald* is considered a Republican paper in a Democratic region.) Some of the results of this poll suggested that certain local radio programs may have a more positive effect on public opinion in Greater Miami than the newspapers have.

When the *Herald's* campaign in favor of a plan for voluntary consolidation of Miami and other local cities with Dade County was defeated at the polls, newspaper innuendo and local gossip had it that gambling interests had succeeded in producing a very unrepresentative vote. Our poll closely corroborated the referendum result.

Again, the *Herald* is constantly engaged in arguing down proposals for legalized gambling in Dade County. Only 9 per cent of interviewees had no opinion on this issue; 74 per cent of those with opinions favored legalized gambling. Almost 90 per cent of those willing to express an opinion on the question whether gambling interests are protected by the law-enforcing authorities of Miami, Miami Beach, and Dade County averred that they were. From 21 to 25 per cent would not risk an opinion. Also, in answer to an open question, Miamians believe that the most important local pressure group is made up of the gambling interests.

The state legislature this spring was faced with the need for increasing revenues by 50 per cent. The *Herald* campaigned for a gross receipts tax. We found that only 28 per cent of the people could explain the nature of such a tax at all, and that only one-third of the people favored a transactions tax when a graduated income tax was offered as an alternative. (21 per cent had no opinion or would select neither.)

Every Sunday the *Herald* runs a half-page featuring a signed editorial by Editor-Publisher John S. Knight printed in easy-to-read 12-point type and adorned by a large cartoon and a banner headline. A recurring theme of these features last year was opposition to the Marshall Plan; yet last fall we found the local public strongly in favor of the Marshall Plan. Knight continued his campaign unabated this spring against the Atlantic Pact. We found as of six weeks ago that only 51 per cent could name three countries that had signed the Pact. Even in this "informed" group only 18 per cent agreed with Knight that the Atlantic Pact made war more likely. Forty-five per cent of the public as a whole and 24 per cent of the "informed" group had no opinion on this matter.

We try to avoid pushing respondents without serious opinions on an issue into stating insignificant and misleading opinions. Most people, middle-class in their thinking, need assurance that they "are not expected to have opinions on all these matters." Accurate measurement of the "no opinion" and "no knowledge" groups may increase reliability and validity of results and thus be important methodologically as well as socially or substantively. Incidentally, our white students who interviewed negroes obtained from 30 to 60 per cent "no opinion" responses. Very likely negro interviewers are necessary for sound polling of negroes, whose opinions in the urban South are coming to be very important politically.

HENRY J. KROEGER: One thing that should certainly be kept in mind is the relative influence of the news columns as opposed to the editorial pages. The way to get results from newspapers is to capture the headlines. The recent election illustrated this, in that Truman captured the headlines although he did not receive support on very many editorial pages.

I would also like to add a word about attitudes toward gambling in Miami. The city lives largely from retail buying done by persons from outside the area. If people assume that visitors are attracted by gambling then they will naturally not wish to shoot Santa Claus.

An example of the relative influence of newspapers and pressure groups is afforded by votes on the city manager plan in Des Moines. The *Register and Tribune* supported the city manager system, but it was defeated by a margin of several thousand votes eight or nine years ago. The paper did, however, give the opposition full coverage. This year the paper again supported the plan and it won by a few hundred votes. This time the League of Women Voters and some other pressure groups campaigned in favor of the measure and apparently swung enough votes to put it over by a small margin.

JOE BELDEN: The Texas poll is probably a pioneer in the field of election *mis*-prediction. We wrongly predicted the outcome of three Texas Democratic primaries even before the general failure of election predictions in the 1948 presidential race. Our average error on all predictions has been only 2.9 percentage points, but we have found that a good record such as that does not count very heavily with newspaper editors who support the Poll. They remember mostly the three incorrect predictions.

Some time before November 2, 1948, we knew two things: (a) we could no longer trust the rule-of-thumb method of disregarding the "undecided" respondents, and (b) in some political situations we were unable to measure what we were pretty sure was happening: last-minute shifts in voting intention. At least on a state level, we suspected these shifts were taking place.

Table 1 gives an example from the recent senatorial election in Texas, which appears to indicate a last-minute change. In the first column we have the results of the poll on which we had to base our prediction. This was conducted on Monday, Tuesday, and Wednesday before the Saturday election. In the second column we see that a check-poll we conducted telegraphically gave us the same results. This was done on Thursday and up through noon Friday —about 18 hours before the ballot boxes opened. We felt pretty safe. Then on the third column you see what happened—the man we had in the lead by one point came in a poor second. Well, a week after the election we asked a sample of voters, identical to the samples we had used in the pre-election polls, how they had voted. You see in the fourth column that we are only off two points, and we have the candidates in the right order. (In the table I have

TABLE 1

	Regular Election Poll	*Telegraphic Check*	*Election Result*	*Post-Election Poll*
Stevenson	40%	40%	40%	42%
Johnson	41	41	34	36
Sample	1,700	500		500

omitted percentages for minor candidates.)

I don't think we can come much closer than 18 hours to the election when we have to survey the entire State of Texas. Either shifts occur at the last minute, or we just have not developed the tools for predicting political behavior in certain situations.

The point I'd like to make is that these things have been going on—at least at our state level—for more than four years, and not much has been done about it. No one has made a good study, although here we have a situation which, if analyzed, might be of help to other pollsters. We do not have the resources to make the study ourselves.

We feel we are going to have to learn a good deal more about the mechanics of politics if we are going to apply any techniques properly to warn us of danger ahead. Any help that we can get from university research people will be most welcome. Our facilities are at their disposal—and the advantages of using a state poll for experimental purposes are considerable: (1) surveys can be operated at lower costs than national studies, yet they afford the necessary large and varied population; (2) our polling machinery is in continuous operation, ready to go; (3) elections come more often on a state than on a national level; and (4) working in a more circumscribed area, the necessary controls for experimental work may be more easily established.

For some time we have publicly shared Mr. Elmo Roper's fears, expressed last night, that political polls may have unhealthy influences. I am talking about the effects a state poll like ours may have. We do not know if the effects are a good or bad thing. We do know that political toes are pinched more acutely in Texas by The Texas Poll than by any national poll. The Gallup Poll is a remote thing to most Texas editors, and it makes little difference to them what candidate is shown leading. But we are told by these editors that the Texas Poll definitely influences political campaigning, if not the voter himself. We have known of several instances in which this influence appears to have been felt. Here is another field of investigation that some one ought to dig into.

There is good indication in our own backyard that polls are developing fears among newspaper editors about our influence in politics. If the Texas Poll happens to show their favorite candidate trailing, their editorial pulses quicken, and since we don't have the advantage of distance that Gallup has they take action against us. One paper, for instance, predicted that a candidate would win by a landslide and refused to print our poll to the contrary. A rival paper sneaked in the results, and the former cancelled our service in a front-page editorial saying the poll was no good anyway. The election proved us right, but we had lost a client. In another instance we seem to have been censored; although the newspaper continued as a subscriber, it left out our results, and by holding on to its contract kept us from selling to other papers in the same area.

We need help in the local and state field to test, experiment, and explore these problems, which we believe may provide results that in turn can be applied to wider problems.

HENRY J. KROEGER: The Iowa Poll,

which is sponsored by the Des Moines *Register and Tribune*, has been used in four principal connections in addition to making basic surveys of public opinion. First, we serve the newspaper directly by making readership studies; these are taken into account in determining newspaper policy. Second, we study various attitudes which concern newspapers; attitudes regarding local newspaper monopolies, for instance. Third, the poll serves advertisers with data which is of assistance in marketing their products. Fourth, we have been undertaking an increasing number of large and small public service projects. We recently made a state-wide survey at the request of the Iowa Bar Association. The Association was interested in finding out what the public thought of lawyers in order to use this information in framing its public relations campaign. Another survey was undertaken on behalf of the State University in connection with its adult education program.

In conclusion, I would like to underscore Mr. Belden's suggestion that increased cooperation between local polls and university research people would be desirable. We would certainly welcome outside help on many of the problems we are facing.

