

Temporal Order and Causal Inference*

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Measures of change in pairs of attitudinal variables can provide important insights into the structure of the political belief systems of mass publics. Panel data reveal evidence of the greater centrality of some idea elements rather than others in the context of short-term dynamic constraint. Specification of the theoretically relevant voter attributes makes it possible to test for expected structural differences connecting policy related predispositions and policy preferences; specification also makes it possible to test propositions involving the reciprocal effects of attitudes and emerging vote preferences. Some of the more helpful specifications disclose the extent to which population heterogeneity produces a blurred image of relationships when analysis is based on the total electorate rather than limited to voters or subsets of voters specified by theoretical criteria.

**Editor's note:* It is with great sadness that I report that the following paper is Warren Miller's final journal article. The piece was accepted a few days before Warren died; I am pleased that he knew the piece had been accepted and I am told this brought him some pleasure at the end. When Warren submitted the piece we agreed that, if accepted, it would need editing. Unfortunately Warren was not able to edit the piece himself.

Ruth Jones, Warren's widow, kindly gave the journal authority to edit this piece in any way I desired. Chris Achen, in consultation with Larry Bartels, kindly undertook this task. Chris felt it would make more sense to incorporate portions of an earlier conference version (given at the 1998 Midwest Political Science Association Meetings) and to excise some portions of the submitted piece. I concurred in this decision. I would like to thank Chris and Larry for their efforts on this piece, and Ruth for her generosity in giving me authority to have the piece edited. It is a tribute to Warren that everyone who knew him knew that he would have wanted his memorial to be the best scholarship possible.

Warren will be most remembered for his contributions to the study of electoral politics. But he also made many contributions to political methodology. Since we all too often overlook contributions like Warren's, I asked Merrill Shanks, Warren's frequent coauthor, to write a brief tribute to Warren as a political methodologist. This journal owes a particular debt to Warren. Our sponsoring organization, the Society for Political Methodology, grew out of a small meeting in Ann Arbor; that meeting might not have taken place without some timely help from Warren.

Chris Achen has also provided some comments on the piece which show how Warren used methodology developed 40 years ago to make a contribution to current debates. We all too often equate political methodology with finding the latest hot method in econometrics, at the expense of losing touch with more fundamental developments that no longer appear to be exciting. Chris' commentary clearly shows the error in that approach.

I am pleased that Westview Press has agreed to endow the Miller Prize for the best article published in *Political Analysis*. This prize will be awarded at the Annual Meeting of the American Political Science Association and will go to the best article published in the most recently completed volume (so the prize awarded in September 2000 will go to the best article in Volume 8, and so on). There are now several Miller awards, the others all being awarded for the best work in some substantive area. But it is fitting that we also remember Warren as a methodologist.

I, and all of us who had the privilege of knowing Warren, will miss him.

Author's note: This article is based on data collected by the Center for Political Studies, University of Michigan and made available by the Center and the Inter-University Consortium for Political and Social Research. The

1 Introduction

THE TREATMENT OF causality in the 1960 version of *The American Voter* (Campbell et al. 1976) was, to say the least, uneven. On the one hand, a self-conscious preoccupation with the topic led to a chapter in the book and the metaphor, The Funnel of Causality.¹ At the same time, the thesis that each discernible phenomenological element probably had multiple causes—and so each cause would have its own set of multiple causes—led the authors to despair of assembling an explanatory structure that would trace the infinite sequence of causes and effects that produce a single vote decision.

In the sequel, *The New American Voter*, causality is again a central theme. The analysis proceeds by elaborating several possible and alternative interpretations of the “effects” of different variables on the vote decision. *The New American Voter* attempts to organize the many causes of voters’ decisions with an explicit model of causes and consequences. The model is provided by a multistage, bloc-recursive structure of explanations in which there is a primary attempt to guard against accepting spurious evidence of causal linkages.

This essay has been developed out of efforts to create “side evidence” relevant to assumptions about the temporal order of variables—cause to effect—employed in *The New American Voter*. The book is based almost entirely on cross-section survey data. This essay is based on panel data that connect successive time periods.

The initial phase of the research reported here focused on the role of party identification as an influence—or cause—with various policy-related predispositions and policy preferences as effects. A very simple algorithm for tabulating bivariate distributions of panel data produced provocative empirical regularities that are a somewhat nonobvious mixture of methodological artifact and substantial significance. To demonstrate the substantive significance of the findings, a series of analytic/theoretic perspectives was explored. The results seemed to authenticate the substantive interpretation that was sought; they also have implications for other, broader, questions about the structure of political attitudes and opinions held by voters in presidential elections. Although the essay begins and concludes with an emphasis on the temporal sequence and causal order of interrelated classes of concepts—reflected in research operations using cross-section data—attention is often drawn, in passing, to other analytic themes.

2 Determining Causality

2.1 Causality and the Analysis of Voting Behavior

In the literature on electoral behavior, one common version of the general problem of establishing causal sequence asks whether party identification “causes” policy preferences

socialization panel data collection was directed by M. Kent Jennings with the support of the Danforth Foundation, the Social Science Research Council, and the National Science Foundation. The 1972–1974–1976 panel data were collected under the direction of the author and with the support of the Ford Foundation, the John and Mary Markle Foundation, and the National Science Foundation. The data for 1980 and 1990–1992 were collected as part of the American National Election Study, funded by the National Science Foundation. Data processing for the manuscript was carried out by Scott Thompson. The manuscript was typed by Joy Erickson. The author benefited from discussions and counsel of colleagues John Geer, Ruth Jones, and Patrick Kenney.

¹The creation of the metaphor preceded the completion of the book manuscript. It was so much a part of our way of thinking and talking about the study of the voter’s decision that a parting gift from the members of the 1958 SSRC-sponsored seminar on electoral behavior was a stainless-steel hospital funnel. The funnel was inscribed, “A funnel has no empty cells.” The vintage of the occasion was inadvertently captured by the accompanying etching on the funnel of the 12 symbols of the 1-column code used with the 12-row/80-column IBM punch card. See Campbell et al. (1976, Chap. 2).

and performance evaluations or whether performance evaluations and policy preferences “cause” party identification. More generally, it would appear that party identification lies somewhere between being an exogenous element which influences many values, perceptions, beliefs, and attitudes, without being influenced by any of them, and, on the other hand, being just another short-term political attitude.

For the Miller/Shanks approach to the study of voting behavior, the problem is more ubiquitous than a singular interest in the theoretical causal status of party identification. Whether in the form of the rather unwieldy 10-stage, bloc-recursive model utilized in our analysis of the 1988 election (Shanks and Miller 1991) or the simplified 6-stage structure adapted for the analysis of 1992 (Miller and Shanks 1996), we attempt to align and combine a veritable host of discrete variables representing many different major concepts in a set of single bloc-recursive equations. In some instances substantive theory suggests a causal sequence for ordering the blocs of concepts—policy-relevant predispositions (values or valued beliefs) can, in the short run, be assumed to lead to references on policy questions of the day.² In other cases the temporal order of the evolution of the variables apparently resolves all such questions (voters’ education can scarcely be caused by expectations about future economic conditions). Yet other situations remain ambiguous: Is one’s party identification determined by prior policy preferences, or do partisans follow a perceived party line on questions of policy? This essay describes an analytic procedure for extracting empirical evidence that may constitute “side evidence” useful in resolving questions of this sort.

2.2 *The Logic of Causal Order*

The point of departure for this discussion, cutting into an extended literature on causal analysis, starts with a too little-cited monograph by James A. Davis (1985). Although Davis ultimately moves to review and renew aspects of the classic work by Alwin and Hauser (1975) on effects analysis, as well as Kendall and Lazarsfeld (1950) and Rosenberg (1968) on multivariate elaboration and Sewell Wright (1921) on path analysis, the single point to be highlighted is Davis’ emphasis on the fact that all of these approaches explicitly treat causality as a process that occurs over time. One way or another, any causal explanation of some dependent occurrence must include the fact that the cause of an effect preceded the effect.

Social scientists tend to think of causality in terms such as “Changes in *X* produce changes in *Y*.” Often, the question presumably answered by regression analysis is, How many units of change in *Y* are associated with, or are produced by, each unit of change in *X*? The very wording of the question assumes that the time–order relationship between *X* and *Y* has already been established. This essay is concerned with the prior need to establish the time-ordered sequence. We begin by noting that posing the question as it is conventionally posed obscures the possibility that changes in *Y* may, in some sense, be “caused” by *X* without any change in *X*. Davis (1985), in his updating of the classic discussions by Lazarsfeld and Rosenberg, makes the crucial point that it may be that observable change *only* in *Y* is the initial defining element indicating the occurrence of a causal process. He argues that two patterns of change in a bivariate relationship constitute two special cases of “the great principle of causal order: after cannot cause before . . . there is no way to change the past . . . one way arrows (indicating the direction of causality) flow with time.” Davis then

²See Zaller (1992, pp. 22–23) for a variant on the definition of predisposition. In general, we are in accord with Zaller, but we do not classify social and economic characteristics, often the antecedents of attitudinal predispositions, as predispositions. Instead, we identify various policy-related predispositions, such as attitudes toward egalitarianism, with relevant antecedents such as race and religion.

specifies the two complementary cases: “Variable X is a cause of Variable Y when—change in X (sooner or later) produces change in Y—or (because some X’s don’t change), Y’s tend to line up with fixed values of X.” Persuasion by political leaders results in followers “lining up” with their respective leaders. This pattern change might be represented by an issue such as affirmative action when partisan voters shift their preferences to match the positions previously established by party leaders. The basic point that covariation, signifying a causal process, may change if *either* the “independent” *or* the “dependent” variable changes is also forcefully argued by Ruy Teixeira (1984).

