

Review Essay

Voters, Absent and Present

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Walter Dean Burnham, *The Current Crisis in American Politics* (New York: Oxford University Press, 1982), 322 pp., hardcover, \$29.95, softcover, \$10.95.

Paul Kleppner, *Who Voted? The Dynamics of Electoral Turnout, 1870-1980* (New York: Praeger, 1982), 206 pp., hardcover, \$29.95.

Conventional political historians tell colorful stories of particular politicians or election campaigns or analyze the appeals of what they claim are the underlying ideologies of groups or of eras. Basing their accounts on the impressions of interested observers as recorded in letters and newspapers, they usually shun open and self-conscious theorizing and straightforward, falsifiable conclusions, and they pay scant attention to election returns, legislative roll calls, and other quantifiable data. Social scientific political historians, on the other hand, specify their assumptions and models, focus more on countable than on "letteristic" evidence, examine electoral systems or strings of elections, and aim to generalize. For traditionalists, questions about trends in voting turnout simply do not arise. The publication of these two books demonstrates both how much the newer approach has changed the research

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agenda of political history and how important questions of turnout have become. Yet reflection on their analysis suggests that the abandonment of the focus on the particulars of single elections and candidates may not be a wholly unmixed blessing for political history.

The first book is a collection of overlapping, provocative essays published since 1965 by Burnham, a political scientist who has contributed perhaps more than any other to the invigoration of American electoral history, while the second, by one of the most prolific and serious of the new breed of social scientific political historians, is the most comprehensive survey of the history of American voting participation yet published.

Both Burnham and Kleppner concentrate on the same major changes in voter participation—the decline in non-southern turnout after 1896, its partial recovery after 1930, and its relapse after 1960. (Burnham views the South as a deviant case, while Kleppner's excellent treatment of southern developments is peripheral to his main concerns.) Both offer similar explanations. From 1840 to 1900, which Kleppner terms “the era of citizen mobilization”, the parties mirrored ethnocultural cleavages between the “psychologically isolated communities” which composed northern society (Kleppner, p. 47). Since politics at the local level “resonated emotionally” (Kleppner, p. 71) with group identity, turnout soared and remained high, despite the facts that national issues were largely irrelevant and that economic policies at all governmental levels were of minimal importance in the voter's calculus. Although voters had substantially fewer years of schooling than they do today and were much less likely to live in urban areas (both traits which correlate strongly and positively with turnout in current surveys), a much higher percentage of them voted regularly in both presidential and non-presidential years. Partly because non-official ballots were distributed by the political parties, the electorate split their tickets much less and voted for candidates for every office on the ballot much more than they do in the twentieth century.

The acceptance of cultural pluralism by William McKinley and the nomination of the pietistic William Jennings Bryan by the previously liturgical Democrats in 1896 untied the parties from their ethnoreligious moorings. No class-based parties succeeded them because of Americans' acceptance of the Lockean liberal

capitalist consensus (Burnham, following his former teacher Louis Hartz); because of the “reforms” of the “Progressive Era”, such as nonpartisan local elections and nominations by primary, which reduced the importance of parties; and because of the adoption of personal voting registration, which made the act of voting more burdensome. Since no viable socialist alternative emerged and no other line of cleavage neatly divided the capitalist parties, and since the Republicans usually easily defeated the Democrats in the North, those in the age cohorts that entered the electorate after 1900 participated at much lower levels than their older counterparts, especially if the newly eligible voters were working class, female, or foreign-born. Although ethnic and religious rivalries continued to be expressed in politics, they did not neatly coincide with party divisions. (Burnham makes an exception for the Smith-Hoover contest, while Kleppner pointedly ignores the 1928 election. Neither cites Allan Lichtman's *Prejudice and the Old Politics*.)

Joining already active Republicans who switched to the Democrats, many previously nonvoting members of the working class surged into the electorate to punish the party of Hoover and to reward FDR's move in the “second New Deal” to more genuinely pro-labor and redistributive policies. Nonetheless, voter registration laws, enacted earlier, presented the full mobilization of the working class (Kleppner), and the historic tendency to keep policy within consensual bounds forestalled a party split purely on class lines (Burnham).

