

KEY CHANGES

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Bruce A. Campbell and Richard J. Trilling. *Realignment in American Politics: Toward a Theory.* Austin: University of Texas Press, 1980. xi + 352 pp. Tables, figures, notes on contributors, and bibliography. \$22.50.

The notion that electoral history may be divided into long periods of stability broken periodically by major shocks has been the central organizing motif of American political history for a generation. Drawing on the simple empirical observation that the balance of electoral support for the major American political parties across geographic units remained roughly the same for a sequence of contests, and then shifted rather suddenly into a new and lasting pattern, V. O. Key, Jr., Lee Benson, Walter Dean Burnham, and others sought to do more than provide descriptive tags for conventional historical "eras." They attempted, by relating political to social cleavages, to explain why voters' decisions stood for so long (for instance, in Benson's "ethnocultural thesis"), and to show how wars, depressions, institutional changes, or intraparty struggles undermined these stable voting configurations (for instance, in Paul Kleppner's view that a common revulsion to the Democrats' failure to avoid economic depression combined with a differentiated voter response to Bryan's fundamentalist Protestant zeal shifted the social correlates of politics in the 1890s). Realizing that since it was based on aggregate election data, their hypothesis of stability and change was susceptible to the "ecological fallacy" of inferring individual behavior from measures available only for collectivities, the critical elections theorists tried, in effect, to supplement aggregate returns for the past with evidence of the long-term stability of party identification drawn from recent surveys. Focusing attention more closely on certain crucial variables and contests, encouraging political scientists to escape their parochial habit of present-mindedness and historians to overcome their predilection for concentrating too much on details and too little on basic patterns, the concept of normal elections broken by swift realignments has been beneficial to both disciplines.

Yet the notion suffers from four major deficiencies, two of which the book under review seeks to remedy: In the first place, the idea of critical realignment, basically an empirical generalization, has no clearly stated

macro- or micro-theoretical underpinning. Just why do individual voters retain or alter their habits? What are the connections between changing institutional constraints and individual electoral decisions? Second, local, state, and national trends have been far from exactly congruent. What explains the divergences, and are they so grave as to undermine the larger generalizations, based as they are primarily on national presidential returns? Third, how can the notion best be statistically measured? How much change is "a lot?" How inert do stable periods have to be? And if different measuring techniques lead to different results, how can they be reconciled? Finally, since the primary purpose of voting is presumably to affect governmental policy, how are votes translated into policy decisions? In their thirteen essays the authors, who are all political scientists, make only a token effort to provide a coherent theory of realignment, recognize but do not attack the problem of different national and subnational patterns, and finesse the issue of measuring stability and change in the electorate entirely by merely assuming that certain elections were realigning and that others were not. For historians, who have devoted too little attention to the connection between voting and policy, the major interest in the volume lies in the six essays which deal with that connection.

Doubting that voters can markedly influence policy during periods of stability because their intentions may be unclear, or because key elected or appointed officials may have little incentive to respond to the voters' wishes, Bruce Campbell and Richard Trilling (following Burnham's argument in his 1970 book on critical elections) suggest in the first chapter that massive realignments play a crucial role in a democracy by periodically reestablishing voter sovereignty. Shaken out of their thoughtless or information-economizing lethargy by social or economic crises, many voters leave their former parties, and, either punishing incumbents or voting on the basis of new issues, help oust extraordinary numbers of sitting politicians, whose displacement upsets formerly settled relationships both within the legislature and the bureaucracy and between the two. While this is a useful skeleton which is fleshed out in several of the succeeding essays, it ignores the fact that major alterations in policy have sometimes taken place even without lasting realignments (for example, the passage of the Reconstruction Acts and the Fourteenth Amendment, made possible by the temporarily lopsided Republican congressional majorities of 1866), fails to account for the fact that some realignments (for instance, the Republican landslides of 1894-96) have not produced important policy changes, and draws too heavily on the twentieth-century pattern of relatively slow turnover in legislative and bureaucratic posts, a pattern which did not hold in the nineteenth century, when, after all, four of the five elections conventionally deemed "critical" occurred.

