Public Opinion Research 1951-1970: A Not-Too-Reverent History*

BY JULIAN L. WOODWARD

By tracing present trends into the future, and simultaneously injecting fanciful predictions of things to come, the author produces a "history" of opinion research which is both stimulating and amusing. The task of disentangling the serious from the whimsical is left to the reader. The paper is based on an address delivered to the American Association for Public Opinion Research.

Julian L. Woodward is Research Executive with Elmo Roper and was the 1950-1951 President of the AAPOR.

USING a prophetic "History of the Future" as a vehicle for evaluative comments on present trends is no new device. My own inspiration to follow this line comes from a document with which Professor McQuilkin DeGrange of Dartmouth College used to regale student and faculty groups on the campus there way back in the nineteen-twenties. DeGrange's paper, entitled, "A History of the Future," was a compound of social commentary with what we would today call science fiction. DeGrange may be said to have "predicted" the advent of television and of something he called a "radium generator," which is not too far from the atomic pile of today. He also predicted (so far unsuccessfully) the development of an anger-dissipating capsule, one dose of which would guarantee rational treatment of all issues. And in a society equipped with the capsule he felt safe in forecasting the development of a world government that really worked.

These parts of the "History of the Future" I gleaned from a copy of the original manuscript which DeGrange uncovered and loaned to me. But one thing about the "History" stands out in my own memory. This was a dramatic incident where a grave international conflict was narrowly averted after the Olympic Games of 1970. It seems that by that period the Games were largely contests between glandular coaches who stimulated the contestants' endocrine secretions with their own

* Presidential Address, American Association for Public Opinion Research, Princeton, N.J., June 23, 1951.

private drug combinations. The international complications arose when an overstimulated contestant died in mid-air while in the act of breaking the world's broad jump record. The judges could not decide whether the record jump should be counted or not.

Perhaps this incident will provide the proper background for what I shall present in the way of a history of the future much more limited in scope than that of DeGrange. What I propose to do is to engage in a few serious and a few not-so-serious predictions confined to the field of public opinion surveying, with the serious and non-serious so carefully mixed that I later can say that the predictions that did *not* come true were not seriously made and that the ones that did were clearly the results of superior scientific foresight and imagination. It might have been better if we pollsters had followed a similar formula in 1948.

Transport yourselves then to the year 1971. The American Association for Public Opinion Research, then having attained both respectability and prosperity, is holding its twenty-sixth annual convocation at a luxury resort in the highlands of Guatemala. There, far away from the mundane world of sampling error and interviewer turnover, the pundits of polling, six hundred strong, are considering the state of their art and making unscientific experiments with their digestion. Since any art or science that makes pretense to respectability must sooner or later set up altars to its ancestors and acquire a history, it is not surprising to find an audience gathered to hear a paper learnedly entitled, "Some Introductory Comments on the History of Public Opinion Polling, with Special Reference to the Period 1951-1970." What follows, my friends, is that paper.

SAMPLING IN THE EARLY 1950's

No doubt there are many of you who are present on this June evening in 1971 who can recall the days of 1951 and the primitive state of our technology at that time. Let me, however, refresh your memories at the same time that I enlighten some of our younger members who have become practitioners since the development of the Universal Roster, the robot interviewer, and the telepathic poll. After all, these modern developments had their origins in the devoted labors of the polling scientists in the early years of the decade, and it is only right that we should accord recognition for the pioneering contributions they made.

Take the field of sampling, for instance. In 1951 we were just

emerging from the great controversy over whether the probability sample was a revolutionary development in polling science, doomed immediately to drive out all baser unquotable forms of sampling, or only an ideal that one should strive for, but hardly hope to reach with the primitive tools then available. It was beginning to be clear that the achievement of the true probability sample of individuals in a national population was, to say the least, an *im*probability, except in special circumstances. There were some parts of cities that even the most conscientious of interviewers balked at going to, there were people almost perpetually away from home, there were unreachable people protected by overconscientious and incorruptible doormen, there were refusals, and there were hotel dwellers, roomers, and sick people who often could not be reached. Compromises, both in the original probability design and in the administration, were inevitably being made all along the line. In the early days these compromises received little attention—sometimes of course they were deliberately played down but more often they were simply not focused on—and the sample still bore the probability label. Finally, along about 1953, partly as a result of formal action by this Association, it became fairly general practice to report the sample design in detail and to describe explicitly how it was carried out. Readers were told that X per cent of the pre-designated respondents were not reached and what, if any, adjustments were made for their absence in the totals. Clients were also told just how the pre-designation was carried out.

