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Public Opinion in America

Moods, Cycles, and Swings

Second Edition

James A. Stimson

University of North Carolina at Chapel Hill

Routledge
Taylor & Francis Group
New York  London
To Dianne Stimson
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The living impressions of a large number of people are to an immeasurable degree personal in each of them, and unmanageably complex in the mass. How then is any practical relationship established between what is in people’s heads and what is out there beyond their ken in the environment? How in the language of Democratic theory, do great numbers of people feeling each so privately about so abstract a picture, develop a common will? How does a simple and constant idea emerge from this complex of variables? How are those things known as the Will of the People, or the National Purpose, or Public Opinion crystallized out of such fleeting and casual imagery? . . .

The working of the popular will, therefore, has always called for explanation . . . what Sir Robert Peel called “that great compound of folly, weakness, prejudice, wrong feeling, right feeling, obstinacy and newspaper paragraphs which is called public opinion.” Others have concluded that since out of drift and incoherence, settled aims do appear, there must be a mysterious contrivance at work somewhere over and above the inhabitants of a nation. They invoke a collective soul, a national mind, a spirit of the age which imposes order upon random opinion.

—Walter Lippmann, *Public Opinion*, 1922
Preface to the Second Edition

The idea of electoral moods is old, older than can be traced. But the concept of public policy mood and its indicator were my own thing when I wrote the first edition of this book. I did not wish them to remain so, for that would have meant that scholars had ignored my attempts to make this idea part of political science and public commentary on politics. The effort—which was considerable—in bringing this thing to life would have been wasted if it had remained only my private possession. I am gratified that it did not.

The mood concept and indicator have become part of political science. Used first by friends and graduate students, then by others in the small set of scholars devoted to longitudinal research on American politics, the concept has worked its way ultimately into the hands of scholars I do not know, writing on topics beyond my knowledge.1 It is not mine anymore.

Some of the new scholarship on mood is my own, written with co-authors Robert Erikson and Michael MacKuen. I correctly anticipated both that I would some day address the dynamic representation thesis and that it would take a while. The product (Stimson, MacKuen, and Erikson 1995) demonstrates that all elected organs of American government are highly responsive to changes of public mood—much more so than I would have anticipated, and I think much more so than is generally believed, and not only by cynical observers.2 So the work is no longer “unfinished,” as I asserted in the original Preface.

Between editions the research program of this book continued at the University of Minnesota and at the Center for Advanced Study in the Behavioral Sciences. Paul Kellstedt and later Larry Grossback assisted in the updating and data management processes that are essential to this ongoing endeavor. I have incurred debts in this second installment to the University of North Carolina at Chapel Hill, to its Institute for Research in the Social Sciences, to Leo Wiegman (executive editor for Westview Press), and to Russell Dean (for assistance in updating the many data displays in the book).

By far my greatest debt, and one for which it is no longer possible to express my gratitude, is to Robert H. Durr. Bob Durr joined this endeavor as the first edition was nearly completed. He essentially took over getting
that manuscript to press and was instrumental in all the research that fol-
lowed, particularly the “dynamic representation” work cited above.
Beginning as a research assistant, he became a source of advice, a won-
derful critic, and, most a all, a friend. His doctoral dissertation at the
University of Iowa took up the problem of explaining the origins of
mood, work later published in the leading journals of political science.
Bob died of cancer in 1996, his tragically short career encapsulated be-
tween the two editions of this book. I wish it were not so.

James A. Stimson
Preface to the First Edition:
An Unfinished Essay?

Looking back, you could see it. For a period, roughly that of the Carter administration, it was afoot. A quiet rumble of change moved through public views of government. None of its pieces was a stunning reversal, none so unlikely as to signal much by itself. But one segment of the public and one issue at a time, the small changes back and forth started instead to be changes all in one direction.

Americans were beginning to turn away from the long-preferred solutions to public problems. They had called upon government to protect health and safety in the workplace, to defend the natural environment, to promote equity in social relations. They had been doing so in these and dozens of other areas for some time. The direction of change most of the time was toward more. And then in the late 1970s they began to want less. You could see it in a tax revolt, rippling across the nation from a start in California. To pay less instead of more is of course the desire of all taxpayers always. But this revolt had a different feeling; it was angry, intense. It had more the feel of deadlock over cherished symbols than mere adjustment of state and local finance. You could sense that its angry leaders were disillusioned people, so accustomed to losing that they did not know how to react to success and were not ready for it when it came.

Deregulation was the great innovation in the national politics of the time. We reached the conclusion that government did too much. Whether to promote economic optimality or from anger at the beast itself, we came to view less government as the solution to problems of disparate industries, disparate areas of American life. “There ought to be a law against that,” we had said for forty years. And then we had passed the law and enforced it. It was a habit of American life. Now we were reversing course, advocating fewer laws. We were advocating more freedom to do the things we had once, in the name of reform, restricted.

Like the states, we began to speak of massive cuts in the federal income tax. Proposals of increasing seriousness called for extraordinary cutbacks that were inconsistent with activist government, not cuts of a few billion dollars for fiscal stimulation, but cuts that would require the eventual unraveling of the welfare state. And the more serious advocates also wanted to index tax rates to inflation, a move aimed at permanently restricting
the national government’s revenue base. It would make the nation like
many of the states, conservative in action (if not in preference) because it
lacked the wherewithal to be anything else.

One could see each of these changes in view as separate. Each could be
an aberration, each explainable as a discrete event in its own terms. But
it was hard not to think something more general was going on, a change
in national mood. Conservatism was waiting in the wings. It would peak
with the ascent of Ronald Reagan, but it was under way before him. The
signs now—if not quite so much then—are unmistakable. And Reagan’s
election would be one more change, explainable in its own terms, with-out recourse to shifting national mood.

In the long view this quiet rumble, this changing all in the same direc-
tion in one period, was not unique. Global reversals had happened be-
fore—and have already again. It had seemed the same in the late 1950s,
but then it was a growth of liberalism, across-the-board changes toward
preference for sharper, more powerful expansions of the national gov-
ernment. Then we wanted government to take a role in health care, to
boost the quality of our schools, to bring equity to race relations. And
again all these things could be seen as separate issues, but movement was
in the same direction for all. Then, as in the late 1970s, it was a movement
of frustration as the growing majorities for change saw no result. (That
the procedural conservatism of American political institutions stymied
liberal majorities was the theme of the “reform” literature of the time.)
And then the dam burst and change came in a flood.

These stories of American public opinion are true to historical fact as I
know it. But they are stories, narrative accounts that weave an interpreta-
tion around necessarily selected highlights. That is not what this book is
to be. Instead, we need to go beyond selection and interpretation and try
to deal with what public opinion was. We need to understand and then
measure this thing in a way that doesn’t float on casual opinions, that isn’t
dependent upon choice of interpretative framework. That is the focus of
this book: developing the concept of mood and then measuring it.

Why Unfinished?

Public opinion, like the stripes on the back of a snake, may be described
for the pure curiosity of doing so. But pure curiosity doesn’t take us far.
We like to think that what we describe matters. Do the stripes make a dif-
f erent snake in some Darwinian sense? If so, then the stripes are more
than description and those of us with no professional interest in her-
petology may find our interest captured.

Public opinion too must matter if we are to regard it worth description.
A story of public opinion as it influences grand movements of public pol-
icy is a better story than one of public opinion that influences only itself. It is the story I will someday tell, I hope. But that isn’t the story of this book, because the telling requires a research enterprise of considerable scale. Movements in public policy must somehow be measured and on some scale meaningful over time. And then a link between public opinion and policy will be found. Or it won’t be.

This book is an unfinished essay in the sense that the link of opinion to outcomes is not demonstrated here. Page and Shapiro (1983) draw it quite directly. Wright, Erikson, and McIver (1987) have similarly demonstrated a powerful link between public opinion and policy in state politics. And since at least James Madison, uncounted authors have been comfortable presuming that opinion has consequence for policy. The link is less than certain. But it is not controversial.

This book is unfinished because it is about public opinion only. Opinion, in the naive version of democratic theory embedded in the American culture, moves governments. In the early jargon of social science research it is the “independent variable.” It is the thing thought to move the other thing, policy. But to sustain an interest in the “stripes” of public opinion—which do, incidentally, look a bit like a snake’s stripes when presented as time series—the reader will initially have to grant a presumption that opinion matters.

The book is about American politics because that is what I know and because the empirical materials have to come from some place. Handling this one case is quite enough work, if never enough generality. But there is a happy message in this essay. Public opinion is about as institution-free as anything in politics can be. And it is the specifics of political institutions that so restrict our ability to create theories of general interest. The happy message, then, is that a model of public opinion that works for the American case ought to transfer across national boundaries with a minimum of difficulty. Its central presumptions, of people with attitudes toward public policies that might move in parallel over time, do not require a Congress or Supreme Court. Indeed, not even elections appear in the “finished” version of this unfinished model. The model ought to be applicable to, if different from, everything from town meetings to military dictatorships. There is nothing American about mood. There is nothing specific about it either.

Public opinion is usually pretty specific stuff. Respondents are questioned on issues arising in a single polity, and the questions themselves are often particularistic. That is unavoidable. Without sharp focus, respondents inattentive to political debate aren’t likely to report much worth knowing about their attitudes. Thus, much of our literature on public opinion is particularistic, telling us, for instance, what proportion of respondents would permit legal abortions given specific circumstances.
of mother and fetus. The issues are specific and bound to place. Often too they are bound to time, a central problem to be confronted in this essay.

But necessary as it is, specificity leads us astray in public opinion work. It leads us constantly to presume that the attitudes we measure mean what the questions ask. Without it we ask questions that produce mush for response. With it we want to believe that the crisp response to sharply framed query is a response to the query itself. And so we catalog opinions according to questions that produced them and in the process forget that there might be something like public opinion (singular) that is far more powerful and far more interesting than the questions that are its indicators. That, at least, is the position I will take in this essay: that the good stuff is diffuse and global, not specific.