THE CHAIRMAN: To broaden our area of discussion, I would like to ask Mr. Bower to talk about an older method of analyzing local elections by studying the returns ward by ward.

ROBERT T. BOWER: One must be modest about the contribution which the technique of analyzing election returns from small geographical areas can make, but in some cases it does throw some light on political behavior.

For instance, we can sometimes test the hold which political leaders have on various elements of the population by determining the socio-economic or ethnic composition of certain areas and then observing how they voted. In New York we can learn something about the appeal of such minor parties as the Liberal and American Labor by locating the areas in which they are strongest and then studying the composition of these areas. The American Labor Party, we found in one inquiry, was not particularly strong in districts populated largely by working men, and the same was even more true of the Liberal Party. This suggests that the labor leaders in the two parties do not actually swing many votes.

We can also gain some insights on the role of issues by using these techniques. People once believed that issues played a large part in swinging votes, but we have found that at times they have little influence or even appear to increase apathy. In the New York Congressional contest in which Isaacson was elected, we found that those districts in which the ALP was traditionally strong had an unusually *large* turnout, while those in which the Democratic Party was in control showed an unprecedentedly *low* turnout. The issues didn't swing the vote here, but rather brought out those who were interested in them, and failed to arouse those who didn't care.

The political influence of population and economic groups can also be traced by this method. In New York City the Jewish group plays a big role because on some elections it will turn out to vote, in others it will stay home, and it will shift from party to party. Therefore its importance in deciding local elections is out of all proportion to its size.

Tentative rules of political behavior can be formulated on the basis of data derived from studies such as these. In the presidential election we found that the ALP maintained its strength during the campaign in districts where it enjoyed the support of a considerable portion of the electorate, but lost strength in districts where it was weak. The same was true of Wallace supporters in general. Such observations lead almost to a principle of political stability: when people of like political beliefs live in blocs they will retain or increase their strength; when they are dispersed they tend to fall away from their previous beliefs in the face of other dominant community sentiments.

Finally, studies of this type will assist polling organizations by pointing to certain factors which should be taken into account in stratification. In polling New York on political issues, one would certainly find it useful to stratify a sample by Jewish vote and by votes cast for Wallace in the last election.

THE CHAIRMAN: Combining this type of local election return analysis with polling often proves very fruitful. In Elmira, for instance, we found that working people who lived among other working people were more likely to vote Democratic than were those who lived in white collar districts.

While there is a great deal more that could be said about the matters we have been discussing thus far, Mr. Dodd has a number of remarks which should prove useful regarding the relation of polling organizations to local governments.

STUART C. DODD: When the Chairman asked for the experience of our Washington Public Opinion Laboratory in relation to some national trend in government, we chose *the trend for government to use polling increasingly*—in spite of the temporary setback last November. For we see polls as servicing popular government—as a new and scientific tool to make government of the people, by the people, for the people more truly government by and for the people. "Polling serves democracy."

To illustrate this trend, consider four of our polls in the State of Washington—one asked for by the Governor, one asked for by the Legislature, one asked for by the Seattle police, and one finding out what the people asked for.

Last winter the Governor asked us to poll the laboring class who would benefit by, and pay for, income insurance when sick through a one per cent payroll tax. Did the rank and file want it as well as their organization leaders who lobbied for it? The Democratic legislature had passed the bill, giving the Republican governor ten days to sign or veto. He proposed to decide with the help of a poll—an item in the trend for government to use polling officially. Unfortunately we had to reply that a scientific sample could not be designed and carried out in three days with the accuracy needed in such a controversial situation. The result was a standing request from the Governor to consult together periodically and to plan needed polls in ample time.

In the case of old age pensions, our State Senator on behalf of the responsible committee and Interim Council of the Legislature asked us to find out what the citizens wanted in caring for the oldsters. The laws had been several times modified recently, an initiative was brewing, and in the confusing pleas of pressure groups the legislature needed to know the true voice of the people. Our poll answered the questions for each issue in clean-cut isolation. Our general finding that the people wanted the State to rise even above its fourth place among the States in generous treatment of its old folk was confirmed by the passage of a liberalizing initiative six months later in November.

A third item in this trend was a municipal use of polling. The police and the housing authorities, helped by a grant from the Mayor, asked us to make a quick survey in a housing project (which was made and reported within a week after proposing it). Negro-white tension there threatened possible violence during a prolonged strike, and after aggravation by two sensational interracial rapings. Our survey showed no dangerous pockets for the police patrol car to watch and indicated no likely outbreak—a finding confirmed during the subsequent year and also by a repeated poll twelve months later which found ten per cent less interracial tension. Some ninety different tensions were found (with the relative frequency of each) and studied with the dozen civic agencies who were best equipped and eager to work for civic integration.

This trend for government use of regional polls was stimulated by the Laboratory, as a tax-supported agency in the University and State College, offering its polling services in a public relations campaign. About half its resources were offered for civic problems to the legislature, to civic agencies through an extension lecturer cruising the State, and to the public directly through our first poll which asked what the people wanted the Washington Public Opinion Laboratory to study.

REPORTS OF CURRENT RESEARCH

(Tuesday, June 21, 1949)

Chairman: S. Shepard Jones, U.S. Department of State.

Participants: Morris Hansen, Bureau of the Census; Angus Campbell, Survey Research Center; Ray Jessen, Iowa Statistical Laboratory; Kingsley Davis, Bureau of Applied Social Research; Clyde Hart, National Opinion Research Center.

MORRIS HANSEN: A major national census brings into focus many research problems. The problems are of the same order as in sample surveys, but in a census their importance is pointed up. For example, in the Census of Population, if we choose a technique which changes interviewing time by 20 seconds per person, it changes the total cost by about a million dollars. This illustrates the tremendous importance of careful and efficient techniques. On the quality side, since millions in public funds are spent, there is an obligation to obtain results that are useful and of which we know the validity.

Research work in the Census has been of two general types: (1) cost and time studies, where we are concerned with the comparative costs of different aspects of the job, such as travel, supervision, interviewing time, rural and urban costs, etc.; (2) study of the accuracy of results. However, we need to know the accuracy requirements for various kinds of data in order to plan effectively. We do not want to purchase a level of accuracy that is not required, since that would represent a waste of public funds, as would also the purchase of results with accuracy below what is needed.

We have worked on the problem of reliability and response bias. For example, we have incorporated tests of various types into surveys so as to evaluate the reliability of the data. This provides guidance as to the improvement of methods. In our monthly studies of the labor force, we have a continuing program of tests and research. In addition to numerous special studies, we reinterview a small sample and evaluate by this means the quality of the original interview.

We have also engaged in what we call "hothouse testing." We set a group of enumerators down in a room, and carry on a demonstration interview. The enumerators record the results. Via such tests we have studied techniques such as the use of check boxes versus verbatim recording of answers at a lower cost than would be involved in field testing.

We have also engaged in a program of pretests in preparation for the major censuses. In such field studies we develop experimental designs to evaluate different methods, such as self-enumeration versus direct interview. We are now moving towards a system of self-enumeration in the Census of Agriculture as a result of these tests, since investigation indicated that self-enumeration altered accuracy very little but involved a significant saving. By such tests we have begun to study other problems also, such as the effect of length of interview, the method of payment of enumerators, the effect of supervision and training on quality and costs of results, etc.

Quality of data has been measured mainly by re-interview procedures. In some instances the re-interview is merely a repeat of the original one; in other instances, the re-interview is a more ideal interview.

To mention a few more areas, we have been studying errors in office operations by sample inspection methods. However, we still need to specify more satisfactorily what should be regarded as a tolerable level of error. We have also been concerned with the mathematical-statistical implications of response errors, i.e., the isolation of component factors—interviewer, respondent, sampling—responsible for response error. We have continued work on sampling problems, such as the study of population characteristics in relation to efficient sample design. In the 1950 census itself, we plan the inclusion of certain experimental designs and we expect to do a quality check on the actual Census of Agriculture.

