

Qualitative Methods

Newsletter of the
American Political Science Association
Organized Section on Qualitative Methods

Contents

<i>Creative Hypothesis Generating in Comparative Research</i> Richard Snyder	2
Symposium I: Ian Shapiro's <i>The Flight from Reality in the Human Sciences</i>	
<i>Introduction</i>	
Elisabeth Ellis	5
<i>Comments and Reflections</i>	
Gerry Mackie	6
Jennifer Hochschild	11
David D. Laitin	13
<i>A Response to Mackie, Hochschild, and Laitin</i> Ian Shapiro	16
Symposium II: Conceptualizing Concepts	
<i>Introduction</i>	
Naomi Choi	19
<i>What Makes a Political Concept Political?</i> Michael Freeden	20
<i>Crafting Explanatory Concepts</i> Naomi Choi	24
<i>Confounding Conceptual Clarity</i> James W. Davis	29
<i>When Can We Apply Our Concepts to the Past?</i> Mark Bevir	33
Book Notes	36
Announcements	38

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Letter from the President¹

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Three years on, the Qualitative Methods section is no longer a newcomer. Section membership has climbed to over 700, and we are now the seventh-largest section in APSA. The newsletter is widely viewed as one of the best in its genre. Section prizes for best book, article, and APSA paper are highly regarded. The section's Web site is active and frequently consulted (<http://www.asu.edu/clas/polisci/cqrm/>). And the section's panels at the annual meetings are extraordinarily well attended (see below). Section officers and members continue to teach at the Annual Institute on Qualitative Research Methods at Arizona State University, which this year is expecting more than 100 attendees.

APSA Panels

Perhaps the most important indicator of any section's health and wellbeing is its presence at the annual APSA meetings. The growth of the section since its creation has been nothing short of remarkable. In 2003, Qualitative Methods started with just six panels. APSA bases future panel allotments on a complicated formula, but most weight is given to the demand for the scholarship represented in the panels and roundtables, in particular by attendance. Because of the large and increasing interest in qualitative methods in the discipline, each year since 2003 we have increased our allotment by five panels—the maximum allowed by APSA rules.

At this past September's meeting in Washington, the section stood out for its high level of participation across a wide range of activities. Indeed, of the 45 divisions, Qualitative Methods had the second-highest mean-adjusted attendance. As a result, the section has again been assigned five new panels, bringing us to a total of 21 panels for APSA 2006. This new allotment makes Qualitative Methods a "top ten" division, as judged by our presence at APSA. We are grateful to the efforts of Program Chairs Gary Goertz and Ted Hopf this year, and to Melani Cammett and Julia Lynch who are currently organizing panels for next year. (Nota bene: please submit your paper and panel proposals now. The APSA deadline is November 15. The section's call for papers is listed on page 40.)

Short Courses

The section's three short courses shared 170 registrations, an exceptionally high level of attendance for APSA short courses. The courses were: Conceptual Innovation at the Intersection of Qualitative and Quantitative Methodology (co-taught by David Collier, Henry E. Brady, Jason Seawright, and Thad Dunning); Strategies for Field Research in Comparative and International Politics (co-taught by Melani Cammett and Benjamin Read); and Fuzzy Sets and Case-Oriented Research (taught by Charles Ragin). Please be on the lookout for short course offerings from the members of the Qualitative Methods section for APSA 2006.

Working Group

During the annual meeting, the section participated in a new APSA initiative, sponsoring a working group on qualitative methods. Through this group, the section sought to make its panels and short courses more valuable to participants in the meeting by providing continuity, coordination, and shared discussion. The working group convened both at the beginning and toward the end of the APSA meeting for a general discussion, and APSA provided certificates of recognition to those who participated fully in the working group.

The working group was also a great success. Although initially APSA envisioned groups of perhaps 20, the qualitative methods working group had close to 70 participants. Throughout the meeting, individuals could identify one another with badges provided by the APSA conveners. During the general meetings, participants engaged in lively discussions about the alternative qualitative methods available for political analysis.

Business Meeting

Andrew Bennett hosted his final business meeting after serving as President of the section for the last three years. The success of the section reported above is due in no small part to Andy's hard work and to the many section projects he has initiated and supported.

At the meeting, the section awards were presented (for information on the winners see the back of this issue) and the new section officers were elected (names are listed on the first page). A special award was also presented marking Alexander George's enduring commitments to the development and application of qualitative methods, with remarks by Jack Levy. The section's reception immediately following the business meeting was dedicated in Alex's honor, and co-sponsored by The MIT Press and Harvard's Belfer Center for Science and International Affairs. Graham Allison, the Center's Director, dedicated the reception to Alex, and called attention to the eagerly anticipated new book by Alex and Andy Bennett on *Case Studies and Theory Building in the Social Sciences* (MIT Press, 2005).

It is a pleasure to begin my presidency with the section in such excellent condition. Thank you for your help in making it so successful.

Notes

¹ Colin Elman and John Gerring doubled checked the data in this letter. In doing so, they also made a number of valuable suggestions.

Creative Hypothesis Generating in Comparative Research

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The following material is excerpted from Chapter 1, "The Human Dimension of Comparative Research," of Gerardo L. Munck and Richard Snyder's forthcoming book, Passion, Craft, and Method in Comparative Politics, which will be published in 2006 by The Johns Hopkins University Press (www.press.jhu.edu). The book consists of in-depth interviews with fifteen leading scholars in the field of comparative politics: Gabriel A. Almond, Robert H. Bates, David Collier, Robert A. Dahl, Samuel P. Huntington, David D. Laitin, Arend Lijphart, Juan J. Linz, Barrington Moore, Jr., Guillermo O'Donnell, Adam Przeworski, Philippe C. Schmitter, James C. Scott, Theda Skocpol, and Alfred Stepan. These scholars discuss their intellectual formation, their major works and ideas, the nuts and bolts of the research process, their relationships with colleagues, collaborators, and students, and the evolution of the field. The excerpt addresses one of the most elusive aspects of scientific inquiry: the process of generating good ideas. All quoted material presented in this excerpt is taken from the interviews in Passion, Craft, and Method in Comparative Politics.

Textbooks and courses on methodology center mainly on the issue of testing ideas, yet they usually offer little insight about the prior matter of how one generates ideas worth testing in the first place.¹ Similarly, professional publications rarely include discussions of how ideas emerge. The interview format employed in *Passion, Craft, and Method in Comparative Politics* allowed for an exploration of how fifteen leading scholars actually do their research, thereby opening a valuable window on the process of formulating good ideas. The interviews show that rich life experiences provide fertile ground for generating new ideas. They also show that experience is not the only path to insight in comparative research. Scholars spend a large share of their time reading; and books, journals, and newspapers all play an indispensable role in the development of ideas. Moreover, directly observing political and social interaction is an important tool for creative hypothesis generating. The interviews highlight five methods that help spark the comparative imagination of the best scholars: (1) "bibliographic sleuthing," that is, hunting for untapped sources in libraries and bookstores; (2) following current events; (3) critical engagement with contemporary works; (4) reading, and rereading, the classics of political and social theory; and (5) real-time observation of political action.

Bibliographic sleuthing, which involves searching, even in a haphazard way, in libraries or in bookstores, can lead to the serendipitous discovery of works that provide new insight.² For example, while rummaging in a used bookstore in Rio de Janeiro, Philippe Schmitter found an obscure book written in the 1930s that triggered his insight that the system of interest representation in Brazil could be conceptualized as “corporatist.”³ Similarly, Theda Skocpol discovered an old, forgotten book on social insurance in the United States in the early 1900s, which argued that Civil War pensions were a major social policy that would soon lead the United States to surpass Europe in the public provision of social benefits.⁴ According to Skocpol, “When I read this, it made me curious, because the mere empirical assertion that, in 1913, there was a lot of government social spending going on that amounted to *de facto* old age pensions cut against the grain of the whole literature that saw the United States as a laggard in social provision. I was skeptical at first...but I decided to look into the matter, because I had a hunch it might lead to something.” Skocpol’s hunch proved correct, as her probing resulted in a novel argument: the United States was actually a precocious welfare state, not a laggard behind European countries. This argument, in turn, played a pivotal role in her book *Protecting Soldiers and Mothers*.⁵

Following current events by reading newspapers and magazines can also serve to generate new ideas. Samuel Huntington says that reading “about what’s going on in the world” plays a fundamental role in his research. He recounts how his observation of chaos, anarchy, and corruption across developing countries in the 1960s, “when everybody was talking about modernization and development,” led to the insight that “there [was] more political *decay* out there than political development. And so I wrote *Political Order in Changing Societies*.”⁶ Reading about current events can work in tandem with bibliographic sleuthing in the formation of new ideas. While reading the newspaper in Switzerland one day, Schmitter saw an article about the role of the Swiss Milk Producers’ Association in the annual price-fixing mechanism for milk. He noticed that this regulatory framework bore a remarkable resemblance to the corporatist systems of interest intermediation he had previously studied in Brazil and Portugal. This realization led him to the library in search of material on Swiss interest group politics, where he discovered an unpublished dissertation from the 1930s on Swiss corporatism. As a result of his newspaper-inspired trip to the library, Schmitter saw that the concept of corporatism could be applied not only to authoritarian countries, but also to democratic ones. This insight anchored his influential article, “Still the Century of Corporatism?,” as well as subsequent works that further elaborated the corporatist model of interest group politics as an alternative to the pluralist model.⁷

Critical engagement with contemporaries is a further way to stimulate new ideas. David Laitin describes how research on the relationship between culture and politics by contemporary scholars like Harry Eckstein, Aaron Wildavsky, and Arend Lijphart provided a compelling foil against which he developed and refined his own ideas: “I was going after Harry

Eckstein from the very beginning. I was arguing against Eckstein’s congruence theory, which posited a kind of direct mapping from one realm—culture—on to another—politics.⁸ In contrast, I said that there was no necessary connection between the cultural and other realms, between say religion and politics...My views also went against Lijphart and also against almost everyone who had been writing on culture.”

Laitin’s critical engagement with the work of these interlocutors helped him formulate his idea that culture both shapes and is in turn shaped by political choices. Similarly, Skocpol notes that arguing against “mistaken others” plays a key role in the process of developing her own ideas:

I’ve always worked out what I was thinking by critiquing work done by others. What gets me excited is seeing that someone else is partly right and partly wrong....My major projects have always been launched with a sense of argument against a received wisdom or an interlocutor, especially somebody important whose work I respect.

Another way to stir the comparative imagination concerns *classic works* of political and social theory. These classics play an important role in the intellectual life of some of the top scholars in comparative politics. Robert Dahl sees himself as having engaged throughout his career in what he calls an “imaginary dialogue” with Plato, Rousseau, and Marx. Adam Przeworski observes, “Reading classics of political theory is extremely important to me. It’s a source of hypotheses, historical information, and great ideas.” Schmitter offers:

For me, engaging the classics is almost automatic. I start by thinking about the nature of the problem on which I want to work, and then I ask myself, ‘Who’s said something about this?’ Sometimes it is simply a matter of having these classic works in your head, having read them.... My first instinct is to go through my own memory of what I have read on political thought.

Finally, Juan Linz notes, “Whenever I start working on something, I usually look to see whether Weber has anything to say on that theme.” To show how he draws ideas and inspiration from the classics, Linz recounts his use of Weber’s concept of “sultanism” to study personalistic dictatorships, such as those of Anastasio Somoza Debayle in Nicaragua and Rafael Leonidas Trujillo in the Dominican Republic.⁹ Because the degree of cronyism, nepotism, and unbridled discretion enjoyed by the ruler was so extreme in these cases, Linz felt uncomfortable classifying them in the same category as regimes like Francisco Franco’s in Spain and Antonio Salazar’s in Portugal. According to Linz,

Weber makes a distinction between a traditional, legitimate form of patrimonialism, on one hand, and the corruption of patrimonialism into sultanism, on the other. When I reread Weber’s section on patrimonialism, I thought, ‘That’s exactly what I am looking for!’ Then I reformulated Weber’s concept in a modern way by specifying indicators of sultanism, like nepotism, cronyism, and the private appropriation of power and wealth.