**Political Science and Journalism**

In American politics there are usually two views of the same public opinion. Journalists and commentators read the polls and make interpretations of what they mean. Opinion analysts of diverse disciplinary background—whom I’ll call political scientists so I don’t constantly repeat that phrase—read the same polls as well. And unlike the journalists’ interpretation, ours is often quite circumscribed by distrust of the data that indicate change, the marginal totals. Because our interests lie elsewhere, the marginal totals are rarely more than a parameter that is useful leverage for a comparison. We don’t often care much about the thing itself.

We coexist; the coexistence is increasingly a peaceful one. Political scientists often read journalistic interpretations of public opinion. Journalists occasionally read our work. Although journalistic practice is somewhat responsive to the more technical work on public opinion, usually the journalists don’t find public opinion scholarship very interesting. The commentators focus on politics here and now. The scholars do not; our excursions into statistical models of decades-old data can be a source of amusement for our journalistic brethren.

There is some respect, back and forth. But we work by very different rules of relevance and evidence. Journalists who write about public opinion often spend a lot of time talking to politicians, themselves astute observers of public opinion. Probably that is a useful focus for their insights, for they get constant input on what is important. We don’t talk to many politicians, and when we do our agenda isn’t “What’s happening now?”

Political scientists who write about public opinion are usually well informed about journalistic commentary. But though we know it, we don’t address it in our work because journalism is not scholarship. What is widely believed by astute observers isn’t a theory or empirical regularity
in need of test. And this is no complaint. That is as it should be. The newspapers cannot set the agenda for scholarship.

Journalists pursue “news” as a criterion of relevance. Change is news. Stability isn’t. Their bias is to see change and novelty that isn’t there. Scholars pursue science as a criterion, which carries another bias altogether. We see the world through theories and models, and that which is not relevant to them we tend not to see at all. Our bias is not to see change and novelty that is really there.

The point to this digression is that journalists have long commented upon public moods and political eras. We political scientists rarely do: Empirical work on public opinion pretty much ignores such ideas. Who is right: the gambling journalists who make much of slim evidence or the cautious political scientists who make little of much? Both—and neither. Journalists, particularly commentators, are more creative, focused on seeing the novelties of political life and bringing meaning to them. Moods and eras are old hat to them; they have seen it all before; they have said it all before. Scholars are more disciplined, and disciplined observation, almost by definition, is not creative. To date, that discipline has largely precluded observation of meaningful movement over the medium term.

I wrote this book, more than other works I have authored, with an intent that it be widely read. I have tried hard to speak to a sophisticated general reader, to break away from the special language of social science that is convenient within its fraternity but a barrier to those outside who prefer their native English. There are occasional points, such as the elections analyses of Chapter 5, where I have had to employ the scholarly toolkit. But I write knowing that those tools will be inaccessible to many readers who, if they are patient with me, will find that the whole message is in the text.

I have tried to limit citations to the scholarly literature. That is easy in part, because this aspect of public opinion, the evolution of general sets of beliefs and views over time, is little studied. But it is hard in part, because to avoid citation is to appropriate merely borrowed ideas for ourselves.

The Essay and the Unfinished Grand Design

The ultimate purpose of this research is the study of representation, specifically, longitudinal representation, or governments responding to shifting public mood. This essay is about public mood only; the representation study is under way as I write. But it is ambitious, and I am plodding. There is reason to expect that its product is some time off.

I have some remarks about public opinion that stand on their own. I have done a good deal of development of theory and operations. I have
collected a great deal of public opinion data. And I am ready to turn my attention away from public opinion; that part of the work is near completion. This somewhat informal essay is a compromise. It is not the ultimate statement of how democratic politics works that I wish to make some day. But it is a statement of completed work that I can make now. And I write it now while it is fresh.

J. A. S.

Notes

1. This process is aided by the public availability of the mood data (in both annual and quarterly versions) on my web site: http://www.unc.edu/~jstimson. I intend for it to remain available and expect to continue to update it for as far ahead as I can now see.


3. Mood as a global sentiment underlying public opinion is an old idea, if not an old scientific idea. My usage of it is most directly influenced by Kingdon's (1984) discussion of "national mood."

4. But in the better jargon of current social research, it quite probably is not "exogenous"—free from the causal influence of anything in the opinion-policy-outcome nexus.

5. This is a point all but unnoted in the newly popular focus on institutions as the central fact of political life. There was a reason the "old" institutionalism fell into disrepute; it was built on particulars, assertions about, for example, House procedures that did not generalize to Congress, let alone legislatures, let alone political decision-making bodies. The "new" institutionalism needs to be wary of the same trap.

Acknowledgments to the First Edition

The research program from which this book derives had its origin, as best I recall it, in a University of Iowa graduate seminar discussion of John Kingdon’s *Agendas, Alternatives, and Public Policies*. My first debts therefore are to John for provoking the discussion and to the students in that seminar (in particular John Heraldson, Eduardo Magalhaes, Glenn Mitchell, Mark Somma, and Steve Nelson) for making it so interesting that I could not put it aside and go on to the next topic. The work proceeded, more or less underground, for two years. During that time virtually everything in the project was tried out on Chris Wlezien, whose critical enthusiasm left its mark.

My largest debt for monetary support is to the National Science Foundation, which funded this research in grant SES-9011807, “Political Eras and Representation.” Support by the University of Iowa’s Bose Endowment was particularly helpful in the earlier, more playful, stages of the research program. An early public presentation, sponsored by Larry Dodd’s American Politics Institute of the University of Colorado, Boulder, was a spur to development. Once formally and more often over food and drink, the work was presented to and tried out on members of the Political Methodology Society at summer gatherings. Among many who contributed something, Neal Beck, Henry Brady, Gary King, Bill Flanigan, and John Freeman left a significant stamp. I am particularly indebted to Chris Achen, who not only suggested the regression estimator I develop in Chapter 3 but also worked through its properties in several lengthy and helpful communications.

Jennifer Knerr, Mike MacKuen, and Tom Smith have been through every page of the manuscript, and all have kept me busy trying to live up to their standards of quality. I fear that I have not done so, but I know their comments moved me in the right direction. MacKuen and Bob Erikson, collaborators in an undertaking to attempt a comprehensive macro formulation of American politics (of which this work should one day be part), have been looking over my shoulder on this for three years. Others who have read, discussed, or criticized pieces of earlier work include Bruce Oppenheimer, Richard Waterman, Kathleen Knight, Carolyn Lewis, Jack Wright, Pev Squire, Cary Covington, Bob Boynton, John
Nelson, Tim Hagle, Art Miller, Lee Epstein, Richard Fenno, Heinz Eulau, Dennis Chong, Ted Carmines, John McIver, Richard Sobel, Warren Miller, and Rick Lau. For a project of short duration (by my plodding standards), quite a number of graduate and undergraduate assistants have contributed their ideas and labors. These include John Heraldson, Glenn Mitchell, Fred Slocum, Suzie Deboef, Bob Durr, Paul Kellstedt, and Barbara Allmart of the University of Iowa and Tami Buhr of Harvard University.

Acquiring public opinion marginals I found is no easy matter. They are all over the place, but one needs to know where to look. Robert Shapiro of Columbia University is a master of the search. I am greatly indebted to him for patiently teaching me how it is done. And often the search was unnecessary because he (and varied coauthors) had already done it so well. Kelly Patterson and Laura Crockett helped to make that knowledge sharing possible. The Roper Center for Public Opinion Research of the University of Connecticut is a principal data source for this research. Marilyn Potter and others of the Roper Center staff have been consistently helpful. National Election Study data were provided by the Interuniversity Consortium for Political and Social Research.

And not by any means least, my thanks go to Larry Dodd for talking me into this project in the first place. Larry, as series editor, and Jennifer Knerr, acquisitions editor for Westview Press, proposed the idea of an extended essay that was the origin of this book.

*J.A.S.*, *August 1990*
Public Opinion?

People in and around government sense a national mood. They are comfortable discussing its content, and believe they know when the mood shifts. The idea goes by different names. . . . But common to all . . . is the notion that a rather large number of people out in the country are thinking along certain common lines, that this national mood changes from one time to another in discernable ways, and that these changes in mood or climate have important impacts on policy agendas and policy outcomes.

—John W. Kingdon, Agendas, Alternatives, and Public Policies

People think about politics. Not often. Not systematically. But they do. Ordinary people do. People, as we say, “on the street,” have views on public affairs. They have concerns about the public order. Sometimes this means the public order only insofar as it affects their lives. But not always. They have ideas of good and bad, of progress and regress. They see the results of government, liking some, disliking others. They connect them, loosely to be sure, to policies. And they come to have views about policies, what works, what doesn’t, what we need more of, what less. This is public opinion.

Usually we overestimate how often people think about politics, how much they care, how thoroughly they see connections between choices and consequences. That, in any case, is our past. Two generations of public opinion scholarship is a corrective, painting a portrait of ordinary people who don’t know much, don’t care much, don’t know or care often. That corrective was sorely needed, for the original views flowed more from normative tilts toward what citizens ought to be than from what they were. For two centuries we tried to see people as democratic theories said they ought to be. And while we did, we didn’t see them as they were.

But the corrective too needs correction. We found that citizens were often not philosophers of the public order. And in finding that, we built a vision of the contrary, citizens as dolts. We imposed models of the informed and thoughtful citizen on the ordinary men and women we
studied. They did not measure up. They did not come close to measuring up. Their ideas seemed ill formed, casually taken up, casually discarded. When we sought structure in individual attitudes, we found instead disorder. We were unprepared to be told that ordinary people didn’t care very much about public life. They told us that they didn’t know very much. And they didn’t seem to mind not knowing. The topic didn’t seem to be worth mastery.