After 1960, both parties, but especially the Democrats, were disrupted by several sets of circumstances: the rise of such issues as civil rights, Vietnam, and abortion, which did not divide the electorate along the same lines as did the partial class cleavage of the New Deal party alignment; the increasing tendency of nominees at all levels to run campaigns independent of the party structures; the changes in the rules of nominating systems which increased the influence of ideologues at the expense of traditional elected pragmatists; and the growing importance of interest groups—Burnham (p. 154) calls it “hyperpluralism”—which offered people a means to affect policies of particular concern to them instead of sending politicians more diffuse messages through the ballot box.

As partisan decomposition in the electorate proceeded and as

the number of incumbent-protecting "collusive gerrymanders" increased, outcomes in Congressional districts became both less competitive seat by seat and less uniform across all seats and party loyalty in Congress decreased. These developments, in turn, reduced the informative value of the partisan cue to both legislators and voters, which further diminished the importance of the parties, and so on. (Burnham, pp. 215–219.) Potential voters, especially those who came of age after 1960 and those who were unrepresented by strong organizations, found politics increasingly difficult to make sense of, parties less and less relevant, and their own efforts to determine policy through voting increasingly ineffective. Consequently, more abstained, particularly working class voters and the young.

In European countries for which comparable data are available, only slight differences in turnout appear between those in high and low status occupations, the well and poorly educated, and the young, middle-aged, and old. In the U.S., on the other hand, gaps in participation between these groups are currently very large and they have grown markedly since 1960, a result, Burnham believes, of the Democratic party's move from the left toward the center of this country's narrow political spectrum. (See Burnham, 124, 152, 184, 188–189.) When no revitalizing "critical realignment" occurred, American politics, with its crumbling parties and its increasingly alienated voters, entered what Burnham pontifically terms a "conjunctural crisis," which is part of a "general crisis of capitalism" (pp. 230, 243, 260). The older, more affluent electorate which remained gave us a government which reduced rich people's taxes and spent money chiefly on care for the old and dying and on preparations for Armageddon.

Burnham and Kleppner both reject the implicit claims of the dominant "Michigan School" of electoral behavior that the *American Voter's* generalizations hold for all democracies for all time and that survey research is always the best evidence with which to validate empirical political theories.¹ Pouring over aggregate data, historical as well as contemporary, for other countries as well as America, Burnham has stimulated students of American politics not so much by challenging the fundamental concepts of Ann Arbor as by throwing doubt upon the stability of the model's parameters. Along with Kleppner, he accepts the view that party identification is of crucial importance, that the electorate can use-

fully be divided into "core," "marginal," and "nonvoting" groups on the basis of their degree and regularity of participation, and that most voters have little knowledge about or comprehension of the political world. What they doubt is that the influence of parties *per se*, over and above the groups they represent, has always been the same as it is now; that the proportions of the core and marginal components of the electorate and the correlates of those components with other social groupings have remained constant; and that politics has mattered as little to the average voter as some Michigan scholars have claimed that it does today.

Yet it may be useful for political historians to question the Michigan orthodoxy more basically, as many political scientists already have. In current surveys, how distinguishable is party identification from vote intention? How much does the often-remarked volatility in panel studies of individual self-placement on the Michigan "seven point scale" from strong Democrat through independent to strong Republican undermine the notion that party identification is a stable "long term force"? Is it clear that the "core" electorates in local, state, and national contests overlap completely? Or might different people, or the same people at different stages in the life cycle, perceive that they have more of a stake in elections at one level than in those at another and therefore participate at different rates in different sorts of elections? Indeed, one would have expected the ethnocultural school's view that local and state elections in the nineteenth century mattered more to the voters than national ones did to lead to a rejection of the concept of a constant core electorate. Finally, are the belief systems of the voters as incoherent as Phillip E. Converse contended in his famous 1964 article, or are voters' constructs simply different from what some political scientists expected and more dependent than they once realized on the clarity of the choices offered by elites?² What model best explains the voter's decision process—a socially deterministic one (e.g., ethnocultural), one focusing on party and personality (e.g., the *American Voter*), or a theory which assumes that the voter maximizes or minimizes on the basis of his preferences and the options open to him (rational choice theory)? By drawing attention to the actions of candidates and political workers, the latter approach casts a different light on the history of American electoral participation than Kleppner and Burnham do.³

Like Michigan's, Kleppner's concepts derive primarily from social psychology or sociology. In this work, for instance, he explains the decline in northern turnout after 1896 primarily as a result of a change in the correlation between two "long term forces"—religion and the political parties. Before that date, the Democrats and Republicans were, he believes, little more than other names for sets of socially competitive liturgical and pietistic ethnoreligious groups. In the new century, he argues, primarily from what seems to me the book's crucial table (p. 78), which gives estimates of voting by members of each party in four probably unrepresentative referenda on prohibition, ethnocultural allegiances did not map one-to-one into partisanship. How members of each circle first came to define other camps as "negative reference groups", how their conflict persisted so long in such a geographically mobile, not very ethnically segregated, society as white America has always been, and how the national issues of the tariff, economic development, inflation, antislavery, and Reconstruction meshed with these locally oriented conflicts, he never satisfactorily explains in this or other works.

