

WALTER DEAN BURNHAM

**THE CURRENT
CRISIS IN
AMERICAN
POLITICS**

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PREFACE

This volume is entitled *The Current Crisis in American Politics*. Yet—as immediately becomes evident with the first essay, which was written in 1964 and published in 1965—many of the data that are generated and analyzed are not current at all. In fact, they go back in some places more than a century into the past. But they have not thereby lost a very considerable relevance, I would argue, to the conundrums with which American politics abounds in the 1980s.

There is justification for such an argument, and it ultimately derives from certain special realities of American politics over time—especially those associated with its “nondevelopmental” character. These realities have been perceived and discussed extensively in the writings of cultural and political historians such as Louis Hartz and Lee Benson.* The work of such authors also makes crystal clear why the past so often seems immediately to confront the present in the United States, and hence why it is so important to include materials from the past in dealing with today’s problems and issues. Comparative analysis is also often of crucial importance in understanding a problem which at first blush might be thought to be pure “native American.” Hartz in particular makes a powerful case that only thus can one become fully aware of just how special some of these realities of American politics are—how profoundly different that politics and its history have been from those of other countries with broadly similar advanced industrial-capitalist economies and societies.

It is perhaps worth noting even in this brief preface that the first of these essays—among the most influential of the lot, it would seem—was written

* Louis Hartz, *The Liberal Tradition in America* (New York: Harcourt, Brace, 1955). For his statement of the “fragment society/ culture” hypothesis, see idem, *The Founding of New Societies* (New York: Harcourt, Brace, 1964), Ch. 1. Lee Benson, *The Concept of Jacksonian Democracy* (Princeton: Princeton University Press, 1961), especially Ch. 13, “Outline for a Theory of American Voting Behavior.”

at a time when the bulk of the research mainstream in American politics had very different agendas and problematics. An imminent, system-wide crisis of political articulation, representation, and public support was in no way generally visible in 1964. This was the age, after all, of Gabriel Almond and Sidney Verba's *The Civic Culture*, the Michigan group's *The American Voter*, and Robert E. Lane's "The Politics of Consensus in an Age of Affluence."^{**} Nor, for that matter, was any such crisis visible to me during the writing of the "Changing Shape" essay in 1964. To be sure, President Kennedy had just been assassinated, an infinitely tragic and shocking event. But presidential assassinations had happened before, and more than once, in our political history without general crises accompanying or following them.

What was visible to me in 1964 (and even more so later, after the crisis did begin to surface) was that the political universe presented by then current academic and journalistic accounts could not plausibly be fitted into the past. Moreover, that past appeared to me then, as it does now, to be deeply relevant to the present. It somehow had to be included, then, if the present was to be described adequately. Several questions were thus ripe for the asking. Has it always been so? Must it always be so? And what substantive difference to American politics, or to the study of American politics, does it make?

Whatever else may have happened in the many years since then, my view that the past must be linked systematically with the present has not changed. When Oxford University Press approached me with the idea of producing a collection of my essays, it seemed appropriate for many reasons to select those which, incorporating such an approach, particularly centered in one way or another on "the current crisis." More especially, they mostly center on one dimension of this crisis, the degeneration of the contemporary American electoral market and its institutions. A fuller discussion of these issues is found in the introduction which follows. It suffices to say here that crisis sequences in any political system are to a peculiar degree chains of events which demand analysis and theorizing about the statics and dynamics of the system itself. The success of any such effort is largely revealed by the extent to which interested readers find that they gain insights and perspectives about today's political world that they did not have before.

A particular note of thanks and appreciation is due to Mr. Sheldon Meyer, senior vice-president of Oxford University Press, and to his capable staff. My relationships with him and the Press are of very long standing. They go back to the collaboration between him, Professor William N. Chambers, and myself in producing *The American Party Systems* in its two editions (1967 and 1975). When Mr. Meyer informed me that he thought there would be a market for a work along the lines of the present volume, and that there were

^{**} Gabriel Almond and Sidney Verba, *The Civic Culture* (Boston: Little Brown, 1963); Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *The American Voter* (New York: Wiley, 1960); Robert E. Lane, "The Politics of Consensus in an Age of Affluence," *American Political Science Review* 59 (1965): 874-95.

substantive justifications for doing it besides, it was easy for me to believe him, and to agree to undertake the project.

