

The Study of Electoral Behavior

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Scholarly studies of electoral behavior have a long and vibrant history. My aim here is to provide a selective summary of that history, focusing on developments that seem to me to have been especially important in shaping the current contours of the field.¹ Since new preoccupations and insights have usually emerged as responses to

¹ Some aspects of this history are treated in greater detail elsewhere. Burdick and Brodbeck (1959) assembled a rich collection of critical assessments of voting research at an early point in its development. Converse (1987) presented a detailed history of survey research through 1960, with chapters on Columbia University's Bureau of Applied Social Research and on the University of Michigan's Survey Research Center describing the organizational history of these influential centers and sketching their scholarly contributions in a variety of fields, including the field of voting behavior. Pomper (1978) analyzed the scholarly impact of the single most important work in the field, *The American Voter*. Converse (2006) provided a mixture of autobiographical and critical reflections on the development of the field, while Sapiro (1998)

salient past successes and failures, the organization of my survey is largely (though not entirely) chronological.

I have, of course, ignored many important contributions and elided many relevant details. That is inevitable, given limitations of space and of my own expertise. Fortunately, much of what is missing here is handsomely addressed in other chapters of this volume. In any event, my hope is that the range of scholarship touched upon in the following pages will provide some sense of the richness and significance of what has been accomplished by scholars of electoral behavior over the past 70 years, while also inspiring the scholars who chart the mainstream of electoral research in the future to tackle some of the many fundamental issues that remain unresolved.

The Columbia Studies

The modern history of academic voting research began in 1940 at Columbia University, where a team of social scientists assembled by Paul Lazarsfeld pioneered the application of survey research to the study of electoral behavior. As occasionally happens with major innovations, this pioneering effort seems even in the light of subsequent advances to have been remarkably sophisticated. Lazarsfeld and his colleagues surveyed 600 prospective voters in a single community (Erie County, Ohio) as many as seven times over the course of the 1940 presidential campaign, with a complex mixture of new and repeated questions in each successive interview, and with

provided a participant-observer's view of the history and mission of the National Election Studies project. Niemi and Weisberg (1976; 1983; 1992a; 1992b; 2001) provided useful summaries of key developments in their editorial contributions to successive volumes of collected articles on voting behavior, while Lewis-Beck et al. (2008) appended similar commentaries to their replications of chapters from *The American Voter*.

additional fresh cross-sections to serve as baselines for assessing the effects of repeated interviewing on the respondents in the main panel.

The results of the 1940 Columbia study were published in *The People's Choice: How the Voter Makes Up His Mind in a Presidential Campaign* (Lazarsfeld, Berelson, and Gaudet 1944). A second panel study conducted by the Columbia team in Elmira, New York, in 1948 provided the basis for an even more influential book, *Voting: A Study of Opinion Formation in a Presidential Campaign* (Berelson, Lazarsfeld, and McPhee 1954). Together, these two volumes defined a set of questions and research methods that have had a variety of profound effects on subsequent work in the field.

Lazarsfeld's panel studies were carefully designed to measure changes in individual vote intentions over the course of a presidential campaign. This focus reflects the intellectual roots of the project in market research on consumer behavior and wartime analyses of the effects of propaganda. Indeed, Lazarsfeld seems all along to have viewed "the psychology of choice" as his real subject matter, and turned to the study of presidential campaigns only when foundation support was not forthcoming for a panel analysis of consumer behavior (Rossi 1959, 15-16).

Given their interests and study design, the Columbia researchers must have been surprised by what they found. Their careful measurement of media content turned out to be of little use in accounting for voters' choices, most of which seemed to be based upon strong "brand loyalties" rooted in religion and social class and reinforced by face-to-face interactions with like-minded acquaintances. Their lavish panel design revealed a good deal of reinforcement of pre-existing political predispositions, but rather little outright conversion. Thus, by the time of their 1948 study, Lazarsfeld and his colleagues were downplaying the role of the parties and the mass media and

elaborating their analysis of interpersonal influence by measuring respondents' perceptions of the political views of their families, friends, and co-workers, emphasizing the homogeneity of these social networks and their tendency to produce increasing political conformity over the course of the campaign.

To their credit, the Columbia researchers did not cling to their preconceptions about the nature of electoral choice, but followed where their data led them. As a result, they found themselves concluding (Berelson et al. 1954, 310-311) that

the usual analogy between the voting “decision” and the more or less carefully calculated decisions of consumers or businessmen or courts ... may be quite incorrect. For many voters political preferences may better be considered analogous to cultural tastes—in music, literature, recreational activities, dress, ethics, speech, social behavior. ... Both have their origin in ethnic, sectional, class, and family traditions. Both exhibit stability and resistance to change for individuals but flexibility and adjustment over generations for the society as a whole. Both seem to be matters of sentiment and disposition rather than “reasoned preferences.” While both are responsive to changed conditions and unusual stimuli, they are relatively invulnerable to direct argumentation and vulnerable to indirect social influences. Both are characterized more by faith than by conviction and by wishful expectation rather than careful prediction of consequences.

Thus, a team led by one of the great sociologists of his era succeeded, almost despite themselves, in producing a classic study of electoral sociology.

If *Voting* was a richer work of *sociology* than *The People's Choice*, it was also noticeably richer in its specifically *political* aspects. Lazarsfeld and his colleagues turned more detailed attention to the role of political issues, stressing the frequency with which respondents ignored or misperceived their favorite candidates' issue stands

when these were in conflict with the respondents' own views. They also demonstrated in considerable detail the extent to which Truman's late surge represented the "reactivation" of latent Democratic loyalties as the salience of traditional class issues came to the fore over the course of the fall campaign. Finally, a concluding chapter by Berelson on "Democratic Practice and Democratic Theory" provided a much-noted—and vigorously criticized—interpretation of the implications of the authors' findings for political theory.² Perhaps not surprisingly, these specifically political aspects of the Columbia studies are the ones that have turned out to have the most influence on subsequent voting research.

The "Michigan Model"

The work of Lazarsfeld and his Columbia colleagues demonstrated the rich potential of election surveys as data for understanding campaigns and elections. The next, and even more important, advance in election studies emerged in the following decade at the University of Michigan.

Ironically, the Michigan team, like their counterparts at Columbia, did not originally set out to study voting behavior. Angus Campbell and Robert Kahn of the

² According to Berelson and his colleagues, the Elmira data revealed that campaigns involve "little true discussion" and "more talk than debate," that "for large numbers of people motivation [to participate in political life] is weak if not almost absent," and that "the voter falls short" of the democratic expectation that he "be well informed about political affairs" (Berelson et al. 1954, 308). "If the democratic system depended solely on the qualifications of the individual voter," they concluded (311), "then it seems remarkable that democracies have survived through the centuries." But in fact, they argued (and this is the point that has been most vociferously challenged by democratic theorists) that "Lack of interest by some people is

university's Survey Research Center had conducted a national survey of foreign policy attitudes in October 1948, and "at the end of these interviews, in order to determine the degree of political interest of the respondents and their general political orientations," had asked respondents whether they planned to vote in the upcoming presidential election and for which party (Campbell and Kahn 1952, 3). After the election—and stimulated by the much-publicized failure of the Gallup Poll to foretell Harry Truman's come-from-behind victory—they decided to reinterview the same respondents in order to "analyze the crystallization of the vote," to "record the personal, attitudinal, and demographic characteristics of voters and non-voters, Republicans and Democrats," and to "assess the influence of various psychological, sociological, and political factors on the determination of the vote" (Campbell and Kahn 1952, 3). They could hardly have imagined that this would be the beginning of one of the longest-running research projects in the history of academic social science.

