Introduction: The Joy of Puzzle Solving

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Political science, especially the field of comparative politics, has been torn between advocates of thick description and detailed knowledge of historical, cultural and social context and those who argue the need for simplifying assumptions and formal modeling to make theoretical sense of a complex world. Personally, since I am, like my colleague, A Wuffle, a member of the California (drive-in) Church of the Incorrigibly Eclectic, I see no need to take an either-or stand in this debate.¹ I think both sides are right; or, alternatively, both sides are wrong -- i.e., the proof is in the pudding and not in abstract debate about methodology or epistemology. A key task for empirically-oriented social scientists is to find interesting features of the world and try to tell us something insightful that will help explain them/understand them better. If -- whatever your methodology -- you can do that, more power to you. If you can’t, find another line of work.² Abstract debate about the epistemological and ontological merits or demerits of particular styles/types of explanation are, in my view, not especially enlightening.

A puzzle-solving approach can be a bridge between alternative methodological perspectives. A focus on solving concrete puzzles derived from empirical observation can force those whose methodological leanings are in the modeling direction to be attentive to real world phenomena at the same time as it can force more ideographically oriented political scientists to look beyond the facts to theoretically-grounded explanations of those facts. A puzzle solving approach forces arguments about the explanatory power of certain (types of) theory to move away from debates about abstract concepts to real debates about real issues, in particular, the power of competing explanations to explain real-world puzzles.

This book offers five important puzzles -- puzzles in comparative politics³ -- and looks at the solutions that certain political scientists have proposed for them.

In three of the chapters (Chapter 1-3) an author looks at the behavior of an individual or an organization such as trade union or political party that, at first blush, just doesn’t seem to make sense: “Why would a political leader gamble on a vote of confidence that he didn’t need to call and the failure of which would seriously harm his party’s future prospects?” (the Kaare Strom chapter); “Why would a trade union conduct a strike that it knows it can’t win?” (the Miriam Golden chapter). “Why didn’t the Japanese Socialist party modify its platform to attract more voters so as to give it a chance of holding power in Japan?” (the Kohno chapter). In these chapters the authors show that behavior that appears irrational is not really so once we understand the full context in which the behavior is embedded.

The fourth chapter, by Richard Anderson, takes its puzzles from a phenomenon that appears counterintuitive -- indeed, virtually inexplicable. “How could a major empire
(the Soviet empire) have dissolved so quickly? Here the explanation involves an interesting new theory: the power of “decisive inaction.”

The last chapter (that by Tsebelis and Stephen) elaborates a formal model of equilibrium behavior in the social welfare system to consider the empirical puzzle, “Why does increasing the unemployment benefits often appear not to significantly increase the attractiveness of the unemployment option?”

Outline of the Book

In chapter one, Golden explains differences in tactics between Italian and British automobile plant trade unions with similar histories of militancy facing similar threat of major layoffs (albeit layoffs cushioned by the promise of severance pay). Organized labor in Great Britain acceded to 30% layoffs at British Leyland without a strike, while Italian unions at Fiat engaged in a more than month-long strike which they lost, after which labor employment fell by a third over the next three years. Rejecting the five most often proposed explanations for the differences in outcomes in the two cases (e.g., the supposed greater influence of Italian shop stewards, or differences in political culture), Golden offers an institutional explanation focusing on differences in seniority rules. She argues that “militant resistance to job loss will . . . occur in situations without effective seniority systems.”

In chapter two, Strom’s explanation of the seemingly irrational behavior by Norwegian party leaders shows how a too simplistic notion of what constitutes rational behavior can go wrong, namely treating parties as unitary actors when they are not. He explains why a non-socialist majority of four parties in Norway, each explicitly committed to dislodging a socialist government, twice failed to agree on a simple vote to that end. He shows that the interests of party leaders (and thus what is rational for them to do given his own career incentives) need not be identical to those of the party they represent. He also shows how sequential bargaining situations can give rise to errors in expectations that lead to errors in judgment.

In chapter three, Masaru Kohno looks at why the Japanese Socialist Party (JSP) clung to its original leftist politics so long, despite the fact that the platforms they advocated made it impossible for them to achieve majority party status in Japan. After considering previous explanations for this self-defeating behavior, including the central role of labor in the JSP’s campaign organization and the strength of key ideological concerns that apparently dominated electoral motives, Kohno suggests an alternative explanation. He links JSP policy choices to certain features of the single nontransferable vote system used for the lower chamber in the Japanese parliament from 1997 to 1994 that helped structure the nature of electoral incentives/constraints that affected party competition in Japan.