The need for this latter procedure was dramatized by a famous scandal in 1952 when a research house was found to be deliberately but confidentially excluding all penthouse occupants from its samples, as being corrupting to the morals and/or morale of its middle class female interviewers. If the interviewers got by the downstairs man and the butler they were corrupted by the upstairs surroundings into asking for more pay; if they did not get by, they were corrupted by the temptation to pretend they had. Since the client who discovered the omission was a manufacturer of private helicopters, you can imagine that the event caused some furore and brought the Better Business Bureau on the run.

One result of the sampling ferment in the early 1950's was a type of approach that was dubbed "possibility sampling" by one of its supporters. A possibility sample was, in effect, simply the best sample that could be obtained under the circumstances, but it sounded like much more

than that when described by media researchers in full-page advertisements in newspapers. Actually, the possibility sample of the period usually contained a good deal more randomness than the old-fashioned quota sample, but it did not altogether exclude judgment or selection bias by the interviewer, any more than it did the judgment of the client's treasurer. The mathematicians went to work to find error formulae that would apply, within limits, to this kind of sample, and by 1953 they had succeeded so well that sampling methods for the time being ceased to be hotly debated. And for four years the efforts of researchers were directed almost exclusively toward reducing the much larger errors that had always existed in other parts of the survey process.

COMPULSORY VOTING AND THE NATIONAL ROSTER

Finally in 1962 another great step forward in sampling came about, almost as a by-product of research being done along other lines. All through the period from 1950 on there had been continued attention devoted to improving methods of election prediction and, after the adoption of compulsory voting in '61 had licked the turnout problem, polls taken a day or two before election with the improved questioning techniques came to predict election results with such accuracy that the need for a complete balloting by everybody vanished. Anyone could see that an officially conducted sampling poll would give the same results in 99 per cent of the contests, and would be a whole lot cheaper. In the 1 per cent of instances where the difference between candidate totals was less than sampling error, successive samplings were taken until the results stabilized. Or else the candidates tossed a coin, it being assumed that public preference between them was too slight to matter greatly which one occupied the office.

Of course if candidates were to be elected by samples of the eligible voters, the sample used had to be a very good one. The obvious need for complete randomness in selection led to the creation of the National Roster which you and I, as well as the government, use so regularly today. In perspective it is hard to see why we were so long in coming to this solution for sampling problems, especially since rosters had been available in European countries for some years. While the number tatooed on your thighs may be slightly disfiguring without cosmetic treatment, it does certainly serve to identify your place in the Roster from maturity to death. And you are now so used to paying your fee for

having the National Urn rotated and a sample of numbers drawn from it, that any other procedure seems antediluvian and absurd. The National Addressographica, which ties up numbers with names and locations of respondents may, like all government bureaus, be somewhat behindhand in recording address changes, but you could not do without it, nor could I. The spread of the plan to other parts of the world further attests its utility. Only last month the Gallup chain got the Roster plan adopted in Tibet (by pointing out that existing sampling techniques there would bias the chances of the ruling Lama in the next pre-election poll). It is rumored that Gallup will use a similar logic when the new affiliate on Mars commences operation next year.

THE STRUGGLE TO REDUCE INTERVIEWER BIAS

Turning now to another great methodological problem of the early fifties, that of interviewer bias, we have even greater progress to report. Many of you will recall the Hyman Report of 1951, which focused attention on the biases, errors, and invalidities of the doorstep interaction between interviewer and respondent. Soon after the report appeared people who had been hiring and training interviewers by mail began to hire supervisors and to talk about "Certified Interviewing Staffs" and "our corps of tested interviewers." Those who had already been field training their interviewing staffs lengthened their training programs, went in for periodic refresher training, and raised the compensation of interviewers to take account of their increased expertness. Everyone experimented with new methods of evaluating interviewer performance and at the same time the personnel testers were called in to help devise better methods for selecting interviewer material.