Over the course of the subsequent decade the Michigan election studies became increasingly institutionalized, with national surveys conducted in 1952, 1954, 1956, and 1958 and analyzed by a growing interdisciplinary team of researchers based in the Survey Research Center (Campbell, Gurin, and Miller 1954; Campbell and Cooper 1956; Campbell and Miller 1957; Stokes, Campbell, and Miller 1958). Warren Miller, a young political scientist trained at Syracuse University, played an increasingly prominent role in the project, including recruiting two promising graduate students: Donald Stokes from Yale and Philip Converse from Michigan's own social psychology program (Sapiro 1998). Miller, Converse, and Stokes would turn out to be the core of the Michigan team

not without its benefits. ... The apathetic segment of America probably has helped to hold the system together and cushioned the shock of disagreement, adjustment, and change" (314, 322).

that made the great leap forward, intellectually and organizationally, that produced the most important landmark in the whole canon of electoral research, *The American Voter* (Campbell et al. 1960).

The primary data for *The American Voter* were from the Michigan surveys conducted in connection with the 1952 and 1956 presidential elections. These surveys followed the same basic design that had been improvised in 1948, with respondents interviewed during the fall campaign and then reinterviewed after the election. Unlike the earlier Columbia studies, the Michigan election studies were based upon national survey samples. Thus, they were well suited not only to develop and test theories of voting behavior, but also to provide an historical record of the considerations shaping the outcomes of specific national elections. As the authors of *The American Voter* put it (Campbell et al. 1960, 8),

Our primary aim in this research is to understand the voting decisions of the national electorate in a manner that transcends some of the specific elements of historical circumstance. But anyone who works with extensive data on a social process as important as a presidential election must feel a responsibility to provide some historical description. Accordingly, much of this volume serves a descriptive as well as a theoretical purpose.

As it happened, description and theory were brilliantly meshed in the most striking finding of *The American Voter*—that Eisenhower’s landslide victories did little to disturb the long-standing loyalties of a plurality of American voters to the Democratic Party. This finding emphasized the political significance of the authors’ fundamental distinction between long-term and short-term forces. On one hand, “Few factors are of greater importance for our national elections than the lasting

attachments of tens of millions of Americans to one of the parties. These loyalties establish a basic division of electoral strength within which the competition of particular campaigns takes place” (Campbell et al. 1960, 121). On the other hand, “it is *not* true that attitudes toward the several elements of politics are only reflections of party loyalty or group memberships or of other factors that may lead to perceptual distortion. ... attitudes toward the objects of politics, varying through time, can explain short-term fluctuations in partisan division of the vote, whereas party loyalties and social characteristics, which are relatively inert through time, account but poorly for these shifts” (Campbell et al. 1960, 65).

By building their account upon an analysis of political *attitudes* of greater or lesser durability, the authors of *The American Voter* clearly hoped to provide an explanatory framework capable of encompassing both impressive partisan stability and shifting election outcomes, including “deviating” elections like those that swept Eisenhower into office. Thus, while acknowledging the important sense in which the political landscape of the 1950s reflected the impact of partisan loyalties traceable to the New Deal or even the Civil War era, they also focused close attention on the short-term variations in perceptions and concerns that differentiated the electorates of 1952 and 1956. For example, they noted the relative paucity and partisan balance of references to prosperity and depression in 1956 by comparison with 1952, and the significant increase in unfavorable personal references to the Democratic candidate, Adlai Stevenson (Campbell et al. 1960, 46, 55). Their systematic weighing of six distinct “attitudinal forces” on the outcome of each election (attitudes toward Stevenson, Eisenhower, relevant social groups, the parties as managers of government, domestic issues, and foreign policy) emphasized the “paramount importance” of Eisenhower’s

popular appeal in accounting for his landslide victory in 1956 (Campbell et al. 1960, 524-528).

In light of subsequent misreadings, it may be worth emphasizing that the partisan loyalties of *The American Voter* were portrayed as relatively stable, but by no means unchanging: “When we examine the evidence on the manner in which party attachment develops and changes during the lifetime of the individual citizen, we find a picture characterized more by stability than by change—not by rigid, immutable fixation on one party rather than the other, but by a persistent adherence and a resistance to contrary influence” (Campbell et al. 1960, 146). This pattern of “persistent adherence” and “resistance to contrary influence,” inferred on the basis of retrospective reports in the cross-sectional surveys of the 1950s, has been handsomely confirmed in subsequent studies based upon more direct measurement in repeated interviews with the same individuals over periods of several years (Converse and Markus 1979; Green, Palmquist and Schickler 2002) and cross-generational comparisons of parents and their children (Jennings and Niemi 1981).

The other major contribution of *The American Voter* was to reiterate and elaborate the finding of the Columbia studies that political information, engagement, and ideological reasoning were far less widespread in the public than most elite political commentators seemed to imagine. The Michigan data suggested that “many people know the existence of few if any of the major issues of policy,” and that “major shifts of electoral strength reflect the changing association of parties and candidates with general societal goals rather than the detail of legislative or administrative action” (Campbell et al. 1960, 170, 546). As the authors summarized their own argument (Campbell et al. 1960, 543),

When we examine the attitudes and beliefs of the electorate as a whole over a broad range of policy questions—welfare legislation, foreign policy, federal economic programs, minority rights, civil liberties—we do not find coherent patterns of belief. The common tendency to characterize large blocs of the electorate in such terms as “liberal” or “conservative” greatly exaggerates the actual amount of consistent patterning one finds. Our failure to locate more than a trace of “ideological” thinking in the protocols of our surveys emphasizes the general impoverishment of political thought in a large proportion of the electorate.

Thus, *The American Voter* portrayed an electorate whose orientations toward politics were strongly influenced by partisan loyalties developed early in life, whose votes in specific elections reflected the overlaying of short-term forces such as Eisenhower’s personal popularity upon these long-term influences, and whose familiarity with and attachment to abstract ideologies and policy agendas was remarkably limited. In the subsequent half-century, every major element of this portrait has been subjected to energetic criticism and painstaking reevaluation using new data, theories, and research methods. In my view, at least, none of the scores and hundreds of resulting scholarly books and articles has succeeded in making a significant dent in the central precepts and findings of what has come to be called the “Michigan model” of electoral studies. While elaborations and modifications in detail have been plentiful and productive, more ambitious revisionists have invariably turned out either to be attacking a caricature of the original argument (which often proves upon rereading to be a good deal richer and more nuanced than its critics give it credit for), or to be even more time-bound in their perspectives than the original authors acknowledged themselves to be, or to be simply wrong about the facts. By the

standards of empirical social science, *The American Voter* has been a work of remarkable influence and staying power.

