Attitude Measurement, Theory, and Prediction

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THE PAST 50 years have witnessed a general, if sometimes grudging, acceptance of attitude surveys to describe the public's mood and beliefs concerning such disparate matters as political events, consumer products and services, advertising and public information campaigns. So established is this acceptance that, today, budgetary considerations are often the only constraints upon the use of attitude surveys in the development of political and marketing campaigns and, more generally, any communications effort. Nonetheless, this acceptance is certain only at the descriptive level, when all that is needed and wanted is accurate measurement of public beliefs, opinions, feelings, and sentiments.

As the focus of interest shifts from the descriptive to the scientific study of human behavior, the status of attitude research becomes more tenuous and ambiguous. There are still many to whom attitudinal data are more interesting than meaningful, to whom such data are inherently "soft" and provide an insufficient base for developing a scientific understanding of how and why we act as we do. Critics of attitude research have always found a receptive audience for their assertions that it is impossible to make reliable predictions of behavior from attitude measurements. The U.S. Department of Commerce went so far as to announce publicly its failure to project reliable estimates of new car purchases from its periodic surveys of consumer buying intentions and abandoned these surveys (U.S. Bureau of the Census, 1973)—confounding those who feel that no bureaucrat would ever voluntarily end any program, no matter how futile.

One possible response to this criticism that attitudes are poor predictors of behavior is that "understanding," and not prediction, is the appro-

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priate scientific criterion for assessing the adequacy of attitude research. Such a response is unacceptable, however, if for no other reason than that most practitioners of attitude research use prediction as a fundamental criterion of science. It is difficult to find any treatise by those committed to the analysis of attitudes that does not posit the existence of lawful relations between attitudes and behavior. Furthermore, a full assessment of research experience leads to the conclusion that it is not necessary to give up prediction as a criterion. A proper articulation of attitude measurement and theory can and does provide a basis for making reliable predictions, and in a way that enhances our understanding of human behavior. Since the study of voting turnout provides an excellent demonstration of this position, on both the micro- and macroanalytic levels, the rest of this paper will focus specifically on this topic.

Microanalysis: Identifying Likely Voters

It has long been a truism in election research that any survey of voter preference is a measurement only of the time the survey was conducted. Any estimate of how the actual vote will be distributed on election day cannot be considered a direct projection from the survey data but, rather, an extrapolation based upon assumptions regarding trends subsequent to the completion of interviewing. Unquestionable as this truism must be, most election research practitioners have nonetheless concluded that an essential ingredient of their methodology must be the prediction of who are the likely voters in their samples. Once it was learned, in the early 1940s, that voting preferences of voters and nonvoters often differ (Cantril and Harding, 1943), it became obvious that if preelection polls were to provide meaningful measurements of candidate strength, some means of accurately differentiating likely voters from nonvoters had to be devised. That is, a *prediction* had to be made about which individuals in the survey sample would vote and which would not.

The importance, and difficulty, of differentiating voters from nonvoters was noted by Stouffer at the time of the Social Science Research Council's investigation of the performance of the 1948 preelection polls. Among other things, Stouffer (1949:210) observed that the potential for error if one did not successfully identify and exclude nonvoters was so great that it presented a sampling bias problem at least as serious as the use of nonprobability methods. The solution to the voter turnout problem, however, could not rest on the application of sampling theory. Instead, it was necessary to develop descriptors that would meet the empirical test of efficiently discriminating between voters and nonvoters.

In 1950, Paul Perry (1960) developed a turnout scale for the Gallup Poll that ranked respondents by their likelihood to vote. The scale met Guttman's criteria for scalability, and a postelection check of voter regis-

Table 1. Comparison of Projected and Actual Turnout in 11 National Elections

	Projected Turnout	*Actual Turnout*
1956	59.3%	61.0%
1958	43.1	42.7
1960	62.8	63.6
1962	46.1	46.5
1964	61.9	64.6
1966	45.5	48.5
1968	60.9	63.6
1970	43.6	44.0
1972	55.7	61.7
1974	36.6	42.0
1976	54.4	54.1

tration books, to identify which respondents in the sample voted and which did not, behaviorally validated the scale. With the development of Perry's turnout scale, one further problem remained before likely voters could be accurately screened out of the total sample: how to determine where a cutting point should be applied to the scale. This, of course, involved making an estimate of what the voting turnout rate would be. Once such an estimate was developed, it would be a straightforward matter to apply it to the turnout scale by treating respondents with turnout scores above the cutting point as likely voters.

