The Evolution of Political Knowledge

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COMMENTARY
Opinion and Action in Postwar America:
Additional Perspectives

PAUL ALLEN BECK

The best reviews of scholarly fields perform three functions. They synthesize the corpus of research in the field, identifying and evaluating the most important streams of knowledge within it. They lay out, explicitly and implicitly, an agenda for future inquiry. They stimulate thought about the difference between what we know and what we should want to know about the field’s primary subjects of inquiry. Donald Kinder’s essay performs these functions admirably for the field of mass politics as it stood at the turn of the century. My commentary merely provides additional thoughts that are provoked by his rich, thoughtful, and (I think) highly accurate rendering of our field.

The Research Synthesis

This essay provides as good a snapshot of the field as one could hope for in around fifty pages. It will be a useful guide to scholars, young and old. I do want to take issue, however, with several of his conclusions.

First, I doubt that we have “enough already about ideology,” as Kinder so succinctly puts it. Scholars, even some mass politics scholars, continue to resist Converse’s great insight that Americans are largely innocent of ideology, so it is important to reiterate it at every turn. Furthermore, even if ideology as a coherent belief system that is shared between elites and masses is rare among the mass public, ideological self-identifications are common, and they are meaningfully related to party identifications and votes for many ordinary citizens. More attention to the role of ideological self-identifications and how it can be squared with the absence of wide-ranging belief systems is necessary.

Second, nor am I as quick as Kinder is to write off material interests as an influence on public opinion. On the vast array of opinions that Americans so freely offer in response to opinion surveys, material interests surely do play an insignificant role. Why should they be important? As Zaller (1992) has so effectively shown, these are typically “top of the head” opinions, often reflecting non-attitudes. As such, they are rooted more in immediate considerations of today’s headlines and question wording than in enduring evaluations of self-interest. Another problem is that our instruments for measuring material interest are often blunt, based as they typically are on standard demographic variables, or insufficiently fine-tuned in national surveys to be connected to non-national
issues (e.g., school bussing). All in all, while I would echo Kinder’s point that the right circumstances for self interest to make a difference don’t come along very often, they also are limited by our measures and our research designs. I am not ready to infer that “self-interest does not take us very far in understanding public opinion,” especially when it comes to opinions that really matter for political behavior.

Third, in an otherwise sensitive treatment of political participation, Kinder makes too much of the costs of registration as a barrier to voting and of declining rates of turnout in American elections. Both points are conventional wisdom about participation in America, to be sure, but neither may be accurate with regard to turnout in recent years. With demolition of the most oppressive legal and administrative barriers and now the Motor Voter Law of 1993, registration is a far less imposing barrier to turnout today than before; and state-by-state differences in its requirements have dwindled considerably. The result is a restriction in the amount of non-voting that can be attributed to its “costs” and an increase in the explanatory power of motivations to participate that are affected by such factors as grassroots party efforts and other campaign activities, voter resources, and civic attitudes. With the lowering of the costs of voting in the U.S., then, in some ways the mystery of non-voting becomes even more difficult to clear up. Scholars need to be addressing this mystery anew.

Moreover, there is considerable question these days about how low American turnout really is relative to levels in other countries and at other times. The problem of estimating actual turnout is compounded on both sides: As was exemplified in the Florida controversy over the 2000 presidential vote, many more Americans vote than are recorded as voting in official statistics. Then there is the problem, in a nation with a large non-citizen population and restrictions on the voting rights of convicted felons in many states, of determining the eligible electorate. These frailties in numerator and denominator of the turnout formula, along with the likelihood that they vary considerably over time and place, make longitudinal and cross-national comparisons of turnout problematic. There is no question that turnout in contemporary American elections is low by most standards, but whether it actually has declined since the early 1960s is more debatable.