Either process of change will produce the familiar evidence of covariation linking cause and effect. Covariation, however, can also be the result of yet a third process in which some third factor, perhaps in existence prior to both X and Y, causes changes in the relationship between X and Y. The diagnostic trick when working with cross-section data is, of course, to establish which of the three processes lies behind any relationship observed at the single point in time. In this essay we are seeking ways of exploiting *panel* data to understand better relationships between and among variables that may be observed in static cross-section data.

2.3 Experiments, Cross-Sectional Data, and Panels

As we proceed, it is useful to keep in mind the difference between the causal interpretation of the results of an experiment and the analogous interpretation of a bivariate relationship produced from a cross-section survey. In the former, covariation between X and Y is *generated* by actually producing different values in a dependent variable, Y, by purposefully varying the subject’s experience with some independent variable, X. Two sets of measurements on each variable, “before” and “after,” may be made, or inferred, demonstrating that the particular change has been worked by a particular agent. However, in the case of the bivariate distributions extracted from *cross-sectional* data, one observes covariation (conventionally expressed as a correlation or a regression coefficient) only between presumably dependent values and independent values, exhibited after the fact, across an array of “subjects.” There is a single point in time (or a series of points in time for successive cross-sections) for measuring both variables simultaneously, with no direct evidence that one set of variations in one variable preceded and thereby produced or caused the second.

Panel data, in some contrast, may be considered quasi-experimental, despite lack of control over the introduction of experimental variables, inasmuch as each “independent” and “dependent” variable can be measured for individual change between two points in time, “before” and “after.” The relationship between them can be measured both times, but, even more, one can note whether change in one variable is congruent with the non-changing value of another.

With data from a two-wave panel it is possible to identify two of the three processes that result in a causal relationship between independent and dependent variables: (1) both variables may be observed to change in a congruent or compatible manner (presumably the effect of a common cause); or (2) as Davis notes, observable change in one variable results in it “lining up with” or “becoming more congruent with” the other. Three-wave panel data are needed in order to observe the third more conventional case in which a change in X subsequently produces compatible or congruent changes in Y. In lieu of a controlled experiment, the three-wave panel is needed because T_1 and T_2 are necessary in order to measure and observe the changes in X, while T_2 and T_3 are needed to observe consequent conforming changes in Y, which may then be interpreted as variations in Y having been caused by changes in X.

Without resorting at least to nonexperimental panel data, it is not possible to observe the temporal sequence that lies at the heart of causal analysis. The best one can do with

data from a single cross section in time is establish configurations that are consistent with the *presumption* of a causal sequence, without evidence that changes actually occurred in the posited temporal order. Side evidence of the greater stability of one variable over another often leads to a presumption that the more stable is the more likely cause, if, indeed, the two are linked causally. And where “strong theory” undergirds an argument, it may provide credibility for the assumption of a unidirectional causal sequence. Successive cross-section analyses may also reveal a succession of changing configurations over time that are consistent with the notion that one variable does become more congruent with another (Miller and Shanks 1982). But with only cross-section data, the assumption of causal sequence cannot be given a crucial independent test to reassure the skeptic. This is why post hoc correlations between two variables are not, by themselves, taken to be sufficient evidence of causality or a causal relationship.

In the midst of his discussion of the rules governing the logic of causal order, Davis (1985) observes that “the rules have nothing to do with statistics. Although the methodological journals teem with crackpot proposals for determining causal order using only the numbers in a data set, most methodologists agree that causal order is a substantive or empirical problem to be solved by our knowledge about how the real world works, not by statistical gyrations.” While I share Davis’ concern for emphasizing the temporal logic of causal processes, I also believe that some very simple ratio statistics based on temporal sequence *can* provide the basis for clarifying the way in which the real world works. The temporal sequence of changing relationships among variables can be captured and made explicit in panel data with the tabulation of sequentially changing values of the interrelated variables. To produce such tabulations, we must stop short of summarizing bivariate distributions as correlation coefficients and retain the detailed presentation of distributions that underlie such coefficients.

2.4 A Tabular Representation of Causal Order

To do this, we shall go beyond Davis’ use of cross-section data to represent (as did Kendall, Lazarsfeld, and Rosenberg in their pioneering work) the logic of elaboration with multivariate tabulations of survey data. Given panel data, in which both variables have been measured at no fewer than two points in time, we can examine bivariate distributions of patterns of *stability and change* in each of two variables. We can identify cases of the one generic pattern in which one variable does not change between T_1 and T_2 , while a second variable does change, *and* changes to better accord with, conform to, or “line up” with the unchanging value of the first. It may be that the second variable is in fact adjusting to yet a third variable, Z , which has an established relationship with X . Nevertheless, when the pattern of an increase in the congruence of two variables exists we will take it as evidence consistent with, or plausibly supporting, an interpretation that the first variable may have in some manner *caused* a change in the second.³ However, as with all assignments

³It should go without saying that all of this makes sense only if there is some underlying directional continuum on which both variables can share meaning, and if there is a clear direction for a potential effect of X on Y (or vice versa). If, in general, both before and after change in X , Republicans are no more likely to favor $Y+$ than are Democrats, and Democrats are no more likely to favor $Y-$ than are Republicans, changes from $Y+$ to $Y-$ are unlikely to provide a causal interpretation of partisanship in our limited scheme of things. Patterns of change between two points in time must be interpretable as providing a better fit, greater congruity, or more agreement between two variables that are reflections of quite different qualities or attributes if the patterns are to be interpreted as evidence of a causal relationship, and not simply an example of a tautology or of reliability in the operational measurement of a single concept. This is the point at which research operations and substantive theory define the variables to be examined and propose reasons for expecting one or another dominant-causal arrow.

of “meaning” to a configuration of data, “proof” of a causal interaction or relationship lies in the match between substantive theory and a methodological interpretation of the data manipulations.

Before considering the classic case of “Change in X produces change in Y ,” let us continue with the less familiar case of the two-wave panel and produce a concrete illustration of our tabulating procedures.⁴ First, let us designate Party Identification as A , and Ideological Self-Placement, perhaps a surrogate for general policy preferences, as B . The choice of these two variables is not accidental. The professional student of voting behavior research will recognize them as two basic attitudes or predispositions important to the interpretation of many elections. We have, however, been uncertain about their causal relationship with each other. Following the 1984 election, Shanks and Miller gave precedence to ideology as a partial cause of party identification because, between 1980 and 1984, the marginal distribution of ideology was apparently the more stable of the two. In 1988 the order was reversed (Shanks and Miller 1990, 1991), and in 1992 we concluded that we really did not know enough to classify one as cause and the other as effect. We have been more than incidentally curious about the possibility of finding a means of clarifying their interrelationship.

In the illustrations which follow, we, for didactic purposes, and to minimize the intrusion of measurement error, differentiate only three categories of each variable. For party identification there are Democrats, Independents, and Republicans; at the same time, ideological designations include Liberals, Moderates, and Conservatives. We begin by defining the possible patterns of stability and change in Party Identification by the nine-celled turnover table generated between T_1 and T_2 . This is generically presented in Fig. 1, where $D = +$, $I = 0$, and $R = -$. We next define the possible patterns of change in ideological location by a similar, substantively congruent, turnover table in which $L = +$, $M = 0$, and $C = -$. With these conventions established, distributions of stability and change for either of the two measures, party identification and ideological policy preference, can be represented as in Fig. 1.

We next assume the essential comparability of the elements within the triads designating 1, 2, and 3 as instances of change in a Pro-Democratic direction, 4, 5, and 6 as three instances of the absence of change in partisanship and, 7, 8, and 9 as three instances of change in a Pro-Republican direction. Similarly, we accept (1) three patterns of ideological preference depicting change in a Pro-Liberal direction, (2) another three depicting no change in ideological position, and (3) three patterns of change in a Pro-Conservative direction. We can next generate the 81 cells of a bivariate distribution of changes in the two variables. They are implicit in Fig. 2. Given the assumed comparability of the elements depicting

		T_2		
		+	0	-
T_1	+	4 (+, +)	7 (+, 0)	9 (+, -)
	0	2 (0, +)	5 (0, 0)	8 (0, -)
	-	1 (-, +)	3 (-, 0)	6 (-, -)

Fig. 1 Transformation of a T_1 - T_2 turnover table to a nine-category distribution of a single variable at two points in time.

⁴Although orthodox references hold X to be the cause and Y the consequence, in our attempt to gain new evidence concerning causal direction, patterns of change must be operationally defined so that the imputed direction of a causal arrow is equally free to flow from Y to X or from X to Y . To avoid the traditional roles for “ X ” and “ Y ,” we use A and B throughout our discussion, with no a priori assumptions concerning the dominant direction of causality.