Only two of the volume's contributors dissent from the book's orthodoxy about the importance of critical realignments. In an essay on secular realignment which emphasizes changes in the party balance caused by the slow growth or decline in the homogeneity of political attitudes within specified groups and by migration and differential population growth across groups, Louis M. Seagull remarks that "critical realignment has received undue and possibly inappropriate attention" (p. 69). And in a chapter which demonstrates the miniscule effects of the Watergate scandal on partisan attitudes and on levels of acceptance of the two-party system, Robert G. Lehnen concludes that "it is time to put aside the distractions created by the promise of finding 'revolutionary' changes and structural realignments and to confront the consequences that day-to-day politics have for most citizens" (p. 131). The other essayists usually keep more closely to Campbell and Trilling's initial framework.

But this framework hardly constitutes the "integrated theory of realignment" promised in the preface. Instead of offering a full explanation of how and why different voters and political entrepreneurs react to crises which render their usual decision rules useless, Trilling and Lawrence C. McMichael seek merely to define terms and describe, with terribly blunt statistical instruments, an instance of apparent realignment in Pennsylvania from 1924 to 1940 in the second essay. (Regrettably, they seem unaware of Allan J. Lichtman's 1976 *American Historical Review* article, and therefore escape having to deal with his very different and much more nuanced portrayal of the same elections.) Since neither of the two main branches of theory about electoral behavior explains major shifts—social-psychological theory views party loyalty as a deep-seated habit largely inculcated in the socialization process, and rational choice theory assumes stable voter tastes—one may sympathize with the authors' inability to supply a theory, yet at the same time wish they had confronted this most significant of all current tasks in the study of realignment.

The essayists are much better at describing the mechanisms of policy change. In a paper relating realignments to the age and social status of Congressmen from 1870 to 1970, Lester G. Seligman and Michael R. King find that losers and winners of congressional elections resembled each other fairly closely during stable periods, but that they were much more sharply differentiated in the years around 1896 and 1932. For instance, winning challengers were on the average about six years younger than losing incumbents in 1928, but seventeen years younger in 1932, and the age gap returned to a more normal nine-year difference by 1938. These differences, which paralleled gaps in social status, at least in the thirties, were common to both political parties. That is to say, in both the Republican and Democratic parties, older, higher status cohorts lost out to younger, lower status

individuals around the time of the electoral shifts. This shift in the character of lawmakers suggests both how new policies get adopted and why, once adopted, they are not repealed when the electoral tides recede. The old guard does not return, for it has been retired from leadership in both parties. Based primarily on the 1930s, Seligman and King's generalization might not apply so well to earlier periods, when the typical level of congressional replacement was much higher, but a longer-term analysis of indices similar to those which they have gathered might explain a great deal about national policy changes even in years when lasting electoral shifts did not occur.

Although Seligman and King do not relate turnover directly to policy, in a parallel essay David W. Brady does examine the relations between gross turnover in Congress and its key committees, as well as changes in the party balance in Congress, and indices of policy change developed by Benjamin Ginsburg in a 1976 article in the *American Political Science Review*. Discovering weaker connections than he had expected (perhaps because of the difficulty of measuring his dependent variable precisely), Brady postulates, without systematically testing it, a model in which, for major policy shifts to occur, voters have to choose a new president with a firm program, plus a new congressional majority. Although this model is so vague—what is a coherent program?—and so close to tautology as to be nearly useless, Brady's results do somewhat undercut those of Seligman and King, but unfortunately neither essay includes comments on the other.

In an excellent review of the burgeoning literature on bureaucratic behavior, Kenneth J. Meier and Kenneth W. Kramer show why rational bureaucrats do not respond to small short-term electoral shifts. Overloaded with information from sources other than the electorate and elected officials, protected by a merit system, by professional or "scientific" standards, by stable clientele relationships, and sometimes by legal autonomy, split on policy goals within and between agencies, established bureaucracies, Meier and Kramer hypothesize, will inhibit major changes unless their personnel are physically replaced. The extent of change from policy area to area, moreover, should be related to the legal independence of the relevant agencies and to the strength of their nonelectoral ties. Since replacement takes time, only a lasting electoral realignment will affect policies in many areas, and the policy shifts should be greatest where new bureaus are created or older bureaus are weakest, as, for instance, during the New Deal, or in state and local governments, which have bureaucracies which are both smaller and less secure than those of the national government. While they do not test these hypotheses, twentieth-century historians might, assuming they can solve the thorny problem of creating measures of bureaucratic strength and of policy change.

