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Contemporary Empirical Political Theory

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Seven Durable Axes of Cleavage in Political Science

Bernard Grofman

Like my colleague, A Wuffle, I call myself a “reasonable choice” modeler and am a member in good standing of the California Drive-in Church of the Incurrigibly Eclectic.¹ I have relatively little interest in extended abstract discussions of meta-issues in political science—about, e.g., the nature of human nature; or whether explanations of political phenomena should focus on individual preferences, institutional constraints, political culture, or norms; or the extent to which the discipline can aspire to being scientific; or exactly what such aspirations entail. I believe that, by and large, the proof is in the pudding; if you claim to have something useful to say about some aspect of political life, then you ought to say it, and let other people decide whether or not what you say makes sense and helps them understand politics.² I have no tolerance for obscurantism, whether it be couched in words³ or in mathematical symbols.⁴

The aims of this essay are twofold: first, to identify seven axes of choice by which political scientists decide what to study and how to study it;⁵ second, to argue against the claim (Almond, 1990) that political scientists are presently sitting at “separate tables.”

My perspective is basically optimistic. I find that political scientists of all stripes usually have far more in common with one another than with just about anybody else. Though they may argue a lot with one another, by and large they are arguing about the same kinds of questions—and brandishing many of the same sacred texts. Moreover, comparing what we know today with what is discussed in, say, Charles Hyneman’s 1959 overview of American political science, I see real progress on a number of fronts.⁶ Also, I see the discipline moving away from 1960s-style “if you don’t do it my way, then what you’re doing must be either trivial or wrong” confrontations between behavioralists and antibehavioralists. Even the cleavage line between ra-

tional choice theorists and everybody else is far less rigid than it is sometimes made out to be. In my view, we are in a period of convergence of approaches rather than one of growing separation—especially between Michigan-style data analysis and rational choice modeling, but also between rational choice and political culture approaches.

APPROACHES TO RESEARCH IN POLITICAL SCIENCE

In this section I will review David Easton's famous distinction between behavioral and postbehavioral approaches to political science, argue that this distinction is more misleading than helpful, and then introduce a typology of my own that reflects a set of differences that crosscuts Easton's dichotomy.

What Is Postbehavioralism?

In his presidential address on the theme of a postbehavioral political science, Easton identified seven basic characteristics of the scientific approach (Easton, 1969).⁷ He then suggested that most if not all of the basic elements of this approach were being rejected by a substantial set of political scientists, whom he labeled "postbehavioralists." I find Easton's list of basic characteristics to be an apt and fair summary of the methodology ascribed to the natural sciences, and I did not doubt in 1969, nor do I doubt now, that a substantial number of political scientists find these prescriptions too narrow for what they want to do. Where I part company with Easton is a) in my dislike of labeling those who reject a substantial number of features of scientific method as "*postbehavioralists*", and b) in rejecting Easton's lumping (in this volume) of rational choice theorists into the postbehavioral camp.

The "post" label, as in "postmodern" or "postbehavioral," has one direct meaning: those who are "post" come later in chronological order. However, labeling something as "post" often clearly implies that "later is better." For example, those who identify with postmodernism can claim the cachet of being "with it," attuned to the most up-to-date intellectual currents, not fuddy-duddies still using antiquated models and methods.⁸ Thus, Easton's use of the label "postbehavioralist" for views he appears to favor⁹ can be seen as a rhetorical ploy to give those views greater legitimacy,¹⁰ especially as many of the antibehavioral beliefs that Easton ascribes to the postbehavioralists can best be thought of as *pr*scientific.¹¹

But my objection to the postbehavioral label is not merely based on its apparent use as a rhetorical ploy. My chief complaint is that the various groups of folks whom Professor Easton puts into the postbehavioral box

are an ill-matched lot who generally find each other's work irrelevant, incomprehensible, and/or distasteful, assuming they bother to read it at all. Using a common label for this range of groups is obfuscatory of useful classification. The term "postbehavioralists" includes scholars with views akin to the those of the romantics, who rebelled against both the precision of science and its disregard of human values, as well as postmodernist obscurantists and exponents of formal modeling (including me), who believe game theory is becoming to political science what calculus is to physics. There is a real problem here of lumping apples, oranges, and cucumbers into the same category.