Variable B (Ideological Self-Classification) T_1-T_2

Variable A Party Identification T_1-T_2	C/L	M/L	C/M	L	M	C	L/M	M/C	L/C
R/D -,+	-,+	0,+	-,0	+,+	+, 0,0	-,-	+,0	0,-	+, -
I/D 0,+	Congruent Change (to +) (7)			B+ Dominates A (4)	Dissonant Change (9)				
R/I -,0				B ⁰ Dominates A (5)					
D/D +,+	A+ Dominates B (1)			No Change (10)					
I/I 0,0	A ⁰ Dominates B (2)								
R/R -,-				B ⁰ Dominates A (5)					
D/I +,0	Dissonant Change (9)			B- Dominates A (6)		Congruent Change (to -) (8)			
I/R 0,-									
D/R +,-									

Fig. 2 Illustrative analytic scheme for identifying the most plausible patterns of causal influence involving reciprocal causality by two variables.

change, or absence of change, in each variable (partisanship and ideology), the 81 cells can be organized and simplified, in summary, under 10 headings.⁵

For example, cases designated (1) in Fig. 2 exhibit changes in ideological preferences resulting in greater conformity with (stable) Democratic preferences. *A* is + (Democratic) at both T_1 and T_2 . *B* changes from - to + (C to L), - to 0 (C to M), or 0 to + (M to L). These three examples of change in ideology creating greater agreement with party identification encourage the conclusion that (stable) party is the cause and changed ideological position is the effect.

Cases in Fig. 2 designated (2) and (3) also show ideological preferences moving into conformity with party identification, but now for Independent and Republican identifiers, respectively.

The complementary instances in which party changes to better fit with stable, unchanging ideology are labeled (4), (5), and (6), corresponding to movement toward liberal, moderate, and conservative ideological preferences, respectively.

In cases labeled (7) or (8), no causal interpretation of the relationship between party and ideology is plausible, as both party and ideology change in the same "liberal" or "conservative" direction between T_1 and T_2 , perhaps in response to change in some third factor or set of factors. Finally, cases labeled (9) show increased divergence or discrepancy between party preference and ideological position, and those labeled (10) underwent no changes at all. Both categories make short-term causal interrelation impossible.

Our categorization scheme involves a number of crude simplifications, and the initial trichotomization ignores information concerning *magnitudes* of change, but none of this seems to do violence to the basic logic governing an interpretation of the *direction* of causality in the relationship between *A* and *B*.⁶ Now, as far as we know, the novelty of this tabular categorization of two variable patterns of stability and changes precludes any reference to a validating literature.

The destruction of cases among the first six categories can simply be taken to indicate the proportion of cases fitting one or the other of two simple structures involving unidirectional causality. Categories 1, 2, and 3 in Fig. 2 suggest that unchanging variable *A* (Party Identification) is associated with, or has "caused," changes in variable *B* (Ideological Self-Placement). The extent to which categories 4-6 occur is the extent to which the order *A*-before-*B* is reversed. The relative preponderance of 1-3 and 4-6 is, therefore, a measure of the extent to which one or the other causal sequence is "dominant" within the population for the time period being examined. There is nothing in the operational definition of the causal patterns that gives either *A* or *B* an a priori advantage as the dominant causal agent. Both patterns are notable for their explicit contrast with the normal presumption that one

⁵Equating the three cases in each set of changes for each variable clearly discards information pertaining to the magnitude of the changes. However, this seems to be consistent with the recent argument by Rabinowitz and MacDonald (1989) favoring directional rather than positional emphasis in models of voter choices based on the perceived relative priority of candidate placements.

⁶The decomposition of patterns of attitudinal stability and change into 10 categories also invites an interesting exploration of the causal interpretations given to T_1 - T_2 changes in the correlation between two variables. The exploration can be introduced by collapsing the 10 categories into 4: (1) changes interpretable as the consequence of causal interaction (1-6), where the correlation between variables increases sharply; (2) changes where both variables move in the same direction (7 and 8), with no substantial change in correlations; (3) changes which lead to divergence (9), which produces a sharp decline or reversal of correlations between T_1 and T_2 ; and (4) absence of change in the correlation (10). For the time interval under analysis, only the changes in the first category (1-6) support a causal interpretation of T_1 - T_2 changes in overall correlations of *A* and *B*, yet each of the four categories has an impact on the overall relationships between the two variables at the two points in time.

variable causes change in another *only* when a documented change in the first is followed by an appropriate observable change in the second.

2.5 *Changing Contexts: The Role of the Exogenous Stimulus*

The presumption that the more stable of two variables united in a causal relationship may actually be the independent “cause of change” in the dependent variable leads to a further elaboration on commonplace ways of thinking about causality. The logic of experimentation, which underlies much of our thinking, usually gives explicit recognition to only two variables in producing an effect: a *dependent* variable which changes as the *independent* variable is manipulated (introduced into the experiment or changed in value). If, however, the presumed independent variable does not change but the dependent variable does—coming into greater conformity with or coming into line with the independent variable, the existence of a *third* contributor to the causal relationship becomes apparent as a logically necessary condition for the production of an “effect.” Something (analogous to the experimenter creating the change in *X*) must have happened to change the dependent variable, perhaps by the activation of *X* or an accentuation of the relevance of the unchanging causal variable *X* for the consequent changed dependent variable *Y*. There is a clear sense in which a third element must be the precipitating prior proximate cause. In the experiment it appears in the form of the process of the experiment. In the present context it may take the form of an aspect of a campaign or a change in electoral circumstance or, perhaps, the sheer passage of time that may occur as citizens reconcile internal conflicts or inconsistencies in their attitudes and perceptions. (This “third element” is not to be confused with the “third factor,” which may be a common antecedent that accounts for the correlation between *X* and *Y*.)

Unfortunately for our analytic model—or, for that matter, most models of voting behavior—we usually have no systematic measure of any of the elements (other than the passage of time) in such a third set. Over the past 40 years the content of election studies has expanded our coverage of all manner of “independent” and “intervening” variables, but we have seldom gone outside the psyche of the citizen to measure the actual instruments of change, the intervention of events and circumstance in the world external to the voter. Fortunately, increasing awareness of the problem has led many scholars to embellish collections of survey data with the addition of contextual data (Bartels 1988). Nevertheless, even the most elaborate “contextual data” usually fall short of systematically measuring the magnitude, intensity, duration, or relevance of contextual interventions.

The failure to assess external or exogenous causes through explicit measurement imposes a possibly severe constraint on our assessments of the causal importance of differing blocs of variables. At the very least it leaves open the possibility that differences that seem to distinguish one election from another in relationships involving vote choice may *not* be obviously due to changes or differences in the internal, intrapsychic factors employed in our analyses, variables that we have designated (and analytically ordered) as more or less independent and dependent. For example, voters’ preferences for particular policies may have been essentially constant between elections, yet produce differences between elections because of differences in the relevance of the policy preferences for the different vote choices. The differences may be due to the activation of those so-called “independent” policy preferences, not to changes in the preferences. As another example, the relevance of one’s party identification for one’s ideological preference may depend on the extent to which the candidates’ campaigns promote or emphasize the connection between party and ideology. If so, whichever of the two variables change, it is the candidates’ behavior that causes the change from one setting to another. In this case we must ultimately find some

way to introduce variations of candidate behavior into systematic analysis if we are to understand the linkages between identification and ideology.

Since the changes we are concerned with at this point all result in increased congruence or conformity with an underlying common theme, it is also quite possible, as we have suggested, that the third element may simply be *time*. The increase in “agreement” or “congruence” or “conformity” implies some resolution of conflict or reduction of tension which may involve no more than the mental process of psychological dissonance reduction. A felt disagreement between one’s sense of ideological location and a policy preference *may be* resolved by changing one or the other. The process of such conflict resolution may be initiated by a proximate external event, but it need not be. Nevertheless, recognition of such basic patterns of relationships involved in disentangling the causes of electoral behavior must certainly lead eventually to changes in the design of future studies and attention to problems of measurement in future data collections.

3 Case Studies

3.1 *Party Identification and Ideology in 1990–1991 and 1991–1992*

For the remainder of this discussion, the problem posed by the lack of information about the crucial element that induces or produces measurable changes across a two-wave panel is set aside. We simply accept the fact of change in one or the other variable without inquiring into the proximate *external* cause. We concentrate on the time-ordered sequence of changes in the two variables. We first turn directly to panel data made as comparable as possible by employing operationally identical measures of two variables, in the first instance Party Identification and Ideological Self-Placement. The first display is taken from the recent American NES panel bridging the off-year election of 1990, the Gulf War year 1991, and the presidential election year of 1992. In Table 1, the second column uses 1990 and 1991 as T_1 and T_2 , respectively; in the third column, T_1 is 1991 and T_2 is 1992.

According to our scheme for categorizing the many cells in our joint turnover table for two variables at two points in time (Fig. 2), the modal category in both time intervals was “No Change” in either variable. However, the receding impact of the off-year election of 1990 and the energizing consequence of an approaching presidential election may have been reflected in the small decrease in the size of the “No Change” group in the second time interval (from 53 to 49%). The same disciplining effect of an approaching national election may also be reflected in the decreased incidence of dissonant change in the same time interval (from 25 to 22%). These both produced more congruence of party and ideology with the approach of the presidential election.