In all these realms we feel we need the guidance and cooperation of others, and the problem on which we need most guidance is that of accuracy requirements and validity of data.

ANGUS CAMPBELL:* The Survey Research Center prefers to work on research pro-

* *Ed. Note:* This summary has not been reviewed by the speaker.

grams rather than on one-shot surveys. There are obvious gains in terms of the carry-over of training and the transfer of information. Consequently, a large proportion of our work is programmatic.

One of our programs is directed by Daniel Katz and was begun three years ago under the auspices of the Office of Naval Research. It involves the study of factors relating to group productivity, defining productivity very broadly. One organization which was studied in the course of this program was composed of six equivalent sub-organizations, all working on the same thing—life insurance. We found that these six sub-groups did not work with equal efficiency, and therefore divided them into high and low productivity groups so as to examine workers' attitudes and supervision in the two contrasted groups. In another study of similar design we have 40 pairs of contrasted high and low productivity units and are just beginning to study the factors involved.

Another phase of this program is concerned with the general problem of morale. We have defined the concept in terms of seven components, and are studying each of these components in a large public utility company in Detroit. Here we are also able to interview supervisory personnel ranging from foremen up to top management, and are trying to study morale as it is related to supervisory practices and to top level management.

A second program is under the direction of George Katona and is concerned with the relation of psychology and economics. This carries on work started during the war in the Bureau of Agricultural Economics, where we were concerned with studies of personal finances in relation to war bond drives. At the end of the war, we started our annual Federal Reserve Board Studies. These were first designed to provide a description of family finances. At that time there were no existing data on the distribution of liquid assets—bonds, bank accounts, etc.—by family units, and this information was of importance. As of now, a psychological objective has been added. In 1945, the Federal Reserve System was not interested in the so-called ephemeral area of intentions to buy. Now they have become very interested. At present we know very little about the predictive value of such intentions—their relation to ultimate acts of buying—and point out in our reports that the predictive value of buying intentions is contingent on a variety of factors, such as the continuation of good times. Surveys of buying intentions are now carried out at six-month intervals, in an attempt to relate changes in intentions to outside factors.

One implication of this research is theoretical. We are not only interested in applied practical research, but in research that has long term theoretical value. In these Federal Reserve Studies we are collecting data which represent additions to the basic demographic statistics of the United States, since such types of data do not exist in the Census.

Another implication of this research is that it has possibilities of providing data to test economic theories about human buying behavior. The Carnegie Corporation has given the Survey Research Center money to bring two economists to Michigan for a period of two years to study this problem. The Rockefeller Foundation, in turn, has made funds available to re-interview our 1948 sample for purposes of providing measures of the reliability of our reports, data on the reasons for changes in buying intentions, and measures of the stability of the economic and financial aspects of American life.

RAY JESSEN: One of our main interests at the Statistical Laboratory of Iowa State College has been with problems of survey methodology. In a way it is appropriate to discuss problems of this sort here, because Cornell University was a pioneer in the application of the survey method of obtaining data (in this case in the field of agricultural economics). Incidentally, the survey designers of those days always used complete censuses (usually of counties) in their work, rather than samples. Their main concern seemed to be with getting accurate data from the observed units (farms). Our interest has been to carry out accurate surveys on a sampling basis, and we are mainly concerned with the statistical problems involved.

All investigations may be regarded as classifiable into two types: experiments and surveys. In the case of experiments we have a considerable body of theory, but I believe we must admit that in the case of surveys the theory has trailed considerably behind. Only recently have we begun to get a good foundation from the point of view of statistical theory. This is not because the survey method is new, but because we haven't emphasized the basic foundations. Small sampling theory since about 1900 has contributed to the development. Our knowledge of sampling has given us a basis for estimating aggregates and averages. However, I believe we are really most interested in relationships, that is, questions of "why" in science. Up to ten years ago statistical theory had done little to illuminate this problem of structural relationships. Techniques and philosophies now being developed by the Cowles Commission may be of help. This group has been developing methods helpful for drawing inferences about the relationships existing in the universe from non-experimental (therefore, *survey*) data. It is true that some work is being done on the problem of studying relationships, particularly by means of the panel survey, but we are in need of statistical theory behind this technique in order that we may know how to draw appropriate inferences about the relationships observed.

At Ames, our work on sampling problems has included the following:

1. The theoretical and applied problems of journey distance for different designs of sampling and for different sizes of sample. We have been particularly concerned with the formalization of this problem in mathematical terms.

2. The problem of handling missing data. Even with repeated call-backs in surveys, one rarely interviews or observes all cases. There is usually the problem of missing data. Thus far I haven't seen much good research that helps us in dealing with this problem.

3. Methods of obtaining quick and simple measures of reliability of estimates based on samples. At present for many sampling designs we don't estimate reliability very accurately—the methods are costly and quite biased. We need to devise schemes to measure reliability quickly and cheaply.

4. Methods of dealing with sampling designs which specify a fixed number

of units for each stage. Most often our presently used sampling designs involve a fixed sampling ratio and a variable number of respondents is taken. In some instances there are reasons for fixing the number of respondents taken.

5. The problem, common to many agricultural and market surveys, of obtaining an efficient sample of a particular sub-class of respondents. Usually one has to survey a much larger group in order to find the desired sub-group.

6. The problem of dealing with more than one unit of observation in the same survey. For example, we may be interested in households, in farms, and in land owners for a particular study and would like to survey them simultaneously. One may find that there may be several different farms "attached" to one household, or several households "associated" with one farm. We are interested in dealing with all these simultaneously, but we must keep our probabilities straight for these different elements if we want to make unbiased estimates from our observations.

7. The best way to collect data for a group of scientists, representing different fields, who wish to assemble a variety of information, such as agricultural, economic, or health data for a given country or area. This problem arose in making a comprehensive study of Crete under the auspices of the Rockefeller Foundation.

For some of these problems we have answers; on others we have only hopes. As I stated earlier, despite general progress in survey methodology, I feel we are still far behind the workers in experimental methodology.

Kingsley Davis: I should like to talk about the reorganization of the Bureau of Applied Social Research and the problems involved in such a reorganization. This, in turn, goes back to the fundamental problem of the relationship of social science research to university sponsorship. We are taking a particular line of development at the Bureau.

One obligation of a university is to provide facilities for social science research. One possible solution is a number of specialized research offices, as is the case at Princeton. The alternative is to establish a generalized Bureau which can take care of special interests but not involve actual separate research offices.

Each of these solutions has disadvantages and advantages. Separate research offices are expensive—each requires its own equipment, staff, plant, etc., and this results in duplication, since total equipment is only used at times of peak research loads. Separate offices also cut down cooperation and inter-stimulation because these offices regard themselves as independent and as competitive. An advantage, however, is that independence results in motivation to produce.

The generalized Bureau also has advantages. Historically, social science has trailed natural science in terms of apparatus needed, partly because social science did not realize the need for apparatus. Thus early social science research projects were done on a shoe string, with limited apparatus. But, granted the need for special equipment, a generalized Bureau can be most advantageous, especially for small universities, since equipment and facilities can be pooled and utilized efficiently with costs within the scope of small universities. It is along these latter lines that our Bureau has moved. It began as a radio research agency, but then moved into the larger area of communications research and gradually became a general social science research bureau. This development has resulted in an enforced cooperation and inter-stimulation.

The following are some of the disadvantages: (1) Not having specialized function, the generalized agency's purposes are not clear to outsiders. If it is a specialized agency, its function is clear. It is hard to explain the generalized agency's function to donors, clients, etc. (2) Another disadvantage is that the generalized bureau may become dominated by its top staff. Other individuals may not see a way for their own development. (3) It may become too big for original work and theoretical contributions. It may, in short, become bureaucratized.

Our solution at the Bureau of Applied Social Research lies between the two extremes. We are setting up divisions with their own staffs, projects and budgets, all working within the framework of a generalized bureau. We hope thereby to gain the advantage of both approaches.