What we found is that citizens, taken one at a time and in the norm, did not seem to be competent by the standards of democratic theory. They could not act as individuals in the prescribed manner. And that may well be true. But should we take them one at a time? Is that the nature of politics? Or is public life instead the life of the herd? We carry normative blinders about this issue. Terms like herd or groupthink express them. We like individuals, distrust aggregations. But one of the plainer facts of everyday life is that individuals do not function as individuals. They are enmeshed in a social environment. They interact. They give, receive, borrow, and steal things from one another. Some of those things are ideas. Some ideas are about politics.

We corner individuals in their living rooms or on their telephones, all by themselves, for a survey. And we come to think of them as individuals, “the respondent.” But “the respondent” is an abstraction of a real person, normally not by himself or herself, whose ideas come only partly—and maybe in pretty small measure—from self. We forget that we are interviewing not a self-contained individual but a spokesperson for the herd. And because ideas are borrowed from the social context, the specific elements of them may be borrowed without much supporting structure.

If our topic is people, then this abstraction as individuals is relatively harmless. If we want to know how people think, then perhaps an order and harmony not of their own creation may be omitted. But if the topic is politics, then it matters a great deal if aggregate opinions are more orderly and meaningful than individual ones. For it is the aggregate that matters in politics. To think otherwise is to confuse our normative preference for individualism with the reality of social order.

Social scientists commonly assert that individuals are real whereas aggregates are abstractions, created for convenience. But except perhaps for hermits, there exist no atomistic individuals whose ideas or context of ideas are wholly their own. The individual too is an abstraction, a slice of the social pie temporarily considered as a unit. This view is so obviously true as to count as a platitude, but it goes against an extraordinary bias to the contrary deeply embedded in Western culture.

Considering the public nature of opinion holding helps explain why the pieces of opinions we get from individuals will sometimes seem so unrelated to one another. For the individual opinion holder they are unrelated.
If the structure of everyday views arises in part from interaction, then we would not expect to successfully recover much of it from individuals. But a more important implication is that we might expect to find structure in aggregates. Opinion, that is to say, may be meaningfully public.

This is not a new idea. We investigate aggregates all the time. We ask, for example, whether men (in the aggregate) have differing attitudes toward women’s roles in society than do women (in the aggregate). Individual men and women are just respondents; it is aggregated men’s and women’s attitudes that answer the question asked. Focusing on national aggregates over time is not novel either. “Trends in public opinion” is a virtual cottage industry.

What is different about this research program and this essay is the addition of a second level of aggregation, over issues. Just as respondents are mere units of analysis, the level at which measurement operations occur in typical public opinion work, here the questions themselves (the usual dependent variables) are mere indicators. Aggregating over people and then over issues to a single measure that varies only over time is the normal procedure of this essay.

The goal of all this aggregation is something deeper than mere response to survey items. It is the shared thing carried by individual people that underlies common response to disparate issues. It is latent, this thing to be called policy mood. Thus it can’t just be asked, like a survey question, for most of us may not be particularly perceptive of subtle changes in what we think of and want from government.

On Politics at the Margin

The focus of this essay is change. But change of a kind that is not the routine stock in trade of political science. We tend most of the time to look for big changes. Because of the cross-sectional approach that dominates our thinking, we want changes to be so pronounced that they are in themselves “findings.” And so our first inclination when we look at time series—collections of measures of the same thing at different times—of anything political is to look for interventions, points at which the differences before and after are so conspicuous that they can be argued to be important.

Public opinion time series sometimes exhibit such changes, but they are usually trivial. Big changes in public response are generally attributable to an event or shift in context that produces a notable difference in what the question means. Page and Shapiro (1989, 4:38–39), for example, report the dramatic move toward support of price controls—overwhelmingly the result of changes among Republicans—that followed Richard Nixon’s 1971 imposition of controls. The explanation for the shift is too obvious to be interesting.
If not big changes, then what? The consistent viewpoint of this essay is that change that matters is change at the margin. The regularities of political life (and not just political life) are to be understood as responses at the margin to changes in the environment (such as public opinion) at the margin. Always looking for the bigger impacts simplifies design a bit, for they are easy to find. But if there are no big (and meaningful) changes to be found, and if the influence is still present, then we had better learn how to think about and model a world like the one of our everyday lives, where the constraints and parameters that matter vary in a narrow range.

To assert that change at the margin of public opinion drives politics is almost the same as asserting that public opinion is stable and predictable. For marginal change can be important change only when the underlying phenomenon is stable enough for marginal change to be noticeable change. The more stable a phenomenon, the more a marginal change in it alerts us to something important.

Is public opinion, and specifically preference for policy alternatives, stable and predictable? It often looks otherwise. We know, for example, that apparent inconsistency of policy preferences—the holding of mutually contrary views—is widespread. And Converse (1964) demonstrates widespread inconsistency for individuals over time as well. This suggests little stability or continuity. But public opinion in the aggregate is another story. There stability and predictability are easy to find, and when we don’t find them it is often the case that “instability” arises from combining similar but not identical queries.4 “Similar” won’t do in time series, for the question-comparability effects—the differences in response produced by alteration of the wording of a question—are often larger than those representing real change.

If we collected series of identically phrased questions over time and asked how well a point in the series, say \( t + 1 \), could be forecast from knowledge available at \( t \), the answer would be very well indeed. For comparable national samples of typical size, we could grow very wealthy by betting against all takers that the next value would be within a five-point range of the last. We would win more often than lose with a narrower, say three-point, range. The break-even point would probably fall within a range marginally less than two. Policy attitudes, correctly measured, are very stable. And when things that are stable change, even marginally, we are wise to take that change seriously.5

If we go looking for small changes that appear to have a cumulative character—I resist using the term trends for reasons that will later become clear—we can find them. They are there to be found. But cumulating change at the margin is of a smaller order of magnitude than the big interventions we usually seek. That is particularly the case when we concentrate on two time points rather than on whole series. It takes some
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discipline to turn away from looking for big effects, but the payoff is worth it: It allows us to see important patterns to which we are otherwise blind.

"Looking," "seeing," and "finding" will be common activities for the reader of this essay. For most of its evidence will be pictures, pictures of data where the reader can see patterns of change. And after our eye is accustomed to the year-to-year changes that signify little, we will look over and over for the more subtle, patterned movements. If the exercise is a success, that subtle change at the margin will come to seem inexorable, and we will come to see the year-to-year movements as noise imposed upon a signal. This essay is about the signal, the gradual movement of underlying sentiment, nowhere large and always dominant.

We are a bit like both tourists and geologists. As tourists we look at nature with a scale that appreciates mountains and seashores—big variations, easy to see. As geologists we look at the mere inches of displacement of adjacent land masses along a fault and infer the coming of earthquakes and volcanism. The two kinds of observation are of massively different scale. While looking at mountains, we can’t see the inches that produced them and will create more. That requires looking away from the big and concentrating on the small but inexorable. That is the focus of this essay: finding movements of public opinion at the margin, movements that cumulate over time to produce a politics that changes remarkably, if not with quakelike suddenness.

It is not easy to see the subtle and inexorable in public opinion. Our traditions, both popular and scholarly, of thinking about what people want from governments are rooted in concern for the present, in “what’s happening now.” That usually leads us to think of levels of public opinion (does a majority support policy x?) instead of changes in it (is support up or down from last year?). And when we do think of change, it is often in the context of then and now; has it changed from some arbitrary time in the past to what it is close to today? Neither of those typical kinds of questions contemplates looking at change as a process, something that marches from here to there in increments, none impressive.

It is hard to see subtle movements, in part because only in the last two decades have we seriously gone about the business of producing the series of public opinion items that permit it. Those series will be seen to contain the necessary evidence. But it is easy not to notice it. And having noticed it, it is easy not to take it seriously. For how much can apparent patterns in one survey item mean? Even after we put aside the hobgoblin of sampling fluctuation, movements in support for a particular policy can always have explanations peculiar to the policy domain—and therefore not especially illuminating of a broader picture. If support for, say, federal government involvement in education goes up (or down), we can
always conclude that it is the result of people’s growing (or declining) concern about public schools. And hence our attitude toward the appearance of something systematic is “so what?”

“Old Politics,” “New Politics”? Discontinuity dominates our thinking about change. Our current events heritage and pretty frequent analysis of time series of two—then and now—usually leads us to look for sharp divisions between present and past. We have a considerable stock of theories and analyses based upon an assumption of a continuous, unchanging past, “old politics,” somehow jolted into a new and discontinuous present, “new politics.” If we then think of change as discontinuity, usually a single discontinuity, we naturally go looking for a single explanation for the single discontinuity. We ask why new politics is different from old politics, and the answer is a chorus: mass communications (by which most really mean television). “Then” we didn’t have it; “now” we do. Although I do not wish to deny the genuine importance of mass communications for our politics, the point to be made here is that this style of analysis is singularly unhelpful for understanding political life.

“Then” and “now,” with TV at the discontinuity between them, is too pat. Virtually beyond disconfirmation, this notion seems to explain things, and in the process prevents us from looking deeper. The problem here is not so much the single variable explanation as it is the starting conception of discontinuity. A pat question calls for a pat answer.

If discontinuity is the natural idea of change where longitudinal observation is primitive (and this may be a natural law), continuity is what we see when such observation becomes increasingly rich. If we observe almost any phenomenon at regular intervals over time, we will see change produced by the cumulation of small and irregular increments. If we do this for a while, look at many traces of many phenomena, the idea of change occurring as discontinuous jumps becomes increasingly an idea of oddball irregularities. Continuous change is ubiquitous. Things move. And that produces a revolution in our style of explanation. For continuous processes must drive continuous change. When something is happening all the time, one doesn’t reach out for a single exceptional change as a worthy explanation. It doesn’t fit.

