1 Introduction: problems and methods in the study of politics

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Political science, particularly in the United States, is often said to be a fractured discipline – perpetually split among warring camps (or "separate tables" to use the late Gabriel Almond's evocative phrase (1988)). Partisans articulate their positions with passion and intensity, yet the nature of what divides them is hard to pin down. At times we hear of a stand-off between "qualitative" scholars, who make use of archival research, ethnography, textual criticism, and discourse analysis; and "quantitative" scholars, who deploy mathematics, game theory, and statistics. Scholars in the former tradition supposedly disdain the new, hyper-numerate, approaches to political science as opaque and overly abstract, while scholars of the latter stripe deride the "old" ways of studying politics as impressionistic and lacking in rigor. At other times the schism is portrayed as being about the proper aspirations of the discipline – between those who believe that a scientific explanation of political life is possible, that we can derive something akin to physical laws of human behavior, and those who believe that it is not. For partisans of the latter view, the stochastic nature of politics and the unpredictability of human action mean that the best we can do is explain specific events – with as much humility and attention to context as possible. At still other times the rivals are portrayed as "rational choice theorists," whose work is animated by the assumption that individuals are rational maximizers of self-interest (often economic, sometimes not), and those who allow for a richer range of human motivations.

There is some truth to all these dichotomous accounts of disagreement, but they are subsumed under a larger, more fundamental question about the proper place of problems and methods in the study of politics. This volume asks: Which should political scientists choose first, a problem or a method? Here too, there are fierce proponents of both approaches. Those who advocate "problem-driven" work claim that it is most important to start with a substantive question thrown up in the political world and then seek out appropriate methods to answer it. These scholars contend that only a problem-driven political science is likely to contribute much

of practical or intellectual importance to the broader communities in which we work. Critics charge that the practitioners of this approach still have little if anything to offer that is more rigorous than the best writings of journalists and historians. The first imperative today, they contend, must be to make political science more of a science. Consequently, they argue that, for now, political scientists must focus on developing more rigorous methods, restricting their terrain of study to topics to which those methods can fruitfully be applied.

The easy and perhaps correct response is to say that political scientists should both develop more scientific methods and choose important questions; but decisions about obtaining and committing professional resources create pressures for individuals, departments, institutions, and the profession as a whole to establish priorities. Besides, it is not clear that either is desirable. A problem-driven approach might lead to a kind of disciplinary chaos, with scholars drawing on methods as diverse as textual criticism and formal modeling to offer a jumble of theories between which no adjudication or comparison is possible. On the other hand, a method-driven political science would preclude some "unscientific" types of substantive questions altogether, crippling the discipline by constraining its scope. There is no simple way to resolve this debate, but the essays in this volume, drawn from a conference held at Yale University in December 2002, attempt to grapple with these questions and suggest corrigible answers.

The book is divided into three parts. The first explores the problem–method divide, and includes essays that describe the rift, reconceptualize it, and illustrate pathologies of both problem- and method-driven work. Part II explores rational choice theory – which has been subject to the most opprobrium for supposed inattentiveness to substantive problems – and attempts to defend it against attacks and reformulate it in response to those attacks. The chapters in Part III explore the possibilities for methodological pluralism, asking whether such a pluralism is possible or even advisable, what such a pluralism might look like, and the changes that pluralism would necessitate in the way scholars approach their own work.

I. Description, explanation, and agency

One response to the problem–method dichotomy as it is framed above is to argue that all empirical research is theory laden, that there is no such thing as a purely problem-driven perspective, since our choices about what to study are invariably influenced by our prior theoretical and methodological commitments. Shapiro takes up this question in

chapter 2. He concedes that theory can sway one's choice of subject, but argues that there is a difference between researchers who choose subjects because they are of intrinsic interest and those who approach them as opportunities for the vindication of their pet theories. He argues that scholars who seek to illuminate the political world should direct themselves to questions thrown up in that world, come to grips with previous attempts to answer the question, and then, if previous answers are inadequate, offer new ones using whatever methodological tools are most appropriate. Method, he tells us, should be subordinate to problem, not the other way around.