In this effort to reduce interviewer bias some interesting techniques were employed. Unfortunately most of them worked better in the laboratory with suitably docile guinea-pig respondents than they did on a stranger's doorstep. Naturally, concealed wire recorders were utilized, but after a few irate citizens brought suit for invasion of privacy, their use in other than experimental studies dwindled. The candid sound camera technique was also employed but this ran into similar difficulties, as did the use of portable lie detectors that were strapped on respondents' wrists. A device known as the Roper Veracitator, based on some of the then-new discoveries in cybernetics, seemed for a time to be a solution to the problem. But the machine itself turned out to have a per-

sonality of its own, and unfortunately a completely whimsical and unpredictable one, so that it would work accurately only in a state of hypnosis. Only Roper himself was consistently successful in mesmerizing his own creation, and he lost interest in the device when he discovered that it was, in a sense, only a projection of his own conceptions of truth and falsity.

The net result of all this activity in the early Fifties was a clearer recognition of the magnitude of the interviewer bias problem but a turning away from any effort to trick the truth out of the respondents. The respondent's right to tell as little or as much of the truth about his attitudes as he wished, or was able, was again emphasized. And attempts to solve the validity problem took the direction of making him *want* to reveal his opinions, with consequent renewed emphasis on rapport between interviewer and respondent and on the nature and form of the question asked.

RESEARCH OBJECTIVES MORE PRECISELY STATED

One other development of the early Fifties that had implications for validity was a greatly increased interest in more exact definitions of the thing public opinion research people were trying to measure. The loose definitions of "opinion" and "attitude" that had hitherto served for pollsters, if not for psychologists, gave way to more precise statements of just what it was that the surveyor was after in each particular piece of research. Did he want to predict behavior such as voting, buying, contributing, attending, supporting in a specific future situation, or did he only want to get at people's more general leanings or dispositions in situations much less specific? Did he want to know how firm and permanent was the leaning, or how much factual knowledge was back of it? Did he want to find out to what extent it was a reasoned conclusion, to what extent a rationalized one? All of these questions had been in the minds of researchers at some time or other, but now there was a much greater effort to state objectives for each study in more specific terms.

How to know whether one of these specific objectives was actually achieved in a given study was always a problem for researchers in the nineteen-fifties. For a time there were two schools of thought with respect to validity. The first, comprising a large share of the commercial survey organizations and not a few of the academicians, still counted on

careful pretesting for frame of reference and on know-how in question wording to ensure a validity that was sensed more often than actually proven. Interpretation of question results was based on "feel" for the meaning of words and phrases to different kinds of people more often than on actual correlations with independent criteria. In describing the attitudinal area under investigation the approach was psychological and anthropological rather than statistical; it was intuitive rather than objective.

The second school was more mathematical and more strictly operational in its approach. When attitude measures were found to be scalable in the Guttman sense they acquired validity by that very fact. The scale described an attitude area that had uni-dimensionality as a principal component. Frequently it was possible to arrive by purely statistical means at two other components or dimensions—intensity and closure—and these helped further to define the attitude measured. And where no Guttman type scales could be developed there was always the possibility that a Lazarsfeld latent attitude scale could be teased out of the data.

For a while there was a tendency among some groups to think that attitudes that couldn't be scaled were too ephemeral to bother with. On the other hand some hard-boiled market research clients were unwilling to pay the extra costs of scale exploration, hinting that a scalogram board was only a high-powered and expensive ouija. But both sides got together eventually and much more scaling was done.

Meanwhile, the Lazarsfeld concepts found unexpected application in the field of dream analysis and in the not-too-distantly related area of advertising effectiveness. It turned out that the major latent attribute in advertising appeal was not sex (which proved completely unscalable) but rather a regressive urge to repeat in adulthood the rhythmic pleasures of nursery experience. When this was discovered, Conover models over the age of five became a drug on the market.