Our Bureau is divided into six divisions: Communications and Political Behavior, Urban Research, Population Research, Advertising and Marketing, Methods and Techniques, and Research Training. We expect to add in the near future a division of "International Area Research" through which we hope to connect our own research activities with those of the international institutes at Columbia which are engaged in training individuals for work in foreign areas. The Bureau will be a holding company for the divisions and will facilitate and coordinate their research.

One further advantage of the generalized bureau is that there is scarcely any concrete research project that doesn't call for different disciplines and interests. For example, we are conducting a study of attitudes and knowledge about venereal disease. This project falls primarily into the Communications Division, but has also a demographic aspect because of the data on morbidity, and therefore is of interest to our Population Division. Also, since one objective of the study is to evaluate a community campaign against V.D., the Research Development Division is a logical agency to cooperate on the study.

Similarly, the Population Division combines its activities with the Research Techniques Division, since population data are dependent on the quality of interviewing and the development of improved statistical techniques. One of our projects, a continuation of work begun at Princeton, is concerned with the high population density of Puerto Rico. Research oriented in the direction of possible population policies requires getting behind the demographic data to the problems of what basic attitudes explain the high birth rate. The Bureau had also done a study of Puerto Rican migrants in New York City. Questions were asked equivalent to those asked of the natives in Puerto Rico and the differences in results will be examined. It can be seen that in these projects we are combining both population research and opinion research.

Our long-range program on the population side involves national inventories —attempts to measure basic trends in the social and economic structure of the countries of the world on a comparative basis. Here again cooperation between divisions is involved, since our Population Division and International Area Research Division will be in close contact.

This gives you a picture of the reorganization, its purposes, and an appreciation of the advantages and disadvantages of various modes of organization. One last advantage which I might point out is that a generalized bureau is more capable of giving the type of broad training needed for social scientists. We hope to emphasize this in the future.

CLYDE HART: Two of our research programs have already been presented in other sessions. One of these was reported in the session chaired by Herbert Hyman, in which you heard of our work in the last two years on the problem of interviewer effects. This work has been sponsored by the joint committee of the National Research Council and the Social Science Research Council under the chairmanship of Sam Stouffer, and has already yielded significant results. It will be completed in the next calendar year and will make substantial contributions to our knowledge of how the interviewer and respondent affect the reliability and validity of survey data. It will also provide a basis for controlling these factors and will have important substantive implications from a social-psychological point of view.

A second major program being done in cooperation with the University of Illinois was reported upon by Mr. Modigliani this morning. This project interests us greatly and is complementary in character to the Katona project reported by Angus Campbell. We are attacking the psychological elements that enter into economic behavior, but are approaching them from the entrepreneurial side rather than from the consumer side. This is a three-year project, financed by the Merrill Foundation and the University of Illinois, with an advisory committee composed of David Truman, Williams College; J. Kenneth Galbraith, Harvard; Albert C. Hart, Columbia; Frederick Stephan, Princeton; Paul Stewart, Stewart Dougall & Associates; Howard Bowen, University of Illinois; and Clyde Hart, National Opinion Research Center. It deals with a crucial field not previously investigated systematically. Mr. Modigliani is Director of the project.

There are two other programs which I shall mention. I have been impressed by the emphasis of the previous speakers upon common methodological problems. At NORC we are also giving detailed attention now to such technical problems as: (1) The execution of sample designs—the degree to which we achieve in practice the sample design as originally planned. We have been logging all our experience with probability samples, and our first report of this type recently appeared in the *Public Opinion Quarterly* in an article by Manheimer and Hyman. (2) Problems of coder reliability in a project under the direction of Herbert Stember. This project involves continuous incidental study of factors relating to the reliability of coding and methods for the improvement of coding. (3) Problems of reliability and validity of attitude and opinion data elicited by interviewers. For the last nine months we have been trying out experimentally the feasibility of a system of affiliation with six universities for common or joint research. We are now attacking with them the problem of reliability and validity, with reference particularly to voting behavior.

We are using the data from the Elmira Study for preliminary testing of our first hypotheses, and plan to go from there into an experimental program. One of the affiliates, the University of Denver, now has in process an independent validity study under the direction of Don Cahalan, assisted by funds from our interviewer-effect project and by a special grant from the Rockefeller Foundation. It is a study of the factors affecting the validity of survey data, and is now in the coding stage. A report will be issued in the Fall.

The other program that I want to mention briefly is in the field of intergroup relations. This again is a program of research and not a single project. It is concerned with discrimination, prejudice, and tension, all of which are, of course, interrelated, but each of which—and especially the last—may vary widely, independently of each of the others. Most research in intergroup relations has taken either prejudice or discrimination as the central concept—except for a few isolated and more or less descriptive studies of race riots and other mob behavior. Frequently it has been assumed that tension varies with these other two sets of facts. In any case, little attention has been given to means of detecting and measuring tension reliably and of determining the causal factors with which it varies. This is the job that we are undertaking first. Miss Shirley Star is now working on a scale for measuring the degree of inter-group tension that exists in a given area or community. The scale, which is now undergoing its first field test, is essentially of the Guttman type. In connection with this field test, partly for purposes of preliminary checking of the reliability of the instrument, information is being collected on the community factors related to tension levels. Bernard Kramer of Harvard University, who is spending the current year at the University of Chicago, is using this latter information in an independent study.

THE CHAIRMAN: The session is now open for progress reports from the audience. I wonder if Jack Elinson would give us a statement of research developments in the Military Establishment.

JACK ELINSON (Department of Defense): The Department of Defense has recognized the valuable contribution made by attitude research during the war by taking the very practical step of moving the Attitude Research Branch up the military hierarchy. During the war, Stouffer, Dollard, DeVinney and associates by virtue of being in a Branch of a Division in the Service Forces of the Department of the Army were forced to go up and down several chains of command before necessary military clearances for some studies could be obtained. The Attitude Research Branch is now in the advantageous position of being in the Office of the Secretary of Defense; this enables the Branch to be of service to all three military Services. Since the war the Attitude Research Branch has also received *de jure* as well as *de facto* recognition by having its organization and mission written into the Charter of the Armed Forces Information and Education Division.

Publication of *The American Soldier* has made it possible for anyone who can read to become familiar with attitude research in the military establishment during the war. Major General Gavin has called these volumes "a monumental contribution to the science of making citizens of a free country win its wars." The peacetime operation carries on much the same kind of work as did the wartime operation but the personnel strength has been cut down to about one-tenth of the wartime staff. Although the bulk of the work has remained intensely practical, "putting out fires," more opportunity has been available to use other than usual survey methods. In peacetime, furthermore, another objective criterion of adjustment to military life has emerged—this is reenlistment and non-reenlistment. Under wartime conditions servicemen did not have this method of escaping from military life or of indicating by overt action their acceptance of it, as the case may be.

Using the criterion of reenlistment, panel studies have been conducted which have furnished evidence on the relation between men's expressed intentions and their subsequent behavior, as well as on the relation between morale attitudes as expressed by servicemen in check-marks and written statements in questionnaires and subsequent reenlistment or non-reenlistment.

It has also been possible recently to build into one of the survey designs a test of the condition of anonymity in response—a condition which had for many years been resorted to on the assumption that servicemen would otherwise not give frank reports on the state of their morale. Preliminary findings indicate that this assumption is only partially true—that in the report of some aspects of morale, the factor of anonymity makes no difference, while in other aspects it has an effect on some individuals. If it will become possible to identify clearly certain areas of questioning which are not affected by lack of anonymity, the degree to which this would facilitate panel techniques such as experimental studies and long-time follow-up studies is obvious.

Most of the Attitude Research Branch reports continue to be classified documents although some have seen publication in various psychological and personnel journals, as well as in unclassified service journals.

Recent studies have been concerned with recruitment and reenlistment, career guidance programs, housing problems, problems of informing and educating servicemen, officers' career patterns and cross-service training, and attitudes of medical officers. In short, as one West Point general whose blouse is bedecked with combat decorations described the Attitude Research Branch: "it has proved itself within small and useful margins of error to be the 'morale radar' of the Armed Forces."