There is also a hint of a very limited visible role for “third factors” that might be responsible for congruent change in both variables or that might be responsible for much more important influences that cannot be disentangled within the time intervals captured by the panel. The number of cases of apparent joint change in the same direction does not quite reach 4% of the panel between 1990 and 1991 and just hits 6% between 1991 and 1992.

Overall, as a consequence of three patterns (no change, dissonant change, and simultaneous congruent change) that elude interpretation of a causal interaction *between* party and ideology, the remaining numbers available for causal interpretation of such a relationship between the two variables are some 18% of the total in the first interval and 24% in the second. Although we may have certain quite specific expectations about which pattern of causal direction will predominate when only two variables are involved (PI to Ideology or Ideology to PI), there is little *a priori* basis for expecting or assuming much of anything

Table 1 Plausible patterns of causal influence between Party Identification and Ideological Self-Placement^a

	1990–1991	1991–1992
	(%)	(%)
Plausible causes		
1. Democratic PI	4.3	7.7
2. Independent PI	4.3	2.7
3. Republican PI	3.7	3.7
Subtotal	12.3	14.1
4. Liberal ideology	.7	3.4
5. Moderate ideology	2.6	2.0
6. Conservative ideology	3.0	4.3
Subtotal	6.3	9.7
Other patterns		
7. No causal interaction; both moved left	1.0	3.2
8. No causal interaction; both moved right	2.6	2.8
9. No causal interaction; divergence	24.8	21.5
10. No causal interaction; no change	53.3	48.8
Subtotal	81.7	76.3
Total	100.0	100.0
(N)	(705)	(562)

^aData are from the NES Panel, 1990–1991–1992, and were made available by the Inter-University Consortium for Political and Social Research. The numbers designating the 10 categories correspond to those assigned to the constructed cells in Fig. 2.

Note: Columns do not add exactly to 100% due to rounding error.

else. In particular, we have little basis for any prediction about the relative incidence of the various patterns we have just identified.

Whether change that fits plausible patterns of causality involving these two generally stable predispositions should hold for 5, 10, 20%, or more, of a 1-year panel, either just after or just before an election, is simply unknown. Are the figures of 18 and 24% for two 1-year intervals high, low, or average? We know little about the likely *pace* of change that may occur or about the conditions that would accelerate or dampen change in the role of either variable. How will the pace of change differ for different variables? What does the approach of a presidential election do to variable relationships formed 1, 2, or 4 years prior? What time interval must elapse under what conditions for appreciable change to occur? Do intervals of a few months, 1 year, or 2 years, or 4 years in any way provide comparable settings for change to occur and be studied?⁷ We are only beginning to understand long-term political change in American politics; we have a comparably meager understanding of short-term change.

⁷Our discipline's lack of precision in understanding conditions promoting change or affecting rates of change plagues the interpretation of public opinion data during a campaign. How much change in horse-race standings should be produced by what events? And, by inference, how much are outliers likely consequences of data collection methods? See Campbell and Garand (2000).

Nevertheless, if our reasoning thus far is sound, we may be able to use our somewhat novel view of bivariate covariation over time to learn about what may have occurred. Establishing the possible predominance of one causal pattern over another is only one product—and conceivably the lesser—of the line of inquiry that is opened up by our tabular description of stability and change among pairs of variables.

3.2 *Party Identification and Ideological Self-Placement: Cause or Effect?*

Now, to examine evidence relevant to our initiating curiosity about the relationship between party and ideology, which pattern of presumed causal influence did predominate between 1990 and 1991 or between 1991 and 1992? In the first time interval, the data in Table 1 support the argument that the causal path more often ran from partisanship to ideology than from ideology to party identification by a substantial ratio of 12.3 to 6.3 (66 to 34). There were almost twice as many cases in which party identification did not change between 1990 and 1991, while changes in ideology produced greater conformity with party as there were cases in the reverse (in which stable ideology was followed by conforming party identification). In the second interval preceding the 1992 election, the predominant direction of causal influence again ran from partisanship as cause to ideology as consequence, but by a somewhat smaller margin of 14.1 to 9.7 (59 to 41). Throughout the 2-year span from 1990 to 1992, the dominant sequence of changes had party identification as primary cause and ideology as effect.

But there is clearly much more to be said. In the first place, the margins of 2 to 1 or 3 to 2 mean that there were large minorities for whom the dominant order in each analytic sequence was “wrong.” In both intervals, substantial numbers of people appeared to change their sense of party allegiance to fit their unchanging ideological positions. This is clear evidence that whichever analytic ordering we might choose it would be inappropriate for some people at least some of the time. This underlies the crucial recognition that any optimal ordering will, as with virtually all models, only be the best “average” fit for a heterogeneous population.

In any case, however, the substantive direction of the net changes seems to fit the political history of the period. From 1990 to 1991 the overall movement was from left to right (6% moving right, as opposed to 5% moving right to left), largely because ideological Conservatives continued to move to the Republican party (Shanks and Miller 1991). But between 1991 and 1992 the direction of movement changed. Possibly as a prelude to a Democratic surge at the polls, movement to the Democratic left outnumbered movements to the Republican right by a margin of 11 to 8 (11.1 to 8.0).

The direction of change among persons for whom both variables changed in congruent directions follows the same pattern: to the Republican right between 1990 and 1991 and to the Democratic left—by a small margin—between 1991 and 1992. All told, by a margin of 3 to 2 (60 to 40), about a fourth of the panel population (22.2% in the first interval, 29.8% in the second) *systematically* changed either their party identification or their location on the ideological continuum (or both) in the 2-year interval.

To extend our inquiry about causal relationships between pairs of variables, there is a variety of two-wave panels that can be explored with confidence that all of the variables have been created by precisely the same measurement operations. The most elaborate, two 6-month panels, come from the intrayear NES panel, January–June–October 1980. The CPS panel of 1972–1974–1976 provides two familiar 2-year comparisons. Given the NES design for off-year studies, there are many panels available from the recent decade. However, some of the most interesting data come from the older Jennings long-term study of political

socialization and attitude change (Jennings and Niemi 1981). The long-term panel from the older Jennings study is made more interesting in theoretical terms because the contrasts in empirical results from analyses of both a parental and a filial panel are so dramatic.

Given the novel nature of our tabular paradigm, henceforth we proceed simultaneously on two fronts: we look for evidence of predominant causality in the relationship between members of various pairs of variables, and at the same time, we explore circumstances that can test the validity of our interpretations. For example, the long-term panel from a portion of the Jennings study provides several configurations that seem most provocative on both fronts.

3.3 *Generational Differences: The Jennings Panel*

The time interval of particular interest to us in the Jennings panel includes a period of rapid changes in American national politics. The first time point for our analysis is 1973, at which point a national sample of high-school seniors from 1965 was reinterviewed, as were their parents. The second time point for our use of these two panels was 1982—almost a decade later.⁸ Among the offspring—for whom ages changed from 25–26 to 34–35 between 1973 and 1982—there was near-parity between the two directional patterns of presumed influence involving party identification and liberal/conservative ideology between 1973 (T_1) and 1982 (T_2). Across the 9 years, some 10% of the younger cohort demonstrated stable partisanship with an increase in ideological conformity with party; in reverse fashion, ideology “pre-vailed” and produced conforming changes in partisanship among 9% of the cohort. In this young, relatively well-educated cohort, whose members could have participated in no more than the two presidential elections of 1968 and 1972 before T_1 (1973), party identification was scarcely more stable—or causally dominant—than the less common phenomenon of ideological self-categorization.

The parental panel, in contrast, had observed—if they had not actually voted in—at least seven presidential elections, often extending back to pre-World War II and the New Deal. The parents’ ages ranged roughly from the late 40s to early 50s in 1973 (T_1), and from the 50s into the 60s in 1982 (T_2). For this relatively mature set of adults, the causal arrow appeared to run from party (as cause) to ideology (as effect) in three of every four cases (16 to 5%). The period of political turbulence during the first 15 years of adulthood for the offspring generation produced responses from them that were quite different from the responses of their parents, who had experienced the relative political successes of the New Deal, World War II, and postwar prosperity. The parental pattern exhibited the dominance of party identification formed largely under FDR and the New Deal. The contrast with the near-parity of party with Liberal-Conservative ideology among their children is a piece of direct evidence suggesting that party affiliation may indeed stabilize and strengthen with age and, thereby, increase its centrality to the processes that shape other political attitudes and perceptions.

Our technique for tracing temporal sequences of change in search of presumptive evidence of the direction of causality may have the important consequence of providing tests such as we have just observed for some of the theoretical properties of the variables under investigation. Such tests may disclose theoretically relevant variations in the stability and centrality of any given variable involved in causal processes of the formation of attitudes, values, and beliefs. To the extent that the results of purposive tests are not blurred

⁸Unfortunately, the first interval of the study cannot be used for our purposes because of the absence of comparable measures of party identification and ideology in both 1965 and 1973. See Jennings and Niemi (1981).

by measurement error and idiosyncratic change, it appears that we will also be repeatedly reminded that the heterogeneity of any "natural" population may defy the discovery of any single, uniform causal process that is shared among all members. At the same time that we learn about the extent to which an ordering of variables is manifestly inappropriate for some members of the population being studied, we may learn more about both the variables and the people being examined.