A satisfactory solution to this problem of estimating turnout rates in advance of an election was not developed until 1956. At that time, following a procedure suggested by Perry, I developed an index number from survey data that accurately predicted what the turnout would be that year. The projected turnout ratio was 61.0 percent compared to an actual turnout of 59.3 percent (Table 1). With some further refinements, this method of estimating turnout has been used by the Gallup Poll for every presidential and national congressional election since then. The range of error during that period is from 0.3 to 6 percentage points, with a mean error of 2.2 percentage points. In 1976, Nicholson, working with Perry, performed a regression analysis of the relationship between the index number and the turnout rates in each election. The index number explained 87 percent of the variance in the change in turnout rate from election to election.[1]

There are two aspects of this experience in predicting turnout that are of particular interest. First, the data used to calculate the index number come from a survey conducted *one month before* election day. That is, in contrast to measures of candidate strength which are only descriptions at a given point in time, there can be no such qualifications about the ex-

[1] Personal communication from Paul K. Perry.

trapolation of the survey data to predict turnout. Nor are we talking about statistical prediction in its most limited sense, that is, mere correlation. We have here an instance of data collected one month before an event that predicts with an appreciable degree of precision the rate of participation in that event. (Moreover, Perry's turnout scale predicts with considerable precision the probability of individual participation in that event).

Second, the items used to develop the voting turnout index are attitudinal. Up to 1956, Perry had experimented with behavioral predictors, such as the number who registered to vote in New York City—a jurisdiction which up to then had annual rather than permanent registration. But this method was abandoned because, aside from the adoption of permanent personal registration in New York that year, these experiments had achieved only moderate success. In contrast, the voting turnout index was based completely on two attitudinal items—one a measure of affect, namely, involvement in the election campaign, and the other a measure of conation, namely, intent to vote. These two items, when considered jointly, have been sufficient to predict accurately one month prior to a national election the proportion of the voting age population that will vote. Perry's turnout scale, from which the two items used to construct the voting turnout index were taken, includes two additional attitudinal dimensions—cognition (for example, knowledge of where one goes to vote) and valuation (for example, how important voting is felt to be). The turnout scale and the turnout index are unlike so many, perhaps most, efforts to predict behavior from attitudes in one crucial respect: the two turnout measures can be related to a theory in which attitudes are conceptualized as multidimensional systems of cognition, valuation, affect, and conation rather than as predispositions to behave in specified ways (Crespi, 1969; Rosenberg and Hovland, 1960). It would be misleading to imply that either the voting turnout index or Perry's turnout scale was explicitly developed from any theory of attitudes. The only theory used in their development was statistical. Nonetheless, once they were developed, it is pertinent to identify a theory of attitudes to which these predictive measures can be meaningfully related.

The voting turnout scale and index demonstrate that instead of thinking of attitudes as unitary phenomena, it is empirically more productive to treat them as multidimensional systems. For example, although the turnout scale meets Guttman scale criteria of unidimensionality, it contains items as conceptually distinct as knowing where one goes to vote and one's intent to vote. Also, both the turnout index and scale require attitudes to be conceptualized as multidimensional psychological systems that influence the *probability* of behaving in a specified way rather than as behavioral predispositions. For example, the scale assigns to each individual a probability of voting and not a prediction that he will or will not

vote. This emphasizes the need to relate behavior to the *total interactive system* of beliefs, feelings, purposes, and intentions rather than analyzing each dimension separately or hierarchically.

If, for terminological reasons, one prefers to restrict the use of the word "attitude" only to affect,[2] that is in itself of little importance—so long as this does not lead to the inference that an analysis of affect by itself is sufficient for an assessment of the interaction between attitudes and behavior. However, if the common practice of restricting the term "attitude" to the affective dimension is continued, it would still be necessary to devise some term that is recognized as applying to the total system. Taking into account the historical scope of attitude research, and the fact that we do have other well-defined terms to use for each dimension, it does seem appropriate to apply the term "attitude" to the total system and not merely to one of its dimensions.

A final characteristic of the turnout scale and the voting turnout index that should be mentioned is their specificity. None of the individual items relate to general attitudes such as political efficacy or alienation. Instead, nearly all the terms relate to a specific form of behavior in a specific context, that is, voting in a given election. This suggests the hypothesis that general attitudes, no matter how great their theoretical interest may be, are poorly correlated with specific actions. If such is indeed the case, this would explain the failure of those who use generalized attitude scales to find any sizable correlation between attitudes and behavior.

Macroanalysis: Declining Turnout Rates

Challenging as the success in developing the turnout scale and turnout index may be, it must be acknowledged that they are both essentially atheoretical measures. In and of themselves they do not contribute to a theory of voter turnout that could, for example, explain why recent elections have been characterized by a declining turnout rate. The question arises as to whether attitude research has anything to contribute to such a theory.