THE CHAIRMAN: The Voice of America, Department of State, is represented here by its research director, Leo Lowenthal. Perhaps he would tell us something of their work.

LEO LOWENTHAL (Department of State): Our research is concerned with the effectiveness of the Voice of America broadcasts to foreign countries. It is a young undertaking. We ask ourselves three major questions: What are we doing? How are we making out? How can we improve? We deal with the first question by content analysis. We examine the image of America as we project it to these foreign countries. We deal with the second question, in countries where it is possible, by trying to determine what kinds of listeners and non-listeners we have and what kind of listening habits prevail. And on the basis of these types of findings we try to improve our programs and to enlarge our listener audience. To answer these questions we need the concerted efforts of all social science and of all public opinion research techniques.

STANDARDS FOR PUBLIC OPINION RESEARCH

(Monday, June 20, 1949)

Chairman: Daniel Katz, University of Michigan, Chairman, Standards Committee of AAPOR.

Participants: Jane Shepherd, Washington Surveys; Hans Zeisel, McCann-Erickson; Alfred McClung Lee, Brooklyn College; Herbert Hyman, National Opinion Research Center; Wallace S. Sayre, Cornell University.

JANE SHEPHERD: Last year we all got aggressive about high standards of research and passed a resolution in favor of them. But in spite of precautions one of the resolutions involved some positive obligations.

Logically enough we decided that we couldn't tell the difference between good research and bad unless three conditions were met: (1) we knew or suspected research had been committed; (2) the report or corpus delecti had been found; (3) an autopsy revealed the nature of the crime. This last condition; of course, allows us to classify the event as either murder—for the client; suicide of the researcher; or just natural death of an idea.

All members of the group from now on are urged to submit themselves voluntarily for investigation. As I come from Washington this is an idea that is very familiar to me.

I should like to suggest, however, that there may be several reasons why it is not always easy for a commercial researcher to rush into print with a full report of his activities. It is necessary to do this if we are to face the issue of standards squarely.

First of all, we must realize that when clients buy research they may have a purpose in mind that would militate against a complete, published report. They may be seeking some competitive advantage by finding things out about their product that they do not wish to reveal to others in the same business; or they may wish to investigate a new product that they do not wish exposed prematurely. The client buying the service is entitled to protection.

The existence of clients raises another problem also. Often the arrangement between client and researcher will have to be such that the researcher cannot entirely control the publication of results. It would be hard to force a newspaper, for instance, always to publish details that the editor thinks would not interest his reader.

Then, too, commercial research is itself a highly competitive business. It is under constant pressure to reduce costs. Under pressure of competition, it must seek new research ideas and methods which would benefit other organizations as well but which, for that very reason, cannot be revealed. Finally, there is the element of sheer human laziness. Articles for publication must usually be written after office hours. Often there is simply not time.

I do not believe that many commercial researchers conceal results because they are afraid of destructive criticism. I say this for two reasons. First, no matter how poor the research organization, I've always found people eager and proud to talk about what they are doing. Usually they use poor methods because they have ridiculously low budgets. In this case they are proud that they've been able to produce so much for so little. Very often, I have observed that such groups have been isolated from the influence of people who know better. Second, I don't believe a member of this group would *deliberately* conceal bad methods because I think any AAPOR member, including myself, is smart enough to rationalize the use of *any* survey method.

HANS ZEISEL: The need for trade secrets is no serious argument against the acceptance and enforcement of research standards. In many cases methods are kept secret not because they constitute valuable and original property, but because they shy the daylight. But even in those rare cases where investments are made in original, competitive research projects, the "secret" is kept only a short time. The reasons for this are twofold: first, there is a continuous voluntary exposure of these secrets to clients and client-prospects; second, all "secrets" are sooner or later challenged. Often rather large research investments are made with little hope to keep the "secret"; McCann-Erickson was the first agency to use, on a regular, systematic basis, the Lazarsfeld-Stanton Program Analyzer, and to invest substantial amounts of money in it—fully knowing that most major agencies would later follow suit in one form or another. To summarize: trade secrets give no valid reason for opposing a code of standards.

How vital standards in our field are, may be illustrated by a survey conducted on one of the major issues in the last presidential campaign. It received nationwide publicity and, only when it was all over, was it revealed that this survey, made by a reputable organization, was based on 525 interviews.

The research standards adopted lately by various associations are quite useless because they are framed in ethical instead of technical terms. I believe in the formulation of technical minimum standards because they would eliminate much wasteful research. Standards for mail surveys are a good example in point. It is a method frequently used because of its apparent cheapness. But although we know by now the requirements for a good mail survey, the majority of them are not worth the paper they are—often lavishly—printed on. Technical minimum standards together with full statements on methods employed in a survey would be highly desirable.

ALFRED McCLUNG LEE: (*The paper read by this speaker is published in full elsewhere in this issue.* *See page 645.*)

HERBERT HYMAN: If we are to advance in the direction of more careful techniques, some provisions must be made for a continuous and more rapid expansion of methodological research. There are some proposals I would like to make in this regard.

First of all, an organization should be set up to deal specifically with methodological problems. It should be composed of impartial and responsible individuals so that the various individual organizations will not be fearful of giving up secrets. Such an organization would have the advantage of pooled resources and therefore be able to carry on projects that would be prohibitively expensive for private organizations. It would be well if a part of the expenses were borne by groups without commercial interests so that impartiality of the entire operation will be assured. A further advantage of such an organization would be the possibility of collecting of secrets and points of views from all the member organizations.

There is another requirement for methodological advance, and that is that research findings be made available to others. Such a central organization as I have proposed could not operate fruitfully unless it were agreed that the findings be published so that all could profit.

There is, however, a further, final requirement. Appropriate credit should be given to the originators of methodological advances so that the incentive to continue to work in this area will not be diminished.

WALLACE S. SAYRE: The opinion researchers have developed in opinion research a powerful social instrument, and are daily making it more powerful. If it is not radioactive or fissionable, it is at least a tremendously heavy blunt instrument.

This is true both of the totality of market research—which now affects many basic business decisions of great social consequence, and which also affects in some way most consumer decisions—and of opinion research on public issues. Taken together, these two types of activity make the field clearly one which is clothed with a public interest—a public interest so important that some effective kind of social discipline over the activity is, I believe, inescapable.

When we seek to accomplish an effective social discipline over an activity important to the public interest, we customarily rely upon two general methods: (1) self-imposed codes of behavior or standards, developed and administered by the group in recognition of their responsibilities to their several publics, and especially their responsibility to the general public interest. This method is most familiar to us in the work of professional groups, but it is widely used elsewhere. (Note, for example, the great current emphasis on "trusteeship" and "responsibility" of business leaders.)

The effectiveness of this first method of establishing public responsibility determines the degree to which we must resort to the second method—namely, (2) externally imposed standards, usually by governmental action or by public disapproval. Externally imposed standards are invariably less pleasant to the members of the group, even though they may applaud the general results.

These generalizations seem to indicate that: (1) the opinion researchers have no real choice about whether or not they shall have a set of standards (the public significance and consequences of their work make such a code inescapable); (2) they do have a choice, for the time being at least, between a self-imposed set of standards or an externally imposed one. This is an important choice, from the insider's point of view, even though it is true that the self-imposed code must anticipate and accommodate itself to the standards which would make up any externally imposed code.

There is, finally, an additional imperative compelling a group such as this to be immediately and seriously interested in developing its own standards. This reason is the aspiration of the group to develop and practice a science. Science is characterized, among other things, by the attention which it pays to certain universally accepted criteria of scientific work. These criteria are effective to the degree to which they are made explicit, self-conscious, disciplinary controls over the members of the group aspiring to be scientific.

W. PHILLIPS DAVISON (*Public Opinion Quarterly*): It is the function of the professional journal to provide one of the means by which standards may be maintained, by publishing legitimate criticism of articles it has carried. The *Public Opinion Quarterly* would welcome more of such critical statements. I fear that many of these are withheld because of fraternal politeness.