You have questions in your own mind that you want to address, and sometimes you read the classics and say, 'well, that's an interesting insight, it illustrates what I was groping for.' So, the more you read and the more you know, the better.

Real-time observation of political action is a further technique for generating fresh ideas. James Scott describes how living in a Malaysian village for two years enabled him to conduct rolling interviews with peasants that helped him see the "subterranean forms of resistance to hegemony, such as desertion and foot-dragging, underneath the placid surface of the village."¹⁰ Scott also emphasizes that "politics is everywhere," not just in the distant and exotic setting of "the field," and he offers a fascinating example of observing political interaction among passengers while riding on a train from New York City to Washington, D.C. Schmitter also highlights the value of observation, noting that his efforts to form new concepts are often stimulated by talking to political actors and listening closely to the words they use to describe what they do. Laitin's discussion of watching a Catalan national dance, the Sardana, while doing fieldwork in Barcelona offers an especially vivid example of how observation can help trigger new ideas:

When the people perform the Sardana they put their little bundles of possessions in the center and dance around them. So, they developed an urban dance that enabled them to protect their property the whole time they were dancing. And they have to count a fairly large number of steps... I saw them counting their steps with their lips, though trying to hide it because you're not supposed to show it.

Thousands of tourists have seen the Sardana, it happens all the time, and the dance itself is relatively boring. But to me it was inspirational, and I asked myself a very simple question. 'Here I am in the most bourgeois city I've ever lived in, with a commercial bourgeoisie that goes way, way back, which developed an urban form of culture in which they can protect their property while dancing. And they count! It's the fundamental commercial function to count.' And then I asked, 'why are people who are so rational and so calculating pushing a linguistic movement that would increase their communicative capabilities by zero? You would think the Catalans would be on this gigantic learn English campaign, which would be tremendously more useful for their commercial dealings. Why are they pushing this language, Catalan, which, if successfully promoted, will allow them to communicate with no more people than they presently communicate with, and which will have no communicative payoff whatsoever?' And I just walked through the town for the next two or three days, sort of like a zombie, asking and re-asking that question to myself.

Watching the Sardana made it easier for Laitin to see that the tools of game theory, especially the concept of "coordination games," offered a powerful and fruitful way to explain

why people participate in language movements that do not serve their material interests.¹¹ Laitin concludes, "this insight in Barcelona pushed my research program for quite a while, in utterly new directions. Fieldwork has that excitement for me."

Conclusion

Hunting for obscure books, perusing the newspaper, critically engaging contemporary authors, reading the classics, and making observations are, of course, not sufficient to formulate important ideas. After all, many social scientists read the newspaper and follow current events, yet few produce works with the impact of Huntington's *Political Order in Changing Societies* or Schmitter's "Still the Century of Corporatism?"¹² Moreover, as Max Weber reminds us, "Ideas occur to us when they please, not when it pleases us."¹³ Hard work, discipline, and maybe even luck are also necessary to develop good ideas, as is intelligence, especially the capacity to recognize an important question, puzzle, or lead when it arises.¹⁴

Although factors like luck and intelligence are difficult, if not impossible, to control, there may still be ways to increase the probability of developing new ideas. The evidence from the interviews in *Passion, Craft, and Method in Comparative Politics* underscores the importance of openness to the possibility of surprise combined with the curiosity, confidence, and drive to follow a hunch. Moreover, by mastering the literature so that we have a firm grasp of the "conventional wisdom," we may be able to enhance our ability to notice puzzling new information. For example, had Skocpol not understood that the standard view cast the United States as a welfare laggard, then she probably would not have seen that the book she serendipitously discovered through bibliographic sleuthing made an argument that cut sharply against the grain. And her fortuitous discovery of this book still might have led nowhere had she lacked either the curiosity to pursue the lead or the skepticism and confidence to question received wisdom.

While there is no magic formula for sparking the comparative imagination, these various methods of creative hypothesis formation are important aspects of the process of generating good ideas.

Notes

¹ This imbalanced focus on hypothesis testing, as opposed to hypothesis generating, is not unique to political science and sociology. See William J. McGuire, "Creative Hypothesis Generating in Psychology: Some Useful Heuristics," *Annual Review of Psychology* Vol. 48 (1997):1-30.

² Bibliographic sleuthing can be done increasingly on the Internet.

³ Mihaïl Manoïlesco, *Le Siècle du Corporatisme* (Paris: Alcan, 1934).

⁴ Isaac Max Rubinow, *Social Insurance, With Special Reference to American Conditions* (New York: Arno Press, 1968; orig. 1913).

⁵ Theda Skocpol, *Protecting Soldiers and Mothers: The Political Origins of Social Policy in the United States* (Cambridge: The Belknap Press of Harvard University Press, 1992).

⁶ Samuel P. Huntington, *Political Order in Changing Societies* (New Haven, CT: Yale University Press, 1968).

⁷ Philippe C. Schmitter, "Still the Century of Corporatism?" *Review of Politics* Vol. 36, No. 1 (1974): 85-131; reprinted as "Still the Century of Corporatism?" 85-131, in Fredrick B. Pike and Thomas Stritch (eds.), *The New Corporatism: Social-Political Structures in the Iberian World* (Notre Dame: University of Notre Dame Press, 1974).

⁸ Harry Eckstein, *Division and Cohesion in Democracy: A Study of Norway* (Princeton, NJ: Princeton University Press, 1966).

⁹ Linz's initial formulation of the concept of sultanistic regime is in Juan J. Linz, "Totalitarian and Authoritarian Regimes," in *Handbook of Political Science*, Vol. 3, Nelson Polsby and Fred Greenstein, eds. (Reading, MA: Addison-Wesley, 1975). See also H. E. Chehabi and Juan J. Linz, "A Theory of Sultanism I: A Type of Nondemocratic Rule," 3-25, in H. E. Chehabi and Juan J. Linz, eds., *Sultanistic Regimes* (Baltimore, MD: The Johns Hopkins University Press, 1998).

¹⁰ The results of this research were published in James C. Scott, *Weapons of the Weak: Everyday Forms of Peasant Resistance* (New Haven: Yale University Press, 1985).

¹¹ On "coordination games," see Thomas C. Schelling, *The Strategy of Conflict* (Cambridge, MA: Harvard University Press, 1980).

¹² Samuel P. Huntington, *Political Order in Changing Societies* (New Haven, CT: Yale University Press, 1968); Philippe C. Schmitter, "Still the Century of Corporatism?" *Review of Politics* Vol. 36, No. 1 (1974): 85-131.

¹³ Max Weber, "Science as a Vocation," 129-156, in H. H. Gerth and C. Wright Mills, eds. *From Max Weber: Essays in Sociology* (New York: Oxford University Press, 1946), 136. Weber further notes, "...ideas come when we do not expect them, and not when we are brooding and searching at our desks. Yet ideas would certainly not come to mind had we not brooded at our desks and searched for answers with passionate devotion."

¹⁴ Luck may be more important in determining the impact rather than the quality of research. When told that he had been lucky in his research, Louis Pasteur replied, "Fortune favors the prepared mind." See Lewis Wolpert and Alison Richards, *A Passion for Science* (Oxford: Oxford University Press, 1988), p. 6.

Symposium I: Ian Shapiro's *The Flight from Reality in the Human Sciences* (Princeton University Press, 2005)

This symposium is based on a roundtable discussion of Shapiro's book, held at this year's meeting of the American Political Science Association in Washington, D.C. We would like to thank the editors of The Good Society (A PEGS Journal) for their permission to reprint some of the material from their forthcoming symposium on the state of the discipline.

Introduction

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One of the surprising results of this roundtable on Ian Shapiro's new collection of essays was the establishment, at least among the panelists, of a large area of uncontroversial common ground, ground that might not have been so uncontroversial in previous years. No one opposed the project of political science in general. No one attempted to transcend politics with a conflict-ending theory of everything. Everyone recommended a focus on the mid-range of political analysis, with some work at very high or very low levels of abstraction. No one blamed individual scholarly shortcomings for the problems of the discipline, nor were social class, ideology, or historical particularity said to be at the root of political science's failure to fulfill its promise. Instead, each panelist (including Theda Skocpol, whose very interesting spoken contributions are not included in this symposium) looked to academic practices, scholarly assumptions, and perhaps most importantly, at professional and educational institutions. It is therefore not at all surprising that when serious disagreements did arise, they had to do with the practice of education in the discipline, and especially with the teaching of political science methodology.

In *Flight*, Shapiro argues for a problem-driven rather than method-driven research practice. By the "flight from reality in the human sciences," Shapiro means the tendency of research-

ers to cleave to attractive theoretical constructions of political life, whatever the evidence presented by political reality might be. Researchers' theoretical commitments not only incline them to ignore unwelcome facts, these commitments prevent them from observing such facts in the first place. Shapiro finds this dynamic at work nearly everywhere he looks: among rational choice theorists, famously, but also among political theorists, appellate court judges, and students of political behavior, among others. Fortunately, he does not find it everywhere, and while arguing for approaches to the study of politics that might be more open to empirical reality and thus more likely to contribute to our knowledge about politics, Shapiro sketches a set of practices under the loose heading of "realism" that should be more fruitful than those currently in fashion.

Shapiro associates himself with the members of what he calls "the mature Enlightenment," and rightly so. He shares their orientation toward incrementalism in the pursuit of knowledge, their preference for practical results over grand theory building, and their interest in bettering the human condition without losing sight of the actual human beings experiencing it. Shapiro *dis*associates himself from a broad tradition of theories that explain everything by adjusting their observations of the complex political world to fit their predilections (this mode of categorization makes for refreshingly strange bedfellows: Bentham, Marx, and Freud find themselves beside contemporary rational choice theory, for example). The intellectual radicalism of Shapiro's approach to the study of politics (see his 1999 *Democratic Justice* [Yale University Press]) is sometimes overshadowed by the individual scholarly controversies in

which the chapters engage. Interesting as each of these controversies is, taken together, they constitute part of an effort to pursue the collection of knowledge about politics without subsuming it under any universalizing narrative.

Shapiro's critique of reductionism applies as strongly to the subdiscipline of political theory as it does to the rest of political science, as he makes especially clear in his chapter on gross concepts in political argument. He argues that most political theorists miss the "relational character" of the concepts they use, taking up the banner of one or another conceptual abstraction while missing the fundamental political point that normative rights are only exercised among actors. Like Kant, who compares the view that property inheres in an object to the view that objects have guardian spirits, Shapiro criticizes theorists who imagine freedom as a sort of bubble shielding individuals from one another. Rather than fall into the paradoxes that inevitably result from the application of unitary concepts to a world of political relationships, political theorists ought to ground their work in the realities of power, institutions, and conflict. Theorists themselves are not immune to these realities: "people have powerful needs for unifying, simplifying, comforting, omniexplanatory foundational concepts, concepts over which flags can fly...the relational structure of their moral and political grammar is...often opaque to them."

If the panelists did not focus on this chapter in *Flight*, they did take up the question of the relation of political theory to the rest of the discipline. Like Shapiro, each of the panelists argues that theory ought to be more closely connected to the rest of the field, though each has a different view of what that would mean in practice. For Shapiro, the empirically informed, normatively sophisticated political theorist ought to produce "problematizing redescrptions" of political reality. Mackie agrees that theorists should be connected to empirical political science, but emphasizes the need to encourage "theorists of all varieties: counter, original, spare, and strange." Laitin seconds Shapiro's call for a "roving ombudsman" role, and Hochschild offers both a biting critique of ordinary political theory and a call for the kinds of illuminating redescription offered by "Karl Marx, Sigmund Freud, Edward Said, Harriet Taylor, and W.E.B. DuBois." For Shapiro, theorists and non-theorists alike need to focus on real political problems while overcoming their need for simple explanations of a complex world.