If an aggregate time series moves between, say, 44 and 45 in successive years, we do not reach for an explanation of disorderly behavior; 44 to 45 requires no social calamity. “Then?” and “Now?” and “What is different about the two?” become silly questions when we are confronted with such continuous change. (And if we had observed only the two times, we would conclude conservatively, probably falsely, that nothing changed.)
Public opinion is no exception. Series of opinions, attitudes, and preferences, adequately measured over time, show continuous change. They move over time in irregular increments, no one of which typically is large enough to call for idiosyncratic explanation. Tested between one year and the next, the changes would only rarely be large enough to be statistically discernible. Looked at over the long haul, series that do not change are the rarities.

Continuous change that matters is usually cumulative. If movements too small to be discernible were also temporary, then their consequences would be too limited to merit much study. We end up studying problems that are instead cumulative (technically, “integrated”). Small changes become interesting when they add up over time to produce change of a scale that matters in the long haul. Most public opinion series have this character. They move up or down, left or right, irregularly over long spans of time—a movement captured better in decades than in years. Put another way, they do not equilibrate in the short term, throwing off deviations to left or right and always returning to some norm. Once moved, they tend to stay moved for a while, and that is why small movements matter.

If public opinion changes continuously, how and why and when it changes when it does is an explanatory problem of some interest. It is one I will nibble at in this essay and will make some progress on at the margin. But much more will remain unexplained than be conquered when I am done. This is not an easy problem. Its solution clearly will not come in terms of old politics and new, then and now.

**Issue Opinions**

Political scientists write of issues and mean one thing. Almost everybody else uses the word *issue* differently. At least since Stokes (1963) we have pretty much settled on the idea that an issue is a debate over public policy. Debates have two sides. A defining characteristic of the political issue, then, is that we must be able to imagine reasonable people advocating either side. Thus education spending is an issue; reasonable people can want more or less of it. “Education,” though, is not an issue; virtually no one opposes it.

In popular commentary almost anything a politician says in a political campaign is called an “issue.” Thus we have crime issues, flag issues, scandal issues, peace issues, and so forth. That usage isn’t helpful, for it puts symbol and image manipulation, political rhetoric, into the same category as genuine political disagreement over what government ought to do. Stokes calls these image positions “valence” issues, as opposed to (real) position issues. Valence issues carry emotion and image but no
public policy content. Emotion and image are real politics, often effective politics, but they aren’t real debates over public policy. A George Bush visiting a flag factory in the 1988 campaign is wrapping himself in the positive valence of the national symbol; he is not raising a “flag issue.”

So too we need to split off claims about what has or has not been achieved from real issues. Peace and prosperity are outcomes, not debates. When parties claim to have done well managing the economy—or claim that the opposition has done badly—they are engaging in sensible politics. But this is no “economy issue.” Issues are debates about valued alternatives. No party values hard times. Rarely does one value war over peace. Economy issues and war issues are possible, but they would involve debates about what ought to be done. Should we stimulate the economy to produce employment or does that raise too much danger of inflation? That is an issue. Reasonable people can take either side. Debates about whether things are going well or badly, in contrast, may be debates, but they aren’t issue debates.

These distinctions are important because real issues behave much differently from valence issues or outcomes. The latter, particularly the valence issues, can be created and manipulated overnight. They can move from nonexistence to real political force in a matter of days. Real issues, representing hard choices between competing values, are stable. They are stable because they can’t move without some cherished value giving ground. Valence issues have no such competition of values. A tug of war in which only one side is pulling is free to move a long way in a short time.

On Trends in Issue Opinion

*Trend* is a word that can be used seriously or casually. Casual use is the more common in discussions of public opinion. “What is the trend in attitudes toward handguns?” we ask, meaning, How have these attitudes changed the last two or three times they were measured? That is a harmless question, but it uses the idea of trend in a way that deprives it of a stronger meaning. The problem with the usage is that the only way an issue series can be said not to “trend” is to remain virtually constant. Any old movement becomes a trend.

Used more rigorously, trend is a powerful idea. And unless it is used rigorously its power is sapped by subjectivity; what looks like a trend to you may not to me, or vice versa. Before we ask, then, whether or not public opinion “trends” (or individual issues do), we need a rigorous definition of *trend*. The one I choose, drawn from the statistical literature on time series is this: A trend is a process that increments or decrements a series by a fixed amount at every interval. A trending series goes off in one
direction, therefore, not for a while, but indefinitely. Regular increments at each point in time do not produce reversible processes; they go in one direction forever.

Many time series, including public opinion time series, appear to move systematically over time. The question of trend comes down to the issue of whether or not that movement is by regular increments. Because very large changes can occur by random processes, the common usage—how different a series is at one point compared to another—cannot be a test of trend. The test is whether or not that movement occurred in regular increments. The common alternative, a series that moves substantially but not regularly, is called drift. Most of what is called “trend” in discussions of public opinion is in fact drift, irregular movements up and down that lack the tendency to go off in one direction indefinitely.

The point of a rigorous definition here is that it matters. In a short span of time trend and drift can look a lot alike. Over the long haul, which one it is, trend or drift, has prodigious consequences. Trending series go off indefinitely into the future always in one direction (which in survey marginals means that the item tends toward a state where one response option is chosen by all respondents and the question ceases to be asked). Drifting series, in contrast, lacking a unidirectional driving force, are likely to change course. After moving more or less in one direction for a while, they can turn and move more or less in the other.

It matters even more for theory. Trend requires explanation. If movement is always in one direction with some regularity, then something, some process, is driving that movement. Quite a number of social indicators are influenced, for example, by population size and so trend inexorably upward, as to date has the underlying driving force. Nothing “just trends” by some mysterious inner driving force. Trend requires process. Process requires explanation.

**On Public Opinion Leadership**

We are accustomed to seeing public opinion as scholars see it, as pollsters see it, as journalists see it, even to some degree as the public itself circularly views reports of its own views. I pursue here briefly the somewhat different issue of how elected politicians see it. I wish to suggest how it might matter. In particular I wish to suggest how it might matter without treating it only as a cause of electoral behavior. The view to be put forward is that public opinion matters and that most of what matters about it is not to be understood as a matter of votes and elections.

“People in and around government sense a national mood,” repeating Kingdon’s words from the outset of this chapter. “They . . . believe they know when the mood shifts.” I consider the matter as a process and ask
the motivating question: What would public opinion look like if you were an elected politician observing it? Where would you look for it? How, most particularly, would you come to know it? What would be the process of coming to know it, coming to be influenced by it?

I begin the answer to these queries with a postulate. It is unconventional, lacks anything like common agreement—some would probably say it lacks common sense. But I ask the reader’s indulgence, because if it is seriously pursued, I think it has some power to illuminate. The postulate is this: Politicians engage in representative behavior because they wish to lead, to have influence on the direction of public opinion.

In the last decade or so we have become comfortable, too comfortable, I think, believing that we understood the central motivations of political life and that they were one, reelection. An interesting assumption in Mayhew’s (1974) most influential work, reelection as central goal of political life seems subsequently to have been reified into a “finding.” It has “everybody knows” status. A thousand times assumed, it has come to seem a fact. But it is not.

Reelection as central motivation was useful as an assumption; pursuing its implications led to insights. It is not useful as an accepted fact, for it has stopped us from asking the old question of why public officials do what they do. And it doesn’t answer that question. It doesn’t tell us why most people are not tempted by political careers but some are. It doesn’t help explain why most of those who start down the path of electoral politics turn away after a brief experience but some stay on. Its implicit cynicism satisfies too easily our desire to know why people act as they do.

Elected politicians do, almost of necessity, desire reelection. Aside from those few who have got themselves into a job they don’t like, the lack of such motivation would be irrational. But the more important question is why they got into the job in the first place. We need to know what kinds of people put themselves in a position to need reelection and why they do so.

What makes politicians want to be politicians? What is unique about the political career? What aspect of a life in politics is more typical of politics than the alternatives the politician might have pursued? Power is usually the first answer to this query, but I don’t think it is a very good one. For it overstates the power typical politicians—the 99-plus percent who are not presidents or U.S. senators and never will be—can reasonably hope to achieve. And then it compares the lives of politicians to the lives of ordinary people. But the people who pursue elective politics in the United States are in no sense ordinary. They are well-educated, talented, ambitious (often intensely so), hard-working people. Because of these attributes, they are the sorts of people likely to rise to the top in any career track, corporate, military, or whatever, where power accrues. It is a dubious proposition that relative success in politics offers more personal power than relative success in these other endeavors.
Politics offers more limelight than many alternatives, it is true. If the corporate executive might have more real power to move people and material things, that power is much more private, much less visible, than say, that of the legislator. But in an era where seeking center stage as a politician also means seeking derision from a public that thinks badly of politicians, the limelight can easily be oversold.

Sometimes politics is said to appeal because it allows one to identify with the center of things, to be associated with the “action.” Probably there is much truth in this answer as well, but it seems to me much more suited to explain the more limited involvement of amateur activists. The appeal of the action seems particularly strong for the political groupies always seen at party and campaign activities. The satisfaction of being able to say that one talks to “the senator” or some such claim would seem powerful enough to explain a few hours devoted to occasional political work but not enough for a lifetime choice.

Perhaps all of these explanations, each with some partial truth, together are enough to explain the choice of politics as a vocation. I am unconvinced of it. I think we need to look further, to find some satisfaction of the life of politics unavailable to any but politicians. Thus the postulate that leading public opinion is a central motivation for politics. Changing how the public feels about issues is something politicians can do. Indeed, it is something even fairly obscure politicians can do. Insofar as the elected politician is not wholly invisible, he or she can choose an issue, put a stamp of identity on it, and try to influence public response. The rest of us are limited to changing personal views, one at a time. This is a postulate, not an assertion of fact, because the only evidence I can offer for it is to say that it appeals to me. That is not evidence at all.