But the fact that all observation is theory laden means that even when a problem has been selected on its own merits, there will likely be disagreement among scholars over how to characterize that problem. Since every piece of social reality is given to multiple descriptions, Shapiro points out, scholars must dedicate as much energy to getting the right descriptive cut at a phenomenon as they do to explaining it. Adjudicating among different descriptions is difficult, and Shapiro registers skepticism that a single standard is possible or even desirable. He explores the suggestion that a description's ability to allow scholars to generate non-trivial predictions might offer a useful standard by which to judge descriptive efforts, but he notes that this is an almost insurmountable standard for most current political research. He concludes that, in the end, description might be a virtue in its own right, especially when it involves "problematizing redescription" which encourages scholars to debunk fallacies, and force critical reappraisals of received wisdom.

In chapter 3, Rogers M. Smith suggests that political science – as it is practiced in the United States – suffers not only from an insufficient focus on real-world problems, but also from undue continued attention to outdated ones. According to Smith, the agenda of American political science remains stuck in the events and concerns of the last century, particularly the Cold War. He points out that this is reflected nowhere as much as in the way the discipline is organized: the fields of American politics, comparative politics, and international relations speak to an era in which the nation-state was the main unit of analysis, the United States was the exceptional nation, and the politics of other nations were of interest only in comparison to it or as friends and enemies on the international stage.

But, Smith tells us, the political developments of the later years of the last century, and the opening years of this one, have ushered in a new set of concerns that should become more central to the study of politics. The rise of new nation-states, the dismantling of old ones, the displacement of populations, the successes of minority groups in pressing for political recognition, and the appearance of transnational groups and movements

lend new urgency to questions of "political membership, status, and identity." Political scientists have not entirely ignored such questions, especially in the realm of nationalism, but Smith argues that "there have been few thoroughgoing efforts to unify theories of national identity formation with accounts of the creation of groups, social movements, and gender, racial, cultural, and religious identities." But if political scientists are to explore the possibility of a more "unified" theory of identity formation, Smith points out, they will not be able to rely primarily on the standard tools of rational choice analysis and large-N statistical models. Instead, they will have to employ methods that are "richly interpretive" – including "textual analyses, ethnographic field work, biographical studies, in-depth interviews, individual and comparative case studies, participant observation research" – which will allow researchers to understand why certain identities appeal over others, and how those identities "are likely to shape conduct."

Anne Norton takes issue with Shapiro and Smith's exhortations to problem-drivenness in chapter 4, noting that scholarly attention to real-world problems has not always (or even often) been salutary. Though she agrees with much of the criticism of method-driven scholarship – pointing out that blind commitment to a particular method "leads to proselytizing, imperialism, and coercive assimilation" – she notes that scholars who view themselves as solving social, political, and economic problems often end up in the service of the state and corporate interests. Through an examination of three episodes – colonialism, reconstruction after the Civil War, and the Cold War – she shows how scholars who engaged the great issues of the day easily fell into the role of providing cover and legitimation to the dominance practices of the powerful. Norton also detects an arrogance at the heart of the problem-solving enterprise – such scholars, she says, are animated by a belief that "their acts will follow their will, that they are able to determine the consequences of their actions, and control the uses of their work." In reality, Norton tells us, such faith is woefully misplaced, as attempts to solve problems invariably create new and unforeseen ones.

Having argued that both problem- and method-driven scholarship facilitate domination – the former of powerful interests, the latter of one methodological camp over another – Norton attempts to redirect the attention of political scientists to a more benign enterprise. Instead of problem solving, she beckons scholars to engage in what Habermas called "world disclosure." In this she echoes Shapiro's call for scholars to dedicate their intellectual energies to the description of political phenomena – both as a first step to explaining them and as a means of questioning accepted narratives and myths. According to Norton, "world disclosure" is the only way that we can address questions that admit of no answers,

to address what she points to as the enduring puzzles of our enterprise: "What is being? What is thinking? What is justice?"