Fourth, to my taste, the Kinder account views the voter far too much as an autonomous decision maker. In recent years, scholars have turned their attention to how much the voter’s immediate context matters—including which media voters follow, with whom they talk about politics, to what organizations they belong, and how much party activity is present in their locale (see, e.g., Beck, Dalton, Greene, and Huckfeldt 2002). In a political system in which virtually all information is mediated through these and other sources, the political bias of the intermediaries can be quite consequential. Adding a social “calculus” to the personal calculus that has dominated mass politics research since the movement
away from the Columbia school in the 1950s has been an important recent development in mass politics research.

Fifth, there is too little politics in Kinder’s account of framing, priming, and agenda-setting. Amidst the dominance of perspectives from micro-level political psychology and its experimental designs, it is easy to overlook the important role played by the nature of prevailing elite political conflict in setting agendas, framing political issues, and priming political evaluations. From this long-hallowed perspective of scholars such as Key (1955), Lipset and Rokkan (1967), Schattschneider (1960), and Burnham (1970), the most important framing, priming, and agenda-setting are the work of parties and their candidates at important turning points and then repetitively across multiple elections—as political cleavages emerge, stabilize, and change in realignments. The transition, for example, from the System of 1896 to the class-based cleavage of the New Deal system was far more consequential for how ordinary people were involved in political life than any specific media or discussion effects. As Schattschneider (1960, 62) so elegantly put it: “What happens in politics depends on the way in which people are divided into factions, parties, groups, classes, etc. The outcome of the game of politics depends on which of a multitude of possible conflicts gains the dominant position.” And, in this process, political elites are the principal agents.

The Agenda for Future Research

Professor Kinder is wise to place on the research agenda attention to: recent developments in the psychological study of attitudes that emphasize unconscious activation; the need for understanding how information is created and disseminated; and what can be gained from understanding the interaction between psychology and economics. Long seen as competing, even conflicting perspectives, economics and psychology are speaking more to one another these days and offer complementary insights into mass political behavior. To his list, I would add the following:

First, and most important, a serious challenge is looming to survey research, which has been the bedrock data collection method for the study of mass political behavior, especially now that it has become a medium for conducting creative experiments. With growing population dispersion, stronger desires for privacy, and more women in the workplace, personal interviewing has become more and more expensive. With answering machines, caller ID, and call blocking, respondents are ever harder to reach by telephone, so the attractiveness of telephone interviewing as an alternative has been undermined. The consequence is that, overall, response rates are down—disturbingly so. The internet may offer an alternative, but it suffers right now from even more serious selection bias problems. More attention needs to be paid to how scholars will collect reliable...
and valid public opinion data in the future, or the entire foundation of our enterprise will be undermined.

Second, our understanding of mass politics is based heavily on studies of national elections and opinion with national samples. Yet, considerable citizen political behavior comes in response to subnational contests and contexts—where, *inter alia*, turnout is much lower, candidates are much less visible, and issues are sometimes decided directly in initiatives and referenda. Interesting research is being conducted on such topics, but we need to encourage more of it and pay more attention to it in developing our theories of mass politics. Systematic study of subnational politics remains, as it has for decades, an important frontier to be explored.

Third, the full development of mass politics theory also requires more reliance upon longitudinal and cross-national comparison. It is a failing of the field, not of Kinder’s particular rendition of it, that it pays insufficient attention to how what we think we know changes across different times even in the United States, much less across nations. For example, it would be useful to specify how much mass political opinion and choice is structured by the power of political parties as institutional actors or by the nature of the electoral system. It is a tall order indeed to require mass politics scholars to be aware of how contingent their knowledge is upon time and place, but this is the necessary next step in the development of our theory.

Finally, in a field that studies opinions and preferences, it is puzzling that we still know so little about their sources—and that attention to this research question has waned in recent years. Partisanship of course is a well-known fount of preferences on many political issues, and its roots in childhood and in the times of first entry into politics are now well established. Attention to the childhood roots of political behavior beyond partisanship, however, dwindled with the inflated promise and, consequently, the disappointing payoff of early political socialization research. Kinder is wise to focus our attention on the role of political communications and on such “first principles” as individualism, equality, and limited government. His own research has pointed to the importance of symbolic politics and prejudice. Nonetheless, there is considerable work yet to be done on improving our understanding of where principles and preferences come from in the first place.

