HISTORY – THEORY = ?

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Joel H. Silbey, Allan G. Bogue, and William H. Flanigan, eds. *The History of American Electoral Behavior.* Princeton, N.J.: Princeton University Press, 1978. xv + 384 pp. Tables, figures, list of participants, list of contributors, and index. $27.50 (cloth), $12.50 (paper).

Despite the fact that it emerged at roughly the same time as the "new economic history," the so-called "new political history" has, it seems to me at least, generated significantly fewer fresh, stimulating interpretations of American history than its cliometric sibling. That we political historians have produced no counterpart to, for example, *Time on the Cross* is due less to our lack of press agentry and desire to shock the bourgeois (on which Fogel and Engerman have no monopoly) than to differences in the character of the social science disciplines on which the new economic and political historians have drawn. Most economic historians have been trained in departments of economics, and have consequently been force-fed that rich blend of rigorous deductive microtheory and sophisticated statistics which distinguishes economics from all the other social sciences. Most political historians, on the other hand, have taken their Ph.D.s in history departments and absorbed political science, sociology, or social psychology as side dishes. More important, these side orders contain considerably less of the micro-theory/statistics spice than economics does. What has distinguished the final products of the cliometric twins is less the economic historians' familiarity with high-powered econometrics (the gap between the two offspring in this respect seems to be closing) than the disjunction between statistics and theory in political history, indeed, the gross underdevelopment of historical (or much other) political theory at all. The work under review, the product of a June 1973 Cornell conference sponsored by the Mathematical Social Science Board, unfortunately illustrates, rather than overcomes, these deficiencies in political history.

Thus, although his reputation as the leading "new political historian" rests largely on his role in originating the "ethnocultural thesis," which represents an attempt to construct a comprehensive, explicit, falsifiable model of individual and group voting behavior, Lee Benson, in an essay

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with Joel Silbey and Phyllis Field in this volume titled, "Toward a Theory of Stability and Change in American Voting Patterns: New York State, 1792-1970," approaches theory from a purely inductive stance. "Our aim," Benson et al. state, "has been to give as intensive and detailed a description of the trends present as possible on the assumption that the complexity of American voting behavior can be properly gauged in no other way. It is in our view a necessary preliminary to the eventual development of a comprehensive theory of stability and change in American voting patterns" (p. 82). Instead of building a deductive model of how individual voters and political leaders might behave in certain circumstances and choosing appropriate statistical methods and data with which to test the model or its consequences, as an economist would, Benson, Silbey, and Field argue in effect that theory will somehow sprout from description.

It may be that they take an inductive view because of the vagueness of the reigning set of hypotheses in the area on which they, as well as the authors of three or four of the other nine essays in this volume, focus, "critical election theory." Not only does the theory fail to specify just how individual voters might respond to the cues of political parties and other groups and how they might decide from time to time to attend to different cues, but it also fails to indicate just what degree of stability or variation it would take to support, invalidate, or force modifications in the theory. What level of correlation coefficients (which Benson, Silbey, and Field use), or "realigning surge" indicators (employed ingeniously and with originality by Nancy Zingale in a chapter on third parties in Minnesota), or individual voting records (recovered by David Bohmer in an essay on Maryland politics in the early nineteenth century which I found the most interesting chapter in the book), or factor loadings (reported in Robert Dykstra and David Reynolds's essay "In Search of Wisconsin Progressivism") is necessary to indicate the presence of factional stability? If different measures produce different results, how do we choose between them?