But grant me the postulate, for it has an implication I wish to pursue. If, in fact, politicians are seeking to influence public opinion, seeking to channel the stream, then it follows that they must stay in contact with it. It is ineffectual to stand outside the currents of values, preferences, and presuppositions and try to bend them to one’s will. To be outside is to shout and not be listened to. The effective political rhetoric, in contrast, is to be able to say, “Listen, my values and preferences are like your values and preferences, and having looked carefully at this thing I have come to a conclusion that I know you would share if you did the same.” Better yet is a public that knows it shares values with the politician. The politician with the most successful “home style” (Fenno 1978) is one whose public is so convinced that he or she shares their values that they become willing to assume the politician will do the right thing.

This value and preference sharing is good reelection politics. But it is also a near requisite for influencing public sentiment. If one’s premises are suspect, all that follows from them is easily dismissed. The implication, one of some importance, I think, is that the politician who would
influence the current of public sentiment must swim in it. What is shared between politician and public is rhetorical leverage to move those smaller pieces that are not shared. In order to lead public opinion the politician needs also to follow it, leading in particulars by following in general.

The significance of this view is that it predicts that politicians will flow with the moving current of public sentiment, a current that moves slowly, with some subtlety, back and forth, changing in tone and emphasis more than direction. And, importantly, this following of currents of public views occurs without invoking electoral sanction as cause. The politician of this account would do it for the positive goal of enhancing a particular kind of influence, not for the negative one of avoiding being one of the “rascals” to be thrown out.

Switching from liberal to conservative (or vice versa) or anything remotely like that is not in question. The behavior of following, I propose, is strategic and true to personal values. It involves lying low when views are out of favor, picking one’s moments to step forward when it looks like they might prevail. But the aggregate result of these behaviors, if they follow the same public opinion signal, will look like movements from left to right or the reverse. For the aggregate will be the result both of numbers and of the volume with which they speak.

We like to keep life simple by presuming that leading and following are mutually exclusive activities. And so in this simple view the politicians are either out in front (which gets our normative approval) or slavish followers, testing the direction of the winds (which we equally disapprove). Simplicity, when it works, is a powerful sword for understanding. I think it gets us into trouble here. Real-life politicians consistently resemble neither heroes nor fools. They are, rather, more prudent, occasional heroes but always calculating. They deal with risk, I think, as do rational investors, never fleeing from it entirely and always making certain that the risk premium—paid in influence—is worthy of it. And like rational investors, they must value good information. I turn next to the questions of where and how they get it.

**Opinion Perception as Double Summation**

If information about public views is vital and collecting it costly, how, we might ask, would a politician go about it? What would be the process of collection and storage? The political professional lives in an information-abundant environment. Information comes, often without being sought, from all directions. Politicians get information just as scholars do from secondary reports. They also get a good deal of information about the views of particular people on particular issues. But this latter information needs to be processed before it can be decision-relevant. The limitations
are much the same as those of the survey researcher analyzing a single respondent. Without a wide range of information on the person’s views, the researcher cannot interpret the particular bit at hand; it lacks the context of this issue as compared to others and this time as compared to last.

Useful, and therefore consequential, opinion is aggregate. Politicians care about the views of states, districts, areas, cities, what-have-you. Individual opinion is useful only as an indicator of the aggregate. For a politician to pay attention to individual views is to miss the main game; he or she is not a political scientist or social psychologist with an inherent concern for the individual. The politician must, as a matter of image, appear to be concerned about individuals, but aggregate opinion is what matters.

But even aggregated views will usually lack direct decision relevance. Politicians must deal with issues at a high level of specificity and then mainly with nonrecurring specifics. Public opinion is by necessity general, unfocused. Focus requires very high levels of decision-context information, too specialized ever to be found in the general public. That is a simple matter of division of labor. We the public are part-time players in this game.

The politician wishing to know public opinion for decisionmaking faces bad news. The information about public views is expensive to obtain, and even the most specific information one can have isn’t specific enough for the decision at hand. Why pay the cost for specifics at all? I propose instead that a rational economizing collector of opinions would use that information to gain leverage on the evolution of public views at a very high level of generality, the level at which it is meaningful for a public mainly inattentive to public affairs.

If the issue at hand, for example, is how long the waiting period should be—seven days, thirty days, none?—for purchase of a particular class of handguns, the question is too focused, too specific, for us to have much hope of learning how the public will respond. The public doesn’t have views at that level of specificity. When we encounter such specificity in survey data, for example, we would be most unwise to treat the answers as if they were as precise as the question. But such specific information clearly speaks to more general proclivities. Does the public want more or less regulation of guns? One can do reasonably well making inferences about the general from the specific.

Thus I propose an information-seeking and -storing process in which consequential opinion is also aggregated over similar issues. To know views on a very specific issue (e.g., what percentage supports a lengthy waiting period for handguns) is useful chiefly because it has implications for a set of related matters (e.g., other specific issues involving guns), which will be the direct subject of political action. It is unlikely that the
specific issue (as stated, for example, in a survey question) will ever be
directly the matter of decision and quite likely that some different, but re-
lated, issue will. Thus the primary information value of the datum is the
generalization it permits.

The process may therefore be conceptualized as processing of informa-
tion bits, where each bit, a datum, is made meaningful by aggregating
over constituencies and over issue domains. For example, one sees a poll
about how white New Yorkers feel about having a black next door. That
is generalized to white New Yorkers’ attitudes generally (summation 1)
and racial policy generally (summation 2). The specific datum is useful
mainly because it permits such generalization. Most of those who process
the information will care neither about New York specifically nor about
“moving next door” specifically. The datum is valuable as leverage on the
larger mood, which the politician must know.

A third possible summation, over time, is different. We lose specific in-
formation when we sum over some dimension. Timeliness is often infor-
mation we wish not to lose. In politics it is often critical to know what is
current and not to confuse it with past or future. Thus we do not sum
over time, because timing matters.

Public Opinion and Private Opinion

Public opinion as a topic is at least as old as democratic forms of gover-
nance. As a focus of systematic analysis, however, its origin is the origin
of survey research on public opinion. Very early in this history scholars
decided that the interesting questions that could be answered with the
survey tool were about individuals, why one had views or behaviors dif-
ferent from another. That was the unique power and principal good of the
survey design; it was a new opening into people’s heads. Surveys could
explain why attitudes developed as they did and how attitudes con-
ected to behaviors, one person at a time. This was then an exciting new
vista, the opening of the black box of individual political behavior. After
years of frustrating speculation about why people did what they did, now we could ask them.

Scholarly analysis of “public opinion” became—and in general remains
today—the analysis of private opinions about public affairs. This individ-
ual (or micro) focus leads naturally to public opinion as political psy-
chology. Its focus on the individual actor raises many of the same issues
as does psychology, and its tools, approaches, and concepts came to be
dominated by those already developed in that discipline. “Public opin-
ion,” “political behavior,” and “political psychology” could be quite dif-
ferent from one another but are virtually interchangeable in common
usage. One could take a course with any of those titles and get the same materials. Much the same is true for the related area of voting behavior, also dominated by micro concepts and psychological analyses.

The study of micro behavior is now a mature scientific subdiscipline. Its numerous practitioners operate with a high level of consensus about the basic questions to be investigated and the tools to be employed. Micro behavior may be considered a paradigm, in the sense of Kuhn (1962). It has all the requisites: leading theories, examples of success, focus on questions to be asked, and methods by which they are to be answered. Its growth changed the discipline from which it emerged, changed what it means to be a political scientist.

The success of this study of private opinions about public affairs may easily be understood as the result of scholars choosing to exploit what their tool, the sample survey, did well—and to ignore what it did not do well. One thing the survey does not do well at all is to inform us about public opinion. If one were to ask the proverbial man (or woman) on the street what was public opinion, he (or she) would be likely to define it in terms of what the public wants. And “the public” is singular. This is not a happy question for survey researchers, for their tool is not much use in answering the query. One can pose a query about public preferences on some issue and report the results—$n$ percent favored $x$—but these marginal totals (or just “marginals”) both lack interpretability and fail to exploit the power of the survey to discriminate between individuals.

The interpretability issue (to be taken up in more detail in Chapter 3), though simple to state, is not at all simple to solve. The problem is that the answers to survey questions are highly dependent upon what the questions ask, and we lack a scientific method for the design of survey questions. Survey designers do their best to pose queries that probe the concepts of interest, but their best is ad hoc and intuitive. Beginning students often see this issue as one of fairness or balance, of question designers slanting questions to produce the answers they want to see. It is much worse than that. The scholar pursuing objectivity, who just wants to see the world as it is, does not know how to ask the right question. Indeed, there is no right question.

This issue is under control in micro analysis, what I have called private opinion. There, whatever its unknown defects, the same question has been posed to all respondents equally. And so whatever “it” is, we can reasonably ask and answer questions about how individuals differ on “it.” But there are as many public opinions on an issue as there are possible questions that might be posed, and, aside from those that show an obvious intent to produce one or another result, they are all about equally good. Thus, apart from some early excesses, reports of survey results are
framed not as answers to “What does the public want?” but as answers to a specific survey question. We say, “To this question, \(n\) percent said so and so.” We do not—cannot—say, “Public opinion on x is so and so.” This is operationism, the defining of concepts in terms of the operations one conducts to measure them, in extreme form. We do it not because we approve of operationism, a naive view of science that forgets the role of theories and concepts in explanation, but because we have no choice.

But the person-on-the-street view of public opinion (as “what the public wants”) has one considerable virtue: It is directly relevant to politics. It is the public opinion that matters. The irony here is that survey research, the tool that makes direct probing of public opinion possible, is the reason scholars turn away from its public aspect. The coming of the sample survey produced a political science rich in scholarship on micro public opinion, the analysis of individual attitudes and behaviors that deals almost not at all with the public.

Macro public opinion, or just public opinion, is the focus of this essay. Never much more complicated than “What does the public want?” it will exploit the fact of thousands of existing public opinion surveys, numerous samples and numerous items probed at numerous times, to lever meaning into each. The whole will be used to bring interpretive context to the parts, to extract real information from the marginal totals that cannot be had from one survey by itself.