The Imbalance in Mass Politics Research

Donald Kinder’s essay provides us with a good account of the present state of knowledge in the mass politics field. Yet, I am troubled by the reality it depicts. To my mind, an imbalance has emerged in the field’s attention to more versus less consequential mass political behaviors. The public is more consequential in
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its political action and its electoral choices, yet we seem to study participation and voting less than we did in the past. Paradoxically, even as ANES is accumulating enough cases over fifty years to support genuine comparisons across different electoral contexts, it seems less central to the field and its very continuation is threatened by funding declines. Despite the attention paid by leaders to public opinion polls, the mass public is less consequential in its top-of-the-head opinions about public policy issues, and perhaps even in those attitudes that can be altered by experimental manipulations. Yet the study of public opinion, buoyed by ingenious survey experiments and attractive theorizing, has become a growth industry. This is excellent and exciting research in many cases, and all of us have learned a great deal from it. Moreover, there is a recognized ebb and flow to the topics of research, as scholars’ agendas are set by new questions, available data, cutting-edge methodologies, and compelling theories. We need to be careful, however, not to let the field’s research agendas become dominated by studies of public opinion to the neglect of research on the more consequential political behaviors manifested in electoral decisions and political action.
COMMENTARY

Lots of Opinion, Not Much Action: The Study of Mass Politics from Key to Kinder

LARRY M. BARTELS

Donald Kinder’s essay, “Pale Democracy: Opinion and Action in Post-War America,” provides an impressively comprehensive, judicious, and engaging tour of scholarly work on mass politics in the four decades since the publication in 1961 of V. O. Key’s magisterial Public Opinion and American Democracy. Since I can find little to disagree with in Kinder’s splendid summary of the current state of the field, I propose to focus here on some issues that both he and the literature he is reviewing seem to me to have short-changed. In particular, I want to argue that, despite the variety and richness applauded by Kinder, contemporary scholarly studies of mass politics have mostly failed to elucidate the role of ordinary citizens in the broader political process. We know a good deal about where public opinion comes from and what it looks like, but depressingly little about how and why it matters.

Key (1961) defined public opinion as “those opinions held by private persons which governments find it prudent to heed.” Not anything going on in the head of anyone willing to sit still for an opinion survey; not merely the plural of political psychology. Public opinion in Key’s sense is a moving force in the governmental process, and comprehensible only in that broader context. “Mass politics” is an even more expansive topic, encompassing (at least in principle) everything from New England town meetings to revolutions; but here, too, some sort of real or potential political impact seems implicit in the term, and tracing that impact ought to be a primary task of scholars working in the field.

In striking contrast, Kinder’s account—and the scholarly literature he was assigned to survey—focuses overwhelmingly on the content, structure, and bases of political attitudes as reflected in opinion surveys, usually examined in utter isolation from the broader political process. We correlate this attitude with that attitude, perhaps appealing to some not-quite-up-to-date psychological theorizing to motivate the exercise. Sometimes we examine changes in attitudes attributable to candidates, the mass media, or external political events, but the flow of causation is almost always from the outside in, rather than from mass political attitudes to consequential political action. Indeed, we seldom study any political act more tangible than (self-reported) voting behavior. It can hardly be surprising that scholars in other parts of political science sometimes bemoan the squandering of so much intellectual energy and
“methodological virtuosity” (Key 1961, vii) on phenomena of such modest apparent political import.