Nor is it clear how the numerous and sporadically powerful nineteenth-century third parties, which Kleppner has treated insightfully elsewhere, fit his present scheme, for they surely muddled the relationships between the major parties and ethnic, religious, or economic groupings. Furthermore, if ethnoreligious issues on the local or national levels brought voters to the polls, there were plenty to stimulate them after the turn of the century. Positive government and piety in politics reached post-Reconstruction highs in the 1900–1920 period, "Progressive Era" battles over liquor or "boss rule" or "Americanization" often pitted ethnic groups against each other, and Republicans usually continued after 1896 to be identified with prohibition, while "wets" generally found nonsouthern Democrats more responsive to their cause. Kleppner's contrast between tightly linked social and partisan systems before 1896 and loosely associated ones thereafter seems too overdrawn to sustain the interpretive weight which he puts on it.

Kleppner also exaggerates the importance of another favorite sociological notion, that of generational change. In order to estimate age cohort differences in turnout during the 1900s and 1930s, he makes the crucial, unexamined assumption that older people continued to participate in the same proportions that they had

previously, and that young people just entering the electorate were responsible for all the gross turnout changes which took place (pp. 68–69, 90–91). Voters, on this account, are so strongly socialized into their habits that only the turnover of generations can alter the political landscape. Yet Kleppner as well as Burnham also adopts V. O. Key's belief that people are more likely to vote in close elections than in those that are one-sided. Since this view implies that the masses or at least campaign workers, who presumably work harder to turn out votes in tight contests than they do in runaways, are rational calculators who alter their behavior with political conditions, it seems incompatible with the assumption that voters never break their habitual patterns.

Burnham's Marxist functionalism has never been more apparent than in this book. "The state is primarily in business to promote capital accumulation and to maintain social harmony and legitimacy" (p. 256). Critical realignment serves as "America's surrogate for revolution" (p. 101). The political "system of 1896" had the "function" of insulating industrial elites from attacks from below (pp. 97, 110, 142). There will be little change in American politics in the 1980s "unless or until the survival needs of the dominant mode of production require it" (p. 264). All the familiar difficulties with functional explanations arise here. Vague in their mechanics and statements, functional interpretations are hopelessly deterministic, difficult, if not impossible to falsify, and confuse effects with causes. Most importantly, the roles of individual actors are unclear. Who speaks for capitalism's needs, and how directly need these representatives of the bourgeoisie be responsible for the changes for such an explanation to be accepted?

In any case, both Burnham's and Kleppner's books—as well as those of many others—stand as refutations to Robert Berkhofer's (1983) contention that social scientific histories are politically conservative. Both, for instance, quote the same statement from Gramsci (Burnham, p. 14, Kleppner, p. 112), and neither views the course of American political history as necessarily desirable or the state as neutral. Burnham, for instance, refers to twentieth-century American electoral politics as "a history of excluded political alternatives" (p. 17).

Each of these scholars leans toward the behavioral side on the question of whether legal or behavioral changes primarily account for differences in northern turnout over time and between areas. (Both concede that suffrage laws have mattered more in the South.)

They conclude that the institution of registration laws explains 20 to 40% of the post-1896 decline in participation and they acknowledge but minimize the effect of woman suffrage in depressing measured turnout. Although Kleppner notes the passage in various non-southern states of literacy tests, secret ballot provisions, laws ending alien suffrage, and those establishing direct primaries and prohibiting candidates from running simultaneously under two different parties' labels, neither he nor Burnham tries to determine the combined effects of these with registration and residency laws, nor do they attempt to assess possible interactions between legal and behavioral changes. (See Kleppner, pp. 58–60, 66, 87; Burnham, pp. 71, 75, 78, 125, 139–40, 155, 169.) Such interactions were probably particularly important in reshaping the political universes of campaign workers in all parties and potential candidates of minority parties, whose actions in turn crucially affected the rate of participation by the masses. Instead, Burnham and Kleppner estimate the impact of registration laws and the enfranchisement of females separately and the other laws not at all, and they assign all residual changes in turnout to the behavioral category. An inadequate research design and a false (because absolute) dichotomy between institutional and behavioral causes undercuts Burnham's assertion that "behavioral rather than statutory change was the crucially important development in the evolution of the American electorate from 1900 to 1940 . . ." (p. 75).