Norton's skepticism of problem-driven social science is amplified by Frances Fox Piven in chapter 5. Piven dissects the uses of what she calls "policy science" – that is, social scientific research conducted in the service of government policy. The standard critique of the marriage of social science and policy is that the linear cause-and-effect relationships posited by social scientists are seldom accurate reflections of reality, and that attempts to build policies on such fictive foundations are thus doomed to failure. Piven registers her sympathy with this critique, but advances an additional one. In reality, she tells us, social science's usefulness to policymakers is not as a guide to the crafting of policy, but as a means of providing scientific justification to partisan initiatives. As she puts it, policy science "comes to be used as a political strategy, providing arguments or stories to elicit support from among those in a wider public whose participation can influence the outcome in a political contest." Piven explores the use of policy science in the debates around welfare reform policy during the Clinton Administration, demonstrating how social scientists were recruited into the nakedly political effort to cut aid to the poor, how they rushed out flawed research in order to legitimize government policy, and how marginalization was the fate of those who did not toe the line.

Adolph Reed Jr. continues this exploration of the nexus of power and social science in chapter 6, but he does so by engaging in the kind of problematizing redescription that Shapiro advocates. He shows how the study of black politics has, from its inception as an academic field in the early twentieth century, unreflectively perpetuated a myth fostered by black elites – that of blacks as an undifferentiated group with unified interests best served by an enlightened leadership. In the political arena, blacks who dissented from the reigning orthodoxy and attempted to advance claims based on interests other than black identity were tarred as misguided or deviant, and black scholars typically followed suit, ignoring the diversity of the black community and refusing to question the "accountability and legitimacy" of the community's leadership. In a sense, the dominant myths of black politics provided a kind of theoretical framework that drove much of black scholarship, precluding potentially fruitful lines of inquiry that were fundamentally at odds with it. Reed argues for a new, more democratic approach to the study of black politics, one that focuses not on the promotion of an artificial consensus based on a fictitious racial solidarity, but on encouraging norms of participation and open debate, and on pressing for the accountability of black leaders.

In chapter 7, John Ferejohn voices his mistrust of the supposed dichotomy between problem- and method-driven work. Though he agrees with Shapiro that every social phenomenon admits of multiple descriptions, and hence multiple explanations, he rejects the idea that one can adjudicate among them, doubting that there can be said to exist a single, correct description or explanation of anything. Method-driven and theory-driven work, he tells us, may yield "uneven or partial explanations," but social phenomena are multifaceted, and the accumulation of one-sided explanations increases our understanding of the totality of the object under study.

Having rejected the problem–method dichotomy as unhelpful, Ferejohn proceeds to identify and explore another disciplinary divide – that between positivist social scientists and those who emerge from a more humanistic tradition, including political philosophers and ethnographers. He reconceptualizes the schism as being between those who seek "external" and "internal" explanations. Traditional social scientists, he tells us, are externalists; they look for causal explanations of human action. Internalists, on the other hand, "explain action by showing it as justified or best from an agent's perspective" – they identify an actor's reasons for behaving in a certain way. These two endeavors, he says, are often seen to be in conflict; after all, if a causal account of action can be advanced, there is no need for a justificatory, internal account, since the actor had no choice but to act in a given way. The debate is really one about agency – the human capacity to act intentionally is the keystone of internalist explanation, while externalists disregard it as unnecessary to explain action.

According to Ferejohn, rational choice theory provides a model for how the two types of explanation can be bridged. He departs from the standard interpretation of rational choice theory, which holds that individuals are simply hardwired to behave rationally, and instead views rationality as a norm to which actors may or may not adhere, depending on the attractiveness of the norm and their capacities for acting in accordance with it. Rational choice explanations, Ferejohn tells us, are thus both internal and external; they are internal in that those who act rationally do so intentionally, in accordance with a norm of rationality; and they are external in that actors' capacities to act in accordance with the norm, and the attractiveness of the norm, are shaped by causal processes. This reconceptualization of rational choice explanations, Ferejohn says, should serve to demonstrate that practices as different as descriptive ethnography and theoretical economics "are, at bottom, normative enterprises aimed at showing how the agents – real or imagined – can act and have reasons to act in ways that comply with norms they accept."