Perhaps ironically, Kinder criticizes research on “action frames” in the social movements literature for failing to provide “empirical studies in natural settings” demonstrating that efforts by movement leaders to frame issues in one way rather than another have real political consequences. Fair enough; but he might just as well have criticized almost any of the hundreds of studies cited in his review on similar grounds. Real political consequences are simply not our strong suit, nor are “empirical studies in natural settings”; our focus, by and large, is on attitudes, and our scholarly tool of choice is the opinion survey.

It is hard not to be impressed by the sheer accumulation of survey data in the field of mass politics over the past forty years. What is now the American National Election Studies (NES) project has grown from a single book based on two national surveys at the time Key wrote into a highly institutionalized and truly national research enterprise; it offers easy scholarly access to a cumulative data file including almost 50,000 individual survey respondents in more than two dozen national elections spanning half a century, and it has generated a cumulative bibliography consisting of approximately 3000 books, articles, dissertations, and other scholarly works. Meanwhile, a single national survey mounted during the 2000 presidential election campaign by the University of Pennsylvania’s Annenberg School for Communication generated 100,000 interviews with 80,000 respondents (Hagen, Johnston, and Jamieson 2002). A wealth of data from Gallup and other commercial polls has become more readily available for scholarly analysis. And the Eurobarometer series, the World Values surveys, the Comparative Study of Electoral Systems, and other cross-national survey projects have begun to erode the long-standing provincialism of U.S. research on public opinion.

All of this is very much to the good. However, the availability of richly detailed survey data has probably distracted scholarly attention from crucial aspects of public opinion that are not readily operationalized in surveys. For example, Converse’s (1964, 245–46) “issue publics”—the various minorities of the public whose “activated” opinions on particular issues of interest are “expressed in the writing of letters to the editor, the changing of votes, and the like”—have seldom been subjected to detailed scrutiny. In part, I suspect, that is because the notion of issue publics fits uncomfortably with conventional democratic ideology; but in part it must also reflect the difficulty of studying small, distinct subgroups in large national surveys. In this case, among others, scholars will have to wean themselves from surveys and devise more ingenious and eclectic approaches to collecting and analyzing relevant data if they are to bridge the divide between opinion and action.

The literature on representation seems to me to be much better developed in

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this respect, thanks in significant part to the pathbreaking study by Miller and Stokes (1963) of “Constituency Influence in Congress.” Their work provided a template for much subsequent research examining the relationship between public opinion and elite attitudes and behavior, and it still serves as a beautiful model of how to employ survey data in the context of a broader and richer research agenda. On the other hand, the basic research design employed in their study and subsequent studies like it—cross-sectional analysis of the correlation between constituents’ opinions and the opinions or behavior of individual legislators—is arguably ill-suited to answering questions about the political impact of public opinion in the aggregate, especially in legislative systems marked by strong party discipline or proportional representation, or when crucial policy decisions are made by executives or bureaucrats rather than by legislatures.

A promising alternative research design focuses on covariation between aggregate public opinion and policy outcomes across political units or in a single political unit over time. The analysis of “dynamic representation” by Stimson, MacKuen, and Erikson (1995) is an ambitious recent example of this approach, relating variations in the liberalism of America’s “public mood” over a 35-year period to subsequent variations in the behavior of presidents, Congresses, and courts. The same authors’ even more ambitious study of what they call The Macro Polity (Erikson, MacKuen, and Stimson 2002) is an even more powerful example of what can be done when survey data are harnessed to a broader research agenda rather than being analyzed in not-so-splendid isolation.

Another quite successful example of the sort of research I have in mind can be found in the literature on presidential approval. Neustadt’s (1960) classic study of Presidential Power called attention to “public prestige” as an important resource for presidents wanting to make things happen in Washington. Mueller’s (1973) pioneering analysis of trends in presidential approval as measured by Gallup Polls identified several important patterns in public support for presidents, including “honeymoon” effects and secular declines in support, punishments for economic slumps, “rally round the flag” responses to foreign crises, and sensitivity to cumulative war casualties. An enormous subsequent literature has refined, modified, and extended Mueller’s analysis of the Gallup data. Meanwhile, Rivers and Rose (1985) have provided a sophisticated test of Neustadt’s contentions about the impact of presidential popularity on legislative support for the president’s policy agenda, and Kernell (1997) has supplied a more general analysis of how and why contemporary presidents “go public,” and with what effect. While none of this work has the psychological richness of the related studies of priming cited (and, in significant part, generated) by Kinder, it is of far greater political significance.