While some scholars have credited the Supreme Court with the role of legitimating policy shifts after realignments, David Adamany demonstrates convincingly through an analysis of recent opinion polls that the public and even political activities are insufficiently aware of the Court's decisions, particularly those which do not overturn laws or executive actions, for the Court to play such a role. He is less convincing in offering an alternative theory. Positing a mysterious inherent tendency for the Court and the executive to join together in opposition to the Congress, Adamany suggests that the Court, allying itself with the "presidential wing" of the prealignment majority party, helps bring on realignment by sharpening policy differences within that party and between the two parties. But even if he could rigorously account for the alleged coalitional tendency and justify his use of the idea that presidential/congressional dichotomies within the majority party pervaded American history (the period from 1869 to 1892 would be particularly troublesome), Adamany only claims his hypothesis works for five of eight assumed cases of national conversion or realignment, and if one insists that the judicial and electoral decisions be closely related in time (why should the impact of the 1905 decision in *Lockner* be delayed until 1932?), he would have a lower box score.

Emphasizing election-oriented speeches rather than roll call votes, which he believes distort congressmen's real views because of the influence of party loyalty, Charles V. Stewart charts changes in congressional opinion on the income tax from 1894 to 1913. Why did Democrats split on the income tax issue in the Fifty-third Congress, why was GOP rhetoric so vehement then, and why was opposition to the tax so comparatively weak and subdued by 1913? Stewart believes that in 1894 the passage of the income tax "threatened to crystallize an alliance productive of more fundamental reform" (p. 279); whereas by 1913 the Democratic-progressive Republican combination seemed much tamer. There are two major problems with this explanation: First, although some opponents feared the income tax would be a precedent for more "socialistic" measures, such a fear is not the same as a belief that passage of a 2 percent tax on incomes over four thousand dollars would itself weld together a radical alliance, as Stewart claims, and is even less a demonstration that it in fact would have solidified a farmer-labor party, as he comes close to saying. Second, Stewart vastly oversimplifies his story by leaving out shifts in sentiment on the protective tariff, for which the proceeds of an income tax were a substitute. In the 1890s protectionists were still puissant and must have realized, though they were naturally unlikely to say so on the floor of Congress, that the development of a major alternate source of revenue would diminish support for the tariff; by 1913, as Susan B. Hansen points out in the final essay in the book,

manufacturing exports had for more than a decade exceeded imports, the free trade constituency was consequently large enough to pass the Underwood tariff easily, and the revenue had to be made up somehow.

Hansen draws on a 1960 *World Politics* article by Anthony Downs in her ambitious exploration of two centuries of American tax policy. Rejecting the view that war or economic development account satisfactorily for tax policy because such factors do not explain very well the timing of changes or differences in tax rates across social groups or counties, Hansen focuses on variations in the political saliency of taxes and in the power of each political party. Since federal revenue consumed only about 5 percent of the GNP in 1870, as opposed to more than 20 percent today, federal taxes could not have been as politically salient then, she reasons, and politicians were probably freer to advocate raising them without suffering retaliation at the polls. Hansen's content analysis of references to taxes in national party platforms lends some support to this argument, for the amount of discussion of tax-related issues grew throughout the nineteenth century, only to be reduced in recent times to ritual pledges of lower taxes, and the two political parties took opposing positions on taxes more often then than they do now. As taxes have risen as a proportion of national income, politicians have increasingly indexed them to benefits or passed decision-making power—and potential blame—to experts. While not sufficient in itself, unified party control of the national government has been a necessary condition for important alterations in tax policy, Hansen shows in an insightful overview of tax policy changes from 1800 through the New Deal, and such sustained control has most often been associated with critical realignments.

While the authors of this book ignore or offer only very unsatisfactory solutions to several of the problems now apparent in the concept of critical realignments, the work should serve as a convenient introduction for historians to the developing political science literature on the connections between elections and policy, and on the policy process itself. Why should all the fun of explaining a century or more of policy be left to political scientists?

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