In particular, if we treat rational choice modeling as a species of postbehavioralism (as Easton does in his chapter in this volume), then it is hard to know what, other than chronology, provides the defining characteristics of postbehavioralist thought; it is ludicrous to regard rational choice theorists as rejecting the seven basic tenets of scientific method identified in Easton (1969). A political scientist trained at Cal Tech or Rochester in the mysteries of positive political theory has far more in common with that apotheosis of behavioralism, the Michigan Ph.D. trained in the intricacies of survey research, than either has in common with the various deconstructionists, postmodern feminists, neo-Marxist state theorists, policy analysts, and even Straussians who, according to Easton, all share with rational choice modelers postbehavioral concerns.

Because even the author of the term "postbehavioralism" does not use it consistently, switching between chronological and content-oriented definitions, I will not use it in the remainder of this essay. Similarly, I will eschew use of the term "behavioralism" in the way that Easton defines it, because I do not find it the best term for distinguishing what many Michigan-trained scholars do from what many Rochester-trained scholars do; both sets of scholars make use of the scientific method, and both are concerned with behavior such as voting choices.¹² Instead, I will distinguish among political scientists in terms of seven (mostly) orthogonal axes:

1. normative versus empirical
2. description versus explanation
3. induction versus deduction
4. scope versus certainty
5. exegesis versus exploration
6. governmental orientation versus policy orientation
7. understanding versus change

1. **NORMATIVE VERSUS EMPIRICAL.** Here the distinction is simply between those who see political science as a branch of moral philosophy and those

who see it as a search for empirical understanding/explanation. The former often see "is" and "ought" as inextricably intertwined; the latter almost invariably seek to disentangle them.

2. DESCRIPTION VERSUS EXPLANATION This distinction, which applies to empirically oriented political scientists, is between those who wish to immerse themselves in insider's knowledge of a delimited domain (e.g., a particular country or region) and believe that little or nothing that is useful can be said about politics without such deep knowledge, and those whose first reaction is to look for comparative analysis to shed explanatory light on particular cases, with those cases often simplified in terms of "stylized facts." I believe there is merit to both sides of this argument, which is why I regard it as mostly a silly one between two straw men.¹³ My own students are encouraged to do comparative analysis but also to know a great deal about at least one case so as to have a "reality check," not just on theoretical propositions that may seek to encompass that case but also on the ways in which variables that purportedly apply to that case get (mis)coded.¹⁴

3. INDUCTION VERSUS DEDUCTION. The distinction here is between those whose first recourse to any question posed is to go about amassing data and those whose first recourse is to think the question through from first principles. Of course, these are ideal types.¹⁵ It is unlikely that any contemporary social scientists would argue in favor of a naive form of hyperempiricism. "Just the facts, ma'am," is an injunction that makes sense only if we have an a priori notion of which of the infinitude of facts are, in a given context, relevant ones. Nonetheless, data-oriented political scientists often act as if almost any sort of regression represents progress, as long as it has lots of variables in it and a high enough R^2 . On the other hand, political scientists with a positive theory orientation often emulate economic theorists by seeking to model a phenomenon in terms of deductions from some limited set of postulates.

However, deductive reasoning that lacks connection to real-world phenomena is a branch of mathematics, not of the social sciences. The work of some positive theorists supports the old saw that "it's not what we don't know that hurts us, it's the things we think we know that really aren't so."¹⁶ Sometimes, math modeling is like an absolutely stupendous basketball slam-dunk—except that somebody forgot the ball.

An exclusive focus on purely inductive or purely deductive approaches will ultimately prove counterproductive. But even if we get a reasonable balance between theory and data, there are other issues of division.

4. SCOPE VERSUS CERTAINTY. The distinction here is between those who search for certain answers to relatively small and manageable questions and

those who are willing to settle for not-so-certain answers as long as the questions are big ones. Some scholars relish being able to say with confidence that their answer is the right one, even if the question they are asking is not particularly momentous. Others are delighted with any kind of real insight, no matter how imperfect or incomplete, into "bigger" questions. These are matters of temperament and provide one of the permanent divisions among political scientists.