Of direct relevance to electoral analysis, parent and offspring cohorts in the Jennings study also reflected contrasting patterns in the relationships between party identifications and specific policy preferences. In 1973 and again in 1982, members of both panels were asked for their policy preferences in two very different issue domains. The first called for approval or disapproval of an activist role for the federal government in domestic affairs, assuring the availability of jobs and the maintenance of a "good standard of living." Among the parents, the presumptive causal interpretation of the connection between these policy preferences and party identification favored party as cause by a ratio of more than 2 to 1 (16 to 7%). Within the cohort of offspring, party was dominant but the ratio was only 12 to 10.

More dramatic differences appeared on the question of whether the United States should have intervened in Viet Nam. Within the parental generation, party again dominated as cause, although by the lower ratio of 16 to 10. However, in the offspring cohort the dominant causal direction was reversed; it ran the other way, from attitudes on Viet Nam as "cause" to changed party identification as "effect," by a solid ratio of 13 to 8. This is more than suggestive evidence that while party loyalties are still crystallizing in young adults, policy preferences that are central to contemporary political debate can play an important role in shaping those loyalties. Interestingly enough, although anti-Viet Nam sentiments were much more often the presumed cause in both generations (rather than prointervention sentiments), the anti-Viet Nam/pro-Viet Nam ratio for the offspring was only 9 to 4, compared to 9 to 1 among their parents. At this point we cannot tell whether the sentiments among the older generation were simply reflecting old "liberal Democratic values" that persisted as new partisan loyalties were being shaped among their children.

Nevertheless, other generational differences that have subsequently been well documented by many other sources were apparent in Jennings' 1982 data. The leading cohort of the post-New Deal generation—the Baby Boomers of the late 1980s and early 1990s—came into their majority as potential voters no more evenly balanced as Democratic and Republican partisans (72 Democrat to 28 Republican) than were their New Deal parents (71 Democrat to 29 Republican). They were, however, much more "conservative" than were their elders. Self-declarations of ideological preference split 49 Conservative, 51 Liberal among the offspring, in contrast to a split of 23 Conservative, 77 Liberal among the parents. These generational differences paralleled the differences in attitudes toward U.S. intervention in Viet Nam. (All of this, of course, among the 75% of families whose children had not dropped out of school prior to their senior year, and at a time before the Reagan-era changes in public partisanship eroded the New Deal advantage given to the Democrats.)

It should be noted that these harbingers of the future were apparent only among the roughly one-in-five of each generation who displayed the patterns of change to which we are attributing causal meaning. Both in the Jennings study and across several other studies that we touch upon briefly in the remainder of this paper, it appears that another 40 to 50% of any given population of citizens eligible to vote show no change in either member of any given pair of variables during intervals of a year or more, either before or after an election. The remainder, often a sizable fraction, is then divided between a handful in which both members of a variable pair change in unison (perhaps reflecting a common third factor) and a much larger set in which attitudes reflected in the pair of variables diverge from each

other and thereby defy any easy causal interpretation. In short, systematic or interpretable patterns of change often seem confined to the cases identified by variables with converging values.

3.4 *A Caveat on Measurement Error*

Before proceeding further in the study of causal relationships between the members of variable pairs, we should note again how little we know about the statistical properties of the analytic procedures we are using. In particular, we know very little about the possible confounding that is the result of measurement error or of random change. Since we generally assume that measurement error decreases the appearance of reliability of measurement [see Green and Palmquist (1990) for an application to the measurement of party identification], it may follow—as we suggested earlier—that an analytic scheme which equates dominance in causal relationships with the greater measured stability of an individual variable is vulnerable to charges of limited validity due to the intrusion of measurement error. The more error, the greater the likelihood of the incidence of “conforming” change; the less error, the greater the likelihood of the appearance of dominating stability. This possibility cannot be ignored, but there is a variety of approaches to a systematic assessment of the seriousness of the problem.

In the course of exploring possible substantive interpretations that might be addressed, we have taken advantage of the opportunity to observe the conventionally measured stability or reliability of one given variable in relation to the measured reliability of other variables with which it has been joined in the search for plausible causal relationships. For example, in the analyses of the 1990–1991–1992 NES panel we examined some 50 variable pairs. In 18 cases, the higher test–retest, T_1 – T_2 , reliability of one variable (compared to the other) was indeed associated with an apparently dominant causal role for the more stable variable. Or, to put it the other way, in those 18 cases a lower T_1 – T_2 intraitem autocorrelation for a variable was associated with the interpretation of the variable more often being “caused” by the other more stable variable. In 32 cases, however, the ratio of presumed cause to effect was contradicted by the low reliability/high reliability ratio. In other words, in 32 of 50 cases one could not attribute the greater incidence of causal direction simply to a greater reliability of measurement of the apparently dominant variable because, in fact, the dominant variable was not more stable, or more reliably measured.

Since 18 of the 50 pairs (a different 18 than above) involved party identification, which is, by a large margin, the most reliable of all political variables, it is of interest to examine them separately. In 11 of the 18 cases involving party identification, party identification did appear to be the more reliably measured, as well as the dominant “cause;” in the other 7 pairs, party was also measured as the more reliable variable, but it was not to be interpreted as the dominant causal variable. Given the figures for all 50 pairs, it then follows in the remaining 32 pairs not involving party identification that only 7 cases were based on greater univariate reliability being associated with causal dominance; in 25 cases the variable that dominated as the putative cause exhibited *less* reliability (a lower T_1 – T_2 correlation) than did the presumed “effect.”

Other less direct tests seem to discount further the likelihood that interpretations of dominant causal direction are in fact simply the consequence of random change or measurement error. Nevertheless, there are circumstances in which a likely intrusion of the measurement problem is not easily dismissed. One such set of circumstances involves the relationship between age and party identification. As we have noted, theory would have it that party identification becomes stronger and more influential with increasing age. And sure enough, in the Jennings panels of parents and offspring, in the 1980 intrayear panel, and

in the 1972–1974–1976 long-term panel, party identification is consistently interpretable as “cause,” much more often among older people than among younger. However, just as consistently, party identification exhibits greater stability (or measured reliability) among older than among younger people. Lacking greater precision in our assessment of the incidence of measurement error or the age-related centrality of party identification, the conclusion that party identification is more often a causal agent among the old than among the young remains at least vulnerable to criticism when the conclusion is based on our two-variable directional flow causal analysis.

3.5 Participation, Education, and the Causal Role of Political Predisposition

On the other hand, some other results of the early explorations of our tabular algorithm seem less vulnerable to criticism based on the presumption of measurement error. In examining the differences in causal patterns for voters and nonvoters, for example, the autocorrelations for virtually all of the relevant variables from the 1990–1991–1992 panel are essentially *the same* for both voters and nonvoters. Nevertheless, in the early 1990s panel, in the 1980 panels, and again in the panels from 1972–1974–1976 there are persistent differences between voters and nonvoters supporting different causal interpretations within each of the two groups of citizens. When testing for causal relationships involving party identification, party is almost invariably featured as “cause,” with policy preferences as “effects” *among voters*. In contrast, among nonvoters between 1972 and 1974, attitudes toward civil rights,

Table 2 Causal dominance within pairs of variables, party identification and policy preferences, 1972–1974 and 1974–1976, by voter participation and education^a

“Other” variables	Nonvoters		Voters with less than college education ^a		Voters with at least some college	
	PI dominant	Other dominant	PI dominant	Other dominant	PI dominant	Other dominant
Ideology						
1972–1974	3	2	11	3	8	7
1974–1976	4	2	11	2	7	5
Aid to minorities						
1972–1974	8	8	15	5	9	6
1974–1976	4	4	18	4	10	6
Rights of accused						
1972–1974	5	6	13	3	8	2
1974–1976	7	7	14	4	13	5
Role for women						
1972–1974	7	10	13	6	9	8
1974–1976	7	4	11	6	8	5
Jobs & standards of living						
1972–1974	10	3	11	5	9	5
1974–1976	9	5	15	4	13	6
Presidential pardon						
1974–1976	5	14	8	10	7	10

^aEntries are proportions of the entire panel population reflecting plausible causal relationships. Number of cases varies, maximum is 1270.

the role of women in society, and the rights of the accused all dominated party identification in the two-variable directional analyses of change, and by margins of 6 to 5, 10 to 7, and 8 to 7, respectively. As Table 2 shows, among *voters* with less than college education, the ratios favoring party as cause were sharply accentuated. By margins of 13 to 3, 13 to 6, and 15 to 4, prior party identification influenced policy preferences on these issues rather than the reverse.

More generally, in both the 1972–1974 and the 1974–1976 intervals, instances in which the variables were relatively equal in the frequency at which they appeared as “plausible cause,” or instances in which party was dominated by the issue-related variable, were found *only* among nonvoters. The contrast was provided by voters with less than college education; among them, party always dominated as a “likely cause,” and by ratios averaging almost three to one. Among voters with some college education, effects were more evenly balanced—party was again always dominant but by much smaller margins. The dominant role of party identification as “the cause” was most evident in the contest with the more or less abstract concept of liberal or conservative ideological preferences.