A much less successful, albeit more typical, example of research on mass politics is provided by the literature on political participation. Obviously, as
Kinder’s review makes clear, a wealth of recent work in this area has enriched our understanding of when and why ordinary citizens vote, attend meetings, contribute money to political causes, and the like. I certainly share his enthusiasm for Rosenstone and Hansen’s (1993) study of electoral mobilization, Verba, Schlozman, and Brady’s (1995) “Civic Voluntarism Model,” and Gerber and Green’s (2000) field experiments on the effect of nonpartisan canvassing, among others works. But what does all this research tell us about the workings of the broader political system? Kinder asserts, much in the spirit of the work he is summarizing, that “Because citizens express their aspirations and defend their interests through participation, failing to take part diminishes the chances that they will get what they need.” But nowhere does he cite—and nowhere can I find in the literature—compelling evidence for that familiar assertion. Scholars of political participation treat it almost exclusively as a dependent variable; demonstrating the political consequences of differential participation is, apparently, someone else’s problem. But whose?

The literature on political participation is certainly not unusual in this respect. The burgeoning literature on political information and democratic competence gets pride of place in Kinder’s survey; but it is striking how little of this work moves beyond the opinion survey or experimental laboratory to investigate the political consequences of pervasive political ignorance. The much larger literature on voting behavior and elections suffers from much the same problem. Kel- ley’s (1983) work on Interpreting Elections and Miller and Shanks’s (1982) analysis elaborating the distinction between important causes of individual vote choices and important causes of election outcomes are rare in emphasizing the broader implications of voting behavior; Conley’s (2001) study of Presidential Mandates is even rarer 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.

Finally, it seems fitting to end where Kinder started, with V. O. Key’s Public Opinion and American Democracy. The most innovative and politically important aspect of Key’s analysis was his emphasis on “latent opinion”—the preferences and beliefs politicians expect to confront in future elections, after today’s policies have had their effects and tomorrow’s political opponents have had their say. That notion of what opinion matters, and how and why, will probably feel familiar to anyone who studies the policy-making process; but it is remarkably invisible in the massive literature on public opinion surveyed by Kinder. In part that is an understandable, if unfortunate, consequences of the sheer elusiveness of latent opinion. But in part, as with Converse’s issue publics, the neglect probably also reflects the obvious futility of relying solely on survey data to measure opinions that don’t yet exist and may never exist.

A few scholars have employed the concept of latent opinion quite fruitfully.
For example, Arnold (1992) has applied Key’s logic carefully and creatively to congressional policy making, while Zaller (1994) has used it to illuminate the role of public opinion in the run-up to the Gulf War. However, neither of these works relies directly on survey data, and neither figures in Kinder’s exhaustive survey of the field. And notwithstanding a few exceptions like these, difficulties of conceptualization and measurement have discouraged political scientists from giving Key’s notion of latent opinion attention commensurate with its undoubted political significance.

Kinder notes in passing that “Key complained about social psychologists hijacking the study of public opinion.” But as something of a social psychologist himself (and the author of an even more comprehensive literature review in the Handbook of Social Psychology from which the present work is derived), Kinder is perhaps not well placed to assess the justice of that complaint, or to pinpoint its ramifications for the field. For what it is worth, my own view is that Key had it right. Scholarly research on mass politics will have taken a big step forward when it begins to focus less on opinion and more on action – and to inherit its preoccupations less from social psychology and more from V. O. Key.