II. Redeeming rational choice theory?

As Ferejohn's essay shows, the place of rational choice theory in the study of politics is a vexing one for scholars. Indeed, many critiques of method- and theory-driven work were initially advanced as critiques of rational choice theory. The most notable such critique, Green and Shapiro's (1994) assessment of rational choice theory's contribution to our body of political knowledge, and their conclusion that the contribution has thus far been paltry, spawned a bevy of rejoinders. In chapter 8, Gary Cox moves beyond reflexive defenses of rational choice theory to explore what, if anything, makes rational choice scholarship different from other forms of social research. He tells us most scholars – not to mention novelists, journalists, and historians – employ some variant of the rational actor assumption, inasmuch as they rely on a model of human psychology in which actors behave in ways consistent with their preferences and beliefs about the world. Under this broad classification, only radical behavioralists (who eschew any assumptions about unobservable mental states), and those who argue that human psychology is too rich to be captured by reductionist assumptions, are outside the rational choice paradigm. The most vocal critics of rational choice theory, Cox tells us, are really critics of a particular brand of the theory, now outmoded, that assumes that actors have unlimited computational abilities and perfect information. Game theoretic models make assumptions about what actors know and how efficiently they act on it, but, Cox tells us, such assumptions should be substantively grounded and adjudged on a case-by-case basis. He argues that wholesale denunciations of game theory are "unlikely to produce methodological or substantive advance, relative to specific diagnoses of what is wrong with the current models and what might be done to improve or replace them."

Alan Ryan concurs with Cox's expansive definition of rational choice explanation in chapter 9, arguing that all attempts at explanation start from a premise of rationality, or as he puts it, rationalizability – we advance accounts that rationalize an agent's actions on the basis of his goals and beliefs. Whether action is said to be taken out of a commitment to some norm, or out of naked self-interest, or in order to express one's allegiances, the story being told is a rationalizing one. But such stories only get us so far, Ryan tells us. Scholars inevitably must direct themselves to asking why an agent was motivated by one reason over another (and whether his professed motivations were in fact his real ones). In this sense, rational actor explanation is only "prima facie" explanation – in order to advance full accounts of human actions, it is necessary to understand the genesis of the beliefs and preferences that are said to cause those actions.

Rational choice explanations are often criticized as being inattentive to the history and context surrounding political phenomena. In chapter 10, Margaret Levi describes an analytic approach that marries rational choice assumptions with historical analysis and in-depth case studies. These "analytic narratives," as they are called by Levi and cocontributors to a 1998 volume, offer explanations that are both sensitive to history and context and, because of their underlying theoretical framework, generalizable (Levi *et al.* 1998).

According to Levi, the analytic narrative approach is explicitly problem driven, in that it begins with historical puzzles that cry out for explanation. Cases are not selected with a view toward fitting them to off-the-shelf models; instead, models are constructed in close proximity to the details of the actual case. The method does, however, limit the range of phenomena that can be studied – the emphasis on narratives of specific events confines scholars to those events whose causal narratives are likely to be tractable, precluding attempts to answer grand questions of broad historical sweep, or in which contingency and unintended consequences play too prominent a role. But, as Levi points out, "by focusing on the institutional details that affect strategic interactions, choices, and outcomes, it can be useful in suggesting likely outcomes under given initial conditions." Thus, analytic narratives can be especially useful for problem-driven scholars who wish to offer advice on the likely outcomes of proposed, real-world policies. The superior usefulness of rational choice theory – and game theoretic models – for policymakers is the focus of Bruce Bueno de Mesquita's contribution.

In chapter 11, Bueno de Mesquita claims that in the last few decades policymakers have employed rational choice models successfully to predict outcomes in a range of political situations. He argues that these successes – and the fact that several firms make a brisk business out of making political predictions using such models – effectively refute claims of rational choice's limited real-world utility. But the predictive power of game-theoretic, rational choice models is not their only virtue, he tells us. The mathematical reasoning employed in such models allows scholars to achieve more logical rigor and argumentative coherence than with what he calls "ordinary language" approaches. The complexity of the political world, often invoked to suggest the impotence of reductionist mathematical models, is actually what makes such models more useful for political analysis than ordinary language, because of the reduced risk of logical error.