The intellectual debts which one acquires over the years are enormously large. Many of them are acknowledged in the Introduction which follows—in particular, those I owe to the scholarship of V. O. Key, Jr., E. E. Schattschneider, Louis Hartz, Lee Benson, and the Michigan team: the late Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes. In more recent years I have profited greatly from my relationships with Professors Douglas A. Hibbs, the late Jeffrey L. Pressman, and my colleague Thomas Ferguson. Nor should I omit the exceptional importance of intellectual give-and-take with my graduate students over the years. It is scarcely possible to name them all here, and it seems invidious to name some without including many others as well. But they should know that their intellectual dialogue has been important in forming the changing shape of my ideas about American politics.

Grateful acknowledgment is made to the following organizations for permission to reprint certain essays in this volume: to the American Political Science Association for “The Changing Shape of the American Political Universe” and “Theory and Voting Research,” which appeared in the 1965 and 1974 volumes of *The American Political Science Review* respectively, as well as permission to publish an American Political Science Association 1981 convention paper, “Shifting Patterns of Congressional Voting Participation in the United States,” to which the Association holds the copyright; to the American Bar Association for permission to reprint “The Disappearance of the American Voter” from the proceedings of the Association’s 1978 symposium on voting participation; to the Academy of Political Science for my essay, “Insulation and Responsiveness in Congressional Elections,” which first appeared in the 1975 volume of its publication, *Political Science Quarterly*; to MIT Press for permission to reprint my essay, “The 1976 Election: Has the Crisis Been Adjourned?” which first appeared in a book published by them in 1978 and edited by myself and Martha W. Weinberg, *American Politics and Public Policy*, and to the editors of *Dissent* for permission to reprint my essay, “American Politics in the 1980s,” which first appeared in the Spring 1980 issue of that journal.

Finally, I should acknowledge a debt, both intellectual and intensely personal, to my wife Patricia. She has been, in St. Paul’s phrase, a “true yoke-fellow” in life’s journey. Her willingness to carry many burdens of family life has entailed no small sacrifice as she has sought to pursue her own professional career. Her energy, cheerfulness, and support have been, most literally, a *sine qua non* for me.

It should go without saying that no one except the author should be held accountable for any errors of fact or any insufficiencies of analysis.

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WALTER DEAN BURNHAM

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The Current Crisis in American Politics

Introduction

The Current Crisis

I

When Oxford University Press expressed interest in publishing a collection of some of my essays, I was flattered; but it also represented a serious challenge. A mundane but very important set of decisions had to be made at the outset of this project: which of my essays to include and which not. Beyond that preliminary stage, the question arose whether the essays selected for inclusion here had any common theme or themes, or on the other hand were related to each other only ad hoc and by virtue of a common authorship. Such a question very naturally arises. These essays were written over a seventeen-year period, from 1964 through 1981, and as is evident from their titles and contents, many of them were written in response to the stimulus of specific events and developments in American electoral politics during that period. This fact is related to another procedural decision that was made early on. The essays in this volume are published as they were originally written: they have not been reworked in order to gloss over analyses, perspectives, or predictions made at the time that today the author might find irrelevant, superseded, or plainly wrong. They, and he, must take their lumps accordingly, in light of what we know empirically and analytically in the early 1980s.

But this decision serves an important purpose that, I think, speaks directly to the underlying question of the unity of the work as a whole. For a unity does exist after all, and in spite of certain changes in the author's perspectives and research agendas over this period of nearly two decades. To explore this argument fully, something of an intellectual autobiography seems required in this introduction, as a means of evaluating the position of these essays in a long flow of scholarly activity centered on American electoral politics. This collection reflects the confrontation of one man with his times on the one hand, and with the dominant intellectual trends within American political science on the other.

I grew up during the Great Depression and the Second World War, in the Pittsburgh area. My parents—"middling" middle class, no more and no less—were the embodiment of the Protestant work ethic; I early, if imperfectly, learned that life was difficult and ringed about by scarcity, but could be mastered by determination and hard work. It was evident very early to them and to others that I had a marked intellectual bent that set me sharply apart from the business-class residential milieu in which I grew up. It is eternally to their credit that they managed the resulting tensions and dissonances with intelligence and understanding.