One hypothesis that relies on attitudes to explain the declining turnout rate relates to Watergate. Simply stated, this hypothesis ascribes declining turnout to a loss of confidence in our political institutions in the wake of widespread revelations of illegal behavior by Richard Nixon and others in his administration. A more inclusive version of this hypothesis ascribes some responsibility for declining turnout to the loss of confidence in the credibility and responsibility of the Johnson Administration during the Vietnam War. Aside from any theoretical inadequacies in either version of the loss-of-confidence hypothesis, it suffers the fatal weakness of a poor fit with the facts.

[2] See, for example, Rokeach (1968).

Table 2. Trend in Voting Turnout in Six Presidential Elections and Five Off-year Elections

	Non-South	*South*
Presidential elections		
1956	66.3%	39.4
1960	70.5	41.5
1964	67.6	46.5
1968	64.4	51.4
1972	59.5	45.7
1976	57.0	48.1
Off-year elections		
1958	52.6	16.8
1962	53.5	25.1
1966	51.2	29.9
1970	49.1	29.1
1974	40.9	25.6

SOURCE: Personal communication from Paul K. Perry.

An examination of turnout rates outside the South during the past 20 years documents the poor fit of the loss of confidence hypothesis to the historical trend. Turnout outside the South in presidential elections increased during the 1950s, with a peak of 70.5 percent reached in 1960. From this peak, turnout started to decline by 1964—that is, before Vietnam became such a divisive issue and well before Haldeman, Ehrlichmann, and Dean were household words—and continued to decline through 1976 (Table 2). Furthermore, a parallel decline in turnout occurred during off-year elections. The low turnouts in 1972, 1974, and 1976, therefore, are a continuation of a trend that started before Vietnam, and later Watergate, became divisive issues. We need an explanation that fits the entire trend line and not merely part of it.

The search for such an explanation is complicated by the fact that a different turnout trend occurred inside the South during the 1960s and early 1970s. In that region the turnout rate in both presidential and off-year elections *increased* during most of this period (Table 2). In the South, therefore, the loss of confidence hypothesis could apply to the 1972 reversal of a trend toward increasing turnout during the 1960s. Nonetheless, the rule of parsimony requires us to seek a more inclusive explanation that applies to both the South and non-South, rather than separate explanations for each region.

An explanation for the deviation of the South from the national pattern of declining turnout should be sought in the processes of regional politics. Most obvious are George Wallace's appeal to his fellow southerners in 1968 when turnout in the South reached an all-time high, and Jimmy Carter's appeal in 1976 when southern turnout again trended upward. Another factor that undoubtedly contributed to increasing turnout

in the South during much of this period is the Federal Voting Rights Act of 1965, which enabled large numbers of blacks to vote for the first time. A third likely factor is the rise of two-party politics in many southern states, a development that gave a meaning to voting in general elections that heretofore had been lacking. One might maintain that if it were not for the loss of confidence caused by Vietnam, the 1972 decline in southern turnout would not have occurred, and that without Watergate the 1976 turnout would have been even larger than it was. However, this is a rather weak argument for what is proposed as a major influence on national turnout trends.

A more satisfactory hypothesis than Watergate is one that relates individual attitudes to institutional change, that is, integrates the micro and macro levels of analysis. Since attitudes develop, change, and atrophy in reaction to socially defined situations and experiences, a useful strategy is to identify trends in attitude change that parallel the decline in turnout, and then to identify the institutional basis for this attitude change. A brief outline should suffice to illustrate the basic thesis that attitude research must be an integral part of the scientific study of human behavior.

One of the better documented attitudinal correlates of turnout is party identification. Those who identify with a political party are more likely than Independents to vote, and the stronger party identification is, the greater is the likelihood of voting.[3] It should therefore be of specific interest to us that the proportion of voting-age Americans who call themselves Independents rather than identifying with a political party has risen sharply during precisely the period in which voting turnout has declined (Gallup, 1976:50). Caution is, of course, necessary in imputing a cause-effect relationship between two concurrent trends, so these comments must be treated as suggestive only. A specific causal process must be defined if we are to progress from mere speculation to hypothesis formulation.

Moreover, cognizance must be taken of two additional concurrent trends, the increasing proportion of adults who have attended college and the increasing use of television as a medium of political communication. The trend in education is particularly noteworthy since a high level of educational achievement is positively correlated with turnout.[4] There is as yet no detailed analysis of the trend in turnout with education controlled, but it is still the case that the positive relation between education and turnout persisted throughout this period. Therefore, solely on the basis of increasing levels of educational achievement, one would have predicted that turnout would be on the increase during the 1960s. One methodological observation that is prompted by the continuing positive relation

[3] Unpublished analyses of Gallup Poll data by Paul K. Perry. See also Campbell, *et al.* (1960:96-101).

[4] Unpublished analysis of Gallup Poll data by Paul K. Perry.

between education and turnout during a period of declining turnout is the inadequacy of normal cross tabulation and correlation for the analysis of institutional dynamics. Longitudinal and tracking studies that take account of changes in absolute rates of behavior in conjunction with correlation analysis are needed for this purpose.