Pale democracy? Well, maybe. But until the study of public opinion and political behavior is more thoroughly integrated into the mainstream of political science, it will be very hard for us to tell.

Notes


2. “Political behavior” is another conventional label for what is essentially the same field; unfortunately, the phenomena designated by that label are often only tangentially “political” and not even tangentially “behavior.” With the field of “comparative politics” having become somewhat more comparative in recent years, the term “political behavior” may now be the most egregious misnomer in political science.

3. Information on the NES organization and access to data, codebooks, and related material are available from the NES website, http://www.umich.edu/~nes/.


COMMENTARY

Then and Now: A Commentary on Kinder’s “Opinion and Action in Postwar America”

JAMES A. STIMSON

Self-flagellation is an occupational disease of the social scientist. It is our custom to fill our journals with complaints that nothing ever done on a particular topic was worth doing. All ill-conceived, ill-designed, invariably that which came before failed to ask the right question. We write like morticians speak. They are in the business of sharing their customers’ grief. Therefore they must learn to affect an air of misfortune; upbeat and cheerful wouldn’t do. It is a business necessity, but evidently insincere.

Aside from such necessary common words as “the,” “is,” and “and,” “unfortunately” is probably the most common word in our introductory paragraphs. We write like morticians and we, too, are insincere. In the long list of life’s troubles, the fact that no other scholar has ever addressed exactly the same question as the present work is not the stuff of poverty, disease, or famine. The claim indeed is pretty often the exact opposite of the truth. “Fortunately,” we should write, “previous work does not ask exactly the same questions, use the same methods, or reach the same conclusions as that which is to follow.”

I find myself in the same logical trap as the charming bumper sticker that asserts, “All generalizations are false.” I am flagellating my discipline for the sin of self-flagellation. But it will do. This is a commentary and logical consistency is a desired attribute only of works commented upon.

Don Kinder is no mortician. This lengthy compendium of what we know about voting, elections, public opinion, and so forth has an air of pride in what we have achieved. Kinder thinks we know a lot about the topic. That is the overriding impression of my reading.

In his telling there is scientific progress on every front. There is even, dare we say it, cumulation of knowledge. I think we see here the danger of letting people practice political science without a degree in the discipline. This is an evident failure of professional socialization.

But my task as commentator is not merely to describe Kinder’s work, but to put its claims to the test. I can’t take him to task for using the wrong estimator. Counting and adding up are mainly what he does. Nor can I chastise him for erroneous design; it is his lifelong reading that is the design of the chapter.

So I have decided to test the optimistic appraisal, to ask whether we have learned as much as Kinder asserts that we have. My design is a time series quasi-experiment, which is to say, a comparison of “then” and “now.” “Now” is
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easy to fix; it is August 2001 (when Kinder’s initial essay and my response were written). “Then” is a trickier decision. Any number of baselines would well serve the purpose of comparison. The time I choose for my beginning point is literally my beginning point, the state of knowledge about elections, opinion, representation, and all the rest when I first came on the scene. That has a huge advantage; I can remember the state of knowledge better than at any subsequent time.

So let me set the stage about “then.” It was 1964. That was when I first encountered political behavior and its most important product, _The American Voter_ (1960). That book clearly defined the agenda of political behavior. Most of the subtopics of the 2001 Kinder essay, for example, are seen in questions raised and analyses performed by Campbell, Converse, Miller, and Stokes.

Here is the design. Kinder asserts what we know at what we’ll call $t_1$, 2001. I will assess what we knew at $t_0$, 1964. And then we can quantify the result: $(t_1 - t_0)/37$. In my assigned page limit I can’t begin the match Kinder’s coverage of subtopics. But fortunately there are several on which the questions had yet to be first raised at time 0, and so we can assign zeros to our knowledge summaries without discussion.