My first conscious memories outside the family-neighborhood milieu were of the Great Depression itself, the Nazi invasion of Norway in April 1940 (which engulfed my mother's cousins and shortened their lives), and the 1940 presidential election. As Bruce Stave has pointed out, unemployment in the Pittsburgh area in 1934 stood at about one-third of the labor force, and it remained extremely high until World War II engaged the services of the "arsenal of democracy."¹ While we never joined the ranks of this "reserve army," one could not live without being aware of its existence and forming some reaction to it. At the time, this reaction importantly included opposition to Roosevelt and the New Deal, though my parents were considerably more moderate in their Republican sympathies than were most of their friends and neighbors. The town in which I grew up gave Wendell Willkie 85 percent of its vote in the 1940 election. As with the parents and the milieu, so with the son. Political socialization and the shaping of party identification worked very much as spelled out later by the authors of *The American Voter*²—except that, in retrospect, it seems rather clear that party commitments in our household were tightly woven into a general view of the world and a perception of political and economic goals congruent with party identification.

One day in the fall of 1940 I took the bus to downtown Pittsburgh, arrayed with several Willkie buttons. (In those days, no one seemed the slightest bit anxious about a 10-year-old making a journey by himself into the core of the "central city.") No sooner had I gotten off at my destination than I was surrounded by several men, far more shabbily dressed than my father. They gave me to understand very clearly that my Willkie buttons were not welcome. I removed them, put them in my pocket, and the scene dissolved without further incident. But this encounter made a vivid and life-long impression. When, later, I read *The People's Choice*³ and learned from Robert Alford's *Party and Society*⁴ that the index of class polarization reached an all-time high in the 1940 election, the arguments of both works fitted readily into this remembered experience. Perhaps this experience shaped the skepticism I was later to feel about the universal generality of certain propositions advanced by academic survey research in the 1950s and 1960s; perhaps not. In any case, this tale reveals my keen interest in electoral politics from early on, as well

as the intense stimulus of concrete experience in shaping one man's consciousness over the very long term.

My friends and colleagues have often noted in me a bent for historical and comparative analysis that is rather unusual among practitioners of American political science—so much so that not a few of them have asked why I didn't choose a career as a historian rather than as a political scientist. There is little doubt that such influence as my work may have had over the years has been as great among American historians as among political scientists, and it is hard to give an answer to the question that does not in the end turn upon the influence of specific intellectual mentors at decisive moments in my graduate education.

The Nazi invasion of Norway, and the enormous reaction to it in my household, was the first event that, as it were, propelled me out of the neighborhood and the city and into awareness of the much larger world outside. World War II completed this side of my education over the next five years in ways too complex to unravel even decades later. By the time I was fifteen, Hitler's Great German Empire had gone down in fire, ashes, and total destruction for which the fall of the Roman Empire alone might provide some parallel, and the nuclear age had begun with the bombing of Hiroshima and Nagasaki. It is hardly to be wondered at that such overwhelming events should have prompted me to consult the meta-historical reflections of Toynbee, Spengler, and others in a search for clues as to what had happened, and was continuing to happen, to the world. I find little reason to doubt, in retrospect, that my preoccupation with long historic sweeps, dramatic events, and the decoding of underlying patterns of meaning in a confusing melee of events had its germination in this early period of my life. That part of my work which has dealt with critical realignments and their systemic importance in the history of American politics very probably grew out of this seed.⁵ By the same token, much of the professional work reflected in these pages is shaped by my long-held view that the problems of American politics are often best grasped and analyzed in a comparative context.

My higher education was obtained at, successively, Johns Hopkins University, the United States Army, and Harvard University. At Johns Hopkins I pursued what was essentially a double major in political science and history. From Carl B. Swisher I learned American constitutional law and jurisprudence and also something about what made a great scholar and his scholarship; from Malcolm Moos, a wealth of detail about American politics as well as something of his infectious enthusiasm about it; and Thomas I. Cook gave me my first exposure to comparative politics and political thought.

Toward the end of my career at Johns Hopkins, one of my other peculiar talents came to the fore: that of amassing large quantities of data that interested me and, as it turned out, appealed to a larger audience as well. During the early 1950s the computerization of political science was still pretty much

in its infancy, and, indeed, the so-called behavioral revolution had also just gotten under way. To an extent that would be considered incredible today, the basic quantitative facts about American politics were scattered around the countryside. There was no question at that time of constructing a machine-readable archive of basic historical electoral data. That was to come much later, in a project in which I had a hand during the 1963–64 academic year: the construction of the election archives at the Inter-University Consortium for Political and Social Research at the University of Michigan.⁶

But in the 1940s and early '50s the only effort to construct a county-level compendium of American presidential election returns had been Edgar Eugene Robinson's two-volume work, *The Presidential Vote, 1896–1932*, and *They Voted for Roosevelt*,⁷ which continued the series through 1944. It occurred to me around 1950 that the series could well be extended backwards through the nineteenth century to the earliest date for which county-level presidential returns were pretty universally available, i.e., 1836. I undertook this labor, which was virtually completed when I entered the army in 1953. Two years later the work was published by Johns Hopkins University Press, and two decades thereafter was reprinted by the *New York Times*–Arno Press in its American historical documents series.⁸ Very much is owed to Professor Moos's consistent encouragement and support for this project, which included lining up external financial backing, then available only in extremely limited form but still crucial to the work's completion.