5. EXEGESIS VERSUS EXPLORATION. Even among those who are primarily empirical in orientation, there is an important distinction between those who think the important answers/insights have already been written down by the great minds of the past and those who believe the process of knowledge gathering is ongoing and cumulative. If political science is a conversation about great ideas, the basic elements of which were laid down anywhere from 200 to 2,000-plus years ago, then most of what modern political scientists do is a waste of time. One would be better off reading Plato, Aristotle, Machiavelli, or Hobbes than studying the budgetary reconciliation process in Congress or the effects of proportional representation on the city council of Ashtabula, Ohio, or reading the works of those who do. How much Aristotle versus how much Ashtabula is an ongoing basis of cleavage within the profession.¹⁷

6. GOVERNMENTAL ORIENTATION VERSUS POLICY ORIENTATION. The distinction here is between those who think of political science as simply the study of government and those who think it ought to be the study of policies and power, especially of "who gets what, when and how," regardless of the domain in which value allocations take place. If we exclude international relations, issues that dominate newspaper front pages—e.g., health care, immigration, crime, unemployment—are conspicuously absent from the *American Political Science Review* or are discussed only in the context of public opinion research. Also, articles in the *APSR* and similar journals are generally "bloodless"—i.e., they do not convey a sense that politics is an activity engaged in by real people and having real consequences. Often the focus is on puzzles—"Why do incumbents get reelected so often?" "Why does the in-party lose seats in midterm elections?" "Why is divided party control of state governments so common?"—or on measurement questions—"Is the increased issue orientation found in voters in recent presidential elections real or is it an artifact of questionnaire wording?"—or on purely abstract modeling.

There are at least three reasons why the subjects discussed in the leading general-purpose political science journals seem largely divorced from the issues that make up such a large part of ordinary citizens' understanding of what politics is all about.

First, the articles in the discipline's most prestigious general-purpose journals are primarily about government and its institutions: e.g., elections, congressional committees, Supreme Court voting patterns. Because of this focus on government and its institutions per se, we find very few articles that *directly* deal with "who gets what, when, and how." Many political scientists have tacitly accepted a division of labor with other social science disciplines, leaving to sociologists issues of race, class, and social inequality and to economists macroeconomic issues and broad questions of public policy but retaining primacy when it comes to Congress and the presidency. Such a division of labor is reflected in the courses taught by political scientists about American politics—courses that focus on institutions and public opinion.

Second, political scientists for reasons of pride wish to distinguish themselves from mere journalists. We do this in part by distancing ourselves from the day-to-day issues of political debate that are the stuff of journalistic commentary in order to take more "scholarly perspectives."

Third, with the exception of a few distinguished political scientists (e.g., Aaron Wildavsky), who have prided themselves on their ability to "speak truth to power," students of policy have generally not enjoyed great prestige. Moreover, to the extent that Bill Clinton's advisers were right that "It's the economy, stupid," political scientists are superfluous; they have very limited training or tools with which to contest claims by economists and so must leave such matters in the hands of others lest they be revealed as the ignoramuses they are.¹⁸ Also, with a few notable exceptions, such as faculty at the Kennedy School (who view Washington as a suburb of Cambridge), U.S. political scientists in academic posts do not usually hold government posts (as their counterparts in other countries often do), and those who hold such positions are not thereby held in greater prestige.¹⁹

The extent to which political science should encompass topics such as power relations inside the family or allocative policies of all types regardless of locus remains one of the dividing cleavages of the discipline. I find it unfortunate that political science has been so preoccupied with government that it has lost sight of politics as the study of power or of authority relations in general (cf. Eckstein, 1969).²⁰

7. UNDERSTANDING VERSUS CHANGE. The distinction here is between those who wish to understand the world and those who wish to change it. Some social scientists may pick topics for research in the hope that their work may, in some small fashion, change the world; others simply pick puzzles they find interesting or work on whatever topic seems convenient because of, say, data accessibility. Some academics pursue a topic to advance a particular political agenda; others have declared various areas of research off-limits on the grounds that the results might tend to reinforce racism, or