A Changing American Voter?

The American Voter was the acknowledged foundation for the entire field of voting research in the decade following its publication. Scholars of voting behavior increasingly relied on the Michigan data and methods, and many of them made pilgrimages to Ann Arbor to learn from the masters, either as graduate students or in summer classes and workshops. Meanwhile, the subsequent statements of the Michigan team (Converse 1964; Campbell et al. 1966; Butler and Stokes 1969) became important scholarly works in their own right, continuing to shape the burgeoning field.

Outside the ivory tower, however, history was afoot in America. The civil rights movement, urban unrest, and war in Vietnam seemed to be superseding the issues of the New Deal era, precipitating a collapse of the rather staid and stable electoral system portrayed in *The American Voter*. In a political world rocked by the Johnson landslide in 1964, the defection of the “Solid South” from the Democratic ranks, and the further fracturing of the putative majority party in 1968 and 1972, what good was a theoretical framework in which “normal” voting behavior was traced to partisan loyalties arising from the Great Depression or the Civil War? In the political atmosphere of the 1960s, critics were especially eager to overturn those aspects of the “Michigan model” that seemed to cast aspersions on the democratic competence of ordinary citizens. With tens and even hundreds of thousands of citizens marching on Washington to demand civil rights, or taking to the streets to protest the Vietnam war, who could continue to believe that “many people know the existence of few if any of

the major issues of policy” (Campbell et al. 1960, 170), or that “the mass is remarkably innocent” of “any direct participation” in the “history of ideas and the behavior it shapes” (Converse 1964, 255)?

How, exactly, was the textbook portrayal of *The American Voter* outmoded? First, revisionists argued that issues had come to play a much larger role in voting behavior than they had played in the 1950s—and that party identification had become much less important, or much more sensitive to changes in the parties’ issue stands, or both. Second, and even more impressively, revisionists argued that the very structure of political thinking had changed significantly since the 1950s, as evidenced by a marked increase in the ideological consistency of mass political attitudes from the low levels described in *The American Voter* or (even more elaborately and persuasively) in Converse’s (1964) landmark essay on “The Nature of Belief Systems in Mass Publics.” These revisionist claims were put forward in various forms by Pomper (1972), Bennett (1973), Nie and Anderson (1974), and others, but received their most extensive and influential statement in a book by Nie, Verba, and Petrocik (1976) entitled *The Changing American Voter*.

By the mid-1970s, the intellectual hegemony of the “Michigan model” was clearly at an end. Even the heirs to the Michigan tradition were getting in the revisionist act, referring in their analysis of the 1972 presidential election to “a spectacular change in the quality of mass attitudes toward questions of public policy” and stressing the importance of “leadership behavior that brings the substance of issue politics into the public domain” (Miller et al. 1976, 754). Somewhat ironically, this particular revisionist analysis was criticized for exaggerating the importance of issues and ideology, mistakenly “implying that the voters rather than the theory” and measurement strategy

had changed (Popkin et al. 1976, 802). Popkin and his colleagues used responses to the Michigan survey's open-ended "likes and dislikes" questions to estimate regression weights for the six-component model presented in *The American Voter* in five different elections, and found that the weights associated with foreign and domestic issues were no greater in 1972 than in previous election years.

Meanwhile, the thesis of increased attitude consistency was subjected to an even more forceful methodological critique put forward almost simultaneously by Bishop, Oldendick, and Tuchfarber (1978a; 1978b), Brunk (1978), and Sullivan et al. (1978). These critics noted that the average correlation among distinct issue positions jumped suspiciously between 1960 and 1964, rather than rising gradually in response to the political events of the 1960s more generally—and that this suspicious jump corresponded with a significant change in the format of some of the relevant issue items. Having focused suspicion on the changes in item format, the critics conducted experimental surveys in which the old and new item formats were presented to comparable samples of respondents. In each case, the pattern of correlations across issues among the respondents who answered the new-style questions resembled those reported by Nie, Verba, and Petrocik (1976) for the 1964-1972 period, while the pattern of correlations among the respondents who answered the old-style questions resembled the pattern reported by Nie, Verba, and Petrocik (1976) for 1952-1960. The obvious inference was that much of the increased ideological "coherence" reported in *The Changing American Voter* was probably an artifact of the change in question

wording, rather than reflecting a fundamental transformation in the structure of political attitudes.³

A second key test of the thesis that the structure of American political attitudes had undergone significant change between the 1950s and the 1970s was provided by data from the 1972-74-76 National Election Study (NES) panel survey, the first major panel study of the American electorate since the original 1956-58-60 panel which had served as the main source of evidence for Converse's (1964) analysis. Repeated observation of party attachments and issue preferences over the four-year period from 1972 to 1976 made it possible to measure the *stability* of these attitudes by comparison with the corresponding attitudes in the 1950s. The result was another significant blow to the revisionist thesis. Despite some decline in the average *level* of partisan loyalty (and despite the intervention of the Watergate scandal and the resignation and subsequent pardoning of Richard Nixon between 1972 and 1976), the *stability* of individual partisanship was just as great in the 1970s as in the 1950s. Meanwhile, the continuity of individual issue preferences (for issues included in both sets of surveys) was no greater in the 1970s than in the 1950s—and thus well below the corresponding level for party identification. Thus, as Converse and Markus (1979, 45) put it, “there has been scarcely any change in the comparative continuity of party and issue positioning between the two eras, despite manifold reasons to expect not only change, but change of major proportions.”

³ Not surprisingly, this controversy continued to simmer in the scholarly journals for several years, with notable contributions from Nie and Rabjohn (1979), Sullivan et al. (1979), Bishop et al. (1979), and Smith (1980). Later, Smith (1989) published a more general critique of the revisionist argument, inevitably entitled *The Unchanging American Voter*.

In this respect as well, then, careful analysis seemed to reveal more continuity than change in the nature of political attitudes between the 1950s and the 1970s. Despite the manifest importance of the intervening political developments, the deviations from the “Michigan model” seemed to be a good deal more limited in scope and magnitude than the more dramatic revisionist claims had implied. Moreover, and most ironically, the revisionists seldom applied their key insight regarding the historical contingency of the attitude structure and voting patterns of the 1950s to their own findings from the 1960s and ‘70s; thus, some of their writings have a millennial tone that seems more than a little quaint in the light of subsequent political history.⁴

Despite these failings, the revisionists seem to me to have made two significant contributions to the study of voting behavior. First, they put the interaction between citizens and political elites more firmly on the scholarly agenda. Key’s (1966, 2) metaphor of the electoral process as an “echo chamber” in which “the people’s verdict can be no more than a selective reflection from among the alternatives and outlooks presented to them” was taken up not only as a likely explanation for changing patterns of voting behavior (Boyd 1972; Pomper 1972; Miller et al. 1976; Nie et al. 1976), but also as a spur to innovative research on elite behavior and its electoral implications (Page 1978).