QUESTIONNAIRE CONSTRUCTION AND CODING

Potentially scalable questions were not the only type experimented with in the early nineteen-fifties. There was increased experimentation with all question writing techniques, and much less dogmatism about the virtues of any particular type of question. By 1953 the so-called open-ended question had especially come under criticism, just as had the single closed-ended question earlier. Foundation support was obtained

for a big research project on informal, open-ended questions and the qualitative analysis thereof. No one was really surprised to find out that answers were recorded incompletely and inaccurately in many instances, or that the coding was also frequently unreliable to a serious degree. Instances were found, however, where the recording and coding were both done with little error, and attention was directed toward obtaining a careful description of the conditions under which such accuracy was attained. There was a tendency to use more pre-coding of answers to open-ended questions, thus allowing the respondent to classify his own answers in relation to a framework that had been developed through pretesting. After all, some framework inevitably would be applied when the answers were coded in the office—why not let the respondent fit his own answer into that framework himself? Where answers were office-coded there was a tendency to use much simpler codes, concentrating more on a few categories directly related to the hypotheses that underlay the inquiry, and avoiding the elaboration of coded categories that was standard practice earlier. Finally, there was a tendency to resort more frequently to pattern coding—a purely qualitative technique. But the pattern that was found to emerge in the mind of the analyst as he read over the question answers was not allowed to stand without checking—only when two or more analysts came up with similar patterns was the summary reported as scientific fact.

As an outgrowth of these various trends in the handling of open-ended question material the whole function of qualitative techniques in public opinion research became somewhat altered. They were used more for preliminary reconnaissance and as a basis for writing fixed-answer or aided-recall questions that presented a greater variety of alternatives to the respondent, but still retained the possibility of quantitative treatment of results. Thus the point of view that Lazarsfeld had outlined as early as 1944 became largely adopted ten years later.

There were other developments in questionnaire construction. Projective techniques were widely experimented with, the questioning in some instances becoming so devious and indirect that analysis of the results came to be a projective test of the analyst more than of the respondent. On some occasions the clients had even to employ psychiatrists to interpret findings they got from the research organization, and it became the practice to submit a brief biography of the analyst—includ-

ing Oedipus complexes, early toilet-training history, and traumatic experiences in childhood—along with the findings he had written. Researchers ruefully called this Dichteration by clients but had to submit.

Some simpler projective approaches proved more useful and less embarrassing, particularly those in which the respondent was shown pictures of people in ambiguous situations and asked to interpret. This doorstep thematic apperception test was carried very far in some instances. Respondents were shown all sorts of things, from unlabelled liquor bottles (to test association of brand name with the shape of the container) to composite photographs of national figures (to find out what kind of profile had the greatest political "oomph" for what kinds of people). One enterprising ink manufacturer had doorstep Rorschachs conducted to find out whether the brand of ink used affected the ink blot interpretations made by respondents.

INTERPRETATION AND PRESENTATION OF RESULTS

I want to mention one other development in relation to the validity problem that was taking place in the early Fifties. This was a growing insistence on the necessity of expert interpretation of polling data. In the early days of public opinion polls each citizen tended to be his own interpreter of what the results meant—he read the individual questions that had been asked, looked at the percentages answering them one way and another and drew his own conclusions. After all, he thought he could see what the results meant as well as anybody. While researchers knew that the percentages often meant something different from what the naïve reader thought they did, it took time to convince editors, commentators, and the public generally that not only were qualifications concerning any single percentage figure necessary but also there was nearly always a need to set it against the percentages obtained on other questions; in other words, to interpret individual figures in relation to the over-all study design.

It had long been a commonplace in other scientific research that only individuals who had really comprehended a study design and also "immersed themselves" in the data were really competent to draw conclusions. The difficulty often was that such individuals were unable to see the broad relevance of their findings or even state their conclusions in sufficiently clear English so that a more broadly oriented person could

see their significance and put them to use. Sometimes the problem was solved by the interposition of a communicator between the researcher and the ultimate user—a scientific popularizer who translated the strange language of the scientist and made his findings available.