In chapter four, Richard Anderson tackles the question of the fate of the Soviet Empire. How could sovietologists have so baldly missed the signs of its imminent dissolution? Was that collapse not really inevitable? How could the former Soviet Union have dissolved so quickly? Anderson offers a theory that ties the timing of the empire’s fall to certain critical choices made by Premier Gorbachev, choices that Anderson characterizes as “decisive inactions.” For Anderson, it was what Gorbachev did not do in response to certain crises (things that everyone in the Soviet Union expected him to do because they were the kinds of responses made by his predecessors in similar circumstances) that made the difference.
In chapter five, George Tsebelis and Roland Stephen makes use of the tools of game theory to study the changes in behavior that occur when one parameter of the social welfare system, the levels of benefits to those out of work, is changed. *Ceteris paribus*, it would seem that increasing unemployment benefits should increase the attractiveness of the unemployment option. Yet they show that increased benefits seem to have only a limited effect on unemployment rates. Why? Tsebelis and Stephen’s answer rests on an analysis of equilibrium behavior; they show that increasing the penalties will affect the behavior of those who monitor eligibility requirements and not just affect the calculations of potential beneficiaries. These changes in law enforcement activities may in part offset the deterrent effect of increased benefits.

Discussion

We may classify the chapters in this volume in terms of how general is the phenomenon that is being explained: ranging from attempts to explain either the behavior or the consequences of the behavior of particular actors in a given country (party leaders in the case of Strom, Anderson, and Kohno) or types of actors (trade unions in the case of Golden); to explanations of behavior cast in very abstract and general terms (e.g., Tsebelis and Stephen’s model of a welfare system and the equilibrium response of its components to changes in particular parameters such as benefit level).

Another useful way to characterize the chapters in this volume is terms of categories derived from the literature on the art of the mystery. Mysteries can be seen as generally falling into three categories: “whodunits,” “howdunits,” and “whydunits.” In a whodunit the central issue is simply pinning down who committed the crime; whodunits are what most of us think of when we think about mystery stories. In contrast, in howdunits and whydunits we often know the identity of the perpetrator from the start. In the former, the emphasis is on exactly how the deed was pulled off. In the latter, we are interested in probing the psychology of the criminal and the motivation and background of the crime. We may think of the chapters by Golden, Kohno and Strom as in large part whydunits, seeking to explaining the otherwise mysterious (because apparently irrational) motivations of particular actors; while the other two chapters can be seen as combining the concerns of the traditional whodunit (perhaps, for the Anderson chapter, better labeled as a “whatdunit”, i.e., identifying a mechanism (the villain) that done the deed), with the howdunit’s concern for the exact nature of that explanatory mechanism.

Since all of the work in this volume can be thought of as falling loosely within the rational choice tradition, still another way to think about the chapters in this volume is terms of a continuum ranging from what is often called “soft” rational choice (i.e., verbal analyses that draw on the idea that people do things for reasons and, if we can understand their reasons, we can help explain their behavior), to the intermediate category of work that looks at game-theoretic equilibria in particular concrete situations, to the extreme of “hard” rational choice, i.e., purely mathematical results divorced from any immediate empirical context. Because this is about the solving of empirical puzzles, none of the papers in this volume fall on the extreme “hard” end of this spectrum. The Golden, Anderson and Kohno chapters clearly fall into the category of “soft” rational choice, and the Strom chapter and the Tsebelis and Stephen chapter are in the intermediate category in that they do offer explicitly game-theoretic models and look for equilibria.
However, as my late colleague, Harry Eckstein (personal communication, 1992) noted, it seems perverse to label as “soft” work which seeks to empirically test models, while reserving the label “hard” for theorem-proving papers whose mathematical results may, in fact, have no empirically testable implications. Even though mathematical and statistical techniques can be invaluable to its practitioners, social science is not a branch of mathematics (but, of course, neither is physics or chemistry). Thus insofar as “hard science” is thought to be better than soft science (and hard-headedness preferred, of course to gullibility), unlike the case of Hamilton and Madison successfully stealing the name, “federalists,” that more properly belonged to their “anti-federalist” (sic!) opponents, I regard the theft of the term “hard” by theorem-provers to be unfortunate. By reserving the term “hard” for purely mathematical work that may have little or no direct empirical application, it denigrates the contributions of those who are trying to make sense of the world.