Mackie disagrees with Shapiro's sharp distinction between problem-driven and method-driven research, demonstrating the fruitfulness of several method-driven enterprises, including his own. Defending rational choice theory, which he would prefer to call "purposive actor theory," Mackie shows that it can result in original insight into some types of problems, particularly those involving interdependent actions. The story of Mackie's arrival at his purposive-actor explanation of the practice of female genital cutting via a fascination with Schelling shows that some method-driven research results in substantive advances in knowledge, a claim Mackie bolsters with the examples of Alan Turing and Hubert Dreyfus. More than the other contributors, Mackie worries that narrow specification of research methods and programs will eliminate the

space in which "discerning observations" about politics are possible.

Like Mackie, Hochschild argues for a broader mandate for political science than she finds in Shapiro's problem-driven approach. She argues for the inclusion of "perspective-driven" work, in which the researcher adopts an unconventional perspective and uses it to illuminate problems and dynamics that were effectively invisible without it. Even though these perspectives are never perfect and often "tendentious and circular," their adoption can reveal "new channels for analysis and evaluation." Drawing on her experience as the founding editor of *Perspectives on Politics*, Hochschild emphasizes the noxious effects that intellectual conformity can have on research in political science. After sketching sharp caricatures of typical submissions from most of the discipline's subfields, Hochschild describes some institutional responses that conduce to better work, and concludes that many political scientists both desire and are able to produce research that measures up to Shapiro's standards.

David Laitin also endorses Shapiro's commitment to the pursuit of answers to substantive political questions by means of acute observation and "problematizing redescrptions" of empirical reality. Laitin argues, however, that in *Flight*, Shapiro commits the same errors he finds in most political science, including especially the error of devotion to ideal presuppositions rather than empirical reality. Laitin develops this argument with an extended hypothetical example involving an undergraduate research project, an appropriate example given that Laitin's and Shapiro's disagreements here and in previous work have centered around standards for political science education.

In his reply, Shapiro responds to Laitin's call to equip students of political science with "a range of hypotheses reflective of the most current research and a set of formal skills." Shapiro notes that particular descriptions of political phenomena will inevitably affect the content of any standardized curriculum presented to students. Given basic disagreement among researchers about the appropriate description of relevant topics (is democracy about preference aggregation? political opposition? something else?), Shapiro prefers to allow the marketplace of ideas to function at the level of curriculum development, and not only there. As he writes, "part of my anti-reductionist message is that people should abandon the search for the one-method-fits-all holy grail of political science. The phenomena that interest political scientists vary too much for this to make sense."

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My criterion for intellectual success is deliberately broad: that creative intelligence be applied such that abundant insights result. That criterion is well satisfied by Ian Shapiro's *Flight from Reality in the Human Sciences*, a collection of his previously published essays on how to do political science. Given my history, it's inevitable I'll discuss the rational choice

question. Before doing so, I want to recognize the essays I won't discuss at length here. The essay on Richard Posner's praxis is an instructive destruction of popinjay jurisprudence. Many a student would learn how to avoid seductive error by working it through. The essay on gross concepts in political arguments is an innovative contribution. It diagnoses the professional prejudices and incentives that lead much of political theory into debates between polarized isms. Too much of this is the consequence of a flight from empirical controversy, says Shapiro. I recall a philosophy seminar on two different formulations of international justice. I objected that the two conceptions, if implemented, would not differ in practical action until after about fifty if not a hundred years of development. "We don't discuss facts here," I was told. Like Shapiro, I think it's better for political theory to be connected to empirical political science, although I wouldn't say that about any individual political theorist. We need theorists of all varieties: counter, original, spare, and strange.

I'm reluctant to participate in methodological struggle for several reasons. I spent a full day in graduate school going through *APSRs* from the 50s and 60s. The most interesting articles were by people who made intriguing observations about the world, despite the observational meat being served between moldy slices of behavioralism or functionalism. The least interesting articles were methodological disputes. As for today, I find it difficult to adjudicate among methods in the abstract. Run that method around the track a few times, and let's see what it's got. The best philosophy of social science might be done by example, or that's how I console myself for failing to have developed views on the philosophy of social science. Finally, I haven't practiced in the profession long enough to confidently evaluate its wide range of efforts. Having established my lack of qualifications, here are my thoughts.

Rational Choice Theory

I do think that *Pathologies of Rational Choice Theory* was a well-deserved polemic.¹ Attending the APSA convention as a graduate student, a friend tipped me that there might be fireworks at a big panel, an attack on rational choice that I hadn't heard of before. It was a great show, but I was sitting near two besuited gentlemen who leered, winced, grimaced, and whispered bitterly at every word uttered by Green and Shapiro. Hang on, I thought, this is science, not religion. Theories, hypotheses, facts are frequently wrong, and all must be constantly subject to relentless criticism. It's not religion, it's not even football, there's no place for cheering your team just because it's your team. Theories and methods should be like clothing which you change as appropriate to the occasion, not like skin which burns with pain as it's peeled away. As I've said elsewhere, I agreed with many of Green and Shapiro's evaluations—for several years the *APSR* was so irrelevant to politics that I rarely unbundled it from its plastic wrapping—but not so much with their conclusions about rational choice.

I am a methodological pluralist, but a critical one: not anything goes. I doubt, for example, whether Freud has anything to tell us about politics or about anything else. I am a rational choice theorist, but in the spirit of Schelling, Davidson, Elster,

not in the spirit of Tullock or Riker. I think the theory is misnamed, confusing both proponents and opponents. It would be better named *purposive actor* theory, in my view. Anyway, I first learned rational choice theory from John Orbell and John Dryzek. Orbell is a social-dilemma experimentalist and from him I learned that the game-theoretic prediction fails if you assume that subjects are motivated only by the material rewards offered in the experiment. Since the model is logically valid, the failure implies that many people are motivated by more than dollar payoffs (and many more interesting findings came out of his research program). From Dryzek I learned the hypothesis that it is not rational choice but the assumption of self-interest which leads to the falsified predictions known as the paradox of voting and the paradox of participation; and I was introduced to the communicative reason of Habermas. From Jon Elster and Diego Gambetta I learned that rational choice can be applied with startling subtlety and insight.

Rational choice, according to Gary Cox, is "any argument that takes actors and their goals as primitives, then proceeds to explain how the given actors' pursuits of their posited goals leads to systematic tendencies in observed behavior."² What was exciting to me about rational choice is that it displaced all the failed behind-the-back theories of social causation and put human agency in the center of our considerations. Humans are not the puppets of subconscious demons nor of systemic function nor of age, income, and education variables. For me the doctrine is not the bonehead assertion of egoism as exclusive motivator, nor the careerist generation of mildly modified mathematical models of zero substantive interest. I deplore the latter developments as much as does the passionate perestroikan.

It is not a theory of everything. Patterns in constraints are exogenous to the theory. The explanatory force of W.H. McNeill's accounts of disease and history—for example, that an imperial metropolis unintentionally fragments and subjugates its rural frontier by the inadvertent export of urban disease—has to do with patterns of constraints rather than with the desires and beliefs of the actors.³ Or consider Jared Diamond's theory of emergence of civilization varying according to the availability of domesticable species and ease of east-west diffusion.⁴ Or the effect of military technology on the conduct of war. Or whether an individual is a bonder or a bridger in a network of social relations.

Rational choice makes a rebuttable presumption that beliefs are consistent with one another and similar because they connect to the world and that desires are consistent with one another and similar because of our similarity as humans. It could be that humans are not mostly purposive, it could be that the folk psychology of belief and desire is somehow illusory, and it could be that there are no tendencies to consistency and similarity. These may be necessary conditions of human agency, but if one denies human agency, or denies the explanatory power of human agency in human affairs, as do some exotic continental thinkers, I suppose they could be done without. The Davidsonian idea is that they must be true much of the time in order for us to make sense of the exceptions.⁵ There are plenty of exceptions: beliefs contradict one another, as do desires; people hold to false beliefs and to heterono-

mous desires; and people make mistakes. Other than the consistency and similarity presumptions, the patterns of beliefs and desires in a population are exogenous to rational choice, and often require independent explanations, as do errors in beliefs and in desires.

Thus, I am not a universalist in Green and Shapiro's terminology: I accept both that rational choice applies in some circumstances and not in others and that it sometimes explains in conjunction with other independent variables. Following Cox, one might say that nevertheless rational choice is more universal than the flotsam and jetsam of concepts left over from the shipwrecks of behaviorism, behavioralism, functionalism, Marxism, Freudianism, and other grand vessels. Could the present methods of rational choice be superseded 20 years from now? Sure, maybe cognitive psychology, neuroscience, and agent-based modelling will be fruitful, or probably methods we have yet to imagine.

Rational choice as I understand it is an interpretive strategy, and game theory especially offers good explanations of interdependent actions, much of the stuff of politics. Shapiro's Chapter 1, about realism and acquiescence to power, puts rational choice into the mistaken logicist camp. Shapiro and Wendt characterize rational choice as Friedman's instrumentalism sundered from the constraint of accurate prediction. That may be an apt description of a line of work in political science, one that occupied too much of the center of our profession for too long a time. If true, however, it describes not rational choice but an erroneous version of rational choice. Rational choice theory is not necessarily instrumentalist, and it is not necessarily opposed to accurate description and prediction! According to Shapiro and Wendt, for logicists (including the anti-evidential and instrumentalist rational choice theorist), acquiescence to power is epiphenomenal, a logical artifact of assumptions themselves not subject to evidentiary considerations. But I say there could easily be a pro-evidential and realist rational choice theory as well, which relies on evidence to infer the (real) desires and beliefs of the relevant actors over actual and counterfactual states of the world.

Shapiro and Wendt justly fault the approaches of behavioralism and noncausal interpretivism to the topic of acquiescence to power. Each is misled by the phenomenal, and thus each fails to detect the cold shadow of power. Gaventa, understood by Shapiro and Wendt as the proper realist alternative to logicism, behavioralism, and interpretivism, resolves the puzzle of quiescence.⁶ But he does so by calling our attention to counterfactuals, as does good game theory. The question is: why do miners acquiesce to their subordination? Such acquiescence is a puzzle in the first place because people don't like being subordinated, so stipulate that. Rational choice suggests that they acquiesce to subordination *in order to avoid a worse alternative*. With that interpretive understanding, one then looks for evidence of what that counterfactual alternative is, what people believe would happen if they defied power. We can ask them what they believe, we can investigate historically what happened to their relatives and friends who did challenge power, we can observe what happens when the believed costs of defiance go low, and from all that we can infer that

they would defy power if they believed that they could get away with it. If, in addition to acquiescing due to a belief about what would happen if one did not, acquiescence is due to the shaping of false beliefs and heteronomous desires in the subordinated by the subordinators, then additional psychological and sociological variables are required for explanation. That is because rational choice proper has no wide theory of preference formation and change.

Problem-Driven or Method-Driven?

A major theme in *The Flight from Reality* is the folly of method-driven political science and the wisdom of problem-driven political science. I shall dispute this contrast. The faults that Shapiro identifies in method-driven work, I say, are the faults of intellectual self-deception, or, most charitably, error. Problem-driven work is subject just as much to intellectual self-deception, I shall suggest. I do not mean to lecture from a high horse, I consider myself just as likely to err in this regard as others, until constrained by the criticism of peers.