As study designs in public opinion research grew more complex, the field went through the transition just described. There were technical people, there were presentation experts, and there were users or clients. The presentation people were always trying to make things too simple to suit the researchers and were having more and more difficulty making them simple enough to suit the client. They tried charts and art work, they tried "n" dimensional models, and finally they began to dabble in hypnotism and direct thought transference. It was found that when unconscious resistances were removed, and the mind of the client laid completely bare, the essential facts from the research findings that were suggested to him in the trance state would come naturally to his consciousness upon waking. Eventually it became possible to put the communicator also under a form of hypnosis so that he acted as a sort of automatic decoder and transmitter of the technical findings. With the three parties in adjoining cubicles connected by telephone the process of dosing the client with usable information went on rapidly.

As you know, we are reaching out even beyond this today by experimenting with the extremely radical technique of recombining all three functions—analysis, reporting, and utilizing—in a single individual. The client hypnotizes himself and becomes a researcher; when the study is done, he reverts to his former personality and has memory for what is significant to him (as a client) in his research findings. In the future (say by 1980) we will probably all be congeries of multiple personalities which can be turned on and off by a psychic switch. An immense increase in human powers is bound to result, along with some minor difficulties concerning social and sexual relationships, some of which Stephenson foresaw in the Jekyll and Hyde epic. Others I may leave to your imagination.

It is obviously impossible in a historical account of this sort to do justice to all the methodological developments that have taken place in the last ten years. I will, therefore, touch on only one more aspect of technique before turning finally to a discussion of the social role of polling as it has been and is being defined by your predecessors and yourselves.

TABULATION—AND THE STRANGE PUNCH CARD BLIGHT

The remaining technical area where progress has been so great that it cannot go unrecorded is that of tabulation, or in more modern terminology, *quantification*. At the beginning of the Fifties the procedure was to use punch cards, one for each person questioned, and to calculate totals by running the cards through machines made by a company called I.B.M. It was even then recognized that the process of punching cards was a time-consuming and inefficient manual procedure, but so long as cards and punch operators were available little thought was given to possible improvements. However, in 1953 came the Great Punch Card Famine, which made necessity once more a mother of invention and eventually changed the whole tabulation procedure.

The full story of the emergence of the strange blight, which affected punch cards on supply shelves and filled them with little holes indistinguishable from those made by punchers, still remains to be told. The bio-chemists, the bacteriologists, the entomologists, and even the F.B.I. all had a go at trying to discover the causal agent—animal, vegetable, or mineral—that lay back of the strange manifestation. But before they had uncovered the culprit the whole industry was desperately resorting to other tabulation methods. People were hiring extra warehouse space for sorting questionnaires into cellular piles and then using various mechanical methods for counting the "yes" and "no" answers in each pile. One researcher fortunate enough to have his office in a community where space was more freely available tried to utilize an out-of-door grass plot for questionnaire sorting. When a wind came up and somewhat rearranged his pile tabulation, he found it far more economical to develop a formula to correct for wind drift error than to do the sorting over. After all, it was only the top questionnaires that had been moved.

It was not long before science came to the rescue of the hard-pressed pollsters, and provided a whole new approach to the problem. The respondent was given an electronic counting box on which he pressed buttons to indicate his answers to questions asked him by the interviewer. The box stored up the answer patterns of successive respondents and when the day's work was done the box was sent to national headquarters where it discharged, or fed back, one-by-one the interview patterns to a master electronic calculator which could be set for any kind of cross-tabulation, scale analysis, or error computation. Soon the indi-

vidual boxes were improved by a device that broadcast the results to the master machine, thus saving time and express charges. Since I.B.M. made and rented the counting boxes and the master calculator, the punch card blight was allowed to run its course unhindered. No one was interested in punch cards any more.

