

HANDBOOK OF LEGISLATIVE RESEARCH. Edited by Gerhard Loewenberg,¹ Samuel C. Patterson,² and Malcolm E. Jewell.³ Cambridge, Mass.: Harvard University Press. 1985. Pp. 810. \$32.50.

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Handbook of Legislative Research is a new collection of review essays assessing the state of political science research concerning legislatures. Many law professors will find it a handy compendium of information.

The editors, all respected political scientists, label their collection an "inventory of research." Part I, dealing with legislators and constituencies, addresses legislative recruitment and careers, the relationship between electoral outcomes and responsiveness to constituents, and a more general overview of the role and responsiveness of legislators. Part II, which concerns legislative organization and leadership, contains articles about the roles of parties and factions in legislatures, the committee assignment process, legislative leadership, legislative staffs, and legislative organization and rules. Part III, on legislative processes, examines standing committees; legislative voting; the legislature's relationship with and oversight of the executive branch; the supposed "iron triangle" of legislative committees, administrative agencies, and interest groups; legislative shaping of policies and budgets; and the utility of abstract models of the political process growing out of public choice theory. Finally, part IV presents some historical and comparative perspectives.

For law professors, the value of this book might not be readily apparent. In fact, however, public law theorists should find the work of substantial importance. Legal realism properly counsels the legal scholar to craft public law theory in light of realistic assumptions about the political process. In recent times legal scholars have not been hesitating to undertake this burden. Public law scholarship fairly abounds with writings proposing some adjustment of public law in light of empirical realities. Consider a few examples. A legal scholar who believes that administrative agencies tend to be captured by the interests they are supposed to regulate

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might propose that courts enforce a nondelegation doctrine.⁵ A legal scholar who concludes that interest groups dominate legislative decisionmaking might counsel courts to strike down statutes that embody deals that promote the private interests of powerful groups rather than public values.⁶ Another legal scholar of a similar empirical but different jurisprudential bent might see the judge's role as an agent of the legislature who must sanction—and perhaps even promote—whatever deals among groups are ratified by the legislature in the form of a statute.⁷

Legal scholars should not instantly modify public law theory in light of the latest nuances in economic modeling or empirical findings. Yet public law theory cannot be divorced from an understanding, partial and tentative though it may be, of the institutional qualities of legislatures and administrative agencies. In addition, we all make countless generalizations about the political process in our legal scholarship and teaching. These frequently appear to be based more on seat-of-the-pants guesswork or ideology than on careful reflection upon the understandings of our compatriots across the campus in the departments of political science, sociology, economics, and so on. The publication of *Handbook of Legislative Research* thus provides the law professor with easy access to important information.⁸

I cannot speak directly to the needs of scholars other than law professors. Many political scientists who teach courses in constitutional law, judicial behavior, and legal philosophy may well have a contemporary appreciation of legislative processes, interest group politics, and so on. I would think, however, that *Handbook of Legislative Research* would still be a convenient and handy resource for them, if they consider its contents significant for their courses. As a consumer of an undergraduate constitutional law course in the early 1970's, I remember studying game theory and a variety of other

5. See, e.g., Stewart, *The Reformation of American Administrative Law*, 88 HARV. L. REV. 1667, 1684-87, 1693-97 (1975) (concluding that reviving the nondelegation doctrine is unlikely to improve administrative law substantially).

6. See Sunstein, *Interest Groups in American Public Law*, 38 STAN. L. REV. 29 (1985).

7. See Easterbrook, *The Court and the Economic System*, 98 HARV. L. REV. 4, 14-17, 45-48 (1984).

8. The book will be valuable to law students as well. It is no secret that law students who lack a social science background sometimes find constitutional law, administrative law, legislation, and other public law courses extraordinarily frustrating. In addition, their fellow law students who have that background often have difficulty relating social science to public law theory. William N. Eskridge and I are preparing materials for a course in legislation that attack these problems. Unlike *Handbook of Legislative Studies*, our materials are introductory and suggestive rather than comprehensive. The student who wishes a richer overview of particular topics now has a ready resource in that book, which is admittedly too dense for quick consumption by novices.

topics in the context of Supreme Court decisionmaking. I do not recall spending much time on subjects I now consider more central to constitutional law: for example, the comparative competence of courts, legislatures, and agencies to handle some aspect or another of public policy; strategies of judicial intervention into politics; and devices that might promote a lawmaking partnership between courts and legislatures based on greater cooperation and less antagonism. If these topics are stressed in political science courses in the 1980's, *Handbook of Legislative Research* should enhance the effort; if they are not, the publication of this book removes any excuse based on the difficulty of ascertaining the state of the art in legislative research.

The reader of *Handbook of Legislative Research* will find one dominant message between its covers—a pessimistic view of the ability of contemporary political science to explain legislative processes and to predict legislative outcomes. Part of the problem seems to be the nonreplicative nature of the methodology of some studies. Case studies are one common method of political science inquiry, and though useful they usually do not lend themselves to the creation of meaningful theories. Overall, *Handbook of Legislative Research* reinforces the generalization that political science has created “good descriptive information about how certain legislatures work, [but only] a very limited set of theoretical propositions that can help to explain these workings.”⁹

Consider a question of major concern both to political scientists and to public law theorists: the degree to which interest groups influence political outcomes. One searches in vain in *Handbook of Legislative Research* for concrete conclusions on this subject. As two social scientists recently stated, “[T]he realm of organized interest politics is so vast—encompassing so many different kinds of organizations and so many different avenues of influence—that it is possible to locate an example to illustrate virtually any reasonable generalization one might put forward.”¹⁰ Any public law theory based in part upon even plausible empirical generalizations about interest group politics is always subject to challenge by theorists who find different empirical generalizations more plausible. Political science provides no easy way to resolve these disputes.