In fact, my most successful application was method-driven. I did just what Shapiro said we shouldn't do. I was fascinated by Schelling's account of conventions as coordination games. I had a hammer in search of a nail. Hume, who had some thoughts about justice as a convention also suggested that inheritance practices are conventional in the same sense. I read everything I could about convention and about variation in inheritance practices. My paper concluded that very little about inheritance practices was conventional in Schelling's sense. The striking regularities in practice, said the best literature on the topic, were mostly due to the local political economy: primarily the local agricultural pattern, secondarily legal requirements. Inheritance is not conventional, because choice of practice is independent or quite weakly interdependent between families. I noted that in contrast marriage choices are strongly interdependent between families, and that in future work one might look for Schelling conventions in that arena. I do not think Shapiro could fault my method-driven exercise, which yielded a useful if tiny conclusion, that inheritance practices are at best only weakly conventional. Where I would have gone wrong, and where he could object, is if in a self-deceptive fashion I tortured the facts about inheritance to make them fit my model. Notice how an honest method-driven investigation sharpens scope conditions of the method, and sends inquiry in the proper direction. Suppose a scholar trains up on social network analysis. Would it be wrong for her to look for problems where the structure of relations is suspected as a key causal factor? Would she add to knowledge if she discovered that, contrary to expectation, the structure of relations had little effect in some area, and explained why?

A year later David Laitin suggested as a paper topic that someone could do a game-theoretic investigation of the painful, dangerous, and puzzling practice of female genital cutting (FGC).⁷ Initial research showed that the practice is associated with marriageability, that it is nearly universal in intramarrying groups, that its features strongly parallel the features of footbinding in China, and that footbinding had ended suddenly rather than gradually in intramarrying groups. Eureka! The

preliminary indicators said that FGC is probably a Schelling convention. Then followed an interplay of theorization and factual investigation that settled into a reflective equilibrium. Wide rational choice theory, supposedly vacuous, provided the key. Why would a family impose one of these painful and dangerous treatments on their daughter? The answer must be: in order to avoid a worse alternative. What is the worse alternative? Unmarriageability of the girl. How could one help end the practice? Only by solving the marriageability problem. How did the Chinese end footbinding? By recruiting the members of an intramarrying group into a declaration of coordinated abandonment, such that daughters retained their marriageability. Would the same method work in Africa? I predicted that it would.⁸ That prediction was fulfilled by unique mass abandonments in Senegal guided by the theory, through 700,000 people in 1500 villages, and planning is afoot to implement Africa-wide. The exercise mostly satisfies Shapiro and Wendt's criteria: it retrodicts empirical regularities, conforms with most of existing understanding but adds considerably more, and allows us to intervene successfully in the relevant environment. Abandonment interventions based on other theories of the FGC practice have uniformly failed.

The account in full is probably what Green and Shapiro would call a partial rational choice theory, but by far its largest part is, to quote Cox again, "an argument that takes actors and their goals as primitives, then proceeds to explain how the given actors' pursuit of their posited goals leads to systematic tendencies in observed behavior." The agency-based approach is also a happy way to promote abandonment of the practice. The account seeks to understand the people involved from their own point of view. In practice, the intervention does not tell people what to do, rather, it discloses that there is an effective way out (and most people decide to go that way). Alternative theories, apparently more humanistic than supposedly cold mechanical game theory, mistakenly understand the people involved as dopes buffeted about by mysterious id or almighty culture or systemic powers, and speak not of abandonment but *eradication* of the practice (as if it were a mosquito). Eradication consistently fails and only abandonment has succeeded.

At the panel, Ian Shapiro responded that my exercise fails the grandmother test (i.e., it is something so obvious that even your grandmother would understand it); this has been his refuge whenever a rational choice application is successful. I suspect he is not well-informed about my work in this area, however. And, I suggest, Shapiro's grandmother test is ill-defined, more rhetoric than argument. Of course, any observed action is to avoid the worse alternative, he replies, but that's trivial. Well, the causal power of the counterfactual was not obvious to Dahl in *Who Governs?*⁹ Dahl failed to notice that people acquiesce to the implicit threat of power because he was blinded by behaviorist methodology. It took years of controversy, conceptual analysis by Lukes and others, and empirical investigation by Gaventa and others to sort it out. The counterfactual shadow of power on acquiescence is obvious in retrospect, after being called to our attention by creative observers. Further, it's obvious today (or is it?) that a light object and a heavy object will fall at the same speed, but

it wasn't obvious before Galileo. Most of mathematics is tautologous, but is nevertheless enlightening because humans lack logical omniscience. Competing hypotheses about FGC and footbinding are almost all behind-the-back theories, and none of them counsel us to consider first the beliefs and desires of the people involved, nor to look for the worse alternative avoided. The purposive-actor approach to FGC and footbinding may be "obvious" in its *presumptions*, but is not obvious in its *results*. It was a revelation in theory, and led to a revolution in practice. Maybe its results will become obvious in another generation, but they were not at all obvious to me as I worked them out, and weren't obvious to anyone before that.

In developing the convention account I did inference to the best explanation. I charted a matrix with theories on the vertical axis and observations on the horizontal axis, marking an X where theory accounted for an observation. Some of the theories were the convention account, the patriarchal sado-ritual syndrome, the mindful body account, functionalism, modernization, ostentation, and the ethnic marker account. Some of the observations were universality, persistence, link to marriageability, practice by those opposed, and the sudden end of footbinding. The relatively simple convention hypothesis accounted for more of the observations than did alternative theories.

The *problem-driven*, inductivist sado-ritual-syndrome theory sought to explain FGC, footbinding, and sati, and taking the three practices together accounted for more observations than did the convention account, even though it neglected to explain why a universal patriarchy caused such extreme practices in three different locations but did not elsewhere. It encompassed many observations because whenever it met an anomalous observation the ramshackle inductivist theory just added another proviso! If footbinding is caused by universal patriarchy, why did footbinding end suddenly when patriarchy did not? Because malicious males also practice "patriarchal reversal," their sadistic pleasure includes both forcing a disfigurement on women and their further humiliation in abrupt reversal of the practice. How about another problem-driven, inductivist theory? Germaine Greer, neglecting to consider the quite different desires and beliefs of the human agents involved, and the different constraints they encounter, lumps together FGC, footbinding, plastic surgery, obsessive self-mutilation, tattooing and piercing, surgery on infants born to ambiguous gender, transgender surgery among adults, adult cosmetic surgery on the genitals, and unnecessary obstetric and gynecological surgeries. The problem to be investigated is why women get cut, and the theory offered is that women secretly desire to be mutilated. If these examples are representative, then problem-driven theory obscures rather than enlightens.

I presented the rudiments of the convention account at one of our best universities, and a senior figure was visibly appalled that I would use game theory to help explain these practices. Instead, this person said, we should turn to something like Freud. There are already Freudian "explanations": Freud hypothesized that FGC assists the transition from clito-

ral orgasm to vaginal orgasm, and that footbinding allays male castration anxiety. The Freudian hypotheses were unique in my abductive inference exercise for explaining *none* of the observed features of the practices. But, hey, they're not rational choice! The problem here is not with problem-driven investigation, not with method-driven investigation, the problem is intellectual self-deception or error. My method-driven investigation could have erred, but did not. The problem-driven investigations could have avoided error, but did not.

Conclusion

Moreover, I want to suggest, theory, method and problem are inseparable (I must add that this implies no relativism about the facts). One's investigations might be more theory-driven at one point, more method-driven at the next, and more problem-driven later. Consider Alan Turing. This mathematician turned to philosophy after his best friend died. He wanted to understand how the mind was embodied in matter, and this led him to quantum mechanics, and then to mathematical logic. He encountered the logical problem of undecidability and provided a solution (which founded computer science). From his point of view he was problem-driven. From the point of view of any nonlogician he was method-driven. (The Turing case is instructive for those national science foundations which require that research be relevant to national goals of peace and prosperity. "Dear Mr. Turing: We are grateful to have received your ambitious proposal to pursue a solution to Hilbert's *Entscheidungsproblem*. I am afraid that the committee has determined that such logical speculations are of no conceivable use in war or commerce. Naturally, we wish you the best of luck with your career.") Turing turned his method-driven discoveries to cracking of the German cipher in World War II, and to the development of the computer thereafter, until his cruel early death following official persecution for being an open homosexual.

I worry that the call for problem-driven work could degenerate into a demand for relevance. It's probably best for many political scientists, and especially for beginning ones, to pursue a concrete problem. But it would not be best to eliminate those who choose to march down unbeaten and apparently irrelevant paths. Turing is one example. Hubert Dreyfus might be another. Why should a (philosophy) department keep around a cantankerous Heideggerian? Dreyfus's resolute phenomenology called the great emperor, the supposed science of artificial intelligence, naked, in his *What Computers Can't Do*.¹⁰ Dreyfus accurately predicted that artificial intelligence implemented as the manipulation of internal symbols by internal rules would fail, and stimulated intellectual revolution in cognitive science. Dreyfus is controversial and I'm sure he's wrong about many things. However, his "obvious" but discerning observations about the acquisition and exercise of human skill led to the identification of a degenerating research program, no mean feat.

Speaking of naked emperors, a third approach begins to emerge in Shapiro's Chapter 5 on what to do about what's wrong in political science, one of *critical reappraisal* (one I followed in the debunking of my *Democracy Defended*).¹¹ He

says that political theorists should serve as roving ombudspersons for truth and right, holding the discipline's feet to the fire (I hope that members of other subfields take part, and that some political theorists are allowed to pursue their peculiar fancies). Science is not religion, it's not football, it is criticism. We always need challenging factual and ethical redescrptions of the world. The best theories inevitably neglect some facts and elide some injustices, not just neglect them but hide them away.

Not only critical reappraisal, but, I would add, *discerning observation* is a worthy goal of political science—so many observations are obvious in retrospect but go completely unnoticed until disclosed by the creative observer, a Dahl, a Fenno, a Gaventa, or a Goffman. Physics is more unified in theory and method than political science. Physicist Richard Feynman was asked the secret of his success, and he answered that (even in physics) those beautiful models leave things out.¹² He always hunted for observations unaccounted for, ignored, repressed in the standard version. He found them—there, under everybody's noses—and then he explained them. My review of ancient *APSRs* taught me that discerning observations outlive the theories and methods which midwived them.

Perhaps critical reappraisal and discerning observation better describe the right direction for political science than the choice of problem-driven over method-driven investigation. Successful applications in political science probably result more from intuition, talent, and luck, however, than from marching behind any methodological banner.

Notes

¹ Donald Green and Ian Shapiro. 1994. *Pathologies of Rational Choice Theory* (New Haven: Yale University Press).

² Gary Cox. 1999. "The Empirical Content of Rational Choice Theory: A Reply to Green and Shapiro." *Journal of Theoretical Politics* 11:147-169.

³ William H. McNeill. 1998. *Plagues and Peoples* (New York: Anchor).

⁴ Jared Diamond. 1999. *Guns, Germs, and Steel* (New York: W.W. Norton).

⁵ Donald Davidson. 1980. *Essays on Actions and Events* (Oxford: Clarendon Press).

⁶ John Gaventa. 1980. *Power and Powerlessness in an Appalachian Valley* (Chicago: University of Chicago Press).

⁷ I do not align myself with Laitin in his quarrel with Shapiro, nor do I agree with his more dogmatic views about how best to conduct political science.

⁸ Gerry Mackie. 1996. "Ending Footbinding and Infibulation: A Convention Account." *American Sociological Review* 61:999-1017.

⁹ Robert Dahl. 1961. *Who Governs?* (New Haven: Yale University Press).

¹⁰ Hubert Dreyfus. 1972. *What Computers Can't Do* (Cambridge: MIT Press).

¹¹ Gerry Mackie. 2003. *Democracy Defended* (Cambridge: Cambridge University Press).

¹² Richard Feynman. 1997. *Surely You're Joking, Mr. Feynman!* (New York: W.W. Norton).