INCREASING PROFESSIONALIZATION

Well, what did all the advances in methodology during the early Fifties do to the public status of polling? Undoubtedly polls became more accurate, but did the public trust them more? For a while the answer was "No." As the technique became more complex it also became less comprehensible to the laity, who could understand percentages on a single question but not the more accurate but at the same time more general conclusions which so-called experts attempted to draw for them from whole batteries of questions. The experts had to qualify again as medicine men, the possessors of a special but incomprehensible magic, before the public was ready to give them the sort of trust it reposed in electronic physicists, industrial chemists, atrologers, and tax accountants.

Three developments were especially influential in gradually altering the situation and in restoring to the pollster the reputation of wonder-worker and magician he had had for a brief period before 1948. The first of these was a new professional status that was gradually being attained by the public opinion surveyor and market researcher. So long as the commercial polling organizations (and indeed some of the university organizations also) were willing to submit competitive bids on research projects and so long as particular techniques were regarded as trade secrets or given fancy labels for selling purposes, both clients and the public could hardly be blamed for thinking of survey designs as shining pre-packaged products to be bought the way an advertiser bought space, or a manufacturer purchased sub-assemblies. Probably some of the survey organizations almost got to thinking that way about their research projects themselves.

Gradually, however, it came to be recognized by everybody that formula jobs were few and far between, that each project was unique in itself—designed to answer a particular set of questions under particular conditions of time pressure, possible cost, and required accuracy, and that what the research organization was really selling was its brains,

know-how and problem solving ability. More recognition was also given to the essentially unpredictable factors in the research process—research was seen as not just a production line process but also as essentially a questing into the unknown, a *search* in which one often finds the unexpected. To undertake to deliver answers at a predetermined price was seen to be a risky business; the researcher needed to have some freedom to adapt techniques to problems as they emerged along the road, and to reformulate hypotheses as the data indicated—provided always that his thinking retained a relevance to the investigation's original purposes. Only the *pure* scientist—a type of researcher seldom found in the social sciences—could be subsidized to follow data *wherever* he thought they might lead.

What the pollster was adopting was the attitude of the *applied scientist*—with emphasis on *both* words. It was also an attitude closely akin to that of the lawyer, the doctor, or any other professional man who sells services rather than products. Public opinion and market research people came to be hired more often on a retainer basis. Or, when a government agency or a business had a problem, it would pick one research organization and put it to work, giving it enough time and cooperation to gain an understanding of the problem. When the research design was produced the client was still free to say, "We don't think we want it done that way or at that probable cost" and go elsewhere, just as the patient is free to distrust his family doctor's diagnosis after it is made and ask for confirmation from another practitioner. But the patient would never think of initially calling in four doctors and asking for competitive plans for cure, and by 1955 businessmen seldom essayed any analogous procedure with researchers. Instead they simply tried hard to pick the best organization available, they exposed their problems to it as fully as their own current information and insights made possible, and then they let it go to work.

This rise in the *professional* status of market and public opinion research was one development making for improvement in the pollster's public status. Two other developments along the same line may be enumerated briefly. First, the more sophisticated experts in government and business were all the time placing ever greater reliance on survey techniques, and this fact impressed people in general. Second, the pollsters, using their improved methods, made some dramatically successful

election predictions (mentioned earlier in this history) and they too raised the pollster's stock in the public eye.

POLLS IN THE PUBLIC SERVICE

What have the public opinion surveyors done with this "second opportunity?" You know the answer as well as I. Having learned the dangers of becoming soothsayers before they were ready for such a responsibility they have this time builded more slowly and soundly. They made no immediate claims to replace the ballot box or to speak the public mind on all issues, but simply said: "We have a device for finding out what a cross section of the public *say* they want done on a variety of national issues, but by no means on all issues. We report public opinion with considerable accuracy, probably with greater accuracy than any other available mass technique can boast of, but of course not with complete accuracy, nor indeed with exactly definable accuracy. Let us interpret the meaning of our results to you and you can safely use our technique as an important new tool for Democracy, a tool that is already good enough to be worthy of more employment than it is getting. But don't ask for what we are not yet prepared to give—don't ask us to probe the unconscious minds of citizens, or report attitudes on subjects where attitudes are unformed, or give an accurate picture of the actual pressure behind a poll majority, a report on *effective* public opinion as distinct from merely articulate opinion. We are working on these problems but we haven't solved them yet."