**On the Plan of the Essay**

The design for this extended essay on public opinion is this: Chapter 2 introduces a series of ideas about how and why public opinion might move over time and how that movement might be related to what governments do. Chapter 3 attacks head-on the problems of analysis of the survey marginal to gain information about public preferences, ending in the development of a main measurement technology and some subsidiaries for cross-validation. Chapter 4 exploits that technology for description of policy moods, for cross-validating the measure, for comparison to a common alternative (self-declared ideological identification), and for a series of decompositions of mood into specific policy domains. Chapter 5 takes up the question whether policy mood influences elections and returns to an old familiar political theme, mandates. And last, in Chapter 6, the focus returns to American politics of the late 1990s and beyond as we develop the implications of what the measure seems to be saying about the current era.

The foundation for this whole scheme is a view of the structure—in particular, the structure over time—of public opinion and the place of public opinion in representation. That is the business of Chapter 2.
N O T E S

1. These themes are clearly articulated in Berelson, Lazarsfeld, and McPhee (1954) and become central to the three-decades-old voting behavior research paradigm beginning with Campbell, Converse, Miller, and Stokes (1960).

2. This is most commonly seen in arguments about the “ecological fallacy” (Robinson 1950). See Erbring (1990) for a somewhat contrary view.

3. The numerous works of Robert Shapiro (see References) in Public Opinion Quarterly are outstanding examples. Smith (1981, 1990) takes up trends across (nearly) all issues in the postwar era.

4. Many of these points about stability and change in opinion series are similar to those of Page and Shapiro (1989), where they are more thoroughly developed in an entire chapter (“The Myth of Capricious Change”) devoted to these issues.

5. This is often unappreciated because of a commonly held and fallacious understanding of sampling theory. It is commonplace to regard differences between independent samples of, say, three points as probably the result of random sampling fluctuation, because parameters from samples of typical size could vary about that much from chance fluctuation in about five out of every hundred cases. But that it could have occurred by chance with small probability does not mean that it is likely that it did. The misunderstanding is coming to believe that an improbable event, a difference so large that it could occur by chance only five out of a hundred tries, is likely. In fact, the sampling error with greatest likelihood is zero. And the range of values close to zero will represent most actual cases.

6. The flag desecration controversy was another story, but that debate arose from a Supreme Court decision issued after the 1988 campaign.

7. This definition has the virtue of an objective statistical test. In the common simple case, for the model $x_t = x_{t-1} + c + a_t$—in English, the series $x$ at time $t$ is a function of (1) its previous value, $x_{t-1}$, (2) a constant, $c$, added at every period, and (3) a random error component, $a_t$—whether the estimated value for $c$ is statistically significant (whether, that is, we can conclude with some confidence that it is not zero) is a test for whether trend is present.

8. Yet a third alternative is simple random fluctuation—with or without ARMA (autoregressive-moving average) error aggregation processes (Box and Jenkins 1976)—around an equilibrium fixed over time. If that were the case, if public opinion were stationary over time, the only changes self-correcting random disturbances, then there would be no interesting variation to observe, and this essay would not be.

9. Smith (1981, 1990), for example, writes of a trend toward general liberalism in the postwar era that “plateaus” in the 1970s. The facts of his analysis are pretty much the same as my own to come. I do not disagree with his analysis; I just am urging a definition of trend that disallows the possibility of abatement or reversal. And, too, there is a difference of emphasis. Some issue domains, most notably racial and cultural matters, clearly produce liberal trends in the postwar era. A phenomenon of central import to Smith, these matters are peripheral in my analysis. It depends chiefly upon the focus. In the domain of culture and values, trends are abundant, perhaps normal (see table 4 in 1990:492). Attitudes toward religion, lifestyles, sex, gender, family, and the like—the “climate” in Davis and Smith...
show dramatic trending change. In the domain of public policy controversies—the “weather”—these critical social issues are but a very small part of the issue set and trend is the exception to the norm of cyclical fluctuation.

10. Whether it is few or many who desire the quickest exit from political life depends on what kind and level of politics we are considering. Barber’s old (1965) but still up-to-date study of state legislators is a useful view of the other side of political life, a side where “going home” may often be the central motivation.

11. Financial gain used to be on the laundry list of motivations to public life, if never very prominently. The evidence now seems overwhelming that financial loss, relative to what could be had elsewhere by people of comparable talent and industry, is the more likely outcome of a political career.

12. This is similar in concept to the Converse, Clausen, and Miller (1965) assertion about the mass electorate that the apparent Goldwater surge in numbers of conservative voters in 1964 came largely from a small group who were very vocal. Were there a rich enough time dimension, I think we would have seen relatively constant numbers of such conservatives and strong variation in their frequency of public expression. In the expression of the times, the conservatives were “coming out of the woodwork.”

13. See Beck (1986) and a response by Weisberg (1986) for intriguing analyses of the directions in which the study of voting might have developed but did not. Weisberg shows that a number of alternative issues in voting flowered before the advent of surveys of voting behavior, each more or less abandoned as the psychology of voter choice became the one ring of a one-ring circus.
Arthur Schlesinger puts it with more color:

Disappointment is the universal modern malady. . . . It is also a basic spring of political change. People can never be fulfilled for long either in the public or in the private sphere. We try one, then the other, and frustration compels a change in course. Moreover, however effective a particular course may be in meeting one set of troubles, it generally falters and fails when new troubles arise. And many troubles are inherently insoluble. As political eras, whether dominated by public purpose or by private interest, run their course, they infallibly generate the desire for something different. It always becomes after a while "time for a change." (1986:28)

Notes

1. We would still benefit from methodological sorts of theories and models, theories of the survey response itself, see Feldman (1990), and Zaller and Feldman (1988).

2. But see Dahl’s (1956) A Preface to Democratic Theory, where alternative forms and their implications are worked out.

3. By public, I mean here the general public, not including numerically minor groups (“issue publics”) so directly involved in process, policy, and consequences as to be responsive to every imaginable policy choice.

4. One can imagine politicians with fixed preferences about policy who nonetheless adjust their tactical views to the currents of the day from the belief that changing the direction of the mainstream requires one to be in touch with it, if not in its center. If we grant the common assumption that professional politicians harbor stronger and more ideological views than the citizens they represent, then the ability to make such tactical adjustments, adopted consciously or otherwise, must be a well-honed skill. This is not a cynical suggestion that politicians adopt views only for expediency—a view I cannot reconcile with firsthand experiences with elected politicians—but rather an assertion that the requisite of influence is tactical adaptation to circumstance, an important component of which is the mood of the times.

5. This assumption too could reasonably be generalized. I do not do so because it is beyond the available data to estimate bounds of acquiescence at all, let alone the distance between them. It is a concept without any but arbitrary measurement in this essay.

6. If this adversarial scenario of public understanding of issue debates is correct, it would lead to the expectation that the ignored side of the trade-off would later give pain and cause its reassertion. If the originally valued alternative becomes similarly undervalued against the pained alternative, we have a teeter-totter pattern of cycling back and forth over the long haul.

7. I do not wish to imply that racial or gender prejudices have disappeared and certainly not that new beliefs in equality imply support for government efforts to
effect a change in status. These are separable issues. What can be shown to have changed is that most once believed inequality itself was right and no longer do.

8. Happily, that decision need not be arbitrary or a priori. Leaving them in the issue matrix for the recursive maximum likelihood estimator, to be developed in Chapter 3, tends to produce communality estimates that converge on zero, giving trending issues no weight in final estimates, whether or not included.

9. The process could be ameliorated by analytic treatment of policy proposals in public media, but that tends to be blunted by the conflict between analysis and entertainment, which is the central consideration for commercial media, particularly television. Analysis makes us informed about choices but isn't entertaining. A profit-driven system of public information, propelled by consideration of circulation or ratings, will therefore systematically fail the needs of democracy.
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FIGURE 3.10c Three Series in Standard Metric and Their Estimated Mean

Implemented for over a hundred series instead of merely three, the problem involves prodigious number crunching. Its logic in the more complicated detail of Appendix 1 is basically that of the illustration, adding only one additional step, a means to weight issue series in proportion to their validity as indicators. Now we have a concept, policy mood, and more than one technology for measuring it. It’s time to get to the thing itself.

Notes

1. Such a consideration underlies the use of the normed liberalism index—Percent Liberal/(Percent Liberal + Percent Conservative)—as the basic measure, rather than one of its components alone. Subtle differences in filtering, including some not even discernible in question texts, tend to produce changes in the “don’t know” categories relative to classifiable liberal or conservative responses. A single component measure (such as the “percentage conservative” of my earlier work) produces systematic error in the face of filtering change. To add a filter to a question where previously there was none, for example, would depress percentage conservative (along with percentage liberal) and produce a false measure of movement to the left. In the same situation the worst effect experienced by an index including both, in contrast, is an artifactual reduction in variance.
2. What comes closest to a standard, and the one I follow, is this: Percentages are calculated from the totals of all actual responses—every possible response from respondents to whom the question was actually administered. The "n.a." responses, typically associated with respondents who for one reason or another (interview terminated, second-wave sample mortality, and so forth) were never asked the question, do not count toward the totals.

3. When it isn't easy, for example, on issues of more or less international involvement, one gets some intuition on the boundaries of domestic policy mood. If the connection can't be coded with confidence, it probably doesn't exist.

4. A more difficult judgment call arises, if rarely, with in-between responses. Almost always these are coded as neutral and contribute nothing to the index values. In a couple of cases, on racial segregation, for example, a response like "leave things about the same as they are now" is pretty clearly a conservative option and is coded as such. All that matters is that such coding decisions be comparable over time.

5. From preliminary analyses foreign policy attitudes were known not to travel the same path over time as the domestic sphere and so are not seriously represented in the data.