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sexism, or whatever. Few political scientists see themselves as having to choose between espousing truth, on the one hand, or social justice, on the other; yet if certain truths are unpleasant, politically destabilizing, or potentially harmful to the short-term interests of the weak and disenfranchised, that conflict can arise.²¹ Also, to the extent that certain topics are avoided because results might be controversial (and controversy may limit a researcher's ability to get future funding), then the insights offered by the discipline as a whole can be skewed. In any case, differences in personal preference on the understanding-versus-change dimension provide another one of the long-standing cleavages in political science—as in other social science disciplines.

TOWARD CONVERGENCE

Political science has been characterized as being in crisis for want of a unifying paradigm, having moved from raw empiricism to legalism to behavioralism to splintered postbehavioralism. We are presently seeing a strong reaction in political science to the hegemonic claims of rational choice modeling, on the one hand,²² and to the older, survey-data focused approach of the Michigan school, on the other. Rational choice theory is commonly critiqued as being a) wrongheaded in its insistence on positivist canons of scientific inquiry, b) highly limited in its ability to account successfully for key features of political life, such as collective action or non-self-interested behavior, c) foolish in making the elegance of its mathematical modeling an end unto itself, and d) politically incorrect because of an allegedly inseparable association with right-wing views.²³ Michigan-style behavioralism comes under a different but equally strong attack: it is accused of being little more than a reporting of survey data that is about as enlightening as a reading of tea leaves in terms of its ability to account for patterns of change in public opinion or for phenomena such as, say, the rise of Ross Perot.²⁴

I do not believe in a lost Eden, a time before the tower of Babel, when all political scientists spoke a common tongue and prayed to the same god. When I look at the body of research that appears in mainstream journals, political science to me appears at least as unified today as at any point in its past (i.e., not very); and the real divisions that are present are the same ones that have characterized the discipline for at least the last seventy years—namely, those along the seven axes discussed in the previous section.

In Search of a New Metaphor for the State of the Discipline

As an alternative to Gabriel Almond's powerful metaphor of political scientists sitting at "separate tables," I suggest the "Chinese dim sum brunch."

In a dim sum restaurant many trays with different items go by, and you take what you like (by pointing at the dish). Customarily, if you come alone (or with a small group), you are seated at a large table with a bunch of other people. Interaction is possible but not compulsory. And although everyone might have a unique combination of dishes, there is almost always some overlap between any two diners' meals, because there are only so many dishes on the menu and because almost all diners consume multiple items. Moreover, diners can see what other people are eating, ask them about it, and then either sample it for themselves or refrain in disgust—duck feet, for example, are an acquired taste.

I am not arguing that the differences among political scientists about methodological and epistemological predilections are trivial. However, I believe the "separate tables" metaphor overstates the case by implying that two political scientists of ostensibly different orientations have less in common with one another than they do with scholars outside of political science who share their epistemological perspective—recall the jape that "two parliamentarians, one a socialist, one not, have more in common with one another than two socialists, one a parliamentarian and the other not." All political scientists, regardless of persuasion, share a great deal—in terms of early socialization and the received litany of "names to conjure with"—that distinguishes them from other social scientists. This fact becomes apparent when one engages in conversations across disciplinary lines. In fact, I would argue that virtually any two political scientists of even a vaguely realist persuasion can rather quickly find common interests,²⁵ though the commonalities need not be the same between different pairs.

I want to say more than that things merely aren't quite as bad as recently painted. I want to argue that there are a number of signs of a "coming together" of rival approaches. One of these signs is the increasing number of technically well-trained younger scholars who are more interested in empirically testable formal models than in either purely inductive or purely deductive approaches; another is the growth of "soft" rational choice and pervasive use of metaphors derived from game theory.

THE GROWTH OF SOFT RATIONAL CHOICE. Game theory is one of the most powerful tools available to social scientists; I believe it will become for political science and economics what calculus is to physics.²⁶ Game theory models are becoming common in many different subfields of the political science discipline; even more important, in my view, game theory metaphors (e.g., zero-sum game, dilemma of the commons, security dilemma) are becoming ubiquitous, as are ideas borrowed from economics, such as increasing returns to scale and transaction costs.