At least one striking exception to this pattern was provided in the 1974–1976 interlude by attitudes toward President Ford’s pardon of former President Nixon. Among voters, as well as nonvoters, the dramatic and controversial presidential pardon produced the dominant pattern of changes. Party influenced the relative approval of Mr. Ford’s generosity much less often than party identifications were changed by attitudes toward the pardon.

When, in Table 3, ideology (self-placement on a Liberal–Conservative scale) replaces party identification in the policy specific variable pairs in the 1972–1974 or 1974–1976

Table 3 Causal dominance within pairs of variables, ideology and policy preferences, 1972–1974 and 1974–1976, by voter participation and education^a

“Other” variables	Nonvoters		Voters with less than college education ^a		Voters with at least some college	
	Ideology dominant	Other dominant	Ideology dominant	Other dominant	Ideology dominant	Other dominant
Aid to minorities						
1972–1974	3	7	5	5	9	6
1974–1976	4	7	4	8	8	7
Civil rights						
1972–1974	2	4	4	9	9	4
1974–1976	2	6	4	6	8	7
Rights of accused						
1972–1974	3	6	4	6	8	7
1974–1976	0	4	4	7	10	6
Roles for women						
1972–1974	2	11	6	9	8	8
1974–1976	1	6	3	5	6	6
Jobs & standards of living						
1972–1974	4	7	4	6	9	7
1974–1976	4	6	5	8	12	9

^aEntries are proportions of the entire panel population reflecting plausible causal relationships. *N* is approximately 1270.

panels, better-educated voters quite uniformly provide evidence of instances in which ideological predispositions shape policy preferences more often than they are shaped by them. Prior issue positions uniformly appear to dominate ideology among nonvoters; the same is true for the less well-educated voters, but by a somewhat smaller margin. On all five policy questions available for examination, the ordering of our three groups of citizens was clear: specific issue preferences shaped by ideological preference were relatively more numerous only among well-educated voters. The other extreme was provided by nonvoters, among whom ideology seldom dominated in the context with specific policy preferences.

The two panels from the 1970s support a series of generalizations of direct interest to our inquiry. (1) In the analysis of *voter* preferences, the order which has party identification preceding ideological self-classifications, which, in turn precedes specific policy preferences, is the dominant order. (2) This is more often the case among less well-educated voters than among voters with at least some college education. (3) Even in the contexts in which this generalization is most clearly true, substantial numbers of voters reflect temporal sequences of change in which party appears to be shaped to conform to stable ideological predispositions or specific policy preferences. (4) Among nonvoters specific policy preferences appear to shape ideological preferences more often than the reverse. (5) Among nonvoters, party was seldom clearly dominant as a cause of policy-related attitude changes.

Age, education, and voter turnout have been treated here as “conditional” variables, introduced to test the ordering of party identification, ideological predispositions, and specific policy preferences. Not unexpectedly, their introduction has also exposed relationships pertinent to broader concerns with the nature and measurement of public opinion. The “effective” use of abstract ideological predispositions is appropriately associated with political involvement (voting) and education; partisanship seems to have its greatest impact in midrange (less well-educated) voters; and the political periphery is most responsive to the discrete and dramatic—Watergate and the Presidential pardon, the changing role of women in society, aid to minorities, and crime. And, being mindful of Converse’s classic designation of “nonattitudes,” the ubiquitous presence of dissonant or discordant change in policy preferences is notable, particularly among the more peripheral members of the electorate, the nonvoters. For our present purposes, these echoes of more conventional contributions to the literature on the formation and organization of political attitudes add credibility to the meaningfulness of our tabular algorithm (see Converse 1962; Zaller 1992).

3.6 *Campaign-Induced Change in 1980*

In the course of the 1980 campaign, the sheer number of persons exhibiting possibly causal patterns in the changing relationships of party and ideology was roughly proportionate to the number in other longer periods captured by longer-term panels. Between January and June 1980, 11% of the 1980 panel members changed their ideological self-designations, bringing them into line with their unchanging party identifications; only 6% reflected changes in party that were in greater conformity with their ideological location. Between June and October of that election year the ratio was even more extreme, with 12% favoring party identification as the presumptive cause and only 4% revealing stable ideological positions matched by congruent changes in party. This shift possibly reflected the shifting balance of long-term/short-term influences borne of the approaching 1980 presidential election.

Interestingly enough, the unique nature of the year of Reagan’s ascendancy is suggested by the fact that, particularly in the first half of the year, Democratic party loyalties

outnumbered Republican in the causal role, while Conservative predispositions clearly outnumbered Liberal among those for whom ideology was the dominant presumptive cause (of changes in party identification favoring the Republicans). In any event, if this pair of time intervals is anything close to typical of presidential election years, the sequence party–ideology predominates before the campaign and overwhelms as the election approaches.

This was true both for older citizens, where the dominance of party identification over attitudes toward governmental services and spending increased every bit as much as it did, and for those under 45. A striking age difference existed, however, in the partisan directions of the dominant influences. Among the older group, two-thirds of those whose party identification apparently shaped their issue preferences were Democrats—only one-third were Republicans. As a consequence, among them issue preferences moved to the liberal side. Within the younger group the partisan division was only 49 Democrat to 51 Republican. And among the younger generations, where issue preferences were much less often shaped by Democratic sympathies than among older generations, issue preferences more often changed to the conservative side. At the same time, however, among the young only 30% of those changing *party identification* (to better accord with policy preferences) were responding to conservative policy preferences, while among the older group a full 45% of the relatively smaller group changing their party were doing so in response to comparably conservative policy sentiments. As an earlier analysis of the 1980 election could only suggest, this analysis documents the fact that younger voters (who changed policy preferences) were both more often Republican and (among those who changed party) more often Liberal than were their seniors—making for an interesting tension among future loyalties and preferences, at least among those young voters changing party loyalties and issue preferences during the election year of 1980 (Miller and Shanks 1982).

3.7 Further Implications Derived from the Tabular Depiction of Causal Influence

Still working with the 1980 panel, there is additional evidence that the configurations of change that we are observing are real and meaningful, not random and ephemeral. In accord with theoretical expectations it is not surprising to discover that “strong” party identifiers reflect virtual unanimity in the absence of “ideologically caused” change and in the dominance of their party allegiances. They also reflect the lowest instance of change in either variable and the fewest cases of “uninterpretable” change. Independents, or voters with no party preference, on the other hand, boast the least stability, the greatest propensity for apparently random change, and a visibly higher incidence of change attributable to ideology.

On quite a different dimension the patterns of attitudinal change seem related to the citizen’s involvement in politics. Citizens who report that they follow public affairs most of the time are least inclined to change political preferences, those who seldom follow public affairs are most volatile, and the latter most often present changes that defy easy interpretation, while the good citizens most often change in interpretable ways. At the same time, citizen interest in the current campaign presents an interesting variant on the theme. Least change in political attitudes is associated with the least attention to the campaign, but it is the middle category—those only “somewhat interested” in the campaign—who are most volatile and most likely to change in unexpected ways. This is reminiscent of the often-observed curvilinear relationship between political involvement and susceptibility to change. Those not even peripherally engaged in the current campaign are least open to change; those paying more attention are much more open, but their change—in the short

run—is often erratic; those most deeply involved do not change much or often (presumably anchored by the sheer mass of their knowledge of politics), but when they do change, the changes seem interpretable.

Because of the rich database provided by the current study design used by the NES, detailed inquiries into the politics of the 1980s promise further insight into the causal influences that shape public opinion. In 1994, measures of predispositions concerning egalitarianism and moral tolerance were added to party identification and ideological self-placement as panel-based measures of policy-related predispositions relevant to voters' policy preferences. Using 1992–1994 panel data in our tabular algorithms, party generally dominates ideology, which dominates moral tolerance, which, in turn, dominates egalitarianism. Something of the unique temper of the times in 1994 may be captured by the extent to which policy preferences on issues such as affirmative action, the death penalty, aid to Black citizens, and term limits appeared to influence all four, presumably more generic, predispositions. Their dominance over egalitarianism was particularly notable. At the same time, or at least in the same period, attitudes toward health insurance and governmental domestic services and spending were changing to accord better with the same predispositions, and with party identification most of all.

4 Conclusion

These many examples illustrate some of the possible consequences of establishing the credentials of our tabular paradigm for appraising the causal direction of the relationships between two interrelated variables. The procedure can apparently illuminate many aspects of the origins and consequences of antecedents of the vote decision as well as suggest a dominant ordering among subsets of variables united in a single explanatory equation. However, given the pragmatic, data-bound nature of this particular experiment in causal explanation, it is difficult to see all of the implications, or lack thereof, in what we have learned. Much that we have observed has been interesting and provoking, but it has been selected for presentation with perhaps too little appreciation of all of the methodological propositions that may thereby be invoked.