So what was the state of play in 1964. _The American Voter_ was then recent, but no longer brand new. (I would have had to have read it in high school to have been more timely.) We had the NES studies (not yet called by that name) of 1952, 1956, 1960; the representation study of 1958; and a tiny, almost unused, off-year study of 1962. “The Nature of Belief Systems in Mass Publics” would have been in press. And the rich analyses that later appeared in _Elections and the Political Order_ (1966), would have been mostly completed, but not yet in my view.

Voting behavior, mainly but not exclusively focusing on why citizens made the choices of parties and candidates that they did, was then in full blossom. To me at least it was a full-blown paradigm in the sense that T. S. Kuhn (1962) elaborated. An obvious success, it begged the reader to undertake more work of the same kind, asking similar questions and using similar methods. The glow of early success made the future look unlimited.

Although voting should be a central topic for any worthy science of politics, scholarship on the vote in the 1960s had a gold rush air to it. It seemed at the time as if a third or a half of all active political scientists in the 1960s cohorts were exploring the vote. The Michigan studies were wearing out card readers—and later tape drives—from heavy use. Called various things, most often political behavior (although there was never much interest in behavior), it seemed as if the voting crowd might branch out and spread their gospel to all the questions that mattered in politics.

Part of the early excitement was that it seemed that quite basic questions could be answered simply and decisively by the next analysis. Was it candidates who
tipped the scale of voting choice or parties? All one had to do to answer this fundamental question, it seemed, was to get measures of both variables in a table—regression was then a bit daring for political science. You could see which was more important and the question would have been answered for all time. Then on to the next question! And if we didn’t yet have quite the right measures to do the test, surely they would be included in the next Michigan study.

The simplicity was intoxicating. And of course it was also false. And that is why the endeavor to explain the vote, which looked so promising then, would be all but abandoned within a few short years. Blossoms are most brilliant just before they wilt. Within a decade or so we would discover from two parallel attempts to bring the best statistical design and technology to modeling the vote (Markus and Converse 1979; Page and Jones 1979) that the voting-choice issue, far from simple, was fraught with difficulties. The challenge wasn’t just to be smarter, to build more esoteric models, it was also to acquire better data, better than we knew how to get. For if everything is endogenous with respect to almost everything else, we needed powerful instruments to estimate the model. We did not have them. We did not know where to get them.

This outcome perhaps deserves the term “unfortunate.” Or does it? All that power was chimera. This endeavor, then pretty clearly the centerpiece of the political behavior program, ended in frustration. Large numbers of scholars (and I clearly remember myself as one) became intellectually unemployed. Prepared to spend their lives attacking a set of questions, they discovered that the questions were probably unanswerable. That had its fortunate side, for a lot of talent was thereby liberated.

Some behavior types slipped into dissipation and wasted lives, e.g., they became deans. Others had to find some other worthy questions for their analytic talents. And much of the expanded agenda of which Kinder writes is the result. Where once one could, with considerable accuracy, describe the crowd who analyzed survey data as students of “voting behavior,” they and their scholarly descendents now spend whole careers doing works that never include vote choice on the left hand side of an equation. It is awkward, for we don’t even have a proper name for the set of topics, elections, opinion, participation, representation, political psychology, media impact, macroanalyses, social identity, campaigns, and more. What they have in common is that they focus on the “public” and absorb much of the talent formerly devoted to “voting behavior.”

What was the state of knowledge in these “other” areas in 1964? Of elections and alignments we had Key’s early works (1955, 1959) and little more. The “realignment” literature would mostly come later, and later still the critical response. “Political economy” was then was a nineteenth-century usage, having little to do with politics or economics. Largely (not wholly) overlooked in The American Voter, the economic side of politics was popular lore about pocket-
books. Better suited to the analysis of aggregate time series, knowledge of political economy awaited a time series scholarship not then part of political science (or much else) and, more urgently, the existence of time series to be analyzed.

Of ideology, the best work that would ever be done was the summation of Converse’s several efforts, then in press. But cognitive psychology has subsequently provided richer theories and better expectations. And the structure so clearly absent from most individual voters would decades later be found in aggregate movements over time.