Thus, whereas Benson, Silbey, and Field conclude that there was too little stability in New York politics before 1820 to characterize it as a real party system, Bohmer contends that such a system did exist in Maryland. Zingale believes that she has found considerable stability in Minnesota from 1890 to 1940, while Dykstra and Reynolds seek to destroy Michael P. Rogin's contention that Wisconsin nurtured a continuous "Progressive" bloc over roughly the same period. Benson, Silbey, and Field dismiss contentions of stability and its mirror image, sudden realignment, for the years after 1893, but in their essay on "partisan realignment" Walter Dean Burnham, Jerome Clubb, and William Flanigan conclude, after finding a large degree of volatility in their data, that, "obviously, the phenomenon of critical realign-

ment survives all this chaotic variability" (p. 58). Is the variation in conclusions merely a function of using different measuring instruments, or does it reflect a lack of consensus on the definitions of change and continuity? And because none of the essayists concentrates on the theory per se, or even confronts the divergent conclusions of different authors who coexist in the same volume with them, critical election theory itself remains in a state of "chaotic variability," critical elections "phenomena" to be explained (if anyone could agree on how to recognize them) instead of subjects of a fully articulated theory.

Or consider Rogers Hollingsworth's essay on "The Impact of Electoral Behavior on Public Policy." In an attempt to answer the common criticism that nonimpressionistic political historians have spent all their time analyzing voting and none discussing policies which the elected leaders pursued, Hollingsworth draws on the least theoretical area in political science, the mindlessly empirical subfield of state and local policy outputs. Like the study of critical elections, this area grew out of some relatively short remarks by the brilliant, if often unclear, V. O. Key, Jr. I have often thought that Key's reputation among historians derived partly from his "humanistic" penchant for ambiguity and complexity (as opposed to the scientist's passion for clarity and simplicity). Key's suggestion of a connection between turnout, party competition, and the level of public services provided by government has often been misinterpreted by political scientists, and Hollingsworth follows their incorrect glosses on the nature of and reasons for the postulated connection. "An increase in the level of voting behavior," Hollingsworth states, "means that competing groups are generating more demands on the political system, which in turn requires political elites to do more to satisfy their clients" (p. 348). Having laboriously collected cross-sectional data for the year 1900 on educational, highway, sanitation, safety, and general expenditures for 154 American cities with populations between 8,000 and 25,000, Hollingsworth correlates normalized forms of these variables with indices or turnout and competition in local elections, and with a dummy variable for whether the elections were nonpartisan or partisan, as well as with per capita wealth. As many political scientists have found on the basis of more recent data, Hollingsworth concludes that the political process indicators have little effect on variations in the levels of public services, while wealth explains a great deal of that variance.

But has Hollingsworth tested Key's theory or any plausible theory? Key, noting that lower class people voted less regularly than those of higher status, believing that the lower orders would usually prefer more services even if it meant a higher general level of taxes and postulating that lower status people usually had even less access to politicians by nonelectoral than by electoral routes, concluded that an increase in lower class voting would typically lead to an increase in the services they received. Likewise, the more competitive the parties or factions, the higher the price of the last vote, which, if lower class members didn't automatically vote, would most likely be a lower class vote; given the postulated nature of their demands, increased competition would therefore lead to more services for the lower classes. But even if all Key's assumptions were correct (if taxes were regressive, lower class people might rationally desire fewer services, or they might, whatever the nature of the tax structure, misperceive their interests), his hypotheses, correctly interpreted, would predict that increased competition and turnout would lead to increased services only for those whose votes went late in the bidding and only for those services they desired. The theory is fundamentally concerned with the *distribution*, not the *level*, of services, and service levels are bad proxies for the distribution of such governmental expenditures as those for police, fire, highways, and often even education. Had he devoted more effort to constructing logical hypotheses before he collected his data, Hollingsworth, whose ingenuity in explaining away his results (pp. 367-71) demonstrates his considerable ability in manipulating concepts, would have advanced the subject more than he did in this essay, which even the editors of the volume found unsatisfying (see p. 345).