Competing with political science for the attention of the law professor is public choice theory. By applying the methodology and

9. Ferejohn & Fiorina, *Purposive Models of Legislative Behavior*, 65 AM. ECON. REV. 407 (1975).

10. K. SCHLOZMAN & J. TIERNEY, *ORGANIZED INTERESTS AND AMERICAN DEMOCRACY* xiii (1986).

assumptions of economics to political science—that is, by formulating abstract models for predicting legislative outcomes based on the assumption that people are egoistic, rational, utility maximizers¹¹—public choice theorists have developed sophisticated and intriguing models of the legislative process. These models readily generate predictions—of unfortunately doubtful accuracy. Indeed, the essay in *Handbook of Legislative Research* by William H. Panning presents a largely critical overview of “formal models” and suggests that, at least in their most abstract forms, they are fundamentally misguided.

I would have liked to have seen more discussion of public choice theory in *Handbook of Legislative Research*. The Panning essay is an extremely thoughtful and useful overview of formal models and their problems, but it may embody too much of the traditional political science perspective.¹² Moreover, with few exceptions, the other essays focus on traditional political science studies, with little attention paid to studies using public choice methodology.

In some instances this omission seems particularly unfortunate. For example, the essay concerning voting behavior by legislators—a subject about which public choice theory ought to have a good deal to say—notes “studies of voting behavior cast in the framework of axiomatic theory,” states that a “synthesis of the axiomatic research and the more behaviorally oriented research on legislative voting would no doubt be informative,” but nonetheless concludes that such a discussion “is beyond the scope of this article.” In context, it is unclear whether the author reached this conclusion on her own or had marching orders from the editors. In either event, I think

11. See, e.g., D. MUELLER, PUBLIC CHOICE (1979). Illustratively, consider Becker, *A Theory of Competition Among Pressure Groups for Political Influence*, 98 Q.J. ECON. 371, 371-72 (1983):

The economic approach to political behavior assumes that actual political choices are determined by the efforts of individuals and groups to further their own interests. . . .

Individuals belong to particular groups—defined by occupation, industry, income, geography, age, and other characteristics—that are assumed to use political influence to enhance the well-being of their members. Competition among these pressure groups for political influence determines the equilibrium structure of taxes, subsidies, and other political favors.

Political influence is not simply fixed by the political process, but can be expanded by expenditures of time and money on campaign contributions, political advertising, and in other ways that exert political pressure. Political equilibrium has the property that all groups maximize their incomes by spending their optimal amount on political pressure, given the productivity of their expenditures, and the behavior of other groups.

12. I cannot pretend to possess the expertise to assess this potential criticism fully. Panning's essay comports, by and large, with my own perceptions of the limited utility of abstract modeling.

Handbook on Legislative Research is made weaker by the omission. Stereotypic criticisms based on "the ad hoc empiricism and casual theorizing of conventional political science"¹³ are fostered by such refusals to broaden the inquiry.

Some social scientists are attempting to build bridges between public choice theory and traditional political science.¹⁴ Panning's essay in *Handbook of Legislative Research* praises these developments. In the main, however, the other authors of this compendium miss the opportunity to suggest methods of integrating the two disciplines in future studies of the topics of their overview essays.

Although comprehensive only within the world of political science, *Handbook of Legislative Research* is a publication of significance to the legal academic community. If consulted, it should promote a public law theory less fettered by inadequate, or stereotypic, or ideologically loaded perceptions of the political process. Of course, even it is dated upon publication. For example, it was prepared before the release, in early 1986, of the first comprehensive study of interest groups in the American political system in over twenty years.¹⁵ *Handbook of Legislative Research* in no way reduces the obligation of public law theorists to remain current about the understandings of social science, but it certainly makes that task easier.

Now, if reputable scholars would only create a *Handbook of Public Choice Theory*, to give us an overview of abstract modeling of political processes, and then publish a third volume collecting efforts to integrate the viewpoint of the first two handbooks, public law theorists would find it convenient to be trinitarian rather than unitarian in their social science samplings. Of course, even if that millenium arrives we would face a much more difficult endeavor: figuring out what to make (and what not to make) of that information.¹⁶

13. Brennan & Buchanan, *Voter Choice: Evaluating Political Alternatives*, 28 AM. BEHAV. SCI. 185, 200 (1984).

14. See, e.g., Shepsle, *Prospects for Formal Models of Legislatures*, 10 LEG. STUD. Q. 5 (1985).

15. See K. SCHLOZMAN & J. TIERNEY, *supra* note 10.

16. For a more comprehensive discussion of theories of legislative behavior and their relationship to public law, see Farber & Frickey, *The Jurisprudence of Public Choice* 65 TEX. L. REV. — (1986) (forthcoming).