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I have two sets of observations on this fine and valuable book, one of which grows from my experience in editing *Perspectives on Politics* for about four years, and one from my own research experiences.

Perspectives provides a useful starting point for this consideration, because the journal exists largely to put into practice Ian's recommendations in *The Flight from Reality in the Human Sciences*. That is, *Perspectives* is intended to publish mainly problem-driven research appealing to a wide array of political scientists, other social scientists, and political actors. Articles in the journal are not intended to be self-referential within political science, and their methods, language, and theory must be accessible to all readers.

Individuals responded to this new venture in distinctive ways, some of which can be categorized in terms of the traditional five fields of political science.¹ Like all generalizations, this one has important exceptions, but I am seeking here to distill a few themes that bear on the subject of this symposium. For example, submissions on security studies within international relations are almost always structured around several dominant paradigms.² As a colleague once put it, the standard IR article consists of pushing a huge rock of theory up a steep hill, in order to roll it down to smash a few pebbles of fact at the bottom. That is, the modal manuscript starts by outlining the three standard theories: realism, liberalism, constructivism (sometimes subdivided into neo-realism, neo-liberalism, and so on). Articles then diverge slightly—some seek to show that these apparently different theories can really be combined to show X; others seek to show that one of the theories is right and the other two are wrong, as evidenced by an explanation of X; a few argue that none of the three quite suffices to explain X, so we need a new theory (or more frequently, a variant of one of the old ones). Our advice, as editors of *Perspectives*,³ usually ran along what became a well-worn track: start with your own central point, reduce or eliminate the literature review, tell us why we should care about your concern, and then engage with it. This seems to come very close to what Ian refers to as problem-driven research.

The modal political theory manuscript has a very different feel.⁴ Most focus on one or several persons rather than one or several paradigms, and they frequently come with a date: "Machiavelli's theory of X, compared with Locke's theory of X, 1527 to 1605." (Theorists will note the implausibility of this title.) An astonishing number of paragraphs start with a contemporary political theorist's last name and then proceed through a brief exposition of what that person says about Machiavelli or Locke, or what that person says about another commentator's view of Machiavelli or Locke: "Smith argues that Machiavelli believes XX. Smith disagrees with Jones who argues that Machiavelli believes YY." And so on, through many Smith's and Jones's. Our editorial advice for revision? First, we had something close to a rule that *Perspectives* would not

publish any theory article with a name and a date in the title—it is a complete turn-off for all nontheorists. Second, rewrite the manuscript so that you start every paragraph's topic sentence with an idea—preferably your own—not a proper noun. Try writing the whole paper with the assumption that your readers care about the arguments but are indifferent to which contemporary scholar is making them; see how far you can get by focusing on the arguments alone. I believe that this advice also comes close to what Ian would urge for "problem-driven" research, appropriately modified for the subfield of political philosophy. That was our intent, in any case.

Papers in the sprawling and disparate field of comparative politics (a.k.a. the rest of the world, for us Americanists) varied greatly. I have space here to describe only one modal type: a case study with a huge capital-T "Theory" frame. When I served on the editorial board of a distinguished university press, we received a manuscript with a title something like "Birth, Life, and Death: Agricultural Communities in Bulgaria, 1757–1782". I thought of that book often while reading manuscripts submitted to *Perspectives*, especially from comparativists. They often presented an intriguing and provocative case or small set of cases, focused on shorter or longer periods of time; our editorial job was to decide why anyone other than fellow specialists would care about that case. That is, we sought evidence that this case really is the grain of sand that illuminates some large and important part of the political universe, or is such an intrinsically important phenomenon that it warrants careful study in its own right. Conversely, is it mainly a case of something or other which the author has not made clear, or is it simply an interesting narrative? Our advice to comparativists, therefore, was often to either build up or cut down the theoretical structure as needed, to really use it in the argument so that it was not merely a portable frame separable from the portrait inside, and to show how the portrait actually changed the frame (at this point the metaphor breaks down). I see nothing in *The Flight from Reality* that would disagree with this advice, but not much that speaks to it either—so I remain concerned that Ian's argument will simply remain beside the point to an important fraction of the discipline of political science.

Perhaps ironically, the modal "methods" paper submitted to *Perspectives* while I was editor resembled most closely what Ian actually does in *The Flight from Reality*. That is, methods submissions frequently focused on what political scientists do rather than on a substantive political issue, and they disproportionately attended to how others are doing political science wrong rather than how to do it right. Also like *The Flight from Reality*, many methods papers sent to *Perspectives* showed great sophistication in using philosophy of science to show that real scientists (mathematicians, statisticians, physicists, biologists...) are much more adept than we are in using scientific concepts or facts. Our main line of advice to authors of these papers was to do more of what Ian calls for—connect methodological and epistemological claims to genuine issues within the political arena to show why the former matter in understanding or resolving the latter. In other words, so what?

Papers in the field of American politics submitted to *Per-*

spectives are the hardest for me to characterize, perhaps because it is the field closest to my own work. Probably their central feature was “business as usual”—reanalyses of surveys in the National Election Studies, institutional explanations for Congressional procedures, or demonstrations that the media shape the political agenda. In this field as in the others, some of the papers were of extremely high quality, and some were a pleasure to read (these were not always the same papers), but I think it’s fair to say that we saw less effort to write for a different purpose and audience in the field of American politics than in other fields of political science.

This too-quick summary of a complicated editorial experience yields two conclusions relevant to this symposium. First, *Perspectives on Politics* received several hundred submissions a year, and it was clear that most authors understood and sympathized with its distinctive mission; many papers were painstakingly and powerfully rewritten. Thus I conclude that more political scientists share Ian’s goal of engaging in problem-driven research about genuine political issues than is suggested by *The Flight from Reality*. Second, practitioners of political science move away from problem-driven research in different ways, often though not always identifiable by subfield. Thus Ian’s strictures and rallying cry seem well suited to some styles of doing political science, but irrelevant to or disconnected from others. It would be very valuable if he or someone else sought to make those more fine-grained distinctions and connections.

I turn now to my second set of comments on *The Flight from Reality*, which grow more out of my own research experiences than my role as a (former) editor. Ian analyzes three types of political science research—theory-driven, method-driven, and problem-driven. I propose to add a fourth: *perspective-driven*, or *vantage-point-driven*, research. Some of this work has clear affinities with what Ian describes as theory- and problem-driven research. After all, theory can be derived or elaborated from one’s vantage point, and then developed in a more systematic and subtle fashion. Problems can be shaped and articulated by starting from a particular vantage point. Furthermore, what *The Flight from Reality* depicts as re-description of old understandings of the world bears a close resemblance to a perspective-driven model. But here I want to press on the differences between perspective-driven research and Ian’s three other models. That is most readily done through examples:

* “Rape is the primary heterosexual model for sexual relating,” Andrea Dworkin once wrote.⁵ From this startling vantage point came research and arguments that contributed to developing the concept of marital rape, laws on sexual harassment, and claims that pornography is a violation of women’s civil rights.

* “Liberal tolerance is the religion of secular humanism.” This is a vantage point that I used to find tendentious in the extreme, but which I now see as empirically correct (that is not to say that I reject liberal tolerance, far from it). From the perspective of a deeply religious Catholic, for example, there can be no neutral stance with regard to

abortion—just as most readers of this essay would agree that there can be no neutral stance with regard to human slavery. Perhaps a woman’s “right to choose” is analogous to a “slave-owner’s right to choose”; after all, in the former case people assert that the fetus is not a legal or moral person, and in the latter case people assert that an African or black American is not a legal or moral person. I, for one, would never have been made uneasy by this analogy if the perspective-driven claim that “liberal tolerance is the religion of secular humanism” had not intruded on my consciousness.

* White Westerners participate in a racial contract that structures society so that blacks (or non-whites) can never attain full legal, political, moral, cultural, or economic equality. This is the claim of Charles Mills, whose most intriguing and infuriating element is that of the “epistemological contract.”⁶ In this view, whites agree to pretend that there is no racial hierarchy, even though it permeates everything everyone does and all structures of western society. If one honestly denies the existence of such a racial contract, that is evidence of how powerfully it has permeated one’s consciousness. The point holds a fortiori for blacks, or people of color more generally; if they produce arguments or evidence that there is no racial contract, that is the most powerful demonstration of its existence. Thus from Mills’ vantage point, both denial and acceptance of the racial contract equally support his assertion of its existence.⁷

* Palestinian suicide bombers are freedom fighters, or terrorists; Israeli soldiers are freedom fighters, or the new generation of Nazis. There is a whole nest of perspective-based analyses in the Middle East, with obvious consequences for research and politics alike.

These examples could be extended, but my point should be clear. The move toward problem-driven research which Ian endorses has a tendency to slide into a perspective-driven definition of the problem to be studied, or judgment about appropriate evidence for analyzing a problem, or interpretation of that evidence. At the extreme, only when the vantage point has been “proven” will someone in this mode of thinking consider the research to have been persuasive and complete.

Sometimes perspective-driven research will be tendentious and circular; that is easiest to see with regard to those perspectives with which we disagree. But sometimes perspective-driven research can be enormously illuminating, liberating a viewpoint that has previously been suppressed (perhaps inadvertently), and opening new channels for analysis and evaluation. The hardest cases occur when a perspective-driven assertion is both tendentious and illuminating. Such scholarship is not of high quality in the sense that Ian is calling for—for example, it usually lacks fair consideration of alternative hypotheses, and it is naïve with regard to basic tenets of the philosophy of social science. But for better or worse, it might change the world—just consider Karl Marx, Sigmund Freud, Edward Said, Harriet Taylor, and W.E.B. DuBois. I would urge

Ian in the next edition of *The Flight from Reality* to add a fourth type of political science to his typology, and to address it with all the acuity we expect from him.

Notes

¹ I will not describe the field of public law here; there were too few submissions for there to be a clear central tendency beyond the proclivity to cite and discuss Supreme Court cases.

² Articles in the other main segment of IR—international political economy—have their own distinctive style, but in this brief essay I am ignoring them.

³ Unlike most academic journals, I and the five associate editors worked closely with a subset of people who submitted manuscripts in order to develop them into a format appropriate for this journal. Our model was more that of an activist editor at a top-notch university or commercial press than that of a conventional journal. All articles in *Perspectives*, however, went through the standard process of double-blind peer review.

⁴ Here I am ignoring interpretivist and post-modern political philosophy. Articles in this genre tend to get very abstract and tangled up in complicated sentences. Our main advice to writers in this arena was to write with less idiosyncrasy and more clarity.

⁵ Andrea Dworkin, *Our Blood: Prophecies and Discourses on Sexual Politics* (London: The Women's Press Ltd, 1982).

⁶ Charles Mills, *The Racial Contract* (Ithaca NY: Cornell University Press, 1997).

⁷ In teaching this book many times, I have found that some students see this claim as strikingly illuminating, enabling them to make sense of a world that heretofore seemed inexplicably wrong. Other students find the claim to be preposterous. Roughly the same split obtains among students for the assertion that “rape is the model for heterosexual relating,” although the divisions among the students usually fall along different dimensions for the two books.

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In *The Flight from Reality in the Human Sciences*, a nicely contained set of previously published analyses and polemics, Ian Shapiro (and several co-authors) defend an approach to the human sciences that is “problem-” rather than “methods-based”.¹ The essays stand against many in the humanities who deny the possibility of systematic knowledge of causal mechanisms in or any serious engagement with the social world. And they stand against many in the social sciences whose formalistic models are not put to serious empirical test, and therefore present solutions to methodological rather than real-world problems. I endorse his two principal points, viz., that it is important to link our research to questions about politics whose answers would be consequential for the quality of our lives, and that we should be more concerned about the truth value of our findings than their theoretical elegance. Moreover, I am much taken by his ideal for the political theorist as a “roving ombudsman” (p. 179) whose “problematizing re-descriptions” (p. 201) of political reality serve to unsettle us from anodyne appraisals of our political world.