AL WATSON (Curtis Publishing Company): It seems to me that there is hardly anyone who would disagree with any of the speakers' statements. I believe a code of standards could be set up by the group as a whole, and if that is to be true, the commended code we now have is entirely inadequate for that purpose. I would like to see more and better standards set up by the Standards Committee, drawn up and endorsed by this group.

CLYDE HART (National Opinion Research Center): As Mr. Lee was reading his excellent paper it occurred to me that notwithstanding Lee's point of view a code of standards does constitute at least a beginning. I would like to amend Lee's proposal by suggesting that AAPOR have a code even if it is in the nature of pious pronouncements. We could then proceed with the matter of implementation.

HERBERT STEMBER (National Opinion Research Center): I agree with Mr. Hart. I think this discussion is not getting to the heart of the problem. The heart of the problem is implementation of standards, not the statement of standards. Implementation means educating clients to recognize good and bad research. The first thing that should be done is for the Standards Committee to have the prospective task of laying out plans for this area—publicizing what we think good research is according to the standards we lay down. If we do this we will ultimately be able to implement any standards we make.

PAUL F. LAZARSFELD (Columbia University): I feel that standards are best developed in the discussion of concrete cases. The Standards Committee should have cases submitted to it, examine them, and then express an opinion which will tend to become law. I would greatly appreciate an opportunity to voluntarily submit controversial cases to the Standards Committee for criticism.

CLYDE HART: I would strongly second the suggestion that Mr. Lazarsfeld made —but not as an alternative to what I suggested. Should we refer a research undertaking to the committee for judgment, we assume that the committee will be able to devise standards to bring to bear on it. I think those standards should be formulated. The development of such standards as we can agree upon through discussion is the first step, even though they may not be lived up to in all cases and although the meaning of them may not be perfectly specific.

MORRIS HANSEN: (Bureau of the Census): But you do not make good techniques by voting them so. It seems to me that a proposal that implies a review and approval of individual projects by the Standards Committee would fall of its own weight.

STUART C. DODD (University of Washington, Seattle): I would like to make three suggestions: (1) that the committee formulate a better statement on standards than available now and circulate it for discussion and next year's acceptance; (2) that the committee be asked to study ways for us to carry out such standards; (3) that a judicial function be established for passing on cases when requested. We need something like the Better Business Bureau that anyone can go to and ask, "Is this poll any good?"

SHERWOOD DODGE (Foote, Cone and Belding): I think that the development of standards can deviate between hypocritical and hypercritical. No standards should be set in terms of an evaluating or descriptive nature. I can't foresee a time when any good will ever come of it. If we give up the idea of setting standards for what a good or bad research job is and devote our energies to setting up standards on what should be put into a report, we should be able to tell what is good and what is not.

THE CHAIRMAN: I think there was no problem in setting up standards for public reporting. The problem is what we should do in addition to this.

THEODORE LENTZ (Washington University): In setting up this code of standards we are setting up something to live down to. We are doing something very misleading in setting up these codes. All the talk is about making standards for the method of finding out something and very little is being said about judging the worth of what we find out.

ELMO WILSON (International Public Opinion Research): Most of us on the commercial side of research have no fear of standards. We would welcome them and hope for them. There is a justifiable fear, however, that we might saddle ourselves with rigid codes which will not take account of advances in science. I think we should report the size of our sample, how the survey was done, and so forth, but I should like to see us think very seriously of some kind of procedure by which research studies may be submitted to the statesmen of this group for criticism. This would contribute to the advancement and knowledge of all of us.

STUART C. DODD: Speaking to Mr. Wilson's fear that we would saddle ourselves with out-dated standards, we should have the standards labeled "Standards of 1949" to be revised each year—then they would grow with research and the profession. In that way we would be able to see whether or not we were keeping up to date in the improvement of standards.

BERNARD BERELSON (University of Chicago): I would suggest that the Standards Committee might prepare a small sample series on studies done by the major organizations in the field—evaluations to be done by the Standards Committee or the Executive Council of AAPOR. These criticisms would not be published but submitted to the membership at next year's conference. We could then see whether this procedure would have a good effect.

THE UTILIZATION OF SOCIAL RESEARCH IN THE FORMATION OF POLICY

(Wednesday, June 22, 1949)

Chairman: R. A. Robinson, Crowell Collier Publishing Co.

Participants: Edward C. Devereux, Columbia University; Hans Zeisel, McCann-Erickson; Stephen E. Fitzgerald, The Stephen Fitzgerald Co.

EDWARD C. DEVEREUX: Recent decades have witnessed a rapid development of interest in the application of social science knowledge and skills in dealing with practical problems. In diverse fields and with diverse results, the social sciences have been utilized. A wealth of experience has already been accumulated. So far, however, no systematic analysis or inventory of this experience has been made. In view of the new demands and new responsibilities lately placed upon this developing social science, it would seem an appropriate time to take stock of its principal achievements, potentialities, and limitations.

As a result of a special conference conducted by the Social Science Research Council on this subject, a little over a year ago, a pilot study on the utilization of applied social research is now under way. Supported by a grant from the Carnegie Corporation, the study is under the general direction of Professor R. K. Merton of Columbia University.

The general purpose of our investigation is to learn what we can about the conditions under which applied social research seems to be most effective as a *component of the responsible policy* decisions. By means of intensive "clinical" analysis of actual experience in a variety of concrete situations, we hope to be able to codify the principal gaps, both scientific and organizational, which stand between the research expert on the one hand and the policy-maker on the other. It is expected that the study will also point out some of the more promising devices by which such gaps have on occasion been bridged.

In approaching our materials, it has seemed essential to take into account at all times the viewpoints *both* of the research expert and of the policy-maker or "client." For every client-researcher relationship involves, among other things, the redefinition of a practical problem as a research problem, a process in which there appear to be numerous pitfalls, deserving of careful analysis. Consequently, we are making every effort to study in each case of policy-oriented research the standpoint of the policy-maker as well as the standpoint of the researcher.

Our study is focused explicitly upon instances in which the expert formulates and executes a specific piece of research oriented towards some practical problem. Although we are not, therefore, directly concerned with the role of the expert as an advisor who draws on generalized knowledge and past experience, it will be interesting to examine the circumstances in which research is short-circuited into advice.

Time does not permit any detailed examination of the many sub-problems into which our investigation is leading. For the purposes of provoking discussion from the floor, a few illustrative problems may be briefly sketched. Among these are the factors conditioning the "demand" for applied social research, certain problems related to the action-value of applied research, the role of the expert in initiating research, the problem of arriving at an adequate "bookkeeping" measure of the value of applied social research, and the background of conditions under which pure research emerges as an adjunct of applied research in business, labor, and government organizations.

HANS ZEISEL: The difficulties of communicating the results of social research arise primarily from three sources: from the structure of our audiences, from the infant-stage of our science, and from the fact that social research is undertaken so often with an eye on subsequent action. Our audiences vary by their point of interest and the amount of time they will spend on us. Professor Stouffer's army research, for instance, was presented in three different ways: in memoranda to the General Staff, in a popular periodical to the army at large, and now in four volumes to the scholars.

The state of our science is reflected in the lack of a body-theory: not only are the very concepts we use newly created every day, but also techniques develop rapidly, and seldom as yet do our findings approach conditions of controlled experiments. Thus we have to prove the validity of our research every day anew. That social research aims, often directly, at social action means that one frequently tries to impart knowledge as well as to change attitudes. Here the question of the researcher's authority becomes paramount. Unfortunately, this is our weakest point. We are not in the position of the physicist or engineer who is believed without question. We have to show why one should believe us and we have two powerful adversaries: the "genius" who knows anyway, and tradition that does not need to know.

Social research is most readily accepted when previous efforts have failed, when it is ordered by the top authority, and when it is backed by management and scientific authority. The Baruch-Compton rubber report during the war was a good example. In the future, presentation of social research will become easier because our science—and with it our authority—will grow; to base action on research will become more and more the fashion. But presentation will also be more difficult in that it will become ever more technical and thus removed from the judgment of the audience which we address.