At the same time, it is clear that we have focused attention more sharply on the temporal sequence of change than is possible with more orthodox uses of correlational or regression statistics. To return for a moment to our earlier description of the patterns of stability and change produced by our tabular manifestations, something on the order of a quarter of each panel were involved in all of the patterns of change we thought interpretable in terms of caused directional change. The remaining three quarters were divided among (2) cases exhibiting no change in either variable and (3) cases of divergence in “opposite” directions. The second of the categories apparently reflects static relationships established at some prior time; the third defies any directional causal interpretation of the interaction between the two variables, early or late. And yet, all three categories play a role in aggregate estimates of the overall relationship (correlation) between A and B at both T_1 and T_2 and, therefore, in changes in coefficients summarizing the relationships between T_1 and T_2 . Isolation of those cases for which an observed temporal sequence of changes in individual variables seems to “make sense” would seem to offer a more direct approach to the analysis of relationships as they change over time.

The panel data we have examined in our search for the temporal order that must underlie valid causal inferences based on cross-section data have almost invariably supported the expectation that prior party identification leads to changes in Liberal/Conservative ideology among voters, more often than stable ideology leads to congruent changes in party

identification. Party has been most dominant among moderately well-educated voters; ideology more often appears in the role of prime mover among college-educated voters.

Among nonvoters, neither party identification nor ideology appears as often (as among voters) as causes of increased congruence in policy preferences, with ideology least often dominant as a cause of change.

Party identification more often appears dominant as a cause of attitudinal change among older voters. In the Jennings parent–child panel comparisons, ideology and issues were more often designated “causative” among young adults (the cohort of offspring) than they were among citizens of an older, parental generation.

Differences associated with voters’ age, education, and political participation are all properly associated with established theoretical expectations concerning the causal roles for party identification, ideology as a policy-related predisposition, and specific policy preferences. Of perhaps equal importance among our findings is the documentation of the extent to which dominant causal influences are countered by apparent causal flow in the opposite direction. One may be comfortable that the sequence $PI \rightarrow ID \rightarrow \text{Issues}$ is dominant among voters, but it is also true that reversals of this causal order may characterize a very visible minority, if not an occasional majority, within the heterogeneous population of the nation’s voters. Finally, the evidence of order and coherence in the organization of mass opinions does not deny the continuing presence of many, many nonattitudes and conflicting preferences, among both well-educated voters and those citizens more peripheral to national politics and elections.

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Warren Miller—Political Methodologist

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The publication of Warren Miller's last individually authored paper in *Political Analysis* represents a welcome opportunity to acknowledge his many contributions to the development of quantitative methods in political research. From his early days at Michigan and Berkeley, Miller was convinced that many important descriptive and explanatory questions concerning political phenomena can be addressed only through the analysis of quantitative data based on an appropriate research design. He was also convinced that such research is not possible without institutions that facilitate large-scale data collection and widespread access to the resulting materials, as well as improved procedures for data analysis.

In the early 1950s these institutions and resources were in very short supply, where they existed at all. Warren devoted his career to developing the institutions and capabilities that he believed were essential to permit political scientists to answer the kinds of quantitative questions that he had in mind. His initiatives in pursuing those objectives are so numerous that they are hard to summarize, for they include specific research designs concerning electoral behavior and representation, large-scale data collection projects in several countries, archival institutions to document and process the resulting data, and methodological training programs and seminars. In combination, these Miller-led initiatives have transformed our profession.

Miller's interest in both quantitative analysis and infrastructural development started early in his career. His master's thesis at Oregon was devoted to general patterns or tendencies in recorded votes cast by elected representatives and discussed alternative scaling procedures for analyzing roll call data. His doctoral dissertation at Syracuse was based on the first Michigan survey of a presidential election (in 1952) and concentrated on the conditions required for "issue-oriented" choices by ordinary voters in such elections. These general interests, and their relevance for alternative "linkages" between citizens and their elected representatives, played a continuing role in most of Miller's subsequent research, but they also played a major role in his efforts to extend the scope of quantitative research to include

colleagues in many other universities. In particular, his work at Oregon led to an early effort to archive roll call vote data so that scholars at all universities could easily use that data.

After publication of *The American Voter* (in 1960), Miller and his colleagues initiated a series of cooperative activities that became known as the Inter-University Consortium for Political Research. The initial objectives of that venture were defined in terms of documentation and dissemination of individual-level data from electoral surveys. From the beginning, however, Miller believed that many other aspects of political science would benefit from the same kinds of archival assistance and that many fields within political science would benefit from the same kind of training program in quantitative methods. Our profession's subsequent development has documented the accuracy of those beliefs, for the Consortium's data archives, dissemination activities, and summer training programs have played a major role in the growth of contemporary quantitative research about politics.

In the late 1960s Miller and the Consortium expanded the initial summer training program to include a variety of advanced seminars devoted to new methods or approaches, in addition to providing several types of quantitative training that were not yet available on many other campuses. Some of those seminars were funded by the Mathematical Social Science Board (MSSB) and provided an early home for political scientists with more advanced training and an interest in mathematical models besides those based on statistical, psychometric, or economic procedures.

In 1977, Miller initiated a collaborative project that was designed to provide continuing support and substantive continuity for future surveys of the U.S. national electorate. From its creation as the National Election Studies (NES), the activities of this enterprise were often motivated by questions that were of fundamental interest to methodologically oriented political scientists, including many participants in the emerging Political Methodology group within the American Political Science Association (APSA). Miller was committed to broadening the scope of participation in NES activities, and that emphasis was particularly evident in the role of methodologically oriented researchers in planning future election surveys. (Methodologically oriented NES participants were particularly interested in alternative survey designs in order to test conflicting ideas concerning causal direction and "endogeneity," including many of the ideas discussed in Miller's paper in this issue). In addition to these NES-related activities, Miller was also instrumental in obtaining early financial support for the Political Methodology group, in order to develop and sustain its annual conference.

In each stage of his career, Warren Miller devoted a great deal of his considerable energies to the development of new resources for quantitative research, all of which were intended for utilization by political scientists in many other universities. These initiatives included innovations in research design, several of our most important large-scale data collection projects, major archival facilities and services, training programs in quantitative methods, and continuing communications within the scholarly community. As I see our increasingly specialized field, it seems highly unlikely that another political scientist will have this kind of pervasive impact.

I conclude with some observations concerning Warren Miller's qualities as a researcher and scholar, based on my own work with him as a collaborator and coauthor. During the 1960s and 1970 I worked with Miller on several Consortium- and NES-related activities, but the two of us did not collaborate on any electoral analyses, convention papers, or other publications. In 1981, however, we agreed to prepare a paper on the 1980 election for the annual meetings of both the American Sociological Association and the APSA. That initial paper began an extended collaboration which led to three essays on the 1980, 1984, and 1988

elections in the *British Journal of Political Science* and which culminated in 1996 with the publication of *The New American Voter*. (Our collaborative effort also provides some of the rationale concerning causal interpretations of statistical relationships that led Miller to carry out the research discussed in his paper in this issue.) During our extended collaboration, I had many opportunities to observe Miller's unique characteristics as a researcher, including his commitment to what he would call the basic logic of quantitative analysis. I suspect that these qualities are partly responsible for the continuing influence of his scholarly work, in addition to his sustained leadership of the major institutions that have shaped our field.

In the early stages of each project with Miller, he was patient, curious, and unusually open to new ideas. He was often enthusiastic about supplementary analyses that might be described as a digression from our current primary question, he was often eager to see if an alternative analysis or approach led to a different conclusion, and he was always open to alternative interpretations of the same statistical results. Given the limitations of our current data and methods, Miller had a very sharp sense of continuing ambiguity concerning the most appropriate interpretation of many results, and he wanted to encourage other scholars to attempt their own criticism or reformulation. He was also committed to documenting the contributions made by previous researchers and to the evolution of our understanding of political phenomena through the open exchange of ideas and results within an international community of scholars.

These collegial qualities, however, always coexisted with a fierce determination concerning the progress of Miller's own (and our collaborative) research. In each of the projects where I could observe his work, including his final paper in this issue, he exhibited an amazing persistence in trying to improve the manuscript, and he repeatedly sought the reactions of others concerning the clarity and coherence of the most important points. Miller wanted to provide answers to major continuing puzzles in the explanation of political phenomena, but he was also determined to communicate his findings and arguments to as many of his colleagues as possible. The intensity of that commitment, in addition to our appreciation of his many scholarly and institutional accomplishments, will remain as a vivid inspiration to those of us who were privileged to work closely with him.

Warren Miller and the Future of Political Data Analysis

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The final sole-authored article by Warren Miller, in this issue, makes a fitting conclusion to an inspiring career. Here again, as so often before, is the well-chosen and significant problem. Here again is the honest, creative empiricism that was his hallmark. Here again is the distinctive, arresting voice that those of us privileged to know him will always remember.