A third example of the inadequacy of theory and the disjunction between hypotheses and tests of those hypotheses is provided by the Dykstra-Reynolds essay. It has always seemed to me that the best answer to Michael Paul Rogin's question—whether the Populists and Progressives voted for Joe McCarthy or not—was the simplest: they couldn't have, since most were long dead. To expect to find continuity in the voting records of the same rural precincts over a forty-eight-year period (1904-52) which contained two world wars and a depression and which witnessed large changes in the state's economic and social structure, a major shift in governmental agricultural policy, and no doubt a great deal of population growth and many shifts in population, is simply nonsensical. Second, Dykstra and Reynolds exclude all towns and cities of over 10,000 in population in 1905, or about one-third of the state's people, from their sampling universe, a decision which no doubt cuts down the total amount of measured variation in electoral behavior and thereby reduces even further the probability of finding a continuous Progressive bloc of precincts. Third, instead of conducting a factor analysis of the whole era's election statistics, they break down the five decades into four periods, which further reduces the variance. Even more important, since the strength of the "La Follette vote" varied markedly between periods, the periodization itself biases the results against finding continuity in Progressive strength over the forty-eight years. In sum, Dykstra

and Reynolds erected a null hypothesis which could hardly fail to be rejected, and then, having biased their sample and their methodology against the hypothesis, proceed to announce that "there was nothing like a permanent bloc in Wisconsin in the first four decades of the present century, at least not in the countryside" (p. 325). No wonder radicals are often skeptical of quantifiers.

All this is not to say that the essays in the volume lack substantive interest or implications for hypotheses advanced in the historical or political science literature. Benson, Silbey, and Field would replace the notion that there have been five party systems in American history with the view that only the years from the 1820s to 1892 witnessed "stable partisan politics" and that even that era can be divided into two major and four minor periods. But it is not immediately apparent just what individual calculus of decision making would produce such electoral behavior or how the amount of instability they find even in the nineteenth century comports with Benson's and Silbey's faith that continual clashes between pietists and their ethnocultural opponents pervaded the era's politics, and they do not tarry long to spell out the implications of their unsettling results.

Several of the other essayists represented in the volume draw out the implications of their research somewhat more explicitly. While she does not relate her conclusions to the work of Anthony Downs and other practitioners of "formal" political theory—a school whose models political historians would do well to attend to—Nancy Zingale does speculate fruitfully on the conditions for the rise, continuation, and decline of third parties in majoritarian federal democratic systems. William N. Chambers and Phillip C. Davis, in "Party, Competition, and Mass Participation: The Case of the Democratizing Party System, 1824-1852," and Martin Shefter, in "The Electoral Foundations of the Political Machine: New York City, 1884-1897," conclude by seeking to eliminate other explanations that organizational effort was the key variable in producing, on the one hand, high mid-nineteenth-century turnout throughout the nation, and, on the other, late-nineteenth-century Tammany domination of New York City. Although both articles rest their cases largely on indirect evidence of the importance of party organization—none of the other variables they introduce consistently explains much variance, and they, in effect, assume that the large residuals are proof of the influence of party effort—their contentions should lead other scholars to reemphasize the process by which social forces are translated into electoral outcomes, instead of ignoring the role of mediating institutions, as "new political historians" too often have.

The role of institutions receives stress also from Jerrold Rusk and John Stucker in "The Effect of the Southern System of Election Laws on Voting

Participation: A Reply to V. O. Key, Jr.," which, like the article by John Shover and John Kushma on "Retrieval of Individual Data from Aggregate Units of Analysis: A Case Study Using Twentieth-Century Urban Voting Data," has been superseded by work published since the summer of 1973 by other scholars. Both of these articles are occasionally inaccurate or misleading, and neither has been updated to reflect even the books and articles which were in press when the Cornell conference took place five years ago.

In fact, the volume as a whole is much less useful than it would have been if the essays, including the introductions by the editors, had been altered to take into account more recent scholarship (none contains many references to work published since 1973), or if the scholars represented in the volume had even been forced to contend with the implications of each other's essays. As they stand, the chapters are so isolated and uncoordinated that it is difficult to see what either the authors or their readers gain by having them published together so many years after their preparation, instead of separately as journal articles some years ago. The volume is less the product of a true symposium than a set of disconnected, atheoretical essays gathered up and, after too long a pause, put between the same expensive covers.

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