⁴ A notable exception is Boyd’s (1972, 446) prescient remark, after stressing “the importance of issues in 1968,” that “Time has told us of the unusual nature of the 1956 election. Undoubtedly, the future will reveal the atypicality of 1968, as American parties seemed to be in a stage of realignment or disintegration. Surely we should expect issues to be more important in these times than in periods of stability in party strength. After what appears to be a transition period, issue voting may once again decline to the level of the 1950s.”

Second, the revisionists and their critics together directed serious methodological attention to issues of measurement, demonstrating both the potential power and the potential pitfalls of introducing new survey instrumentation inspired by new political and intellectual developments. From the viewpoint of continuity, the fact that new issue items produced stronger results than the old items they replaced was a vexing complication; but from the viewpoint of mapping the “issue space” of contemporary American politics it represented an important step forward. As the Michigan surveys have continued through the decades—recast since the 1970s as National Election Studies and supported by the National Science Foundation as a collective scientific resource—changes in survey content have often had the same dual character. Having been sensitized to both the power and the pitfalls of new survey instrumentation, scholars of voting behavior were better equipped to exploit the power of subsequent innovations while minimizing the corresponding pitfalls.

Spatial Models, Retrospective Voting, and Rational Choice

The intensifying interest in “issue voting” that was part of the broader wave of revisionism in the voting research of the late 1960s and 1970s also drew upon a quite distinct source of intellectual ferment—the emerging “rational choice” paradigm, which applied the hypothesis of utility maximization developed in economics to political decision-making. Rational choice theory likewise played an important role in the incorporation of the empirical insights of Stokes (1963), Key (1966), Kramer (1971), and others regarding the electoral significance of “perceptions and appraisals of policy and performance” (Key 1966, 150) into the mainstream of voting research under the rubric

of “retrospective voting” (Fiorina 1981). In each of these instances, as is often the case with successful theories, rational choice theory reorganized existing knowledge, stimulated new discoveries, and raised new questions for scholars of electoral behavior.

The most influential single work in the rational choice tradition was Anthony Downs’s (1957) book, *An Economic Theory of Democracy*. An economist by training, Downs attempted to set forth “a generalized yet realistic behavior rule for a rational government similar to the rules traditionally used for rational consumers and producers” in economic theory (Downs 1957, 3). Since his “rational government” was constituted by rational political parties competing to win office, Downs’s theory of *democracy* was primarily a theory of *electoral politics*.

Perhaps due to its unconventional analytical style, Downs’s work had little immediate impact upon political scientists. It was not reviewed in the *American Political Science Review* until 1963, and was not published in paperback until 1965. Nevertheless, Stanley Kelley, Jr., was prophetic in suggesting, in his foreword to the paperback edition, that Downs’ work would eventually be “recognized as the starting point of a highly important development in the study of politics.” It certainly has been.⁵

Downs’s first and most important contribution was to introduce spatial models of electoral competition to the field of political science. Indeed, for the first two decades following its original publication, the influence of *An Economic Theory of Democracy*

⁵ This intellectual trajectory is nicely captured in Wattenberg’s (1991, 17-20) analysis of citations for the period 1966-1987. In the late 1960s, Wattenberg found, *An Economic Theory*

seems to have rested almost entirely upon a single chapter describing “The Statics and Dynamics of Party Ideologies.” This chapter laid out the now-familiar model in which voters are arrayed along a unidimensional ideological continuum, parties choose policies corresponding to points on that continuum, and voters choose parties on the basis of ideological proximity. Downs argued, as one implication of this model, that the competing parties in a two-party system should “converge rapidly upon the center” of the distribution of voters if most voters were themselves relatively moderate (Downs 1957, 118).⁶

In an important sense, this aspect of Downs’s work merely formalized a line of reasoning that was already familiar to political scientists (for example, from Schattschneider 1942, chap. IV). Nevertheless, the formalization was enormously fruitful. On one hand, it spawned a considerable theoretical literature on various aspects of what has come to be called the “median voter theorem” (for example, Black 1958; Davis, Hinich, and Ordeshook 1970; Plott 1967; Enelow and Hinich 1984; Calvert 1985). On the other hand, it also changed the way political issues and “issue voting”

of Democracy was cited about half as often as *The American Voter*, but by the late 1980s it was cited about twice as often.

⁶ Downs (1957, 117-132) argued more generally that the distribution of voters along the ideological continuum would have significant implications for the nature of the party system, with two ideologically distinct parties arising if the distribution of voters was bimodal, multi-party systems arising if the distribution of voters was multi-modal, new parties arising when expansions of the electorate significantly altered the distribution of voters, and so on. This aspect of Downs’s argument is logically less compelling (depending crucially upon the assumed willingness of voters to abstain if no party is sufficiently close to them on the ideological continuum—a willingness rationalized in turn on the grounds that uncertain voters would be sufficiently “future oriented” to resist supporting a slightly better party in the current election

figured in empirical work on voting behavior. For example, the National Election Studies (NES) surveys began in 1968 to ask respondents to place themselves, candidates, and parties on seven-point issue scales reminiscent of Downs's ideological continuum. Scholars soon began to experiment with new methods of analyzing the resulting data (Weisberg and Rusk 1970; Aldrich and McKelvey 1977; Rabinowitz 1978; Poole and Rosenthal 1984); and relative distances between voters' positions and candidates' positions began to appear as explanatory variables in statistical models of electoral choice (for example, Markus and Converse 1979; Page and Jones 1979; Enelow and Hinich 1984, chap. 9; Erikson and Romero 1990).

Another key strand of Downs's influence on the field of voting behavior stems from his observation that “for a great many citizens in a democracy, rational behavior excludes any investment whatever in political information *per se*,” since their individual choices have “almost no chance of influencing the outcome” of an election (Downs 1957, 245). While this observation was hardly unprecedented (figuring prominently, for example, in Schumpeter's (1950) analysis of the limitations of democracy), Downs focused clearer attention upon its political implications—considering, for example, what sorts of politically relevant information are most likely to be freely available to what sorts of voters. This facet of Downs's book seems to have attracted relatively little attention or interest in the first two decades following its publication, but has been increasingly influential in recent years.

In his work on *Retrospective Voting in American National Elections*, Fiorina (1981) used Downs's insight as theoretical support for his own emphasis upon the electoral

in hopes of supporting a much better party in a future election), and has had less impact on subsequent theoretical and empirical research.

significance of retrospective evaluations of the incumbent party's performance in office. Even uninformed citizens, he reasoned (Fiorina 1981, 5), "typically have one comparatively hard bit of data: they know what life has been like during the incumbent's administration." Thus, the less they know about the details of policies and platforms, the more likely it seems that they will rely upon "retrospective voting as a cost-cutting element" in arriving at a vote choice (Fiorina 1981, 6).