The original title of the 1992 conference that inspired this volume was “Rational Choice Approaches in Comparative Politics.” Since that conference, much important work has been done applying rational choice ideas in comparative politics (see e.g., Bates et. al., 1998a), but the debate about the merits/demerits of rational choice theory in political science has also heated up considerably. There are at least five edited books specifically about the value of rational choice approaches, and several journals (including Critical Review, Rationality and Society and the Journal of Theoretical Politics have had mini-symposia on this topic), and, of course, Green and Shapiro (1994) have written a book-length diatribe against Downsian and other Public Choice models. While the original draft of this volume had some chapters on the nature of rational choice theory, these chapters have been dropped from this volume in light of how much has already been written in, what seems to me, too often a sterile and boring exercise in abstract argumentation. Not only do many critiques of rational choice theory involve attacks on straw-men, or criticisms of empirical work based on standards of evidence and explanation that are set impossibly high and don’t bear any resemblance to how social scientists (rational choice and otherwise) actually conduct research; but, most importantly, debate has had, as far as I can tell, zero effect on the kind of research that people actually do. Thus, the aim of the present volume has been to focus on puzzle solving, not on rational choice, per se.

Moreover, classifying the papers that appear in this volume into “softer” or “harder” forms of rational choice modeling can be quite misleading since, in each chapter, it is the empirical puzzle that drives the enterprise, not the methodology, per se, and the chapters illustrate that country-specific knowledge and analytic tools are, as Barry Weingast (1996) nicely put it, “complementary rather than competing” sources of understanding. Golden, for example, makes use of a model that emphasizes institution-driven incentive structures, and is able to integrate the perspectives derived from that modeling with her knowledge of British and Italian trade union history. Kohno’s chapter, too, seeks to integrate the insights of formal theory with insights derived from detailed institutional analysis of a traditional sort in reexamining the motivations underlying the policy choices made by the Japanese Socialist Party.

To come full circle, my own preferred way to think about all of the chapters in this volume is in terms of their similarities. They all pose important puzzles and seek to answer them. The five chapters deal with voting behavior, interest groups, regime
change, government formation and public policy. Our chapters span northern and southern Europe and the former Soviet Union. How successful a job each of the chapters in this volume does in addressing the puzzle it purports to explain readers must, of course, judge for themselves. But regardless of the merits or demerits of particular chapters in this volume (or in the planned companion volume that discusses six puzzles about U.S. congressional elections), I hope to persuade the reader that it is useful to think about social science as, in good part, a puzzle solving activity.

Along those lines, let me now briefly address some of the questions that have probably already occurred to the reader in the form of five likely objections to my proposal to make puzzle-solving more central in thinking about what it means to do social science:

(1) Is puzzle solving too limited in that it allows us only to deal with trivial/minor issues? Here, I would argue that puzzles (like research topics in general) come in all sizes, from very narrow and fact-specific (e.g., why did Persons X, Y, and Z engage in acts D, E and F, e.g., why did a prime minister and his chief opponents behave in a particular way in a particular crisis), to mid-range puzzles (e.g., why is the U.S. Senate in the post WWII period more Republican than the U.S. House?), to more big-picture puzzles (e.g., why do some societies where women have made major advances in the workplace still have so few women legislators? Or, why do unions strike when they have no chance of winning? Or, bigger still, how could the Soviet empire have dissolved so quickly?)

(2) Is puzzle-solving a distraction from the real job of building general theory? Here, I would first note that puzzle-solving is only one of many possible styles of research and that, while I don’t think there’s been enough of it in the discipline, that is certainly not to say that every article or book (or even most) need to be written within a puzzle-solving framework. The need for general theory building is as strong as ever. But, I would argue that the problem of theory-building is a general problem of making work cumulate and interlock, and that while puzzle-solving offers no readier solutions to that problem than any other approach, it can lead to the development of general theory, especially as we come to grips with particular puzzles that have implications beyond a particular time and place. To move beyond abstraction and irrelevance, theories have to confront concrete facts and seek to explain then.

(3) Is puzzle solving necessarily so selective in the facts that its chooses for its puzzle and the facts that it uses for its explanation that the explanations it offers are of little use? Here, I would argue that all work is selective, even the thickest of descriptions. Any model needs to be selective in a good way, i.e., to focus on a relative handful of factors that have substantial explanatory power. If we look at any theorizing, whether it be Huntington’s clash of cultures, or Wallerstein’s world system theory, to name two approaches that are not regarded as “rational choice” in nature, it seems obvious to me (as I suspect it does to most readers) how remarkably selective they are in the factors they regard as critical, and in their discussion of the phenomenon they are trying to explain. I am a pragmatist. The value of a theory must be judged in the “bang for the buck,” e.g., the extent to which a sparse model has a broad scope.