6. Note that product moment correlations between items drawn from the same survey, as in these examples, are likely to overestimate the longitudinal associations. The problem is that the items will share some portion of their errors with other items in the same sample because each is subject to the same sampling fluctuation. If one randomly drew 1 percent too many liberals, for example, that error would be expected to inflate all measures of liberalism. Because sampling error is random with respect to time, that false association would be seen in year-to-year random fluctuations but not in patterns systemic with respect to time. It becomes trivial in analyses to come, where items are rarely drawn from the same samples.

7. The effects of wording loom very large in a refined time series analysis and are capable of producing considerably more systematic variation that the real change that is the target of study. Wording of questions must truly be consistent to produce the desired information, and this requisite causes unavoidable information loss when two versions of apparently the same issue have to be treated as different series with no a priori relationship. When the different forms don't overlap one another in time, the normal case, there is nothing to be done about it.

8. As a result, the 1950s and sometimes half of the 1960s will disappear from refined or disaggregated analyses to come because the necessary data don't exist. The long-term issues that never left American politics, the cluster conveniently called the New Deal agenda, go untapped during that period. In that period where we wrote about the "end of ideology" (Bell 1960), we did so with a conspicuous lack of measurement of the components of what was said to be ending.

9. When account is taken of a data matrix—issues by time—that has far more missing cells than measured values, principal components or similar models become swamped beyond believability by the estimation of unobserved values and the assumptions that must accompany the process. One common stopgap, analysis of a matrix of correlations computed with pair-wise missing-value assumptions, is also unworkable. Such a matrix cannot be constructed because many items have no overlapping cases with one another. Thus simple though it is in
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10. My motive is to convince the skeptical reader that the movements I will later extract can be seen in the marginals themselves, that the measurement algorithm to be later deployed refines them but does not create them.

11. Throughout this text, I use all to mean all that I can locate. Inasmuch as I continue to find fugitive items and series, it is clear that more exist that I have not yet found. Inasmuch as greater effort is expended locating fewer and fewer such fugitives, the number of those remaining to be found is not likely to be large.

12. The domestic policy category includes defense-spending issues and liberal and conservative self-identification, which, although relevant, is not a strict "policy" measure. The set does not include abortion measures, which follow a different track, or two "ends"-oriented measures of belief in equality for blacks and women. Depending upon which subset of years is considered, the number of distinct items is around 145. About 2,056 individual readings on those items form the domestic set.

13. Sullivan, Piereson, and Marcus (1978:246, table 6) demonstrate an "agree" response bias to the old format questions. Because agreeing is usually agreeing with a liberal change in government activity, the "agree" bias becomes a liberal bias.

14. These questions then become less reliable as time series as well because they are particularly sensitive to the context of the interview. It matters a great deal whether respondents are focused on problems in need of solutions or the cost of those solutions, and previous questions in the survey or last night's TV news might well have primed one or the other of them, so the marginals will be accordingly noisy as a time series. The better alternative forces respondents to consider both sides of the question and by providing its own priming—always the same—should produce a more reliable response.

15. This sort of specification was suggested by Chris Achen.

16. Taking the crucial assumption of representative values for each item, 3.1 is estimable from a linear regression of the form: \( M = \beta Q + \tau T + U \), where \( M \) is a vector of all marginals, \( Q \) is a matrix of (number of items -1) dummy variables, each coded 1 if the current case is item \( i \) and 0 otherwise, and \( T \) is a similar matrix of time dummy variables. \( U \) is the usual regression error vector. The \( \beta \) vector we throw away; it serves its purpose by being estimated jointly with \( \tau \). The \( \tau \) vector is a set of (number of years -1) offsets, each of which may be added to \( \beta_0 \) for an estimate of policy mood at time \( t \) for years 2 and after. \( \beta_0 \) itself estimates mood at year 1.

17. A more complicated model that might allow for estimated validities is available in the linear structural equations (sometimes called LISREL) tradition, but the complexity of ordinary least squares estimation for this problem is already daunting; a fully specified structural equation specification ratchets up complexity to the point where there is very little possibility that the right model would or could be estimated. For the vector notation of note 16 above hides the difficulty of the estimation problem, which comes from sheer numbers of variables and parameters. The regression for Figure 3.9, for example, requires estimation of 120 item parameters plus 40 time parameters plus an intercept. Written in
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(conventional) scalar notation, the “little” equation would consume the better part of a page. Presented in tabular form, the reported coefficients would consume several (which is why Table 3.2 presents only goodness-of-fit criteria).

18. The Michigan election studies, the richest data resource of the time, illustrate the worst case. There the collinearity is perfect. None of the 1956–1960 policy items is continued after 1962, hence there is no possibility within the Michigan collection of separating real changes over time from changes due to format. Gallup items of the time, which continue later, partially salvage the situation.

19. Again the difficult assumption of representative missing values arises. Standardization presumes that we can estimate the mean and standard deviation of the whole series from the available portion, which may be atypical.

20. This as an assumption, not a fact, because the possibility does exist that changes in political context will produce differing responses to the same words at different times.

21. Values for four missing years are interpolated for this illustration to produce at least one measure for each year. This problem is specific to the illustration; it does not arise in full analyses.

22. The addition of ratios could mean trouble were variation extreme. But given basic preference stability and the calculation of ratios only between various administrations of the same item, variation is quite modest, mostly contained within the range of 0.85 to 1.15. Because the issue preferences are scaled as net differences between liberal and conservative preferences and then normed around 50, accidental variations from sampling and rounding can have only trivial effects.
Part of what the figure shows is true, the general shapes of movement of the two series over time. And part, the greater apparent variability of the value trade-offs, is illusion; the series is just a less reliable instrument because it is based upon much thinner materials. And its two-year movements (with in-between points interpolated to produce a continuous line) look starker than the annual “do more” series. The substantial early differentials in particular should not be given much weight; they actually show the same monotonic variation in both series, differing from shifts in scale.

The divergence of the two sorts of series in the latter 1980s is another matter. In this case there is no particular problem with the data, and so the divergence might be telling a story of genuine differences. The time span is too limited (particularly when one of the series is measured only at two-year intervals) for a strong inference, but this is akin to a divergence we have seen before, first in self-identification and again in the second principal component estimate of mood (driven chiefly by concerns of law and order). It is a repeat of a sharp reassertion of preferences for a larger domestic role for the federal government that is not wholly parallel with a more symbolic and value-laden liberal vs. conservative dialog.

This, in a sense, is the end of the story for a limited conception of policy mood. We have seen that it exists. Within the limits of the data and the measurement technology, we have seen that it can be broken into separate pieces and that those pieces usually—but not always—look much like the whole. If the question is restricted to the simple “Do things go together?” the answer is equally simple: yes. For at least the public opinion side of public policy studies (never a particularly prominent portion of that genre), this is a strikingly subversive conclusion. It suggests that the starting assumption, that policies can be best understood by a focus on their uniqueness, is wrong. With few exceptions, what one gets by focus on the specific, not already known from the general, is errors in measurement and sampling fluctuations. This is a portrait of a macro public that derives specific preferences from global attitudes, top down, not one that builds global attitudes from the starting point of specific preferences,1 2 bottom up.

Yet to be determined is how much any of this matters. And most of that question lies beyond the bounds of this essay, if not beyond the bounds of knowing. But one little piece of “does it matter?” is whether it matters for election outcomes, the topic to which we turn in Chapter 5.

Notes
1. The set excludes abortion, military spending, and self-identified ideology. Relationships to each of those domains are taken up later in the chapter.
2. The year 1960 is particularly problematic. In a period where only the Michigan election studies produced policy preference measures at regular intervals (Gallup measures were sometimes frequent but always irregular), 1960 was the final use of “old” format preference items in the election studies. The format change has very large consequences for the marginals, which simply have to be treated as different series. The backwards recursion method thus has no later series with which to connect the Michigan items. (And use of a forward alternative would simply shift the problem to 1964.)

3. A note on graphic conventions: Page and Shapiro (1989), citing Tufte (1983), argue that any display of the marginal percentages of survey responses with less than the full 0–100 percent range of possible outcomes produces an illusion of change greater than the reality. Although I won’t often display such marginal percentages, I will not follow the Tufte convention when I do. The Tufte prescription has a big problem of its own, an illusion of no change in marginals time series where real change is present. We are more often wrong, I will argue, in concluding that public opinion does not change (when it truly does) than in concluding that it does (when it truly doesn’t). Thus I adopt the graphic convention most robust against the greater threat. Collapsing presentation to the real range of variation (instead of the larger potential range) does highlight change. But evidence for the critical inference, that the change is real, must arise from covarying series. Scale cannot produce covariance. Indeed, a reduced scale of presentation where covariance is absent makes change look large and random.

4. The two are not exact mirror images because of changes in the “don’t know” responses over time. But they are very close to it. Both are presented to get closer to the typical presentation of survey marginals.

5. The quotation is repeated from Schlesinger (1986:249).

6. The case is worse yet for the presumed central conflict along the lines of labor vs. management, which had its day on center stage largely at a time when surveys were much less abundant than now and survey researchers evidently regarded repeating a previously posed item as showing a lack of imagination.

7. The issue is a bit more complicated, unfortunately less objective, than just choosing as many dimensions as have Eigenvalues greater than 1.0, because secondary dimensions are typically extracted that meet this criterion from the chance association of the individual series. The difficult decision for these secondary dimensions is whether they tap common structure or common errors.

8. More specifically, the Varimax criterion attempts to maximize the location of variables on either one or another dimension, rather than having them more weakly associated with several. Correlation between variables and dimensions is constant, but rotation of the dimensional space has the effect of reallocating that total association from one to another dimension. There are no better or worse solutions; they are all mathematically equivalent. But there are solutions that are more interpretable or less interpretable. The goal then is to choose from between those equivalent solutions the one that is most readily interpretable.