The increasing use of television for political communications, especially for political advertising but also for news reporting, does have a *prima facie* relation both to turnout and to the decline in party identification. Specifically, it is postulated that the new politics of professional campaign managers circumvents party machinery contributing to the decline in political party strength and the concomitant rise in the number of Independents. These political guns-for-hire have little allegiance to political parties and often completely ignore state and local party machinery in running their campaigns. They have organized nonpartisan fund-raising efforts, sought out the ideologically committed or personality attracted activists, staged media events for television and other national news media, commissioned their own polls to tap grass roots thinking, and, in general, left traditional party machinery to its own devices.

However, one of the most important tasks of political parties has always been to get out the vote of party loyalists, and probably the most important goal of the traditional old politics was to activate the party infrastructure so as to accomplish this task (Lazarsfeld, *et al.*, 1948). But, with this infrastructure increasingly ignored, political parties have disintegrated, so that there are fewer and fewer party workers around to do the tedious door-to-door canvassing, telephoning, and other unglamorous chores that are necessary to get out the vote.

The new politics, it is true, has had a sort of success in conducting primaries, a type of election in which the concerted efforts of a well-organized, well-financed, and dedicated few can be highly effective in winning a plurality. Ideologically committed enthusiasts can galvanize narrow segments of the electorate to vote, even at the risk of alienating a party's rank and file. Even so, it should be noted that turnout in primaries, always notoriously low, has remained low and in some instances even decreased under the new politics. Most important, the new politics has, so far at least, been incapable of developing effective state and national organizations to replace the old party machineries which, if nothing else, were effective in getting out the vote in general elections.

Even under the new politics, special interest groups, such as the AFL-CIO in 1976, have been able to mobilize their resources to turn out their adherents, but such efforts tend to affect particular segments of the public rather than the broad electorate. Meanwhile, the managers of the new politics have staged an increasingly boring TV show, full of sound and fury and, in the opinion of an increasingly better educated and informed electorate, signifying nothing. If this hypothesis is correct, we must pre-

dict that the current pattern of low voter turnout will persist indefinitely—unless the political parties are revitalized, or some new institutional base for generating turnout evolves.

In one sense there is little new in this hypothesis of why turnout has been declining since 1960. Politicians who grew up under the old politics have long been aware of the processes that have been sketched out here. Lyndon Johnson, when asked by a television network producer how politics had changed since the days when he first came to Washington, replied:

> You guys . . . all you guys in the media. All of politics has changed because of you. You've broken all the machines and the ties between us in Congress and the city machine. You've given us a new kind of people . . . Teddy, Tunney . . . . They're your creations, your puppets. No machine could ever create a Teddy Kennedy. Only you guys. They're all yours. Your product" (Halberstam, 1976:65).

But Johnson's intuitive understanding of how the new politics came into being, no matter how perceptive, does not in itself provide a social psychology of political behavior. The new politics hypothesis of why turnout has declined does. It specifies how trends in voter behavior result from attitudinal changes which, in turn, are the consequences of change at the institutional level. The hypothesized process, in capsule, is: The rise of a new technology, television, has provided the stimulus and the means for a new institutional form, the new politics. This has led to the correlative atrophy of the old politics. As a result, there has occurred a weakening of identification with political parties and a concommitant unwillingness to assume a major responsibility associated with the role of citizen—voting.

Not only does the new politics hypothesis attempt to integrate institutional, attitudinal, and behavioral processes into a single causal model, it does so in a manner that can be generalized to a variety of historical situations. Thus, the deviant southern trend toward increasing turnout is also accounted for by this model since there was a rise rather than a decline in two-party politics in that region during the 1960s. The proof of the new politics hypothesis cannot, of course, be based on these informal observations. Rigorous, systematic testing is needed before we can accept it as more than a reasonable-sounding formulation. Nevertheless, it is illustrative of the type of theory that can make attitude research a valuable tool for the scientific analysis of human behavior.

Conclusion

We have considered two rather disparate aspects of research on voting turnout—the development of predictive micromeasurements and of pre-

dictive macromodels of voting turnout. Both aspects have in common the assumption that attitudes have lawful, predictive relationships with behavior. What is rejected is the inference that because much attitude research has failed to demonstrate the existence of such relationships, the scientific worth of the attitude paradigm has been found wanting. No, what has been found wanting is the way so much attitude research has been conducted with, on the one hand, reliance on attitude scales of an unwarranted high order of generality and abstraction and, on the other hand, theory that is overly particularized and concrete. If we reverse this, so that our measurements are highly particular and specific in their behavioral referents while our theory presents us with generalizable models, the goal of developing attitudinal measurements and theories that are predictive can be achieved.

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