MEMORANDUM OF INTEREST

Conference on Issue Voting, Cognitive Processes and Rational Choice

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It seems to me that the study of voting behavior has for many years been beset with grave difficulties, in part intrinsic to the ornery nature of the beast (as I will discuss below), but in part perhaps also because of a certain narrowness of conception of the range of relevant theoretical considerations. I therefore welcome the convening of the present conference and the opportunity for the infusion of new points of view and new initiatives in the field, given the wide latitude of topics broached in the Stimulus Memorandum.

A critic might say that the field of voting study suffers from an identity crisis: no single conceptual model seems to do justice to all the data, and it is therefore hard to know which broader body of knowledge can claim to subsume voting behavior as a special case. (If no such body of knowledge exists, that is the worst identity crisis of all). Let me state the crisis from my point of view as a frequent reader and teacher of the voting literature (albeit a somewhat infrequent contributor), and as a social psychologist who has been concerned with a parallel crisis in the field of attitude theory in general. I have also had a fair amount of experience in practical survey work on behalf of various local and national candidates, and this has usefully broadened my perspective.

The three major general orientations toward voting behavior might be called rationalistic, demographic, and symbolic. They all have one or another major failing. The rational models, as noted in the Stimulus Memorandum,

assume more information processing capability and activity than is realistic for the typical voter, and do not take account of the many cognitive biases on which compelling data are accumulating. The rationalist presumes that voters attend to information pertinent to their self-interests, but one of the most repeatable general findings about public opinion is that voters are generally inattentive. Perhaps this unrealism can be corrected by appealing to "issue publics" who only monitor the one or two issues that concern them, but "issue publics" largely represent question-begging by rationalists. If there is an abortion public, a gun control public, a pro-Israel public, etc., that's fine, but how did they arise? There seems to be no theory which predicts how many and what sort of publics there may be, especially on new issues. Is there an anti-Rhodesia public, for example? A related problem is that it is quite unclear how people might rationally attach themselves to many issues. What will the Panama Canal Treaty mean in a voter's daily life? Is school busing really bad for one's kids, or really good, and if one were an objective, responsible rational decision-maker, how on earth would one go about forming this judgment? Of course the rationalists recognize that differing value systems may enter the decision calculus (Downs was particularly explicit about this), but values are treated as inscrutable givens, rather than as variables capable of change and reinterpretation.

Demographic models assign the variance in voting behavior to the individual's group anchorages, with party identification playing a stabilizing and mediating role between group influences and political expression. There is a potential connection with the rationalistic models in that a group may collectively define and defend the group's interests, thus removing the cognitive burden from loyal individual members. So far so good. The agonizing thing about demographic models, however, is that the unexplained variance decides elections. It is

the defectors from expected attachments, and the floaters without attachments who govern the differences between one election outcome and the next. The variables governing these swings are not well understood, even by recourse to how well each candidate scores on stock issue and personality variables. The problem is that the relevant issue and personality variables causing voter changes themselves change. Who would have known in say, 1970, that trust in government would be a major issue in 1976? Or that the bundle of issues including campus dissent, racial unrest, moral permissiveness, etc. (The "Social Issue" of Scammon and Wattenberg) would have all but vanished six years later? Issues wax and wane, and so do the instant theories that accompany them. The open-endedness of the set of potentially relevant political stimuli is a vexing problem, especially for longitudinal data collection. In the Simulmatics Project of 1960, Ithiel Pool and I tried to define a set of issue clusters which would have constant applicability over the previous and the coming decade. In its broadest aspect, this attempt was an obvious failure. The "Catholic Issue", so central to Kennedy's campaign and to our later analyses, was almost entirely new (Al Smith being too far back to matter). Had we been compiling our data bank in 1955, absolutely no data on this "issue" would have existed. Altogether, I have seen no satisfactory attempt to define permanent issue and personality spaces.

The third general orientation toward voting behavior, the "symbolic" one, has had a checkered history. There was the early exhileration of case studies (e.g., Lasswell's) that seemed to show connections between Freudian psychodynamics and political orientations. Then general disillusionment set in with the failure of large-scale psychodynamically based objective measures such as the F-scale to show more than the most meagre non-artifactual correlational results for large samples. I think there was the feeling that while

psychodynamic factors might explain extreme behaviors in isolated cases, one could not find enough extreme enough patterns in a mass of individuals to yield statistically satisfying results. Racial prejudice was not to be explained in the mass as the projection of repressed sexual impulses. Even at a level less "deep" than Freudian psychodynamics, there were many failures to strongly relate abstract personal dispositions such as Anomie, Alienation, Locus of Control, etc. to concrete political stimuli that were hypothesized to engage those dispositions symbolically. The possibility of convincing empirical relationships wherein political issues function symbolically thus lay essentially dormant until the very recent and very striking work of David Sears, Donald Kinder, John McConahay, and their students.

