

BOOK REVIEW

***POLITICS, POLICY, AND ORGANIZATIONS: FRONTIERS
IN THE STUDY OF BUREAUCRACY*, EDITED BY
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Politics, Policy, and Organizations: Frontiers in the Study of Bureaucracy. George A. Krause and Kenneth J. Meier (editors), Ann Arbor: University of Michigan Press, 2003, 352 pages.

The group on the scientific study of bureaucracy (S²OB) workshop at the fifth biennial conference of the Public Management Research Association in 1999 produced this volume. It includes a preface, an overview, original articles, a conclusion by the editors, consolidated references, contributor information, author index, and subject index. The original articles are organized into three sections: theory, methodological technology, and empirical studies. The contributors are Daniel P. Carpenter, Krause; Thomas H. Hammond; David B. Spence; John Brehm, Scott Gates, and Brad Gomez; Andrew B. Whitford; Steven J. Balla and John R. Wright; Lael R. Kaiser; Kevin Corder; Kevin B. Smith; and Michael J. Licari. The author index expresses the flavor of this volume nicely. Aside from the contributors, the most frequently cited authorities are Terry Moe, Herbert Simon, Barry Weingast, James Q. Wilson, and B. Dan Wood.

This is a valuable book for several reasons—perhaps most importantly because of the exemplars it provides showing the use and power of state-of-the-art formal models in public management. It is less valuable than it might be, however, because its contributors are American political scientists and the models tend to reflect their idiosyncratic concerns. As an academic field, public management is largely concerned with a set of doctrinal issues: What is the role of public managers? What should be the design of a programmatic organization? How should government operations be managed? What public management policies (e.g., financial management, human resource management, procurement, etc.) should be chosen?¹ For the most part, these issues are given short shrift in this volume.

Carpenter's article, "Why Do Bureaucrats Delay? Lessons from a Stochastic Optimal Stopping Model of Agency Timing, with Applications to the FDA," beautifully

illustrates both the value and the foibles of this volume. Carpenter constructs a sophisticated learning model to explain bureaucratic delay. Analysts have tended to treat this phenomenon as a queuing problem. One might call this approach the Lieutenant Kije model of bureaucratic delay, in which a number of harried officials must process an application, for a license or permit say, in a particular order, which may or may not be well understood by the participants. Under this approach, each participant processes the application on either a first-in first-out or a last-in first-out basis, and passes applications that raise unfamiliar or difficult issues to another official, perhaps a supervisor, for resolution. The consequences of such a process are a very high ratio of cycle time to processing time, high mean delays, and considerable variance. Carpenter's model produces the last two outcomes, but it presumes a far more sensible process.

Carpenter predicts time to approval of new drugs by the United States Food and Drug Administration (FDA). He treats this decision as a real option² in which the FDA knows the benefits of approval (expressed in terms of the drug's efficacy, the size of the afflicted population, and the availability of substitutes, plus the political influence of the potential users and of the drug company submitting the application), but not its costs (expressed in terms the drug's safety and, thereby, the threat error poses to FDA's reputation).

Uncertainty can be expressed as a probability distribution. Complete ignorance, for example, implies that every possible outcome is equally likely. Analysts pricing financial options usually model future asset values using normal, triangular, or binomial distributions, depending upon the fit to historical data. In this case, however, costs are truncated at zero. Consequently, Carpenter assumes that safety is a standard Brownian motion or a continuous-time Wiener process, a statistical distribution that exhibits intermittent exponential growth and decay, with an absorbing barrier at zero, which is appropriate given a concern with adverse drug reactions. Carpenter further assumes that time and repeated drug trials will produce better estimates of cost, i.e., they will reduce the FDA's uncertainty about a drug's safety, which is to say the FDA will learn. Finally, the value Carpenter assigns to the FDA's aversion to adverse drug reactions drives the model's optimal stopping point. Implicitly, Carpenter says that the FDA will (should) approve a new drug when the payoff to doing so is equal to or greater than the expected costs of approval *plus* the value to the FDA of further review.

Reading this model is worth the price of the book. It is a revelation. Moreover, contrary to Carpenter's claim (31), this is a typical problem of administrative choice. Many, if not most, important choices involve real options. Only projects that are now-or-never decisions, or projects that can be undone without cost, resolve to a simple question of (risk-adjusted) expected value. All others require a further test: Will the project have greater value in the future than it has now? The answer to this question will usually depend upon whether time will resolve some uncertainty about the benefits or costs of the project. In other words, many organizational processes can and probably should be modeled as learning problems (employ real option analysis).

On the other hand, I am not convinced that the addition of public choice variables—the political influence of potential users or drug companies or even the reputation of the FDA—contributes much of value. As a first cut, I would take the FDA's charge seriously: insuring the safety and efficacy of pharmaceuticals. Both can be measured in terms of quality-adjusted life years (QALY), a measure that combines health status and years of health.