Unlike some hard-core rational choice modelers, who appear to believe that if it isn't a theorem it cannot be a valuable contribution to human

knowledge, I welcome so-called "soft" rational choice approaches.²⁷ I welcome, too, work that seeks to bridge the gap between rational choice and other approaches—e.g., the work by Robert Putnam and others that builds on James Coleman's notion of "social capital" (Coleman, 1990) or on Jon Elster's work on social norms (Elster, 1989). This work blurs the line between cultural and rational choice approaches in ways that I, as an incorrigible eclectic, heartily welcome.²⁸

COMBINING MICHIGAN-STYLE AND ROCHESTER-STYLE APPROACHES. The political science departments at the University of Michigan and Rochester University have received the greatest recognition a discipline can award, that of becoming synonymous with a particular way to do research, of having provided a paradigm that has affected an entire discipline,²⁹ for which the Chicago School of Sociology was famous in the 1920s and 1930s and the Chicago School of Economics is famous today.

The principal fault laid at the door of Michigan-trained Ph.D.s is that they're great with data and don't have much in the way of theory; the principal fault laid at the door of Rochester-trained Ph.D.s is that they're great with theory and don't do much (or do nothing at all) with data. But who says you can't have it all? A set of younger positive theorists—some trained at Rochester (Keith Krehbiel), some at Cal Tech (Matthew McCubbins, Arthur Lupia), some at Washington University (George Tsebelis), some from elsewhere (Thomas Hammond, U.C. Berkeley)—and a number of younger scholars whose principal training is in data analysis—from Michigan (e.g., Dave King and Elizabeth Gerber) as well as from a host of other schools (e.g. Gary King, a University of Wisconsin Ph.D.)—are doing a superb job of integrating sophisticated modeling with empirical research.³⁰ Evidence of this new rapprochement between empirical and modeling orientations³¹ is also found in the special edition of *Public Choice* celebrating the twenty-fifth anniversary of the Public Choice Society, wherein empirically testable theory is hailed by James Enelow and Rebecca Morton (1993) as the wave of the future³² for Public Choice.³³

If ever there was a time for optimism about the revitalization of political science called for in Easton's 1953 classic *The Political System*, that time, I believe, is now.

NOTES

1. See Wuffle (1992).
2. If this be pragmatism, then make the most of it (cf. the last several pages of Wendt and Shapiro, this volume).
3. For example, I have no doubt that deconstructionists have a lot to say, but by and large it's only about what other deconstructionists have said—a circularity

that is not helpful unless your principal interest is in understanding the behavior of university professors or unless you think that pompous gibberish translated from French or German is inherently more enlightening than pompous gibberish available only in English. (My colleague, A Wuffle, has whimsically characterized deconstructionism as the belief that "what I think is more important than what you said or did.")

4. I am not saying that the use of mathematical symbolism per se is obscurantist; rather, for work that appears in journals intended for a general political science audience, what I find reprehensible is the failure to provide nontechnical readers the courtesy of an English-language translation of the key results and at least some clue as to why any political scientist might find them of interest.

5. I am not arguing that any of us has one and only one position on each of these axes. We each have tendencies, reflected to a greater or lesser degree in particular pieces of research, but some of us are more eclectic than others (i.e., have higher variance).

6. Cf. Laponce (this volume).

7. The same list is found in Easton's chapter in this volume.

8. Cf. "Progress is our most important product."

9. Exactly to what extent Easton ca. 1969 subscribed to the views he labels "postbehavioralist" remains a matter of some dispute. Heinz Eulau puts forward the view that Easton (1969) renounced the views he espoused in Easton (1953)—a charge that Easton (this volume) denies, asserting that in Easton (1969) he was more describing than endorsing. Having read both early Easton (pre-1969) and later Easton (post-1969) I can only say that I had no trouble figuring out where the "old Easton" stood—he was a "young Turk" who sounded the charge against a "great books" notion of the discipline and urged political scientists to do empirically and theoretically grounded research—whereas I find it much harder to figure out the "new Easton."