In their work, "symbolic politics" is pitted against "self-interest politics" and symbolic politics wins every time. Sometimes this is not because symbolic politics is so powerful but because self-interests relate so poorly to political response, as in Kinder and Kieweit's bombshell demonstration that individual economic distress correlates zero with tendency to vote against the incumbent party. In other cases, though, as in the demonstrations that stance on school busing correlates substantially with racial prejudice but zero with whether or not one's own children are involved, the "symbolic" relationships are prominent. Despite the impressiveness of the data demonstrations, what these investigators mean by "symbolic" is not entirely clear. Certainly the symbols involved do not reach as deeply into the personality as in older work, evoking instead such concrete attitude orientations as racial prejudice. This is no doubt associated with the success of these demonstrations. But until we have some systematic theory of when something can become a symbol, and what kinds of personal orientations can be evoked by symbols (perhaps at the expense of "rational" self-interest), we will not understand these phenomena

in a unified way.

There is one other deficiency I see in the voting literature, transcending all present theoretical orientations. What the academic tends to ask in a public opinion survey is somewhat different from what the candidate's pollster tends to ask. There is a gap between theory and practice. When I consulted for Cambridge Survey Research last year on surveys for Jimmy Carter, I was acutely aware that knowing what questions were important to ask did not follow from any general theoretical framework, but rather from a sense of what considerations happened to be most salient to the public, or were likely to become so. For example, consider this question from a CSR survey in August, 1976:

> "Which of these statements is closer to your position:
>
> 1. Gerald Ford should be judged by all eight years of Republican Administration that he and Richard Nixon have run.
>
> 2. Gerald Ford should be judged only by the two years he himself has been President."

This question is unique to the particular circumstances of the election; it is not one which would be a standard inquiry for all candidates, such as whether they are seen as warm, competent, "interested in people like me", etc.

The occurrence of novel items in practical survey work is perhaps merely an illustration of the "open-endedness" problem alluded to earlier. I think the point bears repetition, however. Many "issues" of the Ford-Carter campaign were transient happenings (the Playboy interview, the East Europe remark, who "won" the debates, etc.) to be understood in the context of an emerging interpretative structure which the media and the electorate placed on the contest i.e., a "campaign". I do not see voting theory as presently coming to grips with "campaigns". I wrote a long memorandum for Jimmy Carter on the dangers to him of being perceived as "fuzzy on the issues", and how

he might correct this perception. My advice was guided only minimally by findings and theory from the voting literature. Yet advice (from many sources) on this "issue" to some extent modified Governor Carter's behavior and no doubt affected some votes.

Beyond the posing of a theoretical challenge (to which I return below), there are methodological lessons to be learned from practical cases. One of these is that we need much more event-responsive measurement and analysis. By this I mean the development of a body of theory about how to know that an event has had an impact. There are excellent examples in the literature of data series where the same question is asked repeatedly over months and years. I think especially of John Mueller's analyses of presidential popularity and attitudes toward war. With dense enough observations, it is possible to make good inferences about what drives these measurements up and down (e.g., Presidents get more popular in the short run by doing anything decisive; casualty figures make wars less popular, etc.). But there is a similar inferential problem in the course of a short-run campaign: what events lead to spurts or tumbles in candidates' fortunes? My experience suggests that practical pollsters have ways to look for these effects in data, ways which have not been inspected carefully by academic methodologists. I will suggest two such possibilities here, and I hope that the conference may consider others.

If (with appropriate controls) stand on an issue suddenly correlates more with candidate preference than it used to, a campaign event is responsible. An example from the last campaign (plausible but hypothetical -- I do not know of pertinent data) might be the following. Suppose that both before and after the second Carter-Ford debate, attitudes toward Soviet domination of Eastern Europe had been solicited. Imagine further that pro-Ford and anti-Soviet sentiments were moderately correlated before the debate,

and that after the debate, the correlation dropped to zero. The practical analyst, possibly armed with other converging items of evidence, possibly not, would take the correlation shift as presumptive evidence that Ford's thoughtless remarks about Eastern Europe had hurt him.

More subtle than the above "correlational shift" effect is a difference in correlation across groups rather than across time. The principle is this: If an issue correlates with candidate preference within group A but not within other groups, then that issue has a causal campaign influence within group A. A real example comes from the Playboy "issue" of the last campaign. Labeling the Playboy article a "mistake" correlated with opposition to Carter among women but not among men. The most striking difference between men and women was not in the percentage thinking the article a mistake, but in how well this opinion predicted voting intention. The practical analyst would take this to mean that for women, the issue was salient; they talked about it and worked it through. Pro-Carter women needed to rationalize it and say it didn't matter. Men didn't need to because it wasn't as consequential to them.