Yet in the course of making his arguments in these essays, Shapiro steps on several land mines, making his project at

times self-destructive. This review seeks then to clear the field, as it were, making Shapiro's arguments less dangerous to those of us fighting in his trenches who do not want to get blown up with him.

In its expressions of disdain for methods-based research, this book is mired in self-contradiction. Its title castigates the human sciences for their focus on method that entails a flight from reality. While “reality” is never defined in these essays, I surmise that Shapiro is referring to events and outcomes that are consequential for our safety, our values, or our opportunities. Yet the text is virtually empty of any supported claims about that reality.² In Chapter 1, for example, addressing the issue of consent, we are treated to a war of methods. We meet up with logical empiricists (of two types, logicists and empiricists), interpretivists, conventionalists, Marxists, post-Marxists, Gramsci-ites, minimalists, utilitarians, philosophical idealists, realists (of two types, commonsensical and scientific), and Foucauldians, but hardly a person who could give or withhold consent. To be sure, we get John Gaventa's theory about coal miners but the contact with their world is at best indirect, as Shapiro and his co-author Alexander Wendt take on the role of philosophers, who we are told (p. 50) need not be worried about saying true things about the world. Is this opening essay then not a flight from reality?

Chapter 2, on the debate over rational choice that Shapiro and his collaborator Donald Green instigated, is similarly mired in methodological pilpul. This argument in this chapter is solely about a methodological debate instigated by the authors. It first argues that rational choice theorists avoid reality. It then reminds us that Green and Shapiro, in the book that instigated the debate, showed this to be the case. It then summarizes the defense of rational choice critics in light of Green and Shapiro's charges. Finally, Green and Shapiro elaborate with indelicate wit on the paltriness of the defense. To the accumulated knowledge about the world (which on p. 88, they name as their criterion for social science success) this chapter adds nothing. Method rather than problems motivated this chapter. Their antidote to the flight from reality, we can surmise, is further flight!

In Chapter 4 as well, called “Gross Concepts in Political Argument,” readers get buffeted between utilitarians and deontologists, and after that between foundationalists and contextualists. And in the course of these debates that are internal to political theory, Shapiro promises as the goal of this essay to “outline a more complex account of the relationship between the structure of political grammar and people's beliefs about that structure” (p. 154). I wonder if knowing either the structure of political grammar or people's beliefs about that structure constitute problems that should serve as the focus our research? In getting mired in these subdisciplinary (within political theory) debates, Shapiro seems to be running alongside his colleagues fleeing from reality.

At one point (pp. 178-9) Shapiro expresses “discomfort” in that his prescriptions to remedy “what's wrong with political science...replicate...the disorder under discussion,” but he regains his bearings by taking the role of “roving ombudsman(a)n for the truth and the right.” In that essay (Chapter 5) he

makes a series of subtle distinctions in order to differentiate the bad guys (who do methods based research but hide this behind an apparently thin concern for problems) from the good guys (who do problem-based research even if they sometimes get all-too-enamored with their favorite methods).³ My guess is that there would be low inter-coder reliability if Shapiro asked research assistants to code social science contributions based on this distinction. But the more important point is that learning the results would teach us nothing about the political world. The very distinction Shapiro goes to such pains to uphold implicitly undermines the point about problem-based research that is the declared theme of the book.

Inter-coder reliability would be low in any event because Shapiro is remarkably obscure about what constitutes a problem, writing off—it seems—the accomplishments of a two-thousand-year-old dialogue on political questions that merit scrutiny. Indeed, one of the critical essays of Green and Shapiro's *Pathologies of Rational Choice* (by Ferejohn and Satz, 1995) raised a problem with their notion of a "problem." Ferejohn and Satz pointed out that before we can address problems, we need a theoretical characterization of what exactly needs to be explained. Green and Shapiro, in Chapter 2 of *The Flight from Reality*, are rather cavalier about dismissing this criticism. At one point, they offer basic methodological guidance that is relevant, they argue, "Regardless of how one got to the question to be addressed..." Any problem will do, it seems.⁴

More is at stake than contradictions and troubling distinctions. The bigger issue is in Shapiro's vision of the discipline that needs to be passed on to the next generation. On the one hand, his book exhibits ironic contempt for what most political scientists write, but nonetheless wants to protect the status quo in how our not-very-disciplined discipline is taught. On the other hand, the essays in it reveal a taste for undergraduate papers in inverse proportion to his distaste for those of his professional colleagues.⁵ It seems to be that the grievous failures of professional political scientists as compared to the achievements of Yale undergraduates should suggest to the discipline's critic—if we assume that the former have had full advantage of our haphazard curricula while latter have not—that the status quo curriculum yields intellectual value subtracted! Shapiro should therefore be open to changes in that curriculum. In the book's final chapter, criticizing my call for change, he instead defends that status quo.

Allow me to develop my response to his critique with a pedagogical just-so story. Suppose a student who is designing an honor's thesis asks Professor Shapiro, "Why is there so much evil in the world?" I doubt he would let a thousand problems bloom, and tell the student that now she has her problem she can go on without method. I think he would point out to the student that political science, unlike religious studies, has not much to say about evil, but we do have a long tradition on addressing the problem of injustice. He could push the student to refine the problem as a concern for injustice, perhaps pushing her to Plato in *The Republic*, or Pitkin in *Wittgenstein and Justice*, or Rawls in *The Theory of Justice*. The student might come back and say she did not mean injustice, but rather the killing of innocent others. The professorial

response might then push the student as to whether she was interested in homicide or war, each with a well-developed literature that gives theoretical characterizations of a related set of explananda. My point is consistent with Ferejohn and Satz—problems aren't out there like snowflakes in the Sierras, each with its own shape. We work in a discipline where problems are structured, for which we have theories and expectations. Contributing to the two-millennia-long tradition addressing these problems is part of what we mean by doing political science. Maybe because Shapiro knows the canon so well, he underestimates that by teaching it we are disciplining our students such that they are able to give theoretical characterizations of what needs to be explained.

Suppose further that the student wants to write about injustice. After solving, for purposes of her thesis, the conceptual problem of what in her theory constitutes acts of injustice, professorial advice would push her to reckon the magnitude and distribution of injustice, as a check on her intuition that it is pervasive.⁶ Suppose she found that in some polities her intuition was supported, but in other polities, injustice was rare. This would be the beginning of a search for new knowledge. She would be impelled to ask, "Why is there justice in Megalomania but not in Ruritania?" which are otherwise quite similar in culture and history? She would now be mired in reality, rather than fleeing from it.

But there is more! Suppose she found that injustice is more pervasive the smaller the polity. Would she not be impelled to ask "why?" A useful exercise would be to think why those in power in smaller communities exploit their position more than in large polities. A variety of hypotheses could present themselves, but one useful way to think through the problem would be to ask why individuals in different settings would want (or be able) to take such advantage of the weak. Suppose she conjectures that the smaller the polity, the greater the information elites had about the habits of the poor, the better able to dominate them through extortion. Formalizing this conjecture in a model would help her draw out logical implications, ones that if tested could add (or subtract) confidence in the conjecture. For instance, it follows from an information model that linguistic minorities could more easily hide information from dominant elites. Suppose she were to find that such minorities faced greater injustice than the poor from the same linguistic culture as the rich. This would compel a loss of confidence in the first theoretical conjecture. She would then be pushed to search for a different mechanism that drives the Megalomania/Ruritania difference. This would then send her on an exciting research spiral, going back to her conceptual model, reexamining the data, and reformulating the models until a satisfactory answer—one that could even explain levels of injustice in polities not part of the original dataset—is found.

I have just walked Professor Shapiro's honor's student through what I have called the "tripartite method" (Laitin 2002). It sees the political theory tradition as the arena in which problems are conceptualized and theoretical expectations drawn. (Students must be disciplined to enter into this tradition.) It sees statistics as a necessary tool to assess the magnitude and distribution of the problem that is to be addressed, and

one source for conjectures to explain the sources of the problem. It sees formalization as an invaluable tool allowing researchers to draw broader implications of their conjectures, to assure these researchers that the theory is internally consistent and consistent as well with the logical implications of the theoretical model. And it sees narrative as an essential component of any research program such that we can assure ourselves that our postulated theories account for real historical processes.

The curriculum that I advocate in “The Political Science Discipline” (Laitin 2004) and against which Professor Shapiro rebels (in Chapter 6), equips students to follow these essential research steps.⁷ Students disciplined by our canon get guidance on consequential questions, their normative significance, and the range of answers that have been proffered under quite differed contextual conditions. Students who have studied comparative politics will not only be attuned to the realities of cross-polity variation, but will have the skills to set up unbiased statistical and narrative tests of their hypotheses. And students who have studied institutional analysis will have a range of hypotheses reflective of the most current research and a set of formal skills that will enable them better to focus on mechanisms that translate values on hypothesized causal values to outcomes. Shapiro I fear keeps his head in the sand, denying that our methods-conscious colleagues have developed a range of skills that give weight to their hue and cry when they observe inferential errors by rivals and colleagues.⁸ In contrast to Shapiro, I take it as an obligation that we equip our students with those skills.

It is my mission—and here I believe that Shapiro stands on my side—to lead a charge back to reality, and following Marx in his “Theses on Feuerbach,” to help our students fashion a better reality. Our difference turns on the question of whether we (and our students) need to be disciplined in order to be effective interpreters and shapers of reality.

Notes

¹ An earlier version of this paper was presented at the American Political Science Association Annual Meeting, Washington D.C., September 2005.

² Shapiro’s recent work on injustice in South Africa and inequality in the United States are hardly flights from reality; my comments here are not on his life’s work but only on the essays in the volume under review. As assessment of his empirical work, to see if a more assiduous attention to methods would have added value, would be an interesting complement to this review.

³ He dismisses as methods-driven the work of Kathleen Bawn, who analyzes ideology as a device for a group to maintain solidarity, and of Russell Hardin, who analyzes constitutions as coordinating devices (pp. 105-06). Yet he does not consider these works as “problematizing redescrptions” (p. 201) and therefore worthy of praise.

⁴ In Chapter 5, Shapiro seeks to delineate a problem independently of theory: “theory-laden descriptions [of problems] fail on their own terms: there is no need to challenge the theory that leads to them to challenge them” (p. 185). I find the entire discussion of what constitutes a problem as impenetrable as the fragment just quoted.

⁵ Shapiro’s Yale colleagues are the exception here. I count twelve

colleagues from Yale whose work gets positive mention. One hypothesis explaining this great success rate for Yale scholars (as compared to the failures everywhere else) is that Shapiro, as a long-term and quite successful department chairman, has recycled promotion (and retention and reference) letters as well as retirement encomiums as his source for what is good about the discipline. As for the work of Gerber and Green (2001)—two of his colleagues at Yale—Shapiro holds back his full fire. Gerber and Green choose their problem (whether it is more efficient to phone or visit people if you seek their vote) based on its tractability with the experimental method that they are promoting. Probably drawing on a retention letter, Shapiro reports their answers to be “decisive” and only warns that their approach “risks” falling into “method-drivenness” (pp. 196-97).

⁶ Professor Shapiro pushed himself to do this in his original publication of this essay, to assess the magnitude and distribution of Justice Posner’s inconsistencies across domains of the law in the application of his wealth maximizing rule. This was a powerful confrontation with reality, but Professor Shapiro excised it from this reprinting, confessing on pp. ix and x that it was not worth his time to update the dataset. So much for his supposed commitment to reality.

