

1993

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Recommended Citation

Sorauf, Frank J., "Book Review: The Supreme Court and Partisan Realignment: A Macro- and Microlevel Perspective. by John B. Gates." (1993). *Constitutional Commentary*. 343.
<https://scholarship.law.umn.edu/concomm/343>

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THE SUPREME COURT AND PARTISAN REALIGNMENT: A MACRO- AND MICROLEVEL PERSPECTIVE. By John B. Gates.¹ Boulder, Col.: Westview Press. 1992. Pp. xvii, 253. Cloth, \$55.00.

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The search for order, even meaning, in American political history is first of all the search for significant periods within the 205-year span of government under the Constitution of 1787. Presidential terms serve that purpose about as well as a list of the kings and queens of English history—milestones along the route of history that tell the distance but little more. Scholars find far greater analytical power in periods of party ascendancy—periods in which voters adhere with some stability to the two major parties and in which, perforce, one of them, the “majority party,” governs and puts its stamp on public policy. Even more useful analytically is the study of the great upheavals, the realignments, in party support brought about by shifts in the electorate.

It would be hard to exaggerate the avidity with which historians and political scientists have seized on alignments and realignments as the basic units of historical measurement in the last forty years or so. It was the maturing of sample surveys (i.e., polls) that sparked the explosion, since they provided for the first time reliable data on the party preferences of individual voters. The bibliography in Gates’s book runs to more than fourteen pages, substantial indeed even though some of the entries deal with the courts per se rather than realignment. Harold Bass’s bibliography in another recent book on realignment more generally runs to a staggering thirty-one pages and more than 700 entries.³ Realignment is indeed a flourishing industry among scholars of American politics.

Not surprisingly within so large a scholarly domain, there are sub-domains and tribes of specialists, each with a particular agenda of questions to debate. What fundamentally is the nature of the attachment (loyalty? identification?) of individuals to political parties in a nation with no tradition of formal membership in them—and how does one measure it? Are realignments caused by the conversion of voters from one party to another or by the sudden infu-

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3. Harold F. Bass, Jr., *Background to Debate: A Reader’s Guide and Bibliography*, in Byron Shafer, ed., *The End of Realignment? Interpreting American Electoral Eras 147-78* (U. of Wis. Press, 1991).

sion of new voters overwhelmingly into one of the parties? Do all realignments have the same dynamic and major characteristics—is there just one kind of realignment, fairly uniform regardless of time and context? And, indeed, will realignments go on “forever?” Or do we now see the end of realignments as party loyalties attenuate and as we linger in a dealignment that doesn’t seem to want to progress to the next realignment?

And then there are the questions about the relationship of realignments to policymaking (and policies made) in the Congress and the Supreme Court. Gates’s book obviously falls into the latter category. The relationship of realignments to policymaking, however, is in fact two questions. Do the policies made in Congress and the Court *before* the realignment help to define its critical issues and thus to bring the realignment about? And *after* the realignment, does the Court, reflecting the majority views of the old alignment, inevitably find itself at odds with the Congress that is the electoral product of the new one? Gates tackles both questions.

In fact, the Gates study is the most systematic and comprehensive one on the realignment-Court nexus in a literature graced by the work, *inter alia*, of Robert Dahl, Richard Funston, Jonathan Casper and David Adamany. Certainly his data base is wider—all instances of Supreme Court invalidation of both state and federal policies in the periods both before and after the major realignments. For good measure Gates even treats the somewhat inconclusive dealignment (and, a few would argue, partial realignment) beginning with the elections of 1960 and 1964. It is a net wide enough to catch 743 cases. To repeat, it is the inclusion of both national and state policies *and* the before and after impacts of the Court in the realignment that marks the broadened scope of this book.

Gates’s examination of the Supreme Court’s involvement with the cycles of alignment and realignment in American history is systematic, rigorous and monographic. There are precious few personalities or anecdotes here. It is serious, empirical social science, although one should hasten to add that the use of numbers is restrained and the statistical apparatus relatively simple. Each chapter—one realignment per chapter—has the same architecture, the same progression of topics; the reader always knows the neighborhood, even if at the price of predictability. There are also many, perhaps even a few too many, previews and summaries to sustain the thread of analysis. In short, the book is intended for the serious reader, and while the prose is certainly not a “quick read,” it is lucid and it requires no translation into colloquial English.