Yet for many students of public opinion and voting, this article, however charming, will have a slightly old-fashioned air. Nearly all the empirical claims come from cross-tabulations. Standard errors are not much in evidence. No underlying dimensions are

Author's note: I am grateful for comments from my colleagues Nancy Burns, Don Kinder, Doug Lemke, Ken Kollman, and Roy Pierce, and also to Jim Johnson and Brad Palmquist, but, of course, they are absolved from responsibility for remaining errors and infelicities of expression.

postulated, no normal curves of error are assumed, and thus no probit analyses are conducted nor scaling techniques employed. Measurement errors are discussed, but no distributional assumptions are made and no formal corrections are applied. The analytic methodology uses almost exclusively the formal statistical tools of the 1950s, and *everyone knows* we have progressed well beyond that in political methodology.

What ought to be done instead, it will be thought, is to correct all these omissions. Let us add some econometric and psychometric structure. Let's make all those assumptions that prop up our conventional tools and underlie much recent innovation. Let's make the analysis suitable for maximum-likelihood estimation and Bayesian analysis. Let's presume we have normality, independence, linearity, random errors, underlying interval scales, and everything else we need. These are the techniques of modern political methodology; their presence validates statistical work in contemporary political science. If we would just add all these routine Nineties assumptions, it will be said, how much better this article would be.

In my view, this way of thinking about Miller's article is quite mistaken. Yet there is much to learn from the conventional wisdom, for its misjudgments illuminate brilliantly the troubled current state of political opinion and voting research, and they help explain, too, why political methodology has contributed less of substantive value than it should.

The dominant style in statistical applications to political science is characterized by three working principles:

- The relationships among key variables should be expressed parametrically, with strong assumptions about the functional forms relating measured quantities and at least equally strong assumptions about the probability distributions for unknown quantities.
- Researcher time and computer capacity should be used to generate parameter estimates and their sampling distributions under the assumed model. Exploring the data intensively before estimation to get the assumed model right is less important. At most, a few specification tests should be done at the end.
- Deriving explicit functional forms and distributional assumptions from mathematical political theory is a frippery. Theory rarely provides much help anyway, and in any case, linear functions with long lists of exogenous variables will control for the many things we do not know.

High-end work within this dominant framework often looks quite impressive, particularly to the uninitiated. Mathematical formulas for unfamiliar distributions may appear; intensive new-fangled computing may be reported; revisionist findings may be trumpeted. New computing tools produce scientific advances!

Sadly, all too little of this work can be trusted. Empirical research remains just as good as its inferential foundations, and no better. If the assumed functional relationships connecting the variables are wrong, then no amount of computing will produce anything useful. Dumping a list of exogenous variables into a linear link function inside a maximum-likelihood equation is not serious empirical research. Simply assuming a convenient distributional form—a normal, exponential, or negative binomial distribution, for example—pays no attention to the data. And when other scholars have applied decision theory and game theory to the behavior under study, ignoring what they have to say virtually guarantees that the statistical results will fail to speak to theoretical debates.

Too much of our "best" methodological work ignores all these concerns and rushes off to its calculations. The results may stun the innocent. Yet however majestic the enterprise may appear, the truth is that to scholars with a little experience at it, the work is often rather

formulaic. Indeed, at some level, we all do it sometimes. Its great appeal is that it is so much quicker and less intellectually demanding than coaxing the data to tell us something new, consequential, and truly reliable.

In consequence, fields of political science that rely on methodological advances get much less help than they need. Public opinion and voting research are cases in point. Consumer theory in economics, whatever its imperfections, is an inspiring mixture of rigorous theorizing and closely linked econometric work with potent policy implications. [A recent example is Deaton (1997).] In contrast, genuine voter theory in political science barely exists. Indeed, we commonly proceed as if nothing of the kind were necessary. Most of our work consists of ad hoc regressions and probits and more elaborate statistical setups, each with linear lists of variables taken freely from casual rational choice thinking, social psychology of various vintages, and whatever demographics the survey researcher might have collected. The usual lengthy collection of explanatory variables mops up the variance and hacks away the outliers and residuals that might have taught us something. Repeated admonitions have long been sounded from both outside and inside the profession (e.g., Freedman 1985; Kramer 1986; Manski 1995), but they go largely unheeded.

Why should anyone believe such research? Actually, and contrary to what disciplinary outsiders sometimes think, we don't. Article by article, we profess belief and sometimes manage to convince ourselves; but in truth, not much of real theoretical power cumulates after years of work. Result: we have no first principles to teach undergraduates and no agreed foundation from which to talk to policymakers about voter turnout, campaign finance, or how well democratic representation is working. We don't know, and they know we don't know.

We claim to be studying our data. But real data analysis has a character incompatible with most of our current practice. Its principles are different.

- To argue that a variable y is caused by another variable x , begin by showing the reader the simple bivariate plot or cross-tabulation of how y changes when x changes. Interpret what it means substantively. Then do the same in key subgroups of the sample to see how the relationship varies. These simple steps prevent many a blundering misspecification.
- As control variables and statistical adjustments are added to eliminate counterhypotheses, remember that measures, distributions, and functional relationships are not justified by their computational convenience. They have to be argued for with empirical evidence, one by one. Careful and detailed investigation with multivariate cross-tabs, partial regression plots, and other data-visualization techniques, carried out in conjunction with a substantive expert, has to be done first before any parameter estimation is attempted. Semiparametric and nonparametric estimators, as they become better codified and more familiar, will eventually help prevent specification error at the estimation stage, too.
- Formal theory focuses data exploration and guards against overfitting a particular sample. When other scholars have developed relevant models, their suggestions to us should be welcomed—not naively or dogmatically adopted, but welcomed.
- With neither prior mathematical theory nor intensive prior investigation of the data, throwing half a dozen or more exogenous variables into a regression, probit, or novel maximum-likelihood estimator is pointless. No one knows how they are interrelated, and the high-dimensional parameter space will generate a shimmering pseudo-fit like a bright coat of paint on a boat's rotting hull.

The important empirical generalizations in political science due to V. O. Key, Donald Stokes, and Philip Converse, along with more recent work on "the democratic peace" in

international relations, illustrate the power of creative data analysis. Few, if any, of their generalizations emerged from elaborate statistical models at the frontier of methodological research. They came instead from intensive study of plots and cross-tabulations. Those simple data-analytic methods build foundations. Of course, we should not stop there. We have the tools now to build higher than those who preceded us. Too often, though, we want to skip the basement construction. Little wonder there is no skyline.

No one article can hope to satisfy all methodological canons, and this final article by Warren Miller is no exception. Yet it illustrates lucidly what is needed, for its remarkable inferential power stems from the simplicity of its design. Here we need not rely on religious faith in normality, linearity, and the other customary dogmas. Instead, biennial panel data are exploited to answer a simple question using only cross-tabulations: When a voter's political opinions and party identification are divergent, which one usually adjusts? For all but college-educated voters, the answer is: Opinion. Most of the time, party identification controls voter opinion more than the reverse, probably because popular opinion is structured by party elites. The drastic implications for democratic theory are too obvious to need spelling out here.

Special features of Miller's data make his methodology work. For example, party ID and opinions may both be construed as trichotomous (liberal/Democratic, centrist/Independent, conservative/Republican), so that harmony and dissonance are easily recognized. Then Miller simply counts how often dissonant opinions revert to harmony with unchanging party preferences, and how often the reverse occurs. The result is a very simple test of two "error-correction" models: either party ID adjusts to opinion or opinion adjusts to party ID. The unusual ordinal character of Miller's data should have made the connection to all the statistical work on error correction much harder. Instead, in his hands, the investigation became beautifully simple. Recognizing fecund research designs is the first principle of skillful data analysis.

Miller has given us here a hard core of evidence on how political opinions relate to party preferences over time. Perhaps only measurement error remains as a potential counterexplanation, though Miller gives some evidence that it has not biased his findings. In any case, any subsequent theory of voter behavior will have to account for the consistent patterns found here. For those uncomfortable with the conclusion, specification error offers relatively little hope; the data largely speak for themselves. Subsequent theoretical work will find it hard to dodge Miller's challenge, and that is what a good investigation's methodology should accomplish.

Perhaps more than ever before, political science needs tools from its methodologists, tools to generate reliable empirical generalizations. Statistical "findings" that logically depend on six impossible things we must believe before breakfast are useless to us. Nonlinear regression analysis and maximum-likelihood theory provide us with enormously valuable machinery, but no matter how sophisticated, estimators based neither on formal models nor on patient, painstaking data analysis generate only bad science. We need real science, and so we will have to learn to look and learn to really see. That is the example Miller leaves us.

Finally, a personal note. The political science profession will long remember Warren Miller's intellectual creativity and institution-building. What looms even larger in my memory, however, is his character, particularly his unstinting generosity toward younger colleagues. A great many pertinent stories have been told by those he helped, but one bit of history is particularly relevant here.

At a fragile stage of its early life, the Political Methodology Section of the APSA found itself without money to hold its first meeting. Funding agencies had declined to support a group of young scholars so unproven. But Warren Miller thought the Section had a

future, and his faith made the meeting possible. He never mentioned his role again, and he didn't profit from it. He just spoke up forcefully in a committee meeting and got the money allocated.

It is only fitting, therefore, that his final article should appear in the official journal of the Methodology Section: and more fitting still, if my judgment is right, that the article should point the way toward the methodological future. There, some young scholars may come across it, hear its message better than we do, follow up its example with new techniques of their own, and thus find themselves, like many young scholars before them, in Warren Miller's debt.

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