For a variety of reasons my three-year service in the United States Army, beginning in the last phases of the Korean War, was also a most important "higher education" influence in my development. In many respects I had hitherto led much too sheltered a life. This of course was now totally a thing of the past. Not only was I introduced to, and challenged by, a much wider range of human experience than I had known, but this experience changed many of my perspectives on the nature of social reality in the United States. The result was the beginning of a deep skepticism about many of the manifest values that were commonplaces among my generation and social class. Things were obviously not what that cosmology had taught me to expect them to be. This distancing was reinforced by a profound, year-long exposure to Russian language and culture given me by the Army Language School. The "mission" of this training, from the army's point of view, was to produce people who could knowledgeably participate in the communications side of its Cold War activities. But the Language School had other effects too. It was crucial in broadening my own horizons from the rather narrow, America-centered perspectives I had had when I entered the service. The culture to which we were exposed is profoundly different from our own, and the language—also different in fundamental ways from English—provided the key to some of the puzzles involved. While so very different from our own, this Russian culture has many rich and many sympathetic features, which my fellow trainees and I came to appreciate keenly as the year passed.

Some of my friends were eventually to make careers that, in one way or another, employed these skills and perspectives. I did not, though I gave the matter serious thought on entering graduate school in political science at Harvard University at the close of my army service. The Soviet political system was not a congenial subject of study for me, to put it mildly, and in the 1950s much Soviet-area analysis struck me as seriously deficient in important ranges of data, and as tending to lead its practitioners onto political and scholarly paths I did not care to travel. At the same time, it is hard to over-emphasize the importance of this experience and others that built on it later in my graduate career at Harvard. Again I was confronted with world history at its most cosmic, this time with the Bolshevik Revolution and its consequences at the center. My own aptitudes and talents continued to propel me in the direction of the study of American politics, but in a changed and more intellectually troubled context. This context came to include not only historical analysis of American political evolution, but a continuous internal incentive to place such study in a comparative context in order to deal with its problems and riddles, and in order to make it comprehensible.

Leaving the army in 1956, I entered the graduate program in political science at Harvard University. The most profound and far-reaching influences on my subsequent intellectual development were exerted by V. O. Key, Jr., Louis Hartz, and Barrington Moore, Jr.—the first energizing my interest in American electoral politics; the second opening my eyes to the dominance of the “liberal tradition” in American political culture and consciousness; and the last, in what was quite simply the greatest course I ever attended, giving me my first sustained exposure to the ideas of key modern social theorists and critics. All of them—and others too—cumulatively deepened my commitment to work with a historical, comparative, and theoretical focus.

For a time I thought that the specialization to which I would devote my subsequent career lay in the field of American public law (after all, I had been preoccupied with parties and elections for a very long time already!). For a number of reasons this did not work out well in the end. Then the Social Science Research Council approached me and inquired if I would be interested in spending the 1963–64 academic year at the University of Michigan, working on feasibility studies for retrieving the mass of electoral data for major offices across the political history of the United States. We moved rapidly from feasibility studies to being “present at the creation.” The project rapidly turned into the groundwork for the historical data archive of the Inter-University Consortium for Political and Social Research at Ann Arbor. In the process it became very clear to me that in all likelihood my best work in the years immediately ahead would be done in the analyses of electoral phenomena in the United States. Only somewhat later did it begin to occur to me that such analysis could be the point of entry for a new understanding of the ways the American political system as a whole functioned as pressures for change grew with changes in economy and society.

II

Mine was a rather peculiar entry into the study of American voting. This field had been, preeminently, the center of the "behavioral revolution" in American political science that had gotten fully under way around 1950. The citadel of training in this area had long been the Survey Research Center (later, the Center for Political Studies) at the University of Michigan, led by that remarkable team of scholars Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes.⁹ Their work forms an essential part of the classic corpus of contemporary voting analysis. In many respects praise is therefore superfluous, and criticism should be read with the constituent importance of the Michigan group's work always in mind.