(4) Is puzzle-solving so limited in the way that it explains a given phenomenon that it offers not real explanations but only pseudo-explanations which beg the question of true causation? For example, Anderson’s chapter tells us how Gorbachev’s decisive
inactions allowed the Soviet empire to dissolve so quickly, but it is easy to claim that Anderson does not address the (perhaps allegedly much more important question) of how Gorbachev came to be in a position to act (or rather, fail to act) as he did. Here, I would emphasize that any explanation takes some facts/phenomena as given, even though, for other purposes those very phenomena would themselves, need explanation. No explanation is ever complete. There is nothing unique to rational-choice approaches or puzzle-solving approaches in having this limitation. To put it simply, it is often much more straightforward to discuss the validity of a given explanation than it is to debate, in the abstract, what the properties of a good explanation must be.

Most social scientists would agree that any explanations must be judged in the context of what it is that it is trying to explain, that any explanation must be judged in the context of competing explanations, and that any explanation must be judged in terms of the kinds of evidence it offers and the accuracy of the description it gives of what it is that is being explained. We need not resolve questions such the limitations of the nomological model -- questions perhaps better left to the philosophers of science -- to decide when we have, in social science terms, a reasonable explanation for a phenomenon such as, say, variations in the proportion of women in national parliaments.15

(5) Is puzzle-solving just another name for rational choice? In a word, my answer to that question is no. While I count myself a “reasonable choice” modeler and all the essays in this volume are more or less “rational choice” in tone, there is nothing whatsoever in the puzzle-solving approach that requires that the answer to the puzzle be a game-theoretic equilibrium or even a story about preferences and constraints on choice. In fact, some of the papers that my students or I have written that are puzzle-oriented offer solutions that are explicitly institutional but not incentive-driven (e.g., Thomas Brunell’s explanation (Brunell, 1999) of why the Senate has been more Republican than the House), and there’s nothing to say that the best solution to some puzzle won’t involve, say, political culture (e.g. Putnam’s work on North-South differences in Italy). In some particular instance, we may approach a puzzle inspired by ideas from rational choice theory – or we may not; we may make use of formal modeling to help us solve puzzles -- or we may not. Which methodological axe has been sharpened is not the issue. Insight into a problem is what counts.

I would like to believe that proposing we focus more on concrete puzzles, and showing examples of papers that do so, might actually influence the research choices of a next generation of graduate students. None of the standard methodological treatises or “scope and methods” primers I ever read treated puzzle-solving as something central to what social science is all about, and I have deliberately written this introduction in a rather provocative fashion (emphasizing the theme of puzzle-solving and who-dunits, why-dunits and how-dunits) with that aim in mind. Looking at what we do in concrete puzzle-solving terms forces empirical relevance, discourages methodological dogmatism, and it can convey a spirit of excitement to our students.16 Puzzle solving can be fun.17
ENDNOTES


2 For some who find empirical work too daunting, there is always theology.

3 In a companion volume, ‘Six Puzzles About Congressional Elections,’ my co-authors (Thomas Brunell and William Koetzle) and I consider questions such as: “Why do we see virtually certain midterm loss in the House, but not in the Senate?” “Why has split-ticket voting exhibited a pattern of rise, then fall and, most recently, rise again?” and “Why have the Republicans generally done better in recent decades for the Senate than for the House?”

4 Another example of this type of conundrum: “Why did Italian cabinets topple with great rapidity but the politicians who toppled with them return to power again and again?” is discussed in Mershon (1996).

5 I have been reading about one mystery story a day for nearly forty years and I own over 5,000 titles. Mostly I read mysteries for relaxation, but I also read them because I am interested in puzzles.

6 Recall, for example, the old Perry Mason courtroom dramas on TV where we learn the identity of the criminal when he or she is trapped by Mason into an incriminating confession on the witness stand (or sometimes, even more implausibly, from their place in the back of the courtroom watching the trial).

7 See e.g., the “locked-room” mysteries of John Dickson Carr, or the TV mysteries solved by Columbo where we know who the killer and Columbo, himself, intuits the identity of the killer very early in the program, but then has to break the killer’s alibi.

8 See e.g., some of the psychological novels of Ruth Rendell.

9 That said, as I have written elsewhere (Grofman, 1993c), I regard game-theory to be as central a tool to political science as, say, calculus, is to physicists. However, I strongly reject the implicit or explicit view espoused by some modelers that anything that lacks a result about a game-theoretic equilibrium cannot be a major contribution to social science.