⁷ Shapiro’s apparently willful misreading of my essay and the course it recommends is egregious. I give in the course a primacy to political theory in addressing consequential real-world concerns that have motivated participants in the tradition for two millennia. Only after these concerns are defended and the various arguments about them brought to light do I reduce these concerns to variables for purposes of comparative analysis. Shapiro criticizes me for a variable centeredness that ignores the very problems that draw students into our discipline. He not once mentions that I integrate political theory into the discipline in a way not very different from the way he advocates when he writes that “My account... is incompatible with a view of political theory either as the queen of the disciplines or as an isolated subdiscipline within political science” (p. 173).

⁸ The replication norm that Gary King (1995) has instilled in the discipline has led to more transparent statistical models in our papers and almost instantaneous error correction in our journals.

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A Response to Mackie, Hochschild, and Laitin

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I will not comment on Jennifer Hochschild's illuminating discussion of the different ways in which various kinds of submissions to *Perspectives on Politics* fail to be problem-driven, and on the wise editorial feedback she gave to the authors. Her editorship of the first three volumes has set a high bar for those who will follow. She has done a major a service not only for the political science profession, but also for those outside it who want to learn what political science can add to their understanding of politics.

Turning to Hochschild's second set of remarks about perspective-driven research, I agree with the substance of her point. However, Occam's razor counsels against creating a new category here. What Hochschild has in mind goes not so much to the generality or otherwise of the claim as to its arresting character: describing something in a new way that pulls you up short and makes you think about the phenomenon differently. This I deal with in *Flight* under the rubric of "problematizing redescription." Examples I give are recharacterizing the Westphalian system of sovereign national states as *global Apartheid*, or recharacterizing bipartisan agreement as *collusion in restraint of democracy*.¹

Such alternative descriptions invoke a theory that is not usually associated with the phenomenon in question. I agree with Hochschild that this kind of redescription can be illuminating even when it overreaches. This is because it highlights an unexamined assumption in the prevailing way of seeing things. Most people take it for granted that respecting national sovereignty, within some limits, is morally benign and that bipartisan agreement is a good thing in democratic politics. The redescriptions at least make us wonder. But to go beyond being arrestingly thought-provoking, there has to be more: a two step process, I argue, by which one shows first that an important feature of what needs to be accounted for is missed by the prevailing characterization, and a demonstration that the proposed recharacterization speaks to the inadequacies of the prior account. One mark of success will be persuading skeptics and adherents of the old view, as distinct from those who were already partisans of the proposed new view. This, I argue in *Flight*, is where so much that travels under the rational choice banner does so poorly.² An example of success—though not a rational choice one—that I discuss is the reexamination of industrial policy in capitalist democracies through the lens of liberal corporatism rather than pluralism. This enabled researchers to understand a great deal more of how industrial policy comes to take the form it does, and the different roles played by business, organized labor, and government in that process, than the competing Marxist and pluralist visions that had prevailed hitherto.³

Gerry Mackie's comment leaves me unpersuaded of the advantages of method-driven research as he describes himself as having engaged in it. Granted, scouring the social world for Schelling-conventions would be dishonest if one pretended to find them when they were absent. Mackie is to be commended for not doing this. Nonetheless, it is far from obvious that Schelling-conventions are the best place to start when trying to understand a particular phenomenon such as inheritance practices. It would depend on what one wanted to know about them, but I would start with the existing theories of how they came to be established in a particular setting and then ask what—if anything—those accounts failed to explain. If that was something I wanted to know, then I would cast about for alternatives and see how well they did in accounting for the phenomenon in question.

As for Mackie's discussion of foot-binding and female genital cutting, I fail to see why one has to crank up a bunch of game theory to get to the hypothesis that, if someone does something that seems harmful to themselves or someone we have reasons for thinking that they care about, then there may be some greater harm they are trying to avoid. Nor does one need it to suppose that, if the greater harm were removed, then the people in question would no longer engage in the lesser harm. Indeed, one need not even know what a coordination game is to consider this a plausible possibility. Mackie's suggestion that Dahl failed to consider possibilities of this sort in *Who Governs?* "because he was blinded by behavioralist methodology" scarcely meets the objection. Nothing in *Who Governs?* implies that people do not accept lesser harms to avoid greater ones, and nothing in Mackie's research involves looking at anything other than behavior.

Mackie's comment seems to me to be unnecessarily self-flagellating in that his major book, *Democracy Defended*, is manifestly a problem-driven enterprise.⁴ In it he takes on the literature that was motivated by the theoretical possibility of voting cycles first observed by Condorcet and formalized by Kenneth Arrow, and he asks whether such cycles actually occur in practice. His exhaustive investigation of virtually every alleged instance of a cycle in the literature of the past several decades reveals them to have been misidentified, supporting his conclusion that the supposed threat to democracy is chimerical. Is there a real problem here?, Mackie asked, and answered: no. We are all in his debt for having debunked this spurious though influential literature.

I have considerable admiration for David Laitin's empirical work in political science, but his comment on *Flight* is disappointingly obtuse. A good title for it would be "Shooting the Messenger with Non-Sequiturs," inasmuch as he tries to direct a series of barbs at me that for the most part fail to engage with the argument of the book. When he does engage, he gets it badly wrong.

The first non-sequitur is Laitin's lament that although I complain of a flight from reality in the human sciences, "the text is virtually empty of any supported claims about that reality." *Flight* is a collection of methodological essays, not a summary of my substantive work. As he acknowledges in a garbled footnote, my substantive work is published elsewhere.⁵

Moreover, only by engaging in the most tortured imaginable reading of Chapter 1 could anyone take Wendt and me to be saying there that philosophers “need not be worried about saying true things about the world.” We defend philosophical realism as a desirable basis for social research on the grounds that it does not bias the enterprise in ways that the going alternatives do, but we also argue that Roy Bhaskar and others are mistaken in believing that a commitment to philosophical realism by itself entails the truth of any particular theory about the world. That, we argue, is a task for social research, not armchair reflection.

By the same token, my criticism of Laitin’s work in *Flight* is not directed at his substantive work on Africa, language and political identities, or civil war, but rather at his proposal to do for political science teaching what Samuelson did for economics teaching. This proposal does indeed rest on a denial of reality, but not for the reasons Laitin reports me as having given. His claim is that political scientists should teach a universal introductory political science course, the syllabus for which Laitin has already designed. I argued in Chapter 6 of *Flight* that the type of standardization he seeks has been a bad development in economics, divorcing undergraduate teaching for the controversies at the research frontiers of the discipline and alienating serious scholars from undergraduate teaching. The practice in political science, where the plurality of introductory courses reflects the reality that scholars disagree on what the basic problems of politics are, is, I argued, superior. It is curious that this sometime student of hegemony misses the irony of his proposing that current teachers and future generations of students all be disciplined by his view of these matters.

Laitin’s attempt to sidestep this by saying that he gives “primacy to political theory in addressing consequential real-world concerns that have motivated participants in the tradition for two millennia” scarcely meets the objection. There are many ways of reading the classic theorists in the tradition. As I noted in my original discussion, the basic challenges about democracy that Laitin extracts from the tradition reflect a view of democracy in which solving Arrow’s problem is important. On my view, by contrast, preference-aggregation is comparatively unimportant; things like fostering political competition and opposition matter more. And I noted that many political scientists who teach about democracy would disagree with us and with one another more than Laitin and I do. In this context, suggesting that there is an accepted view of the matter that can be extracted from the tradition “involves kidding ourselves or kidding our students. I guess the latter is worse, but the former has little to commend it either.” Kidding the outside world in order to extract funds from granting agencies, which he comes close to advocating as well, hardly seems much better.⁶

My preference is that we continue teaching undergraduates in ways that reflect the disagreements at the research frontiers of the discipline, betting for the long haul on the competition of ideas instead of captive audiences for all the reasons that John Stuart Mill maintained that we should. In the short run, this will at least yield courses that engage teachers

and students more than serving up chapter three of some textbook in week three of every semester in every department in the country. Fortunately, it seems unlikely to me that the diverse reality of the political science discipline will bend to Laitin’s will any time soon. But I find it unfortunate that he believes that it should. Contra his suggestion that I am opposed to changing the political science curriculum, I have no desire to stop him from teaching it as he sees fit and trying to convince people that he is right. My own introductory course (based on a rather different account of the Western tradition of political theory than Laitin’s) seeks to do just that. When I turned the course into a book, rather than propose that it become the universal standard, I noted in the preface that I would be satisfied “if instructors find it a helpful teaching tool, yet feel the need to argue with it as they teach it.”⁷ That seems to me the appropriate aspiration.

A second non-sequitur concerns Laitin’s charge, piggy-backing on Ferejohn and Satz’s critique of *Pathologies of Rational Choice Theory*, that advocating problem-driven research allegedly ignores the fact that there is no pre-theoretical way of characterizing problems. I will not repeat our response to Ferejohn and Satz here, which can be found in Chapter 2 of *Flight*. The matter is taken up in greater depth in (and indeed is the central topic of) Chapter 5, though you would never know this from reading Laitin’s comment. In that chapter I note that every political phenomenon admits of multiple true descriptions, each of which invites a different type of explanation—reflecting the inevitability of theory-ladenness. Much of what is contentious in political science comes down to disagreements over which description is most apt, with partisans of different reductionist enterprises—Marxist, rational-choice, feminist, functionalist, and others—proposing their preferred cut as the right one.⁸ No architectonic venture of this kind has much of a track record of success in political science; indeed, this is a major reason why no single conception of what the discipline is or should be has won the day. Against all such ventures I argue for an anti-reductionist view of political explanation, one that jettisons the assumption that a particular cut will be the right one for the varied kinds of things political scientists study. My approach places the burden of justification for adopting a particular cut for a particular phenomenon on the researcher rather than on the skeptic. Shouldering this burden includes showing why the proposed cut illuminates more than the going alternatives—something that is seldom attempted in practice.

A third non-sequitur concerns Laitin’s allegation that my criterion for praising work is that it be authored by Yale colleagues. Tempting as it is to call this a low blow, that would suggest that it actually gets off the ground. No matter that I criticize Robert Dahl’s behavioral account of power and defend John Gaventa’s realist account as superior. As for his claim that I “hold back” my “full fire” with respect to my discussion of my colleagues Alan Gerber and Donald Green, Laitin both misreports what I say about their field experiments and gets the import of my discussion of them exactly backwards. I never claim that “Gerber and Green choose their problem (whether it is more efficient to phone or visit people if you

seek their vote) based on its tractability with the experimental method that they are promoting.” In fact, I have no idea how they chose this particular problem. What I *do* say, which Laitin ignores, is that *if* the research agenda for political science starts to be driven by what can be studied by means of field experiments, this will exclude the vast amount of what draws people to political science in the first place. This is the partial list I give: “the effects of regime type on the economy, and vice versa; the determinants of peace, war, and revolution; the causes and consequences of the trend toward creating independent central banks; the causes and consequences of the growth in transnational political and economic institutions; the relative merits of alternative policies for achieving racial integration, such as mandatory bussing, magnet schools, and voluntary desegregation plans; the importance of constitutional courts in protecting civil liberties, property rights, and limiting the power of legislatures; the effects of other institutional arrangements, such as parliamentarism v. presidentialism, unicameralism v. bicameralism, federalism v. centralism on such things as the distribution of income and wealth, the effectiveness of macroeconomic policies, and the types of social policies that are enacted; the dynamics of political negotiations to institute democracy. I could go on, but you get the point.”⁹ Apparently Laitin didn’t.