It is, I think, accurate to say of this work that both its intellectual strengths and its limitations arose from the conceptual models derived from social psychology and small-group research that informed these authors. Their work added immeasurably to the identification of the attitudinal characteristics of the mass electorate. To some extent, however, this amplification was purchased at a price: a view of electoral politics that was singularly suited to the specialized case of the United States in the "Augustan age" of Dwight D. Eisenhower. In its initial stages, quite naturally, this work was not oriented toward comparative perspectives—that was to come later, and to full flower. It is scarcely surprising either that, with no national survey of fully acceptable research quality existing prior to the first Survey Research Center study of 1952, any longer-term time dimension was essentially absent. This also was to come later, notably in the "replication" study of 1976 performed by Norman H. Nie, Sidney Verba, and John R. Petrocik, *The Changing American Voter*,¹⁰ and in many other studies. Ultimately, however, the largely micro-oriented research strategies of modern survey research in its first years made comparative sense, as Erwin Scheuch pointed out in 1966, only in a country that was politically organized so as to lack the kinds of fundamental macro-level problematics that were commonplace experiences in European electoral politics.¹¹ As such problematics inescapably and continuously bring major issues of value to the fore of analysis, the capacity of American political scientists to ignore them (or, the same thing, treat them as givens rather than as problematics) was congenial to a behaviorist orientation that sought to develop only value-free generalizations.

Even in this early period, criticisms and reservations were expressed by political scientists deeply concerned by the spread of campaign technologies that were programmed to engineer consent while debasing the currency of political discourse. In particular, the late V. O. Key, Jr., with his profound commitment to democracy and his sensitivity to the multitude of devices by which it can be undermined, raised the alarm toward the end of his life about the growing triumph of market-research perspectives in political campaign and scholarship alike.¹² Key's own work was *sui generis*: an extraordinary

mixture of wit, the telling anecdote, rigorous analysis (with heavy reliance on simple but powerful aggregate data bases), and preoccupation with macro-level issues affecting the health and the future of democracy itself in the United States. My own intellectual debt to him—and to a few others, such as E. E. Schattschneider—is as obvious as it is incalculable. He was, of course, the first to set forth clearly the phenomenon of the critical election as an analytic problem, and to suggest something of its potential importance as an entering point for the study of system-level political dynamics. His classic *Southern Politics* remains not merely an example of what the anthropologist Clifford Geertz has called “thick description” at its best but a major statement of the consequences for policy when party competition is destroyed and electorates are shrunk to a fraction of their potential size.¹³ By the same token, his analysis of *American State Politics* evaluates the incredible array of constitutional and legal devices by which Americans, ever distrustful of political power and ever hostile to the development of a true state, have fragmented political resources and hamstrung the democratic representation of interests that only political parties appear capable of providing.¹⁴

Needless to say, epigones must find their own more humble place among the massive structures that giants built before them. What Key did was to suggest, in an integrated way, a whole series of research agendas that had a very different focus, orientation, and purpose from those that dominated much of the field of voting behavior and electoral politics at that time. My own work has been designed to carry some of them further along into a later generation. As it happened, American politics moved into an explosive crisis very shortly after Key died in 1963. This crisis, in its successive phases, has dominated that politics almost from that day to this, and seems certain to continue to unfold during the 1980s as well. Inevitably the developing crisis of regime in the United States has served to bring major, if not fundamental, conflicts over political values to the fore in its wake, and, as the authors of *The Changing American Voter* demonstrate, to reveal a much wider range of potentialities for political attention and consciousness among a mass electorate than might have been imagined around 1960. As the field of study begins to reveal sudden and profound changes in some of its most important parameters, analytic and descriptive challenges increase. Maybe these large changes have increased the salience and utility of my own work among those with particular interest in American politics, maybe not. In any case, they have had the effect of shaping the nature and focus of this work over time as the crisis matures and deepens.