What became a literature on participation was already well rooted in The American Voter in 1964. Recent work enriches, but hasn’t fundamentally changed our understanding. On representation, the pioneering work of Miller and Stokes (1963) inspired by the sweep of its ambition, but foundered on problems of design. The sample survey had little leverage on the question at hand. Connecting the views of individual survey respondents to those who represented them in Congress was unable to cope with elections as an aggregation of social choice, the wrong level of analysis. Getting closer to the linkage awaited the powerhouse data of the Wright, Erikson, and McIver (1994) state-level analysis and the longitudinal focus of my own coauthored work (Stimson, MacKuen, and Erikson 1995; Erikson, MacKuen, and Stimson 2002).

On voting itself we have learned some things since The American Voter; chiefly in the burst of scholarship of the early and mid-1960s. But that wonderful book is still a pretty good summary of what we know. Where some of the answers have changed with new data and revisionist perspectives, the agenda is still remarkably intact. The American Voter tells us how much typical voters know or care about politics, little, how they structure their ideas, or fail to, the range of considerations that end up in the voting decision, the quasi-permanence of identification with party, the interpretation of election outcomes, and more.

Other areas of scholarship are almost wholly novel. The power of heuristics to solve what once seemed insurmountable cognitive requirements and limitations is virtually all new knowledge. I take the on-line processing theory to be worth more emphasis than Kinder gives it. I see it as wonderfully powerful to solve the dilemma that has haunted voting since Key argued with Campbell, Converse, Miller, and Stokes that voters could at the same time lack the information necessary for purposive action and still seem to act with purpose. Our understanding of media effects is both old and new, but what we now regard as the correct version is wholly new. Unlike the work on voting, the early media analyses are cited chiefly these days in arguments that they were wrong in their most fundamental conclusion.

Partisanship is a matter not to be taken up in passing. It is impressive in the complexities its throws at scholars seeking easy understanding. But we do know one thing of fundamental importance, that though stable it is not fixed. And the
things that move it comes from normal political events and circumstances. We knew the stability story, still accurate in its main outline, from early work. We have learned that small but politically consequential changes move around that stability. A more general message emerges, one that early survey analysts looking for big relationships were bound to get wrong; change that is consequential for politics happens at the margin. Although partisanship is impressively stable, the small changes that do occur have impressive consequences for the outcomes of politics.

So, How Much Is New in Thirty-seven Years?

Kinder suggests that most of our knowledge in these areas is new. Although he doesn’t say it directly and I may be adding unintended interpretation, that at least is my take on the issue, that what I can count as new knowledge, divided by the elapsed time in gaining it, is a more than satisfactory payoff for our efforts. What then about the future?

Topics for Future Research

My heading here is a familiar one, perhaps painfully familiar. Just as we often begin our papers with the “unfortunatelies,” bemoaning the evident stupidity of all who came before, we end them with suggestions for future research. This, too, is disingenuous. We write, in essence, “Here is a list of topics I might have studied, but did not find worthy of my time. You, the reader, should do the work that I chose not to do.” So what is it that I should suggest (while I do something else)?

Actually, it was Kinder’s task, a brief effort in an otherwise lengthy paper. Perhaps that’s as it should be. I think our ability to anticipate the twists and turns of scholarship founders on the fundamental path dependence of the process. It is probably little better than financial markets’ notable inability to forecast turning points, and partly for reasons that are similar. It would be intriguing to view others’ glimpses into the future from times past to see how often they got it right and how often they were slipping off the intellectual cliff, undermined by work in progress as they wrote. I suggest this, tongue in cheek, as a topic for future research.

Notes

1. Always the empiricist, I did a word search on the electronic draft I have and found no instances of “unfortunately,” and one of the rarely seen opposite, “fortunately.”
2. They were unemployed, but not homeless. They had Ann Arbor.