Fiorina documented the importance of retrospective voting by including variables tapping voters' evaluations of economic conditions, foreign policy, presidential performance, and other politically relevant conditions in models of voting behavior which also included such traditional explanatory variables as party identification and issue positions. He concluded that "the effects of retrospective evaluations on the vote are pervasive, though often indirect" through their impact on partisan loyalties or expectations regarding future performance (Fiorina 1981, 175).⁷ This work provided further empirical support for Key's (1966, 150) claim that "voters, or at least a large number of them, are moved by their perceptions and appraisals of policy and performance"—and thus that elections provide an important form of *post hoc* political accountability. It also helped to integrate vibrant strands of research on presidential

⁷ Fiorina (1981, 80) usefully distinguished between "simple" retrospective evaluations (based upon "more or less direct experiences or impressions of political events and conditions") and "mediated" retrospective evaluations (involving assessments of political figures or institutions, such as government economic performance). In a subsidiary analysis, he established that the latter "are not purely artifacts of personal bias," but also reflect "the impact of political reality" (Fiorina 1981, 129). Nevertheless, the role of retrospective evaluations in models of voting behavior remains highly problematic. While complexities regarding the causal status of issue preferences and partisan loyalties have received a great deal of scholarly attention, parallel complexities regarding the causal status of retrospective evaluations are often overlooked.

approval (e.g., Mueller 1973; Kernell 1978) and economic voting (e.g., Kramer 1971; Tufte 1978) into the narrower survey-based literature on individual voting behavior.

Burgeoning interest in retrospective voting represented one response to the challenge of low-information rationality in the realm of electoral politics, but it was by no means the only response. In the early 1990s a spate of books and articles examined how prospective voters use “information shortcuts” (Popkin 1991) to make reasoned electoral choices in the absence of detailed knowledge about policies and platforms. For example, Lupia (1994) showed that voters in a California insurance referendum used the position of insurance companies as a cue in formulating their own preferences regarding a complicated menu of alternative proposals. Meanwhile, Page and Shapiro’s (1992) portrayal of *The Rational Public* stressed what Converse (1990) referred to as the “miracle of aggregation”—the tendency for randomness and error at the individual level to cancel out in a large electorate.

Subsequent research has suggested that neither cues nor aggregation are likely to be fully effective substitutes for a better-informed electorate. Bartels showed that uninformed voters in six presidential elections did “significantly better than they would by chance, but significantly less well than they would with complete information, despite the availability of cues and shortcuts” (Bartels 1996, 217).⁸ While information effects were smaller at the aggregate level than at the individual level,

⁸ Lau and Redlawsk (2006) analyzed the same elections using a somewhat different, less demanding standard of “correct voting.” They found that from 58% to 80% of voters in each election chose the candidate consistent with their partisanship, issue positions, and other attitudes and beliefs. The authors reported being “pleasantly surprised by these results,” which they took as “validat[ing] the efficiency of heuristic-based information processing”; however,

actual election outcomes departed from hypothetical fully-informed outcomes by an average of three percentage points. Fournier's analysis of Canadian elections produced similar results on both scores, while also providing evidence that "collective biases in preferences are essentially unaffected by the unfolding of campaigns" (Fournier 2006, 60). Meanwhile, Achen and Bartels (2002; 2004; Bartels 2008, chap. 4) have argued that even supposedly straightforward retrospective voting is significantly skewed by systematic errors such as myopia and misattribution of responsibility for good or bad times. These findings suggest that much remains to be learned about the nature and extent of "rationality" in electoral behavior.

The Search for Causal Order in the Electoral Universe

The theoretical account of voting behavior offered in *The American Voter* drew heavily upon the metaphor of a "funnel of causality," in which proximate influences on voting behavior were themselves subject to explanation, at least in principle, in terms of temporally and causally prior forces (Campbell et al. 1960, 24-37). Temporal priority and causal priority were inextricably linked: the "'conceptual status' of each measurement of an independent variable" was said to involve "location on a time dimension," with events "follow[ing] each other in a converging sequence of causal chains, moving from the mouth to the stem of the funnel" (Campbell et al. 1960, 25, 24). Thus, for example, personal evaluations of the parties' current presidential candidates might be explained, in part, in terms of more general partisan loyalties

they offered no clear answer to their own obvious follow-up question: "is 70% correct enough?" (Lau and Redlawsk 2006, 88, 263).

which existed before the candidates themselves emerged as significant objects of political consciousness.

The “funnel of causality” provided a convenient framework within which to pursue both a comprehensive program of electoral accounting (by “concentrating on a cross section of measurements at a point close to the dependent behavior”) and a more selective strategy of explanation in depth (by “rang[ing] freely in time back through the funnel” in search of historical, social, or institutional antecedents of proximate electorally relevant attitudes). Electoral change could be incorporated in this framework through the “political translation” of external, non-political factors into politically relevant considerations, as when voters brought their assessments of the Great Depression in the 1930s or Eisenhower’s military career in the 1940s to bear upon their vote choices in the 1950s (Campbell et al. 1960, 33, 25, 29-32).

By the mid-1960s, methodological advances filtering through political science in the wake of the behavioral revolution were beginning to transform the “funnel of causality” from a conceptual framework into a more concrete causal model with potentially testable statistical implications. Goldberg (1966) examined a variety of alternative multi-equation models relating parental influences, sociological characteristics, party identification, and partisan attitudes to vote choices. The models that best fit his data assigned “a pivotal position to party identification” as the “encapsulator of political socialization”; a “rational calculus” of voting, while by no means absent, was “operative against a set of predispositions dominated by party identification” (Goldberg 1966, 917-919).

Whereas Goldberg’s analysis was limited to *recursive* causal models (in keeping with the logic of the “funnel of causality”), scholars in the subsequent decade began to

take seriously the possibility of *reciprocal* influences among the various antecedents of electoral behavior. For example, Brody and Page (1972; Page and Brody 1972) cautioned against taking the correlation between perceived issue proximity and vote choices as *prima facie* evidence of “issue voting,” on the grounds that perceived issue proximity might reflect psychological *projection* of voters’ own issue positions onto their preferred candidates (harking back to the analyses of Berelson et al. 1954), or *persuasion* of voters on specific issues by candidates preferred on other grounds, rather than (or in addition to) the impact of issue voting in the usual sense.

A few years later, Jackson (1975) published the first analysis of voting behavior based upon an explicit non-recursive causal model. Jackson’s model allowed for reciprocal influences between party affiliations and party evaluations. He concluded that “party identifications are highly influenced by people’s evaluations of what policies each party advocates relative to their own preferences,” and that “party affiliations have little direct influence on the voting decision except for people who see little or no difference” between the parties’ issue positions (Jackson 1975, 176). This was a major challenge to the theoretical status of party identification in the “Michigan model,” and a methodological challenge to more conventional statistical analyses that ignored the possibility of reciprocal influences among the various antecedents of voting behavior.