10. Recall that the Bolsheviks took for themselves a label connoting that they, not the Mensheviks, were the majority; Nixon claimed for his supporters the title of "silent majority"; Madison and other supporters succeeded in attaching the popular term "Federalist" to the constitution of 1787, forcing its opponents to become anti-Federalists, despite the fact that their confederal approach had at least as good a claim to the label "federalist" as did advocates of what William Riker has called "centralized federalism."

11. Much nonsense has been written by social scientists about how the scientific method ain't what it used to be—e.g., claims that positivist notions of science have been shown to be falsified by the work of Thomas Kuhn on paradigm shifts or by other recent work in the philosophy of science that demonstrates, i.a., that the link between scientific findings and empirical reality is problematic in that observations are inherently theory-laden. Space does not permit a discussion of philosophy of science issues here. Suffice it to say that I believe that political scientists should not allow such misleading claims to get in the way of their aspiring to do political science à la the seven tenets identified in Easton 1969. Of course, I also recognize that the seven tenets are an idealized portrait of science, because the day-to-day research of scientists does not look like the philosophers' vision of same.

12. However, I also argue below that we are moving toward convergences in political science research methods in which Michigan versus Rochester distinctions no longer have clear meaning (see below).

13. In ongoing work with Arend Lijphart, including a joint course on "The United States in Comparative Perspective," he and I are developing the argument that we can often best understand the peculiarities of particular countries—e.g., so-called "U.S. exceptionalism" (with respect to, say, low levels of voter turnout, or low levels of descriptive representation for women in Congress and state legislatures, or low levels of unionization)—by seeking to explain outcomes in that country in terms of variables drawn from crossnational analyses.

14. My colleague Rein Taagepera, a specialist on the Baltic states, tells a revealing story (personal communication, March 19, 1996) about some crossnational data analysis he did on the consequences of electoral laws. The Estonia data from pre-World War II were an outlier from his theoretical predictions, and he couldn't understand why. He just wasn't satisfied. Eventually he managed to track down an alternative source that reported raw data, and he recalculated the values of interest—to discover that his previous source (and several other sources that had taken that source to be definitive) were simply wrong, and wrong in a big way. Knowing Estonia, he knew that something just didn't seem right.

15. Nonetheless, the reader can undoubtedly fill in examples of scholars whose work falls far more clearly on one end of this continuum than the other, and similarly for the other axes of cleavage discussed here.

16. A particularly humorous example of this comes from a recent article in the *American Political Science Review* by two top-notch positive theorists, David Austen-Smith and Jeffrey Banks. They prove an elegant mathematical result to the effect that, when voters are free to be strategic in their voting choices, three-party competition under proportional representation leads to an equilibrium in which the party with the highest seat share and the party with the lowest vote share will form the government. They then contrast this result with that under two-party winner-take-all elections where it is taken to be true that "the party with the most votes adopt the median voter's position, and this is surely the final policy outcome." Based on their modeling they conclude that "the popular conception that, in contrast with simple plurality schemes, proportional representation leads to legislatures—and hence to final policy outcomes—that reflect the variety of interests in the electorate seems mistaken" (Austen-Smith and Banks, 1988: 417).

Unfortunately, Austen-Smith and Banks, like some of the economists they emulate, don't appear to understand that the truth of an empirical proposition does not necessarily follow from a mathematical model from which it can be logically deduced, even if that model is an incredibly elegant one.

When we look at real politics: 1) there is no evidence that their model of three-party competition under PR is an accurate characterization of coalitional choices in three-party systems; and 2) there is a great deal of evidence that, in two-party plurality elections, party policy positions do not converge (Grofman, 1993a).

Of course, the work of Austen-Smith and Banks is only preliminary, as they are the first to acknowledge, and, because the assumptions of their model are so clearly

spelled out, it is easier for us to figure out how we can improve on **their** model to develop one whose implications are better in line with what we observe. Still, it is bothersome to see an article that purports to tell us something **about** politics that is so utterly unconnected to empirical evidence. (In fairness, I've written plenty of such articles myself. I just try not to make that the only kind of article I write.)