The statement I have just quoted was made in 1954 by a member of this Association at a Congressional hearing on a State Department request for polling funds, a request which, incidentally, was turned down! Since that day we have made great progress on these specific problems and have also come a long way on the road to wider public service. We have, in fact, become a clearly indispensable part of the whole process of government. As a profession we not only conduct the present-day substitute for the old ballot-box election, we also operate the Continuing Referendum, a periodic public opinion survey operated under combined government and private subsidy by four cooperating survey organizations.

As most of you know, the primary purpose of this twice-monthly national survey is to report the wishes of the public to government. I use the term "wishes" rather than "attitudes" or "opinions" to empha-

size the fact that it is the public's ideas on ends, rather than on specific means to reach those ends, that are measured. The world was, no doubt, simpler in 1951 and people could perhaps be expected to have an opinion on whether the chances of peace with Russia were greater if we bombed in Manchuria or if we didn't or whether we should or should not adopt a particular form of subsidy for farm prices. Today we recognize that questions of this sort are too difficult, and require too much information and background of experience to make an intelligent judgment, for the individual citizen to have a competent opinion about them. Consequently we tend to confine our questioning to *values*, asking people whether they want such and such a service at such and such a personal cost to them, or whether they think such and such a procedure for resolving a particular dispute between different interest groups is fair and equitable, or not. Nowadays we seldom ask people to pass on the merits of the arguments presented by parties to a dispute. To weigh the arguments requires technical knowledge and experience the public simply does not have. Consequently, we only try to find out whether the people think that the contending factions are willingly submitting their case to the proper expert commission or arbitral tribunal. When the public expresses doubts on this point, those doubts are put on the record, and they prevent a decision in favor of the side which in the eyes of citizens is not prepared to take its case to a court and come before that court with clean hands. No doubt you will recognize this whole point of view on what the public can and cannot effectively do in a Democracy—it was expounded by Walter Lippmann in his book, *The Phantom Public*, almost forty years ago.

As you are aware, we also do several other things in the Continuing Referendum. We find out how strongly people feel about the things they would like to see done; that is to say, we develop a public hierarchy of values for any given period—a sort of "how-much-do-you-care index." And we find out how people react to the personalities of those in leadership positions—a "whom-do-you-trust index." The problem of picking able and honest and responsive government officials is a vital one in any society. The "whom-do-you-trust index" helps greatly in this respect, but we now have another tool that is even more valuable—this is the Commonweal Motives Test.

The Motives Test is so new that some of our guests from WAPOR who are meeting with us may not have heard of it. As the name sug-

gests it is a test of those motives that are important in a good citizen and true servant of the public. The test consists of two elements: (1) a set of probing questions designed to explore the subject's motives by placing him in a set of hypothetical situations involving moral choices, and (2) a threefold check on the essential veracity of his responses. The old lie detector procedure has been adapted to provide one form of check for conscious prevarication. A type of narcotic drug is used in a second form of check to inhibit resistance to truth telling. Finally telepathic communication with the lower layers of consciousness is resorted to in order to get some idea of the emotional response evoked by the questions.

No one can be a candidate for public office without passing the Motives Test, and an office-holder can always be required to retake the test whenever a poll majority requests it. We pollsters set the norms for test performance by testing general population samples—to be a public official one need not be as pure as Caesar's wife, but one does need to make the first quartile.

With the election poll, the Continuing Referendum, and the Commonweal Motives Test we pollsters have at last come of age and attained an important and honorable place among the cadres of experts who serve our Responsive Democracy. As our techniques continue to improve and our knowledge of human nature grows greater, we will serve our fellow men with ever increasing effectiveness and in a variety of ways not yet even dreamed of. There is only one condition—we too must always be able to pass the Motives Test. Since as a profession we have had from the very beginning a scientist's conscience and a strong sense of obligation for public service, I, for one, have no fear that we will ever fail to meet the test of good citizenship. I only say that while the top quarter may be good enough for public servants in general, it should never be good enough for us who are scientists as well as citizens. Let *us* always be in the first decile.