III

By about 1964, then, it had become clear to me that certain important questions needed to be addressed. Probably the most enduring and important of all these questions turned on the causes, characteristics, and implications

(theoretical and empirical) of the periodically recurring critical-realignment sequence in American political history.¹⁵

Critical realignments are extraordinary upheavals in the flow of American electoral and policy history that occur under conditions of abnormal and general crisis. Realignment episodes involve a major increase in ideological polarizations among parties and political elites, more or less abrupt but thereafter durable shifts in the nature and social location of party coalitions in the electorate, and major changes in the shape and direction of public policy. Elsewhere I have referred to such sequences as "America's surrogate for revolution," and in fact they have had many revolutionary characteristics. Fundamentally different, then, from the electoral norm in the United States, critical realignments have historically recurred at remarkably regular intervals across American political history, at least until the Second World War. Demonstration that this sequence has empirical reality—and not merely in patterns of voting—implies that there are also historically specific "party systems" occupying the space between one realignment and the next. This in turn leads to the hypothesis that these party systems and the realignments themselves can be subjected to systematic comparison with each other.

Space will not permit here an extensive review of this set of phenomena. I have written one book on the subject, and propose to produce in the near future a more extensive and integrated study with this at its core.¹⁶ Others have done so as well, notably James D. Sundquist.¹⁷ And it does seem to be the case that at least some historicans and political scientists have found the party-systems model of use in organizing their work.¹⁸ It is perhaps enough to say here that this area of inquiry is fairly fully reflected in the essay "Party Systems and the Political Process," Chapter Three in this volume. And though all of the essays can be read independently of each other, the others are also linked together in one way or another by critical-realignment and party-system questions.

But no less important than these issues are those that surround the evolution of the American electorate (and American democracy as a whole) over the past century. In this respect, my earlier work compiling the basic data of nineteenth-century American elections provided the perfect "deep background" for skepticism about the mainstream arguments that appeared to extrapolate certain timeless general properties of "mass electorates" from contemporary survey-research concepts and instruments. For it was evident that the shape of the nineteenth-century data was not readily compatible with many assertions in the mainstream literature. For example, levels of formal education are often asserted to be a powerful associative explanatory variable—pointing toward high levels of political cognition and increasing propensity to vote as education level rises. But then how do we account for the fact that, while the general level of education sharply declines as we recede into the past, the turnout rates go up, reaching a maximum a century ago (outside the South, anyway) that compares very favorably with the fully

mobilized electoral politics of advanced industrial-capitalist countries in Western Europe? If “surge and decline” help to explain Eisenhower’s victory in 1952 and the subsequent Democratic victories in the 1954 congressional elections, why is it that no such phenomenon could be detected in the aggregate data before 1900?¹⁹ A full analysis of these longitudinal files reveals, in addition, not only that electoral participation began a spectacular and protracted decline after 1900, but that parties began visibly to disintegrate at the same time.

What were the explanations for all these changes? What is the balance of probabilities when it comes to accounting for the behavior patterns of nineteenth-century electorates—so far as we can retrieve evidence of such behavior—in terms of the basic frames of reference found in survey-research models of the 1950s and 1960s? A preliminary, and in many ways imperfect, attempt was made to address some of these issues in the first chapter in this collection, “The Changing Shape of the American Political Universe.” This article occasioned some comment and not a little criticism, especially from Professor Philip E. Converse and his student Professor Jerrold G. Rusk.²⁰ Eventually these criticisms prompted a rejoinder, “Theory and Voting Research” (Chapter Two in this collection), which is best understood as a rather sustained balance-of-probabilities argument based on circumstantial (if pretty extensive) evidence. Some may regard such controversies as mere ego trips by the participants. This would, I believe, be an unnecessarily personalist view of the matter. A reasonably accurate view of the past is a matter of exceptional importance, not only in bounding comparisons between it and the present, but also in ordering a clear statement of the temporal and sociological limits of any would-be generalization based on the present alone. To my mind, at any rate, the balance of probabilities supports the view that nineteenth-century Americans behaved politically in very different ways from those of today, and that a primary reason for this could be found in sociological and economic conditions that were optimal for participatory electoral politics on a scale that has no counterpart today. As always, the reader is invited to make his or her own judgments and balances of probabilities.

As is now very well known, the United States has by far the lowest electoral participation rates to be found in any Western country. They are not only the lowest, they are incomparably the most class-skewed. While the average turnout among Swedish or German manual workers approximates 90 percent, recent presidential-year turnouts among their American counterparts hover around the 50 percent mark. Among the upper-middle classes, on the other hand, the participation rates would tend to cluster around 90 percent and 80 percent, respectively. In off-year elections (when, as it happens, most American governors and other state officers are now elected, along with the U.S. House of Representatives and one-third of the Senate), these turnouts are more abysmal still, averaging outside the South about two-fifths of the potential electorate in 1974 and in 1978. This was not always the