STEPHEN E. FITZGERALD: If public relations is to become a profession, and fill that social vacuum which called it into being in the first place, both its rate of growth and extent of growth will very largely depend on effective cross-fertilization with the techniques of opinion research. In the light of this I want to discuss three questions which, it seems to me, opinion researchers have not yet solved in getting their work effectively utilized.

The first has to do with effective presentation. It seems to me that those who are in charge of writing and editing research reports must give weight not only to the basic significance of their findings—considered as objective and interesting facts—but also to the need for methods of presentation which are at least comparable to those methods used generally today in the presentation of the business reports which management uses in the formulation of policies.

The second question is that of merchandising. Let us assume that the discoveries have been made. Let us also assume that the discoveries have been adequately presented in written form. They must still be "merchandised"—by which I mean that they must be brought *effectively* to the attention of those in a position to take action. This problem of merchandising is certainly not restricted to the research field. Public relations firms, advertising agencies, and other organizations in related fields, have for years faced the same problem. In advertising and public relations, however, it has been aggressively tackled and reasonably well solved. It has become common practice to make very careful advance plans which will make it possible for reports, memoranda, technical documents, and programs to be merchandised to all the people who are likely to be in a position to make use of the information. This is not just "salesmanship." It is a matter of "follow through." To be most effective, I suggest, opinion research must be regarded as part of a total process: and the process does not end when the report is written.

The third problem is more subtle. It concerns weaknesses which tend to creep into a research report when persons admittedly expert in research venture into specific recommendations for public relations activities implied by their findings. The research expert, however competent in his own field, is often not equally expert with respect to the practice of public relations. Consequently, some reports tend to go to pieces in a welter of proposals for action which go beyond the true province of the researcher.

It occurs to me that public relations people and research people would all benefit greatly if they could work toward a closer integration of their related but separate skills. Speaking as a public relations man, I think it would be fair to say that we generally would welcome a closer integration with opinion research. This could well have advantages for all of us. On the one hand, it would help make the public relations field more mature and more professional; on the other, it would help to develop new areas for the utilization of research in the formation of policy.

JANE SHEPHERD (Washington Surveys): For public relations purposes, would you want survey findings which are related to the basic principles of human behavior, such as the academicians would like to have?

STEPHEN E. FITZGERALD: Yes. Although the interpretation should not be pushed to a point where it becomes so tenuous as to look foolish.

CHARLES E. SWANSON (University of Minnesota): With relation to the problem of making up reports, I would like to point out that intelligent use of such readability formulae as those of Flesch and Dale-Chall can serve a very useful purpose.

LESTER GUEST (Pennsylvania State College): There is an unfortunate gap between reports designed for professional readers and those for non-professionals. This might be solved in part if material, even if directed toward the professional, is made just as simple and clear as possible.

THE CHAIRMAN: I agree heartily with the last observation. It is often difficult to know just how much editing should go into a report to make it easily readable; but the decision should certainly be in the direction of simplification, whenever this is possible.

HANS ZEISEL: That is all very well, but the trend is strongly in the other direction, because the subject matter is becoming more complicated. Just look into Stouffer's *The American Soldier*, or see where economic theory is going; without knowledge of advanced calculus you cannot follow it any more.

SHEPARD JONES (Department of State): Our experience in the government has been that policy makers as a rule do not have time to study the sort of report demanded by the technician. This sometimes leads to a need for two separate reports—one for those familiar with the technical literature, and one for the policy-maker.

STEPHEN E. FITZGERALD: I am not as discouraged as Mr. Zeisel about the problem of simple presentation. What we should recognize is that there are two different types of presentation and that one report often will not do for all consumers.

JOHN RILEY, JR. (Rutgers University): It seems to me that Mr. Zeisel is correct in pointing out the trend toward increasing technicality in research reports. Furthermore, I do not believe that we can reverse this trend without running the danger of oversimplifying our ideas in the process of communication. Such oversimplification would undoubtedly have a deleterious effect on the ultimate course of research. We must continually attempt to educate the users of research to see how *much* they can understand, rather than how little.

EDWARD DEVEREUX: In any event, an alert research group is usually three or four jumps ahead of the people who are going to read the report. Perhaps the best solution is to strive for a fairly simple report for the layman, and then include a technical appendix for the professional audience.

THE CHAIRMAN: It is clear that the problem we have been discussing has a large number of facets which we cannot consider today. Perhaps at some future conference we should schedule a session on semantics in order to go further into these language problems.

MINUTES OF THE BUSINESS MEETING OF THE AMERICAN ASSOCIATION FOR PUBLIC OPINION RESEARCH, ITHACA, NEW YORK—JUNE 21, 1949

The meeting was called to order in the Memorial Room of Willard Straight Hall at 4:15 p.m. by President Wilson.

The minutes of the 1948 meeting, as printed in the Proceedings, were adopted without objection.

The Secretary gave a brief report of the eight meetings held by the Executive Council during the year. He noted the fixing by the Executive Council of the fiscal year of the organization to coincide with the calendar year, and the decision to make members' subscriptions to the *Public Opinion Quarterly* also run for the calendar year. He reminded the members of the action taken by the Executive Council to endorse on behalf of the Association the investigation of the election polls made by a committee of the Social Science Research Council.

The Treasurer reported on receipts and expenditures of the Association for the period January 1 through June 19, 1949 as follows:

Cash balance on hand January 1, 1949		$1371.93
Less expense paid in 1949 but attributable to 1948		
Postage	$23.76	
Stationery	16.27	
Telephone	47.80	
Messenger service	2.30	
		90.13
Net worth of AAPOR January 1, 1949		$1281.80

Income January 1-June 19		
203 membership dues @ $10.00		$2030.00
		$3311.80
Expense January 1-June 19, 1949		
Public Opinion Quarterly 203 subscriptions @ $3.75	$761.25	
Princeton University Press for 1948 proceedings	500.00	
Postage	40.41	
Stationery	9.07	
Telephone & telegraph	44.68	
Messenger service	.60	
Name tags (Suchman)	27.50	
		1383.51
Cash balance June 19, 1949		$1928.29

The Treasurer also presented estimates for receipts and expenditures for the remainder of the fiscal year 1949 which indicated a probable net worth of the Association on January 1, 1950 of $1589.79. The corresponding figure for January 1, 1949 was $1281.80. The Treasurer pointed out that the Association was thus probably in the black for the year, but not in a position to undertake any large additional continuing commitments with its present indicated income.

Under the heading of unfinished business, the President reported on his negotiations with the State Department over the granting of a visa to Laszlo Radvanyi to attend the Conference. The Association was informed that as an organization it could file an appeal from a probable State Department rejection of Radvanyi's application for a visa, and the Executive Council voted to file the appeal. When Dr. Radvanyi was informed of this action by the Executive Council he replied that because of pressure of University duties he would be unable to come to the conference this year in any event. There the matter rested.

Reporting for the Publications Committee, the Editor, Phillips Davison, asked for guidance from the Association in connection with plans for the 1949 Proceedings. He said that the Committee could publish the Proceedings in three different formats: (1) in a form identical with the 1948 Proceedings as a part of an issue of the *Quarterly*, (2) as a separate pamphlet supplement to the *Quarterly*, and (3) as a larger pamphlet using photo-offset or some cheaper reproduction process than actual print. On a show of hands from the membership, a majority expressed their opinion that the 1948 Proceedings had been adequate for their purposes.