Using QALYs to measure safety and efficacy would have the analytical advantage of applying the same metric for benefits and costs. (Presumably the latter would have to be weighted to reflect FDA's emphasis on safety.) Having done this analysis, I might then be prepared to ask the secondary question of how much political influence adds to our understanding of the timing of FDA approvals or, perhaps, what does the timing of FDA approvals tell us about political influence. These questions have some intellectual bite. Nevertheless, putting them first is a case of the tail wagging the dog.

Moreover, many of the chapters in this book are even more idiosyncratic in their perspective than Carpenter's. The theme that runs through the book is congressional control of executive branch agencies.³ This is a uniquely American obsession. Of course, the volume has other themes, including several of interest to students of public management. In their conclusion, "An Agenda for the Scientific Study of Bureaucracy," Meier and Krause suggest that we would progress faster if we focused our attention on five areas: "getting inside the black box of bureaucracy, taking theory more seriously, exploiting the variance in bureaucracies, making an explicit return to the structural aspects of bureaucracy, and adopting methods that correspond to the data generating processes of bureaucracy" (293). They further call for a "new organizations approach" to the study of administrative organizations, one that is rigorous, logical, analytical, and statistical, but applied to timeless issues of administrative organization. Sure! But, what's new? How is this agenda different from the one that academics publishing in journals such as *Administrative Science Quarterly*, *Academy of Management Review*, and *Academy of Management Journal* have been pursuing all along?

For example, "Donut Shops, Speed Traps, and Paperwork: Supervision and the Allocation of Time to Bureaucratic Tasks" by Brehm, Gates, and Gomez, and Whitford's "Adapting Agencies: Competition, Imitation, and Punishment in the Design of Bureaucratic Performance" would both look smart in *Administrative Science Quarterly*. Meier and Krause emphasize their methodological contributions. Brehm, Gates, and Gomez model the allocation of a fixed total amount of labor effort as a Dirichlet process, a conjugate form of the multinomial distribution that estimates the probability density of a particular set of discrete frequencies p_1, \dots, p_n , where the frequencies sum to 1. To say the very least, this is an intelligent, albeit risky, solution to a difficult compositional data problem. Whitford uses computer simulation to model organizations as complex adaptive systems. Computer simulation is not a new approach to organizational analysis, but arguably, we finally have the tools we need to use it well—both of these articles do so. Lacking the kinds of game-theoretic models needed to pursue fully the rational-choice agenda with

respect to organizational design (Gibbons 2003), perhaps the time has come to reconsider alternatives. Among the most attractive of these are the organizational process models elaborated by scholars in the 1960s and 1970s: Herbert Simon, John Patrick Crecine, John Padgett, and others. They deserve a second look and, for that look, computer simulation is the tool of choice (Bender 1990, 2003; Green and Thompson 2001; Jones 2002, 2003).

However, interesting as these articles are for their cutting-edge methodological tools, they are, perhaps, even more so for what they say about the design of programmatic organizations. Brehm, Gates, and Gomez show how people, organizations, and tasks interact to create and sustain the kinds of purposeful efforts and cooperative relationships needed for organizations to outperform markets (157-8). They conclude that functional organizations characterized by a high degree of task specialization should select employees with the right mix of functional preferences, and design contracts and remuneration schemes that sufficiently reward them for performing intrinsically unsatisfying tasks. In contrast, organizations designed around interdisciplinary teams must recruit employees who have heterogeneous preferences. Mutual observability and supervision are also important, but the latter should be focused on the employees who are most amenable to supervision.

Whitford's findings are directly relevant to performance measurement, benchmarking, and collaboration. He models a set of organizations in which goal setting, search and innovation, and linking performance and search are the keys to increasing performance, and then subjects them to three levers of redirection: comparison, punishment, and imitation. He concludes that competition and comparison will better the average performance of these organizations, but this achievement has two costs (182). One is short term instability (even the best performers will experience oscillations). The other is that some organizations may lag ever further behind performance leaders.

The articles in the last section of the volume rely on standard research methods and generally arrive at unexceptionable conclusions. In "Consensual Rule Making and the Time it takes to Make Rules," Balla and Wright find that both agency and rule characteristics influence the time elapsed between publication of a proposed rule and its promulgation. Deadlines, fewer rules at the last juncture of the rule-making process, and agency independence all result in swifter promulgation of final rules, but not consensual rule making.

Keiser explains "Why it Matters Whether State Bureaucrats as Opposed to Federal Bureaucrats Administer Federal Programs." The answer is that local and state officials are not exclusively concerned about compliance with national regulations, and they may also take the costs to the local unit and the benefits to its residents into account. Actually, aside from the effect of a moralistic local culture, the estimated coefficients reported in this article seem surprisingly small. It may be there just isn't very much variance that can be explained by pursuit of local interests, because, perhaps, officials in all states rationally pursue approximately the same fiscal and local benefit objectives. It is hard to tell. I found Keiser's descriptions of data used, derivation of specifications tested, and analysis performed somewhat cryptic, even opaque.