What is the epistemological status of such inferences? It is unclear. Both the "correlational shift" and "group correlation differential" effects are loosely reasoned. There is typically not enough information in the data to rule out alternative explanations. (Note that neither effect is based on panel data). Still, these effects appear compelling in practical terms. It would be very useful to articulate the logical basis for the practical claims, and to explore other intuitive inferential strategies employed by campaign advisers looking at survey data.

Let me return to my critique of the three theoretical orientations toward voting behavior. What other possibilities are there? How can the failings in these orientations be overcome? While aware of the vanity of supposing that if

yet another orientation were adopted, it would solve all previous problems while having no defects in turn, I nevertheless would like to propose exploration of a newer orientation.

Instead of trying to understand voting behavior as a peculiar phenomenon in its own right (peculiar though it may be), I find it helpful to try to establish continuities with other, broader psychological problem areas. One such continuity is suggested by the tension between the symbolic and rationalistic approaches. The symbolists assert that voters frequently respond not to the political stimulus as it is, but as something else which it stands for. The rationalists assert that voters typically respond to the stimulus as it is, or at least as it has implications for self-interest. The controversy revolves around how the voter takes the stimulus, and this is (among other things) a general problem in cognitive psychology, the problem of how people derive understandings from linguistic stimuli. My orientation is toward this "understanding" problem.

There is a major new trend in cognitive psychology to understand understanding as the selection and instantiation of structured knowledge chunks, variously called "frames", "schemata", "scripts", "episodes", etc. This trend has been accelerated, or perhaps even caused by major new developments in the field of artificial intelligence. There has been considerable progress in programming computers to "understand" natural language input dealing with ordinary social reality. Roger Schank and I recently completed a book called Scripts, plans, goals, and understanding, about some of this work here at Yale.

I expect to see the development of a "new look" in content analysis, vastly improving upon static elementaristic schemes by the search of text for larger and more dynamic episodes, in terms of which people really make sense of political stimuli. Schank already has students working on programs which scan

newspapers with political understanding. Meanwhile, I am trying to work out a theory of the ways in which political stimuli can be metaphorical. While I do not believe that voters are never rational (or as I prefer to say, "instrumental"), I think that in the more important and interesting cases they relate to the electoral process metaphorically: by "sending a message to Washington", by "voting their consciences", by recognizing that it's "time for a change" or that we need to "restore trust in government", and so on.

This is a memorandum of interest rather than a position paper, and I will not develop further the details of what I mean by metaphorical responding. But I should indicate how I see this point of view as contributing to the broad agenda of the conference. I think my emphasis on metaphors and other structures for political understanding such as "scripts" can sometimes lead to the development of new questions and question types on surveys, and I would be interested in some discussion along these lines. But that is not my main suggestion.

Throughout this memorandum, I have argued that the open-ended nature of the campaign process, with its potential for presenting novel issues to the voter, is a severe analytical problem. I have also argued that we need more event-responsive measurement. Now notice a glaring asymmetry in the data we have available to improve our understanding of open-ended campaign dynamics. We know a lot about public response to issue questions and candidate preference items we devise. But we know next to nothing systematic about the campaign stimuli available to voters. To my knowledge, there is no handy record of what slogans and appeals various state and national candidates have used in their TV, newspaper, radio, billboard and circularized ads; no record of what headline or TV news stories appeared about which candidates on what dates; no compendium of dated events with indirect implications for the claims and promises of the candidates. I propose the establishment of an archive of campaign stimuli.

I may be wrong in supposing that none presently exists, but I see no mention of it in the Stimulus Memorandum. In any case, it seems to me a vital methodological aid if we are ever to understand the relationship between political activity and voter behavior. The Stimulus Memorandum in fact seems implicitly to make this very same point (p.4): "... the conference will be concerned both with cognitive processes involved in decision making and with the specific inputs that the voter considers in attitude formation and voting decisions" (underlining mine).

There are a number of questions raised by the possibility of such an archive. Let me mention just two, hoping that these and others will prove appropriate agenda items at some point during the January conference. An immediate mundane question is whether the quality of the archival data could be better in monitoring current campaigns than in reconstructing past ones. I think the answer is yes, since campaign literature tends to get lost and forgotten afterwards, and TV stimuli are not as readily recovered as is newspaper coverage. A systematic "survey" of ongoing channels is a feasible operation (I once did it for a fluoridation referendum). An entirely retrospectively collected archive, furthermore, would not be such a unique data service, since an individual investigator can always go the library for old newspapers himself.

A more substantive question concerns how archival material might be compacted or summarized or quantified. Survey responses are reduced to convenient data matrices, but it does not seem that a campaign stimulus archive would be similarly reducible. Yet if there is no reduction, it will not be clear how to relate these stimuli to survey response changes. This reduction problem needs a lot of consideration, and in my view such consideration is of vast theoretical importance. The stimuli should be stored in terms of what they "mean". To work on that problem is to work on the fundamental information processing issue in voting behavior.