9. I am always aware of, and therefore discount, parallel movements that could arise from sampling error where two series are derived from the same samples, as is the case with several abortion items in the General Social Survey. Parallel movement is also seen in independent samples.
10. A *New York Times*/CBS News survey of July 25–30, 1989, gives some support for the assertion. Following the Supreme Court’s *Webster* decision, 37 percent of respondents were willing to express an opinion about that specific decision (not just abortion in general). The other 63 percent said that they didn’t know enough about it (59 percent) or didn’t answer (4 percent). Of the 37 percent with opinions, 29 percent correctly interpreted the decision as one that gave states more power to restrict abortions; 2 percent over-interpreted its effect as making abortion illegal under most conditions; 5 percent got it wrong, saying that it imposed fewer restrictions; and another 2 percent didn’t answer a follow-up asking what the decision did. In a context of generalized ignorance of virtually all political facts, those are impressively high numbers.

11. Because we constantly err in thinking the past was more like the present than it was, it is worth recalling that in the time before *Roe v. Wade*, when abortion was a state issue, some of the more permissive abortion laws were found in the South. The presumed reason at that time was that abortion divided Catholics from Protestants, and there were few Catholics in the South. The rise of activist opposition to abortion among evangelical Protestants is a more recent phenomenon.

12. The aggregate data do not permit an inference that individuals also function global to specific. But neither are they inconsistent with it.
send messages. Individual voters too may communicate policy content in their votes. But they always do so to some degree, and they always speak contrary to one another, on both sides of real policy debates. Mandates arise from the net preference of one side over the other, and that is a system-level property.

What the mandate idea has lacked all along is an unambiguous measure of the purported message. Election results can be close or one-sided, mixed or decisive. But without knowledge of a preference message that might be expressed in votes, we have no electoral evidence that cannot be dismissed as the result of other factors. Policy mood is such a measure of message. Married to election outcomes, it suggests clear-cut evidence that occasional elections in the modern era send messages from the governed to the governors.

I turn last to a more speculative look at the future, giving in to the temptation to ask, What is the mood of the moment and where is it taking us?

Notes

1. This, however, is a much easier case. All survey houses are trying to measure the same thing when they ask partisanship questions. The very high correlations between scale and individual series and, in particular, the .985 correlation with the most numerous, the Gallup series, suggest both great measurement reliability and that we needn’t have worked so hard to get an optimal measure; Gallup would have done fine.

2. In the first edition of this book, I observed an apparent weak relationship but suggested that it might not survive better evidence. That is what has occurred; eight additional data points succeeded in demonstrating that the relationship was not really there.

3. The simple statistical evidence is mixed. A product moment correlation of .36 suggests a connection. But with $N = 11$ that correlation is less than the requisite for the customary .05 significance level.

4. The 1980 contest was a three-candidate affair. Carter, the incumbent president, received only 42.4 percent of the total vote. And the Democrats captured but 49 of 538 electoral votes and lost their long-time control of the U.S. Senate. In terms of what one might reasonably have expected, given party strength and incumbency, it was the worst Democratic showing since the beginning of the New Deal.

5. The $t$ statistic for mood is 1.87, just short of the .05 critical level of 1.94, with 6 degrees of freedom. In a similar model (not reported), with mood paired with incumbency, it is easily significant, with $t = 2.44$.

6. The quotes indicate a certain tongue-in-cheek skepticism about calling this model fully specified. There are other variables that reasonable theories would suggest as candidates. But with only eleven cases, a three-variable model is about as far as one can go without starting to tailor variables to particular data points.
7. The 1976 contest is a difficult coding decision. Gerald Ford was the incumbent president, to be sure, but not having been elected, except in his Michigan congressional district, his incumbency status was dubious. I have coded 1976 a nonincumbent election. The results to be presented are affected by this decision, but the effect results mainly in a trade-off of explanatory power between incumbency and party, leaving mood little changed.

8. The variable is coded in such a way as to be always positively associated with Democratic voting, no matter whether that party is in or out of power.

9. Alaska and Hawaii lack results before statehood, and the Alabama contest of 1964 did not provide voters an opportunity to vote for Lyndon Johnson, the national Democratic candidate. These three states hence do not appear in analyses exploiting the regular, pooled data structure.

10. The goodness-of-fit contrast between national and state is what one would expect from sample size. $R^2$ is higher (.13 to .08) with the small sample, where capitalization on chance is unavoidable. But adjusting for degrees of freedom lost, which reduces the small sample fit to a trivial .01, has no effect at all on the large sample estimate. The fit here nonetheless does not suggest confidence that the bivariate specification is correct, which indeed it is not.

11. Disposable income, a variable that does not appear in the model, merits some comment. This all-purpose, elections-predicting and -forecasting champion, when added to the model, produces evidence that it may be given more credit than it deserves. Specifically, disposable income invariably fails to be statistically significant in specifications that include mood. Without mood in the model, it performs as expected. With mood present it drops out. The two variables are highly collinear, with $r = .58$ ($N = 9$). Mood always dominates disposable income in a statistical sense; when they compete to predict outcomes, mood is always highly significant and barely diminished whereas the income coefficient goes to zero. If they are accidentally related, this suggests that income may be a good election-outcomes predictor because it is spuriously related to policy mood (never before measured). But they may not be accidentally related. Durr (1993a) explicitly postulates a causal scenario in which greater relative prosperity causes policy liberalism. These results must be classed as suggestive only, because they are based upon such a small number of independent measures.

12. I deal with the technical side of pooled regression models in Stimson 1985, where the theory, terminology, and application of pooled dummy variables (LSDV) and error components (GLSE) models are treated in detail. My interpretation in the text can be understood without technical background.

13. The effect is a permanent shift, but that cannot be modeled with such a short time series, particularly when the one-time effect of 1964 is so dramatic that it must decay. The heart of the “solid South” for Kennedy and Johnson in 1960, the states of the Deep South totally rejected the Democratic ticket of 1964, the effect not greatly different in Alabama (where Johnson was not even on the ballot) from neighboring states (where he might as well not have been).

14. In other analyses, not reported, where states that cause problems for one or another reason are deleted, the estimate rises into the 0.45 range.

15. The variable is number of Democratic winners over number of seats with elections. This proves better behaved as a national aggregate than alternatives such
as percentage Democratic of the national vote for Senate. Because the same states are not contested in successive elections, which states are contested influences the national popular vote as a function of state size and the state's Democratic or Republican tendencies.

16. Perhaps I should use we, for I have contributed to this literature, and the thrust of my own contributions is very like the mainstream I here call into question.

17. And 1964, one of the two most interesting cases, cannot usefully be addressed with the Michigan surveys because a wholesale changeover in issue measurement technology that year obviates the measurement of change in issue preference.

18. Mandates would not have to be limited to presidential contests, but we seem unwilling to find the grand messages in midterm contests—even very one-sided ones—that we draw from the presidential contest. Some midterms that suggest mandates would be 1958, 1966, 1974, and 1978, the partisan direction of each consistent with policy mood. Each also forecasts victory for the opposition in presidential elections to come, suggestive evidence that the outcomes are more than just an adjustment to the previous presidential election.

19. Using outcomes as total seats won (instead of changing hands) produces nearly identical outcomes.

20. The measure here, a four-year change in mood, seems unimpressive, a result of the fact that the change is computed from a year that is itself almost the most liberal on record.

21. In the case of the Senate, the "large" seat gain is a mere two seats, tying the best Democratic performance thus measured in presidential years. What that number fails to show is that the Democrats reelected the best Senate class on record (1958) and even added two seats on top of that. If the measure were numbers of seats held, 1964 would still mark the best Democratic year—and 1980 the worst.

22. Indeed, their analysis employed many of the same National Election Study policy preference items that make their way into the estimates of policy mood.
government spending. Although cuts in the end were quite small, relative to the talk of cuts, perhaps the more important impact of this decade and a half of budget concern was that we lost interest in even thinking about expanding the government role, whereas “How much to expand?” was the question that had dominated earlier decades.

I write in the year when the first balanced budget of a lifetime for many citizens is in prospect. The genie of expanding government, still well within the bottle, seems likely to emerge in the longer run. When we get used to the idea that the United States federal government can actually afford all of the activities it undertakes—and we haven’t yet—it seems all but inevitable that we will again begin to dream up new things it might do to make life better for its citizens. “There ought to be a law,” someone will say. And we will pass that law, and we will enforce it. Then the story begins anew, not with “The End,” but with yet another cycle.

Notes

1. This is not a necessary truth, for we can imagine a politics much like we experienced in the 1950s, when both party groups were so close to the center, and so diverse within themselves, that a perceptive electorate might have experienced both as moderate. But the politics of the last four decades has witnessed quite dramatic ideological polarization of the party system. Those who were mismatched to their party (generally southern conservatives among Democrats and liberals on the Republican side) have changed parties or left politics.

2. And we now know that the recovery that would eventually be credited to Bill Clinton had already started in the third quarter of 1992, when the numbers on growth and prosperity started an upward movement still unbroken as I write in 1998. The 1992 third quarter numbers, released before the election, suggested recovery in progress but were apparently discounted by voters who had already decided against Bush. This is not surprising since two previous “recoveries” had failed to be sustained, and a single quarter’s indicators were not enough to document a trend.

3. And in fairness to historical fact, he was quite probably right. The economy was moving on a positive course without activist intervention from the White House.

4. To call the acts of the Gingrich House of Representatives “policy” is a bit of an overstatement. To be sure, the public was exposed to a lot of radical talk as the House moved on proposals that would have seemed well off the edge of the ideological map at other times. But the most radical acts of the 104th House failed to win support even in the Republican U.S. Senate, let alone from the Clinton White House, and did not become law.

5. See Erikson, MacKuen, and Stimson (n.d.) for an extensive analysis of this issue. There it is documented from individual level analysis that the modal preference of self-styled “conservatives” is for increased government spending on a
range of programs in the domains of education, race, cities, welfare, health, and the environment, each conventionally understood to be liberal. Although “conservatives” don’t wish to spend as much as “moderates” or “liberals,” they join both of those groups in wishing to increase programs, on balance, rather than cut them.
References