Although he has played a central role in collecting and making available county-level election returns for the whole country and in popularizing the uncomplicated analysis of time series in electoral research, Burnham has never fully exploited these data nor employed the more sophisticated methods for teasing meaning out of series developed by statisticians and econometricians. While admiring his ingenuity and the suggestiveness of his analyses, readers must be frustrated at Burnham's repeated tendency to be satisfied with "preliminary overview[s]" (p. 187) and to examine the data from only a few areas or at too aggregated a level (e.g., p. 220).

A clever data analyst himself, Kleppner is more thorough than Burnham, often making use of county-level data for the whole country, and he relies somewhat more heavily than Burnham does on multiple regression. Indeed, this is by far Kleppner's most methodologically sophisticated work. One's frustration with

Kleppner, rather, arises from his insufficient explanation of his often quite peculiar choices of variables and specifications of equations and from his tendency to fall back on "eyeball" techniques and to discard or undercut his own results from fancier methods when the outcomes of such procedures do not support his preconceived notions (e.g., pp. 26–27, 43, 57, 73, 184 n. 36).

Since he believes that political interest in the nineteenth century centered on state and local issues, it seems odd that Kleppner almost always uses data from presidential and congressional returns. Often held on different dates in that period, state and national contests may have involved somewhat different sets of electors. Second, Kleppner generally divides his data into three regional groupings by state ("metropole," "periphery," and "border"), but does not justify these groupings theoretically or, apparently, shift them over time. Why should voters in the census category "East North Central" behave differently from those in the "West North Central"? Should citizens of Maine and Vermont really be classed with those of New York City? Are Angelenos and denizens of the San Francisco Bay Area as parochial in the 1980s as they were in the 1880s, or should they now be classed in the "metropole"? Third, why does Kleppner not just include all of what he believes are important independent variables in his equations, instead of confusingly running regressions using subsets of variables (e.g., tables 4.3, 4.5, 5.3, and 5.7)? Fourth, does his "under 21" variable really reflect the proportion of young voters in the electorate, since few under 21 could vote, or is it not in fact an indication of the middle aged, who were more likely than the young to have children in this age range? Likewise, does his proxy for education—the proportion of those aged from 5 or 6 to 20 who were enrolled in common schools—adequately capture the educational levels of those over 21? Fifth, Kleppner's theory relates competition between the parties not to gross religiosity, but to membership in competing ethnoreligious groups. Why, then, in his ambitious attempts to use path analysis to model the connections between religion, party competition, and turnout before and after 1900 (tables 3.4 and 4.9) does he use the percentage belonging to all churches as his basic indicator of societal effects? Why does he not include other potentially important societal determinants of turnout, such as wealth, in these models? Why does he merge figures from five presidential elections in the first

table and seven in the second, instead of testing whether the parameter values of the model differ from election to election? Why does he conclude that religious membership underlay the 1876–92 turnout pattern when, by his chosen standard of statistical significance, only 7 of 18 path coefficients for northern subregions are significant, and three of those are not in the predicted direction?

The tendency to seek deep meanings in evanescent happenings is the seemingly unavoidable occupational hazard of the commentator on current politics. Few perform the role of pundit more sensitively and none, with a more comprehensive knowledge of American electoral history than Dean Burnham does. Yet while at times he underlines the election-specific nature of political outcomes, as in his recognition that the 1980 election reflected primarily “a vote of no confidence in an incumbent administration which had stumbled into an inchoate economic conservatism” (p. 289), he perhaps more often characterizes the conditions produced by sequences of such events in such orotund phrases as “a generalized crisis of legitimacy” (p. 260). To what extent is the decline in northern turnout since 1960 the product of some sort of fundamental transformation of society or politics and to what degree is it, on the contrary, the result of transient factors?