How would these challenges be resolved? Scholars could hardly be blamed for hoping that more sophisticated methodology would rapidly lead to a new substantive consensus. If allowing for the possibility of reciprocal causation eroded the apparent primacy of party identification in structuring political attitudes and perceptions—as a

comparison of Jackson's findings with Goldberg's and others seemed to suggest—then the “Michigan model” would have to be substantially revised or abandoned.

Alas, things did not prove to be quite that simple. Indeed, the next notable development in the search for causal order in voting behavior seems in retrospect to have crystallized the qualms of many scholars about the fruitfulness of the whole “causal modeling” program: the back-to-back publication in the same issue of the *American Political Science Review* of articles by Markus and Converse (1979) and Page and Jones (1979) reporting alternative simultaneous equation models relating party identifications, issue preferences, candidate evaluations, and vote choices.

Despite various differences in model specification, at the heart of each of these analyses was an equation in which overall candidate evaluations (as measured by the difference in “feeling thermometer” scores for the two competing candidates) were related to three major causal factors: party identification, comparative issue positions, and evaluations of the candidates' personal qualities. Unfortunately, the structural similarities between the two analyses were not sufficient to produce similar conclusions. Perhaps not surprisingly, the discrepancies between the two sets of results were most stark in the case of party identification—the variable at the heart of the “Michigan model,” and the primary target of revisionist critics for a decade or more:

◊ Markus and Converse (1979, 1064) reported that party identification was a strong (indeed, the only systematic) influence on evaluations of the candidates' personalities, with a standardized regression coefficient of .44 and a *t*-statistic of 15; Page and Jones (1979, 1082) treated party identification and personality evaluations as totally unrelated, except

insofar as exogenous personality evaluations influenced overall candidate evaluations, which in turn influenced party attachments.

◇ Markus and Converse (1979, 1059) did not allow for any contemporaneous effect of issue stands on party identification, but allowed party identification to influence issue stands indirectly (by influencing candidate evaluations, which in turn affected respondents' issue positions and their perceptions of the candidates' positions through processes of persuasion and projection, respectively). Page and Jones (1979, 1083) modeled both direct and indirect links between party attachments and comparative policy distances, and estimated much stronger effects of policy distances on partisanship than of partisanship on policy distances.

◇ Markus and Converse (1979, 1066) estimated a direct effect of party identification on voting behavior—over and above its effect on candidate evaluations—in situations where a prospective voter's evaluations of the two candidates differed by less than about 50 points on the 100-point “thermometer” scale. Their results suggested that a one-point difference in partisanship (for example, between a “strong” Republican and a “weak” Republican) had a direct effect comparable to a 15-point difference on the “thermometer” scale. Page and Jones (1979) did not allow for any direct effect of party identification on the vote.

◇ Markus and Converse (1979, 1069) concluded that “while partisan predispositions are unlikely to dominate the process completely at given stages where the candidates are being assessed, these loyalties appear to make repeated inputs of substantial magnitude throughout the process.” Page and Jones (1979, 1088) concluded that “the effect of partisanship on the vote varies considerably across elections, depending largely upon the nature of the candidate pairings and the extent to which current policy issues conflict or coincide with established party cleavages.” They added

that “when party loyalties do enter in, they do not function purely as fixed determinants of the vote; those loyalties can themselves be affected by attitudes toward the current candidates. Even short of major realignments, party affiliations are effects as well as causes in the electoral process” (Page and Jones 1979, 1088).

What was one to make of these seemingly contradictory findings? If two teams of highly competent analysts asking essentially similar questions of the same data could come to such different conclusions, it seemed clear that the results of causal analysis must depend at least as much on the analysts’ theoretical preconceptions and associated statistical assumptions as on the behavior of voters. Nor was one set of assumptions clearly more reasonable than the other; indeed, critics of either analysis could easily point to restrictive assumptions that seemed to strain plausibility. Pending stronger theory, or better data, or both, the search for causal order in voting behavior seemed to have reached an unhappy dead end.

Out of Their Heads and Into the World

The apparent failure of causal modeling to answer fundamental questions about voting behavior produced a variety of disparate reactions. Some electoral analysts have continued to pursue the quest for causal order using models and methods clearly recognizable as successors of those used by Markus and Converse and Page and Jones. For example, Goren (2005) used panel data and structural equation models to suggest that partisanship affects, but is surprisingly unaffected by, voters’ allegiance to supposedly fundamental “core values” like equal opportunity and moral tolerance.

Other analysts have sidestepped the knottiest complexities of causal modeling by reviving the assumption of recursive causation embodied in the “funnel of causality” posited by the authors of *The American Voter*. For example, Miller and Shanks (1982; 1996) developed an elaborate recursive model arraying most of the important factors identified in previous studies of voting behavior in seven distinct causal stages. By assuming that factors assigned to each of these stages could influence those in later stages, but not vice versa, Miller and Shanks produced a detailed accounting of direct and indirect effects of each factor on the vote.

Lewis-Beck et al. (2008) undertook a much more literal replication of *The American Voter*, not only applying the basic analytical framework of the earlier work but replicating dozens of specific tables and figures using data from the 2004 NES survey. While acknowledging that “Methods for analyzing data have developed considerably over the past half-century” and that “the funnel model has not always been treated well in the subsequent literature,” the authors argued that “the attitudinal model in *The American Voter* ... is no less correct than the alternatives” (Lewis-Beck et al. 2008, 15, 26, 28).

Notwithstanding this nostalgic look backward, few scholars of electoral behavior have been content merely to recycle the theories and methods of the past. However, equally few have been content to hope that new theories and statistical wizardry might untangle the causal complexities that emerged in the 1960s and 1970s. Instead, the most common impulse among electoral analysts of the past quarter-century has been to change the subject. Rather than building ever more complex and comprehensive models of individual voting behavior, they have focused on more tractable questions.

As a result, contemporary voting research has become increasingly eclectic and opportunistic. Scholars have relied on a broader array of relevant data, including not only the succession of National Election Studies surveys initiated by the authors of *The American Voter* but also a variety of other large and small surveys and laboratory experiments. Even more importantly, they have been increasingly inventive in getting outside the heads of survey respondents and into the broader political world, supplementing individual-level data on attitudes and perceptions with systematic measurement of variations in the political context of electoral behavior. This emphasis on exploiting contextual variation has provided invaluable causal leverage for analyses of voting behavior. It has also paved the way for more successful integration of voting research with broader streams of scholarship on American electoral politics.