Gates’s conclusions are equivocal. In the author’s words: “The

evidence . . . does not consistently support either the policy conflict role following critical elections or the agenda-setting role before critical elections." (The "role" apparatus runs through the book and can, if the reader wishes, be safely ignored.) The Court's contributions to realignment agendas differed in the four instances under study, and the Court's conflict with the elected branches after realignment is clear only after the realignment of 1932. It appears, in other words, that we have hypothesized too great a judicial involvement in the politics of realignment, a result, I suspect, of inferring too much from the decisions of a conservative Supreme Court between the two wars and the epic battle centering on the Court in the 1930s.

With hindsight sharpened by work such as Gates's, it now seems that we have ignored a good deal we know about the Court and its decisionmaking in positing the Court-realignment nexus. (For their part, scholars of the parties have paid insufficient attention to the nonparty context within which party realignments take place.) Not all of the issues over which the Court has constitutional differences with the states and Congress become realigning issues; realignment has never involved all of our policy politics and differences. Moreover, the issues salient to the appointment politics of Justices may not be the critical issues of realignment either. And Justices do change their positions even on possible realigning issues; the shift of Justices O'Connor, Kennedy and Souter to constitutional middle ground on the abortion issue reminds us of that.

As for conflict with Congress and the President post-realignment, the hypothesis of the nexus underestimates the capacity of the Court for change and adaptation. Have we so soon forgotten the 1937 "switch in time" that saved nine? In such conflict within the separation of powers, the elected branches are not without their weapons, and the Justices, too, are in varying degrees sensitive to their anomalous position as life-long mandarins in a mass democracy and, thus, variously sensitive to broad popular opinion. Nor are the Justices incapable of changes in their own worldviews. In the metered prose of one of the great sentences in American jurisprudence—Benjamin Cardozo's famous dictum in *The Nature of the Judicial Process*—"[t]he great tides and currents which engulf the rest of men do not turn aside in their course and pass the judges by."⁴

Any exploration of the relationship between basic political shifts in the mass American electorate and the decisions of the Supreme Court inevitably raises the most fundamental issues: the

4. Benjamin N. Cardozo, *The Nature of the Judicial Process* 168 (Yale U. Press, 1925).

nature and origin of conflict between the Court and the elected branches, the impact of mass and specialized opinion on the Court and indeed the extent of "political" decisionmaking on the Court. A careful reading of the Gates book will stimulate many meditations on these and similar themes. To take only one example, Gates presents a convincing case that the partisan affiliations and auspices of the individual Justices are only marginally related to their voting on the Court. So, the addition of new data to the debate—on the roots of judicial decision in this case—shows us once again that reality is more complex than we would prefer.

Specialists on realignment and its various manifestations will find additional, more technical grist for their mills in the Gates book. First, on method, Gates is persuasive in arguing that the Court's constitutional relations with the states must be included in assessing the Court-realignment nexus. Changes in the policy agenda may be sparked and given new salience in the states as well as in Washington. More broadly, the more we explore realignments and the more we learn of their roots and dynamic, the more varied and complicated they seem. Realignments seemed at one time to combine explanatory power with a seductive parsimony. The seduction has been very real, but the parsimony was probably always illusory.

And where does Gates leave scholarship on the nexus? Certainly he has shifted the presumption from one of validity to one of doubt; the final weight of the evidence and the carefully drawn conclusions simply can't be avoided. For the proponents of the nexus three options occur to me. First, they can admit defeat and give up. Second, they might try to recast the argument by broadening judicial influence (or "role") to include patterns of policy interpretation as well as invalidation. But such a new conceptualization of judicial muscle raises the most formidable problems of measurement. (Talk about the loss of parsimony!) Third, they can pitch in to reconstruct a new and more complex set of hypotheses, a set that would specify different judicial contributions or impacts in different realignment contexts or dynamics. If we concede that not all realignments are alike, that is to say that the work of the industry has really only begun. Stay tuned.