Some curious paradoxes parallel the increasing class participation gap. First, there are apparently few systematic differences on political issues between voters and nonvoters (Burnham, pp. 169–70). If this is true, then nonvoters may be satisfied to let others who hold the same views represent them in the electorate. Under these conditions, no crisis of legitimacy or representation exists unless the nonvoters’ consciousness is deemed false. Second, turnout declined at the same time that federal redistributive programs were first growing, then becoming institutionalized (up to 1980), and when partisan disagreements in Congress and in many state legislatures were becoming more pronounced. As the government’s role made more of a difference to lower-class voters and as the parties offered starker choices, the lower strata, *ceteris paribus*, should have become more, not less active. What other things weren’t equal? Third, even though Democratic presidents have, as Burnham charges, moved rightward, and even though the increased polarization of party elites would seem to have been caused by a dramatic Republican shift to the political right, there

were larger issue differences in 1981 in California between Democratic activists and the masses of Democrats than between Republican activists and either Republican or Democratic masses (Burnham, pp. 197–99). These facts suggest that it is not to the voters or to the lower party elites, but to the presidential candidates, successful and unsuccessful, that we should look for clues to the turnout decrease.

The Democrats have had an unusually bad string of luck—two Kennedys assassinated when their nominations seemed highly probable, and one, who otherwise would surely have been nominated, besmirched by scandal; two incompetent outsider candidates, McGovern and Carter; another president who practiced the military anticommunism which most of his predecessors and successors have been content just to preach. Would participation have sputtered so if the Democrats had nominated a publicly untainted Kennedy in any election between 1968 and 1980, if Johnson had carried out his 1964 pledge not to “send American boys to do the fighting” in Vietnam, if Nixon’s dirty tricks had not thrown Muskie off track in 1972, if Carter had managed to bring the Iranian hostages home in October 1980, if a shift of a few thousand votes in Iowa and New Hampshire had allowed Mondale to avoid a deeply divisive nomination battle that distracted the attention of the voters and his organization from the incumbent?

Attitudinal correlates of the fall in voting may also be transient. In the face of widespread governmental wrongdoing and open flouting of the reasoned views of wide sectors of opinion during the Indochina War and the Watergate scandals, a decline in the voters’ sense that their actions can affect general policy and an increase in the distrust of leaders seems natural, even healthy. That such politically traumatic events should have affected first one party, and then the other repelled voters away from both parties into independency or away from electoral politics altogether. Is it not likely that the Republicans would have achieved long-term majority status if, in addition to Vietnam, LBJ had been caught in a Watergate-type scandal, or, conversely, that the Democrats would have if Nixon had been elected in 1960 and 1964, had followed the same policies in Southeast Asia as Kennedy and Johnson did, and had been caught in something like Watergate?

The French *Annales* school has long disdained politics as the mere “history of events,” preferring to concentrate on longer-term

economic and demographic structures. Although American social scientific political history is in no danger of becoming lost in the *longue durée*, the increasing and otherwise admirable tendency to see through campaign triviality to more fundamental, less rapidly changing factors should not bias us so much as it often does, in these and other books, toward downplaying at least the combined effects of seemingly superficial events. The new political history has not yet absorbed all the useful lessons of the old political history.

NOTES

- 1 Space limitations obviously prevent an adequate review of the works of the "Michigan School" and its critics here. Since the original publication of Angus Campbell and colleagues' *American Voter* (1960), its strongly stated, though occasionally temporally, and more often geographically qualified, generalizations have been considerably modified, in part, no doubt, because of Burnham's trenchant criticisms. Among the most significant reformulations are Norman Nie and colleagues' *Changing American Voter* (1976), and the essays in the second edition of *Controversies in Voting Behavior* by Richard G. Niemi and Herbert F. Weisberg (1984).
- 2 Converse, "The Nature of Belief Systems in Mass Publics," originally published in 1964, is reprinted conveniently with one prominent critique, Norman H. Nie's and Kristi Andersen's "Mass Belief Systems Revisited: Political Change and Attitude Structure," in Richard G. Niemi and Herbert F. Weisberg, *Controversies in American Voting Behavior* 1st ed. (1976) pages 85-137.
- 3 Burnham mentions the work of Anthony Downs, but does not really exploit it. For an effort to apply a rational choice-inspired approach to the history of voting participation in America, see my "Suffrage and Political Participation," in Jack P. Greene's *Encyclopedia of American Political History* (forthcoming).

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