Congressional elections have provided obvious opportunities for exploring the impact of political contexts on voting behavior. Pioneering studies by Kramer (1971), Mayhew (1974), Jacobson (1980), and others relied entirely on district-level data. However, the bolstering of congressional content in NES surveys beginning in 1978 facilitated more detailed analyses of, for example, how members' activities in Washington and in their districts contributed to their electoral security (Cain, Ferejohn, and Fiorina 1987). Franklin (1991) used survey data from the National Election Studies' 50-state Senate Election Study to analyze the impact of campaigns—and of candidates' campaign strategies—on the clarity of prospective voters' perceptions of senators' issue positions. Kahn and Kenney (1999) merged the same survey data with detailed content analysis of ads and news to investigate how campaigns affect voting behavior in Senate elections. They found that more intense campaigns caused voters to rely more heavily on partisanship, issues, and presidential approval than in low-key races.

Even at the presidential level, leverage for contextual analysis has been greatly augmented by the growing length of the National Election Studies time series, which now encompasses more than a half-century of American political history. For example, Markus (1988) exploited temporal variation in national economic conditions across a series of eight successive NES presidential election surveys to effect a merger between aggregate-level and survey analyses of economic voting. Aggregate-level analyses by Kramer (1971) and others demonstrated that election outcomes are powerfully affected by the state of the economy, but they could shed no real light on whether economic voting is grounded in voters' personal economic experiences or in their perceptions of collective economic conditions. Analyses of survey data by Kinder and Kiewiet (1979) and others suggested that assessments of collective economic conditions are much more consequential than personal economic circumstances. However, since the actual state of the national economy at any given time is a constant, *variation* in assessments of national economic conditions within a single election survey must reflect partisan biases or other forms of idiosyncratic "perceptual noise" (Kramer 1983, 104). By pooling survey data from eight elections, Markus (1988) could dispense with *assessments* of national economic conditions and instead relate voting behavior to *actual* national economic conditions (which vary across election years) and individual economic circumstances (which vary both across and within surveys). He found substantial effects for both collective and personal economic circumstances. Voters who were themselves "better off" were about 8% more likely than those whose economic circumstances were unchanged to vote for the incumbent party's presidential candidate, while each additional percentage point of election-year growth in real disposable income increased the incumbent party's vote share by about 2%.

Vavreck (2008) used the NES time series to explore another aspect of economic voting—the extent to which candidates’ strategic decisions to emphasize or deemphasize economic issues alter voting behavior. She argued that incumbent party candidates in prosperous election years and challengers in slow-growth years should and usually do focus primarily on the economy, while candidates disadvantaged by economic conditions should and usually do run “insurgent” campaigns emphasizing other, more advantageous issues. By merging survey data with detailed content analysis of campaign ads, speeches, and news coverage, Vavreck found that candidates who pursue strategies consistent with their political circumstances are more successful in shaping the campaign agenda to their advantage. More broadly, Gilens, Vavreck, and Cohen (2007) showed that the shifting focus of voters’ likes and dislikes of presidential candidates over the past half-century mirrors the shifting content of campaign advertising—and that the policy content of ads has produced a more policy-focused electorate, despite significant declines in the reach and policy content of campaign news coverage.

On a much shorter time-scale, analysts have also exploited temporal variation within campaigns to shed light on the electoral impact of a variety of political events, including primary election outcomes (Bartels 1988), debates (Johnston et al. 1992), and campaign advertising (Johnston, Hagen, and Jamieson 2004). The last of these works effectively exploited both temporal variation (focusing on shifting vote intentions over the last four months of the 2000 presidential election) and cross-sectional variation (contrasting “battleground” states with those in which little or no campaign advertising occurred). Matching survey responses from the 2000 National Annenberg Election Survey with detailed data on advertising patterns in specific media markets, Johnston,

Hagen, and Jamieson tracked prospective voters' responses to changes in the volume and content of campaign ads as well as to news coverage and other aspects of the national campaign. Their analysis suggested that George W. Bush's razor-thin victory hinged on the fact that "at the end, critically, the ad signal became decisively unbalanced" in his favor in the battleground states (Johnston, Hagen, and Jamieson 2004, 13).

Hill et al. (2008) elaborated Johnston, Hagen, and Jamieson's analysis by focusing closer attention on the rate at which advertising effects decayed. Their results supported Johnston, Hagen, and Jamieson's account of the electoral impact of Bush's late advertising surge: most of the effect of any given ad on vote intentions evaporated within one week, and "Only the most politically aware voters exhibited ... long-term effects" (Hill et al. 2008, 24). In a separate paper (Hill et al. 2007), the same authors found even shorter half-lives for advertising effects in a variety of state-level and congressional races, reinforcing the impression that voters can be powerfully swayed by television advertising in the days just before an election.

Scholars have not only *exploited* variation in electoral contexts but also *created* contextual variation through experimental manipulation. For example, Iyengar and Kinder (1985) used doctored television newscasts to examine priming effects of news coverage; Ansolabehere and Iyengar (1995) measured the responsiveness of experimental subjects to positive and negative campaign advertisements; and Mendelberg (2001) compared the effectiveness of implicit and explicit racial appeals using both experiments and observational analyses focusing on the "Willie Horton" incident in the 1988 presidential campaign.

In recent years, laboratory experiments have increasingly been supplemented by large-scale field experiments. Much of this work has focused on assessing the effectiveness of get-out-the-vote efforts (Green and Gerber 2008). However, in the early stages of a 2006 gubernatorial campaign in Texas some \$2 million of advertising was randomly deployed across 18 of the state's 20 media markets, allowing for an unusually straightforward and powerful test of the impact of advertising on evolving candidate evaluations and vote intentions. This large-scale experiment revealed strong effects of television advertising, but those effects were even more ephemeral than in Hill et al.'s (2007; 2008) analyses of non-experimental data: a major ad buy producing a 7% shift in vote intentions one day later but no discernible effect two days later. As Gerber et al. (2007, 26) put it, this “pattern of abrupt change and equilibration” in vote intentions in response to campaign advertising “appears to be inconsistent with a model of rational learning.”

Elections and the Political Order

The final sentence of *The American Voter* called attention to “the influence relations binding the electoral process to the other means of decision in the political system” (Campbell et al. 1960, 558). The title of the same authors' subsequent collection of essays, *Elections and the Political Order*, signaled a continuing interest in the relationship between electoral behavior and the broader workings of government. The pieces collected in that volume varied in style and focus, including survey-based interpretations of specific elections (Converse et al. 1961), historical analyses of the dynamics of party competition (Stokes and Iversen 1962), and a path-breaking analysis of political representation (Miller and Stokes 1963), among other contributions.

Subsequent analysts have made significant headway along each of these lines.

Nevertheless, the aspiration of situating the study of electoral behavior firmly within a more general understanding of American politics seems to me, five decades later, to remain largely unfulfilled.

Scholarly interpretations of election outcomes continue to be produced with some regularity, but they have continued to be stymied by the difficulty of generating convincing estimates of the impact of specific issues, candidates and campaign events. Indeed, it is probably no coincidence that the most detailed and ambitious works of electoral interpretation since the 1970s have been based either on recursive causal models (Miller and Shanks 1982; 1996) or on tabulations of the reasons offered by voters themselves for supporting one candidate or the other (Kelley 1983). While both of these approaches have significant limitations, both also have the substantial virtue of facilitating straightforward accounting of the potentially distinct bases of individual and collective electoral choice—a crucial prerequisite for illuminating the broader political implications of voting behavior.