In the absence of the chairman of the Nominating Committee, a member of the Committee, Francis Waters, presented the following proposed amendment to the Constitution and moved its adoption:

> ARTICLE IV
> *Section 1.*
>
> The officers of the Association shall be a President, a Vice-President (who shall be deemed President-Elect for the succeeding year), a Secretary-Treasurer and an Editor.
>
> *Section 3.*
>
> The Vice-President shall serve for one year, *and shall then succeed to the Presidency.* When the Executive Council rules that the office of the President is vacant, the Vice-President shall become President.
>
> *Section 6.*
>
> The Vice-President, Secretary-Treasurer, and elected Executive Council and Standing Committee Chairmen shall be chosen by the membership in the following manner. The Committee on Nominations shall, at such convenient time prior to the annual business meeting of the Association, invite each member to designate a nominee for each of these offices. On the basis of these suggestions, the Committee on Nominations shall prepare a slate of nominations, which shall be mailed to each active member one month before the annual business meeting. *It shall be binding on the Committee to nominate two members for the office of Vice-President and two members for the office of Secretary-Treasurer. The Committee may at its own discretion nominate either one member or two members for the balance of the slate.* The Committee shall present this slate at the annual business meeting, together with any other nominations for elected offices submitted to the Committee by an active member, on the day preceding the annual business meeting, endorsed by twenty active members of the Association. The elections of officers shall take place at the annual business meeting, and the Secretary-Treasurer shall certify and announce their results.

In explanation of the amendment, Mr. Waters pointed out that the membership of the Association comprised two groups—academic people and commercial researchers, and that his Committee felt that an alternation of a representative of each group in the presidency was desirable. He also supported the proposal to have the Vice-President automatically become President in the year following his vice-presidential term.

It was explained by the President that under the Constitution an amendment must be proposed at the Annual Meeting, where if it is voted by a majority of the membership, it comes immediately into effect. Failing an affirmative vote of the majority of members in good standing, the amendment must be approved by a majority of those present, and then subsequently ratified by mail ballot. The President noted the absence of a membership majority at the meeting.

In response to a question, Mr. Waters said that the plan of alternating an academician and a commercial researcher in the office of president was not incorporated in the amendment but the Committee hoped that it would become an understood policy of the Association. Mr. Roper doubted whether it would be always possible to distinguish between the two categories, an added reason for leaving the matter within the discretion of succeeding Nominations Committees, rather than embodying the plan in the Constitution.

The amendment was carried by vote of a majority of those present, but not a majority of the total membership.

Mr. Waters then presented the following nominees for offices of the Association: President, Paul Lazarsfeld, Columbia University; Vice-President, Julian L. Woodward, Elmo Roper; Secretary-Treasurer (two-year term), Matilda White Riley; Conference Committee Chairman, Clyde W. Hart, National Opinion Research Center; Nominating Committee Chairman, Herbert H. Hyman, National Opinion Research Center; Standards Committee Chairman, Daniel Katz, Survey Research Center; Member at Large on Executive Council (three-year term), Paul Stewart, Stewart Dougall and Associates. He moved that, in view of the fact that no other nominations had been presented to the Committee within the time limit fixed in the Constitution, the Secretary be ordered to cast the unanimous ballot of the Association for these officers.

Mr. Alfred Lee spoke against the motion, reporting his opposition in principle to single-slate nominations. He felt that it was an unwise and undemocratic precedent to set, and one that would make a bad impression on the Association if allowed to stand. Mr. Raymond Franzen supported him in his position; although approving the candidates on the slate, he rejected the idea of a single-slate list of candidates. He noted, however, that the fault would be corrected in subsequent years by passage of the amendment.

Mr. Stember asked how the slate had been drawn up and Mr. Waters explained that the Committee action was based on a mail poll of the membership. The nominees, he reported, were the choice of the membership, with two exceptions—one where a nominee declined to serve and one where a choice of the members could not have been nominated and still allow of an alternation between academic and commercial members.

Mr. Lee offered a substitute motion that the slate of nominations be recommitted to the Nominating Committee with instructions to the Commitee to produce an amended slate containing two candidates for each of the three principal offices. The motion was seconded by Don Cahalan. On a division the substitute motion was adopted by a vote of 28 to 17.

Under the heading of new business, John Riley presented a resolution drawn up by a sub-committee of the Executive Council. The Resolution read as follows:

"Resolved that Article II Section I of the Constitution be amended by substitution of 'six standing committees' instead of 'five' and by inserting in the list of committees 'Committee on Research Development.'

"Resolved that the Constitution be amended by inserting the following as Section VII, Article VI:

"The Committee on Research Development shall be composed of five members elected by the Executive Council. Each member shall serve for a period of two years, except that two of the five members elected in 1949 shall serve only one year each. The chairman of this Committee shall serve *ex officio* as a member of the Executive Council.

"The function of this committee shall be to assist members of the Association in the planning and conduct of research projects of basic nature and assist members in procuring the funds and facilities required for all research projects. The committee shall not undertake research on its own account nor commit the Association to any research program of its own."

Mr. Riley moved the adoption of the constitutional changes proposed in the resolution. In response to a question he expressed his opinion that the proposed committee would function in two ways, 1) it might stimulate the development by individuals or groups of plans for research on important methodological or other problems and 2) it might examine plans presented to it, endorse or reject them, and support requests to the Foundations for funds to carry out projects it endorsed. The Committee would not carry out research under its own auspices.

The resolution was then put to a vote and adopted for submission to the membership by subsequent mail ballot.

Mr. Manuel Manfield moved the adoption of the following resolution:

"Resolved that the Executive Council be empowered to recognize as local chapters ten or more active members of the Association in local areas requesting such recognition and to prescribe the procedures and regulations under which such chapters shall function: and that at the next annual meeting of the Association the Executive Council submit for the approval of the membership such rules and regulations as they deem necessary for these local chapters."

After discussion, the motion was adopted.

Ross Beiler then moved the adoption of the following resolution:

"Resolved that the Association establish a committee to foster regional and local polls."

It was agreed that instead of establishing a new Committee to carry on these activities the matter would instead be referred to the Committee on Research Development, when that committee is established, with instructions to carry out the intent of the motion. On this understanding the motion was carried.

Stuart C. Dodd moved that the Association request the Executive Council to investigate the problem of establishing closer liaison with the World Association for Public Opinion Research and take such action toward that end as it deemed feasible. He suggested the following as actions which the Council might take under consideration:

1. Scheduling a session for WAPOR members attending the next AAPOR conference.

2. Setting up some form of interlocking membership between the Executive Councils of the two organizations or possibly establishing instead a special AAPOR committee on liaison with WAPOR.

3. Inviting WAPOR to meet jointly with AAPOR in 1950.

4. Arranging better provision for reporting poll results from outside the United States, either in the *Public Opinion Quarterly* or the *International Journal of Opinion and Attitude Research*.

5. Consider the feasibility of a joint-dues arrangement.

Mr. Dodd's motion was seconded and carried.

Mr. Berelson moved that the Association express its thanks and gratitude to Cornell University for its hospitality, to John W. Riley, Jr. for his work on the program, and to Edward Suchman as chairman of the Committee on Local Arrangements. The motion was carried.

On motion, the meeting then recessed to await the revised report of the Nominating Committee.

The meeting reconvened at 10:15 p.m. and Mr. Waters presented the following slate of nominees for 1950 offices:

President: Paul Lazarsfeld, Columbia University; Bernard Berelson, University of Chicago.

Vice-President: Julian L. Woodward, Elmo Roper; William Lydgate, American Institute of Public Opinion.

Secretary-Treasurer (two-year term): Matilda White Riley; John W. Riley, Jr., Rutgers University.

Conference Committee Chairman: Clyde W. Hart, National Opinion Research Center.

Nominating Committee Chairman: Herbert H. Hyman, National Opinion Research Center.

Standards Committee Chairman: Daniel Katz, Survey Research Center.

Member at Large on Executive Council (three-year term) Paul Stewart, Stewart Dougall and Associates.

The members present proceeded to ballot on these nominations, and the following were elected: President, Paul Lazarsfeld; Vice-President, Julian L. Woodward; Secretary-Treasurer, Matilda White Riley; Conference Committee Chairman, Clyde W. Hart; Nominating Committee Chairman, Herbert H. Hyman; Standards Committee Chairman, Daniel Katz; Member at Large on Executive Council, Paul Stewart.

The meeting then adjourned at 10:45 p.m.

JULIAN L. WOODWARD
Secretary