Yet Bueno de Mesquita does not prescribe these methods for all lines of inquiry. He argues that the choice of method will invariably rely on the substantive concerns of the researcher; large-N statistical research

for those who wish to establish general relationships between political phenomena, historical case studies for those who want to explain specific events, and textual criticism and discourse analysis for those who wish to advance normative arguments. Additionally, he argues that different methods should be used in tandem – for example, while game theory can explain actions given an actor's beliefs and desires, other approaches to the study of politics (he singles out constructivism) can help scholars understand from whence such beliefs and desires emerge.

III. Possibilities for pluralism and convergence

Even if we accept the formulation that scholars should seek to answer important questions, and that methods should be selected with a view toward what will best help us answer the chosen questions, there is unlikely to be agreement on which problem–method combinations will be most apt. It is conceivable, even likely, that scholars driven by the same substantive interest will approach their questions from methodologically distinct positions, and that each will have strong reasons for choosing one method and not the other. In chapter 12, Alan S. Gerber, Donald P. Green, and Edward H. Kaplan advance a claim for their own methodological predisposition – field experimentation. They argue that the lack of experimental controls and randomization procedures in non-experimental, observational research means that scholars have little means of assessing the bias inherent in such studies. Accordingly, they tell us, whatever we "learn" from such research is likely to be illusory. The authors admit, however, that experimental research is often costly or (especially in such areas as international relations) impossible, and so concede that observational research will remain the only option for some fields. Still, in the end such fields may be doomed to inexactitude, since at some point a ceiling is reached where refinements to estimates of causal parameters cannot be made without experimental research.

It is hard not to conclude from Gerber, Green, and Kaplan's essay that methodological pluralism is a sad fact of life, to be tolerated only because some lines of inquiry are inhospitable to field experimentation. In chapter 13, Lisa Wedeen articulates a more sanguine view of the usefulness of different methodologies. The same substantive problem can be explored with different methods, and "different epistemological commitments sometimes yield divergent questions, sources of data, and explanations of political life." She examines three different approaches to the study of democracy: large-N statistical studies that seek to establish relationships between democracy and economic factors; a Wittgensteinian approach that seeks to understand the meaning of such terms

as democracy, representation, and accountability; and an interpretive approach that seeks to uncover how democracy is lived and practiced.

Taking Przeworski *et al.'s* (2001) study of democracy and economic growth as her starting point, she illustrates how large-N studies require scholars to make simplifications and use proxies that may not adequately capture their variables of interest. Thus, democracy is reduced by Przeworski *et al.* to the holding of competitive elections, whereas in reality elections may capture only a small part of what democracy is about. Wedeen examines the "qat chew" in Yemen – gatherings of men in which a mild narcotic is consumed – and how such gatherings entail a form of political participation that cannot be captured by any study that equates democracy with elections. Wedeen does not try to pronounce one method superior to others, but rather endorses a methodological pluralism, echoing and illustrating Ferejohn's conviction that methodologically diverse accounts of a single phenomenon can build our understanding of it.

In chapter 14, Rudra Sil explores attempts to resolve the tension between calls for methodological pluralism and the idea of social science as a "unified" enterprise. Those who try to unify social science, like Gerber *et al.*, push for a single methodology, or like King, Keohane, and Verba (1994) offer general methodological principles that purport to hold for all types of research. At the other extreme are those who resign themselves (or actively encourage) a methodological anarchism "consisting of incommensurable research products that can never be evaluated relative to each other." Sil notes that the former forces upon all the ontological and epistemological commitments of a few, while the latter bodes ill for disciplinary progress. Instead, Sil promotes what he calls "constrained pluralism" in which scholars embedded in "research communities" with distinct substantive and methodological orientations participate in a "complex division of labor," each providing different insights into different types of problems. He suggests that breakthroughs in our understanding of real-world phenomena occur when eclectic scholars draw upon the insights, practices, and empirics of different research communities to suggest new understandings and lines of inquiry.

William E. Connolly's contribution in chapter 15 is also concerned with the possibility of methodological pluralism. He argues that pluralism is only possible if scholars reconceptualize their methodological commitments, understanding them as "existential faiths" rooted in unverifiable beliefs about the world and the way it works. Methodological debates, in Connolly's view, are akin to religious ones, and like religious ones, conversions are possible, but faith is often durable even in the face of disconfirming evidence. Connolly advances his own orientation to the study