10 As to whether it is harder to prove a theorem than to do good empirical work, in my view, that depends upon the nature of the theorem and on how good the empirical work happens to be. Also, some people’s talents lie in one direction rather than another.

11 My own contributions to the debate about the empirical contributions of Public Choice approaches include Grofman (1993a, 1993c; 1996); see also various essays in Grofman (1993b), including my introduction to that volume. In this debate, Green and Shapiro (1994) and other critics of rational choice have been aided by the arrogance of some of the more dogmatic rational choice modelers who sometimes appear to confuse technical
elegance or the level of mathematical difficulty required for a proof with the degree to which a result helps us make sense of the world.

12 One of these proposed chapters, by my colleague, A Wuffle, on his approach to “reasonable choice theory,” has subsequently been published as Wuffle (1999) and deals with some of the more general issues related to rational-choice bashing. Although written partly tongue-in-cheek (as is the case for all Wuffle publications), Wuffle (1999) expresses views which I strongly share. In particular, Wuffle (1999) argues, i.a., that

There is no such thing as the rational choice model of any given phenomena, only a rational choice model; different models are based on different assumptions. Empirical science is about testing competing models. Just as God is in the details, so is the power of rational choice in the secondary assumptions. Arguing in the abstract about which phenomena can or cannot be explained by rational choice models is not useful; indeed, arguing in the absence of considered evidence about which phenomena can or cannot be explained by (rational choice) models is downright stupid. Empirical science is about testing competing models. Demonstrating that some particular phenomenon cannot be well accounted for by some particular rational choice model demonstrates only that some particular rational choice model cannot explain that particular phenomenon; such a demonstration cannot invalidate the search for rational choice explanations of behavior. Hence, for example, [the failure of decisive voter models of] turnout cannot be "the paradox that ate rational choice theory." Demonstrating that some particular phenomenon cannot be well accounted for by some particular rational choice model may be of value to the advancement of science (a la Popper's falsification thesis), but, contra Popper, science advances most by what comes to be known not by what is shown to be false. Hence, debunking some particular rational choice model is of limited value unless one has something better to put in its place. Empirical science is about testing competing models.

Wuffle also asserts:

Saying that behavior is rational is not the same thing as saying that it is perfect; what is rational to being q at time t is a function of circumstances and information. What may be rational for being q at time t, may not be rational for being q at time t + 1, given that being's new circumstances and information. What may be rational for being q at time t, given that being's circumstances and information, may not be apparent to an observer who isn't walking in that being's shoes. [Also], (f)ew people do things for only one reason.

13 That is certainly not to say that nothing useful has been said in this debate.

14 Other authors who have looked at the strike decision have also emphasized how seeming irrationalities can be explained in the context of expected longer run consequences of strikes on the labor-management bargaining game. Sometimes unions strike with no chance of victory, since unions that forego strike activity will find it
substantially harder to withstand pressures from management. There must be a credible strike threat, and that may require actually engaging in strike activity, even if unsuccessfully.

15 An insightful discussion of the properties of a good explanation, albeit one focused on evaluating the merits of particular analytical narratives, may be found in Bates et al. (1998b, 14-18.) See also Wuffle (1999).

16 Here, I would note some interesting parallels between the approach outlined in this introduction and the recent work on “analytical narratives” by scholars such as Bob Bates and Margaret Levi (see esp. Bates et al. 1998a) – work which I hold in high regard. While this work is much more explicitly historical, like the puzzle-solving approach, it is very concrete in its desire to account for particular events or outcomes. Indeed, the authors see their approach as intended to occupy a “complex middle ground between nomothetic and idiographic approaches” (Bates et al. 1998b, 12). Also, while its focus on choices and decisions, and stress on viewing institutions as “subgame-perfect equilibria” eschews the kind of methodological eclecticism that I have espoused, Bates and his co-authors are in no way guilty of the kind of dogmatism found among some rational choice theorists. In particular, they view their work as a “complement to, rather than a substitute for, structural and macro-level analyses” (Bates et al. 1998b, 13). However, despite my strong sympathy for their attempt to “locate and explore particular mechanisms that shape the interplay between strategic actors and thereby generate outcomes “ (1998b, 12), which finds resonance in my own previous work on the effects of electoral systems (see esp. Davidson and Grofman, 1994), and even though I would fully agree that actors do try to “reason backward” to anticipate the likely consequences of their actions, I would wish to enter a caveat about the emphasis of the analytical narratives approach on insights derived from equilibrium results. However, detailed discussion of this issue would take us well beyond the scope of this introduction.

17 I wouldn’t be a political scientist myself if I didn’t find doing social science a lot of fun.