According to Korder, in “Structural Choice and Political Control of Bureaucracy: Updating Federal Credit Programs,” ceilings and funding sources affect the growth of these programs, congressional commitments are subject to revision, and the choice of policy instrument—direct loans, loan guarantees, or insurance—matters. Loan guarantees, for example, appear to be especially responsive to political control by elected officials. Moreover, Congress frequently reorganizes and updates these programs to amend eligibility, terms, and instruments and, thereby, who gets what, when and how.

In the antepenultimate article, “Administrative Structure and Social Democratic Results: The Case of Education,” Smith demonstrates that the governance structure of schools (public or private), although not the degree of discretionary authority vested in teachers, affects the objectives they pursue. Private school teachers are twice as likely to stress academic excellence over basic literacy skills as public school teachers and a whopping 22 times more likely to rank moral/religious indoctrination ahead of basic literacy. Smith concludes that a likely consequence of school privatization, especially schemes featuring portable vouchers, would be increased service differentiation, with most institutions oriented to specific market segments and segregated along cultural, ethnic, religious, or social class lines. Finally, Licari describes an interesting case of “Bureaucratic Discretion and Regulatory Success without Enforcement,” that suggests that regulators may sometimes be given more credit for programmatic compliance than they earn.

Judged by its provenance and aims, the only real weakness of this volume is one that Meier and Krauss frankly acknowledge: “Bureaucracy scholars overstudy federal regulatory agencies. . . . Federal regulatory agencies, however, have characteristics distinctly different from those of other bureaucracies. . . . This overfocus on regulation calls into question how general the findings in the literature are.” (301) They suggest two solutions: greater attention to public agencies at the state and local level, especially schools, and additional cross-sectional, cross-national research. To both suggestions, particularly the second, I can only say, amen. I would also note that, in my opinion, the articles in the empirical section of the volume tend to use too many words and not enough equations to explain/derive the models they estimate.

Finally, reading these papers, especially Carpenter’s and Krause’s,⁴ reminds me of a minor but longstanding source of irritation. I wish that the authors of articles making rigorous formal arguments would provide tables listing all the notational symbols they use in the order of their appearance and that editors would impose common notational usage and definitions throughout the volumes they edit. Now that I no longer edit this journal, I think journal editors should do likewise.

NOTES

1. Michael Barzelay (2001) formulated this statement of the public management agenda. It distinguishes public management from the academic fields of public administration, public policy, political science, and public economics and finance, which it intersects, based in part on its greater overlap with the academic fields of strategic management, business process

management, performance management and management control. Spence's analysis in this volume, "The Benefits of Agency Policy Making: Perspectives from Positive Theory," (a truly lovely piece of reasoning, by the way) could be interpreted as addressing the first question, but not answering it. Spence does not merely rediscover information asymmetry, the crux of the modern principal-agent problem, he restates Carl Friedrich's prescient version of the conundrum faced by all public servants—figuring out who their principals are and what they need or want (1968: 425). At best, however, all this does is remind us that normative/doctrinal arguments necessarily begin with theories of public value (Hood and Jackson 1991).

2. Real-options analysis is an elaboration on the valuation of financial options such as puts or calls. It can be contrasted to the standard economic approach to decisions involving future consequences, which calls for the analyst to measure a project's risk-adjusted expected net present value. However, the net present value approach implicitly disregards many options. One could first invest resources to gain a better understanding of a project's payoff or to determine its best configuration; time itself may resolve certain uncertainties. Each option has a value, therefore, which should (and can, thanks to new computer programs based on Monte Carlo analysis) be factored into determining the overall value of a project. (Monte Carlo analysis generates random values to determine the potential outcomes of alternative scenarios). A real-options analysis lets you gauge the project's value with all the options taken into account. More often than not, the result of real-options analysis is higher than that of a net present value analysis: flexibility is itself an asset.

3. To be fair, Hammond's "Veto Points, Policy Preferences, and Bureaucratic Autonomy in Democratic Systems" considers an array of legislative arrangements, including continental and Westminster-style parliaments.

4. A careful reader will have noted that I haven't mentioned Krause's "Agency Risk Propensities Involving the Demand for Bureaucratic Discretion." While formal modeling is a prerequisite to the formulation of meaningful empirical models, I don't think public management's axiomatic base is sufficiently firm or broad enough for us to draw strong conclusions from mathematical reasoning alone. To be sure, we have all done so (Choate and Thompson 1988, 1990), but the practice shouldn't be encouraged.

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