If scholars in recent years have devoted too little attention to interpreting election outcomes, they have devoted even less attention to understanding other people's interpretations of election outcomes. Conley's (2001) study of *Presidential Mandates* and Grossback, Peterson, and Stimson's (2006) analysis of *Mandate Politics* are surprisingly rare in focusing on how the elite political community, happily ignorant of the latest regression analyses of NES survey data, interprets the verdict of the electorate. Here, as in many other instances, the ready availability of detailed survey data seems to have distracted us from more consequential aspects of our subject

matter for which the attitudes of voters happen to be of distinctly secondary importance.

Another notable blind spot is the interrelationship between electoral behavior and the party system. Much of the best scholarship in the latter realm has focused on “critical elections” and “realignments” predating the era of detailed survey data (Key 1955; Key 1959; Burnham 1970; Sundquist 1983). While Mayhew (2002) has cataloged the empirical limitations of this historical genre, we have yet to develop an equally compelling alternative account of how the interaction of party elites and masses defines what elections are about at any given time. Carmines and Stimson’s (1989) analysis of the racial realignment of the 1960s as a process of “issue evolution” provides an illuminating start in that direction, and Adams’s (1997) application of the same framework to the evolving role of abortion in the party system of the 1970s and ‘80s underscores its utility. In both cases, the authors found strong evidence that changing views among partisan elites preceded and contributed to partisan change in the mass electorate. But how and why party elites take the sides they do on issues like these, how their choices are shaped by correct or incorrect beliefs about the likely responses of supporters and opponents, and how “evolution” with respect to any one issue spurs or depends upon other changes in the political and social bases of party coalitions are all topics deserving much more sustained scholarly attention.

Finally, and perhaps most importantly, scholars of electoral politics have much more to learn about when, how, and to what extent election outcomes shape the course of public policy. At a very general level, political scientists recognize—even if many ordinary Americans do not—that elections have significant consequences. Since the 1970s, the ideological gulf in voting behavior between Democratic and Republican

members of Congress has widened considerably (Poole and Rosenthal 1997; McCarty, Poole, and Rosenthal 2006). Democratic and Republican senators representing the same states typically differ more in their voting behavior than senators of the same party representing the most liberal and most conservative states in the country (Bartels 2008, chap. 9). And Democratic and Republican presidents have historically presided over vastly disparate economic fortunes for middle-class and poor people (Bartels 2008, chap. 2). However, despite having observed these impressive partisan contrasts, we are far from having a detailed understanding of the policy consequences of election outcomes on an issue-by-issue and context-by-context basis.

The most ambitious attempt by contemporary scholars to integrate analyses of public opinion, electoral behavior, party politics, and public policy is Robert Erikson, Michael MacKuen, and James Stimson's (2002) volume, *The Macro Polity*. Building on a series of related studies of "public mood," presidential approval, "macropartisanship," and dynamic representation, Erikson, MacKuen, and Stimson developed an impressively comprehensive "system model" in which the broad policy choices of elected officials both reflect and help to shape broad currents of public opinion. Although the authors of *The Macro Polity* stressed the direct responsiveness of governmental policy to shifts in public sentiment, their findings imply that the policy changes that would be produced by shifting from the most liberal public mood on record to the most conservative public mood on record are dwarfed by the changes produced when a typical Democrat replaces a typical Republican in the White House (Erikson, MacKuen, and Stimson 2002, chap. 8).

These partisan disparities in policy cast considerable doubt on the political relevance of the median voter theorem developed by Anthony Downs, Duncan Black,

and their successors. They also cast considerable light on the political consequences of electoral politics in the contemporary American setting. Elections matter, and thus so does electoral behavior. But if election outcomes largely drive policy, what drives election outcomes?

Erikson, MacKuen, and Stimson's analyses of election outcomes focus on "rational retrospections" based on the state of the economy, judgments of ideological proximity, and shifting partisan loyalties and presidential approval reflecting these and other factors. Inevitably, in an analysis focusing on 12 presidential election outcomes, the ratio of explanatory variables to data is disconcertingly high—I count 17 distinct regression models with an average of 3.7 parameter estimates each, plus additional analyses of congressional election outcomes. However, even aside from the inevitable fragility of the empirical results, which the authors duly note, there is the nagging question of how much is really being explained. Much of the impressive statistical performance of these regression models turns out to be attributable to "transient macropartisanship"—shifts in partisan sentiment over the course of the election year that are "volatile and essentially uncorrelated with the other variables of the model These otherwise unaccounted-for causes explain a considerable share of the outcome" (Erikson, MacKuen, and Stimson 2002, 272). If the future course of public policy is powerfully shaped by "volatile" and "unaccounted-for causes" of election outcomes, is *The Macro Polity* really such a well-oiled democratic machine? This is one point at which broad integrative analysis will have to build upon a more detailed understanding of electoral behavior.

For that to happen, scholars of electoral behavior will have to keep the big picture clearly in view. In the opening paragraph of the preface of his synthetic large-scale

survey of *Public Opinion and American Democracy*, V. O. Key, Jr. complained that the sociologists and social psychologists who had taken up the study of public opinion and elections since the 1940s had produced “a large body of research findings ... whose relevance for the workings of the governmental system is not always apparent” (Key 1961, vii). Too much current work is equally vulnerable to that criticism. However, the best contemporary work, like Erikson, MacKuen, and Stimson’s, reminds us that that need not be the case.

Although Key didn’t say so, a compelling model of what a *political* science of electoral behavior might look like is his own classic study of *Southern Politics in State and Nation* (Key 1949). Writing in the dark ages before extensive survey research and powerful computing technology, Key drew upon detailed observation, over 500 interviews with politicians and political observers, and rudimentary analysis of aggregate voting patterns to produce a masterful portrait of a political order in which electoral behavior meshed seamlessly with party politics, political culture, and the prevailing realities of economic and social hierarchy. No reader of Key’s book, then or now, would be likely to question the relevance of electoral behavior for the workings of the governmental system he portrayed—or the potential relevance of electoral behavior for undoing it.

Of course, there is no reason for scholars in the 21st century to limit themselves to data and methods of the sort available to Key in the 1940s. With respect to style and technique, the best future work in the field is likely to resemble *The Macro Polity* much more than it does *Southern Politics*. Nevertheless, the people who produce it could do much worse than to draw inspiration from the breadth, depth, and political sophistication of Key’s analysis. That sort of broadening and deepening seems

essential if future scholars of electoral behavior are to achieve the degree of intellectual influence and political relevance aspired to—and sometimes attained—by such past masters as Converse, Lazarsfeld, Miller, Stokes, and Key himself.

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