17. Although I was an undergraduate at the University of Chicago well after the heyday of Robert Maynard Hutchins and the Great Books program, key elements of that program remained alive when I was there, albeit in watered-down form. My knowledge of the classics of political theory comes more from **my** undergraduate than from my graduate education. When I was informed by Joseph Cropsey at the beginning of his graduate course on Aristotle's *Politics* that Aristotle had asserted that the soul was divided into three parts (named, as I recall, **something** like the appetitive, the vegetative, and the dormative), I decided that I was not a political theorist, at least in the sense that Cropsey would have in **mind**, and dropped the course.

18. Of course, a few political scientists are also well-trained in **economics**, but they tend to respond to policy questions in the same fashion as an **economist** without bringing "something special" to the analysis as a result of their background in political science.

19. See discussion of politics as a "policy science" in Lowi and Harpham (this volume).

20. In this context, an important contribution of feminist writers has been to remind us that "the personal is the political" and that politics is not merely what happens when Congress makes a law or the Supreme Court decides a **case**. For example, feminist scholars have called attention to the real "gender gap"—i.e., gender-rooted differences in power.

21. On a personal note, as someone who has frequently served as an **expert** witness on behalf of racial minorities in voting rights lawsuits, my own primary commitment to truth rather than to social justice has gotten me into **trouble** with one or two of the attorneys for whom I have worked. They would have **preferred** that I downplay (if not suppress entirely) results that were uncomfortable for their side. Luckily, almost all the civil rights attorneys with whom I have worked are well aware of the pragmatic point that the credibility of an expert witness, **once** damaged, is hard to repair.

22. The greatest tribute to the success of rational choice modeling in **political** science is the number of people who now feel compelled to attack it (see Grofman, 1993b, 1996 forthcoming).

23. I am somewhat sympathetic to the second and third critiques, but I believe critiques one and four to be almost totally misguided (see Grofman, 1993b, 1996 forthcoming).

24. Moreover, it is sometimes asserted that neither rational choice models nor survey research have much to contribute to our understanding of the **bloody** events in Bosnia or Rwanda. This charge, too, I regard as quite misguided, but **space** does not permit an elaboration of my views here (on the application of rational choice modeling to the disintegration of Yugoslavia see, e.g., Posen, 1993).

25. Here by "realist" persuasion I mean what Wendt and Shapiro (this volume) mean by that term.

26. Of course, just as most applications of calculus cannot be viewed as contributions to physics, so not every application of game theory is a contribution to political science.

27. Harry Eckstein (personal communication, 1995) has made the intriguing point that the labels for "hard" rational choice and "soft" rational choice have been foolishly inverted. Only work that offers empirically testable (and falsifiable) hypotheses deserves the name of "hard science," and soft rational choice has at least as much of that as the largely theorematic contributions of the "hard-core" modelers.

28. As I have argued elsewhere (Grofman, 1993b, 1996 forthcoming), rational choice models need not posit that actors are driven solely by narrow egoistic perspectives.

29. Some would say "infected."

30. The scholars I mention are almost entirely from American politics simply because this is the disciplinary subfield I know best. Similar rapprochements of formal modeling and data analysis are taking place in subfields such as international relations and comparative politics. Names of scholars from these subfields are omitted simply due to my ignorance of their work, not from a belief that they don't exist or that what they do isn't first-rate.

31. In fairness, political scientists able to combine sophisticated modeling with sophisticated data analysis have been around for a while, as witness the work of relatively senior folk such as Kenneth Shepsle (committee assignments, legislative procedures, cabinet portfolios), Norman Schofield (cabinet coalitions), and John Ferejohn (budgetary rules, pork-barrel politics), to name but three. What is new is how many younger political scientists now possess an impressive combination of technical skills and substantive concerns.

32. Enelow and Morton (1993) also cite the works of several of the younger scholars I named above as especially promising in this regard.

33. Indeed, as evidenced by the 200-plus members of the Society for Experimental Economics, even some economists are recognizing the need to demonstrate, rather than merely assert, that various economic "truths" are self-evident.

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