Perhaps Laitin thinks I pull my punches because I note that there is a class of phenomena that do lend themselves to field experiments, to wit, “the study of behavioral variation in settings where the institutional context is relatively fixed and where the stakes are comparatively low, so that the kinds of interventions required do not violate accepted ethical criteria for experimentation on human subjects.” But part of my antireductionist message is that people should abandon the search for the one-method-fits-all holy grail of political science. The phenomena that interest political scientists vary too much for this to make sense. We should avoid the shell game where the successes of one method are compared to the failures of another which is then judged to be ready for the scrap heap. With methods, I argue, as with people: “If you focus only on their limitations you will always be disappointed.”¹⁰ Here it might be worth reiterating that Green and I never declared rational choice explanations to be worthless; only that their indiscriminate application in political science had not yielded identifiable advances in knowledge. Indeed we suggested that rational choice explanations might be expected to do well when the following conditions hold: (i) stakes are high and players are self-conscious optimizers; (ii) preferences are well-ordered and relatively stable; (iii) there is a clear range of options and little scope for strategic innovation; (iv) the strategic complexity of the situation is not overwhelming for the actors whose strategic capabilities must not differ significantly from one another; and (v) actors have the capacity to learn from feedback in the environment and adapt.¹¹

Even within an appropriately specified domain, we should never lose sight of the fact that all the methods political scientists employ run into enormous difficulties when matched with the complexities of actual politics. Most quantitative studies are plagued by the bad data that scholars have to work with.

Case studies might be unrepresentative and more often than not are selected either because of the idiosyncratic interests of the researcher or on the dependent variable—or both. Public opinion surveys are weak guides to what people believe for a host of well-known reasons. Interviews involve their own problems of potential bias and manipulation. Rational choice applications often run afoul one or more of the strictures mentioned above. Archival work limits the researcher to what happens to have been preserved and is available. Given this reality, often the best that can be done is to surround the problem with multiple methods rather than opt for one. Some of the best work in political science does exactly that. Eric Schickler’s *Disjointed Pluralism* and Dan Carpenter’s *The Forging of Bureaucratic Autonomy* are recent examples that come to mind.¹²

As for Laitin’s claim that in my account of problem-driven research “any problem will do,” I go out of my way to warn those who might be seduced by the promise of field experiments (they confer the advantages of experimental controls without being subject to the questions about external validity that typically plague lab experiments in psychology) that part of what spawned the model-mania of the 1990s was the disaffection with the trivial, if tractable, questions that consumed a good deal of 1960s behaviorism in the study of American politics. As a result, I argue, the mainstream of political science that they came to define “seemed to others to be both utterly devoid of theoretical ambition as well as detached from consequential questions of politics; frankly boring. To paraphrase Kant, theoretical ambition without empirical research may well be vacuous, but empirical research without theoretical ambition will be blind.”¹³ Making a convincing case for the importance of the problem under study is integral to problem-driven political science on my account. It trumps methodological tractability. When the problems that are recognized as important are difficult to study with the available methods, the task of political theorists is to keep them on the agenda “and challenge the ingenuity of who are sufficiently open-minded to devise creative ways of grappling with them.”¹⁴ Hopefully, Laitin agrees.

Notes

¹ Ian Shapiro, *The Flight From Reality in the Human Sciences* (Princeton University Press, 2005), 199-203.

² Hence my criticism of Bawn on ideologies as devices for maintaining group solidarity and Hardin on constitutions as coordinating devices (*Flight*, 185-6) that David Laitin finds perplexing.

³ *Flight*, 199-202.

⁴ Gerry Mackie, *Democracy Defended* (Cambridge University Press, 2003).

⁵ The footnote is garbled inasmuch as my recent work on South Africa is not about injustice and my recent work on injustice is not about South Africa.

⁶ *Flight*, 207-8.

⁷ Ian Shapiro, *The Moral Foundations of Politics* (Yale University Press, 2003), xii.

⁸ See especially *Flight*, 187-8, 199-203.

⁹ *Flight*, 197.

¹⁰ *Flight*, 198-99.

¹¹ *Flight*, 94.

¹² Eric Schickler, *Disjointed Pluralism: Institutional Innovation and Development in the U.S. Congress* (Princeton University Press, 2001); Daniel Carpenter, *The Forging of Bureaucratic Autonomy: Reputations, Networks, and Policy Innovation in Executive Agencies,*

1862-1928 (Princeton, 2001). Laitin might complain that Schickler was once associated with the Yale department.

¹³ *Flight*, 198.

¹⁴ *Flight*, 203.

Symposium II: Conceptualizing Concepts

Introduction

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A symposium on the idea of “concepts” is a tricky matter. For one thing, the relevant issues have a way of persistently reflecting back on themselves. And when they do, the task becomes a knottier one of trying to conceptualize what it means to conceptualize, which hurls us headlong into debates about theory and practice and the endless loops associated with those. The usual editorial challenges of characterizing a plurality of perspectives are thus compounded in this case. The initial condition of recognizing the contestedness of conceptual meanings, for instance, must be that participants in a debate about it speak in their own diverse terms, traditions, aims, and demands, where possibly a plurality of conversations ensues. The job is made all the more thorny by the fact that what an adequate description of the terrain of debates about concepts should look like is itself an open question, depending on the various purposes one could invoke for thinking about them.

But I leave these admittedly not-so-trivial issues aside for the moment to hit on the equally significant points of convergence among the views presented here. For what these authors are doing as a group is drawing our attention to the often neglected set of considerations about how we should understand concepts even before we try to do the various things we could do with them. And there is good reason to commend this general stance, regardless of where one stands on some of the particulars, because so much of what passes for scholarship in political science today proceeds without a trace of due attention to issues like the scope and boundaries of concepts (cf. Choi, Freedon), the real world contestation over meanings and usage (cf. Davis), and legitimate versus illegitimate conceptual applications (cf. Bevir). It is not an exaggeration to say that most of the research emanating from the scholarly mills today evinces little or no concern for these at all. Dominating our top journals are arguments of the ilk that try to demonstrate the impact of x on y with a narrow explication of the statistical significance thereof, e.g. articles that identify the particular causes of institutional change, voter turnout, or the impact of GDP on democratic consolidation, or the possibilities for a “democratic peace,” to name a few.

The overwhelming emphasis of the discipline is squarely on attempts to model regularities in the social world, typically by isolating variables and demonstrating their seeming causal affectabilities against each other. Whatever one’s view about

this, closer attention ought to be paid to the ways that concepts actually operate, for example, to the functions they serve in academic research versus real world politics (cf., Freedon, Davis). Doing so could lead us to insights that advance our pursuit of knowledge and may even elevate the character of the discipline as a whole. To be sure, it would alter not only the kinds of research outcomes we get but also the process of inquiry itself (cf., Choi, Bevir). If these authors are right, then paying closer attention to concepts would heighten our awareness of the interaction and inter-relations between variables, in addition to merely their discreet causal effects. It may help us to resolve some messy operationalization issues for particular concepts, or at least motivate us to come up with adequate ways of explaining processes, events, actions, or speech that are by nature essentially contested and irreducibly contestable.

To begin, Freedon makes a concerted effort to draw specific parameters around concepts that are characteristically political in nature, even while simultaneously showing us the impossibility of establishing boundaries that are static, or in any way pre-determined in their content. Part of the difficulty lies in what he lays out as the fundamentally evaluative nature of political concepts. He offers a taxonomical analysis of the relative normative weights that political concepts carry in practice—at the intersecting levels of significance, legitimacy, and intensity—that set political concepts apart from any other run-of-the-mill concept. In studying politics, therefore, we have reason to be alert to the structural aspects of concepts that do not readily meet the eye but nonetheless operate in multiple and dynamic ways—by discursive, illocutionary, or even subversive means.

Choi is much less comfortable with the notion of the “political” as a separate or privileged category of concepts. Rather, she argues that all human action and social practices, including political science (itself a social practice not unlike those we seek to gain knowledge about), are each imbued with whatever meanings its practitioners attach to their concepts, and are therefore coextensive with the theories, purposes, and beliefs of its participants. What she argues is required for explanation-giving is a “family resemblances approach” to concepts that makes possible hermeneutical accounts that elucidate the particular concepts through which the meanings of agents operate. She contends that while concepts essentially constitute social practices, they are vague, by nature, because of the variable ways that people can construe meanings. Thus the vagueness of concepts is something we as political scientists should try to get more comfortable with and not try to skirt or pretend away; certainly not by various means of conceptual abstraction and reification, or mechani-

cal explanations of processes that bypass the agents' concepts altogether.

Davis points to several examples and domains in which the aim of clarifying concepts that have inherently fuzzy borders shows itself to be self-defeating. The dilemma lies in the conflict between the twin social-scientific virtues of analytic differentiation and conceptual validity, and the inability of even the most conscientious methodologist to capture the pervasive variation in the realm of empirical observation. His exemplary case is the concept of democracy and the controversies surrounding efforts to construct valid data-sets—evinced by the proliferation of sub-types and “diminished subtypes”—which he argues provides further evidence for his contention that much of the social construction of meaning takes place at the boundaries of our concepts rather than their cores.

Bevir takes up the particular issue of “anachronism” through which he shows how we can legitimately treat our concepts as valid for other times and places. He offers a philosophical argument for the universal applicability of certain concepts we hold, such as “action” and “procedural individualism.” The grammar of our concepts implies that such concepts apply across the board, in that by deploying our concept of action, for instance, we are committed to the concept of intentionality that it entails, despite the wide-ranging historical or cultural dissimilarities we would want to recognize in the content of such concepts. He argues that the grammar of our concept of “meaning” also commits us to a procedural individualism that precludes us from postulating meanings that we can not attribute, at least in principle, to certain people. These concepts are sufficiently abstract and capture general human faculties or capacities, but they do not prescribe specific content to those faculties or capacities. Thus he argues that the danger of anachronism arises only when we attempt to re-enact past beliefs, ideas, or intentions in a way that ascribes to people particular beliefs or intentions that they could not have held.

So where's the pay-off in heeding any of this? What we have here, after all, is a methods symposium. So what *qualitative* techniques in particular, if any, can be offered to help sort through the questions raised by these articles? Most of the authors do intimate, and in some places quite explicitly recommend, some practical measures that are entailed by the view of concepts the author is expounding. Readers will not, however, step away from here with the sense that what is being offered is a research program, a blueprint for explanation-giving, or a full epistemological theory that would have to underpin such ends. Indeed, the upshot of some of these arguments, though by no means all of them, is that methodology as a separate rarified scholarly activity may be superfluous. For Choi, the unease that this might bring can be diffused by doing away with the need for codifying techniques for proceeding into inquiry in the first place. She writes, “if the concepts embodied in our research techniques cannot reasonably be expected to be those of the participants of the practice we are studying, then they cannot rightly be used to explain them.” For Davis, the fuzziness of some of our most cherished and staple social scientific concepts, like “democ-

racy,” “peace,” and “war,” simply precludes their useful, not to mention reliable, operationalization. He writes, “rather than producing increased inter-coder reliability, efforts to increase operational precision have generated new lines of contention.”

Thus, depending on the perspective of the reader, what follows in these pages can appear to require some massive changes in the discipline. Perhaps more than many of us could comfortably admit. (How hard, after all, are we willing to bite the hand that feeds us?) On the other hand, the explication of what supports the political-science-as-usual model can be quite consistent with a more conservative view of our scholarly practices. Either way, paying due attention to the inherently decentered nature of the concepts we study can only move us toward garnering greater relevance in the world at large than presently seems to be the case.

What Makes a Political Concept Political?

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There is one area that political theory has barely begun to probe: what makes political thought *political*?¹ Not ethical, not philosophical, not historical, but political. What kind of everyday thinking is entitled to be called political thinking, and how should we, as scholars, try and make sense of such thinking? I take the political concept as the basic unit of political thinking—and by “concept” I mean a unit of meaning and of understanding that is incorporated in a unit of language, a signifier. The meeting of “concept” and of “politics” creates two dynamics in opposite directions. The first dynamic infuses thinking with the inescapable characteristics of concepts in general. Concepts are ambiguous, indeterminate, vague, and inconclusive. *Ipsa facto*, political thought will necessarily display those components as well, and I have dealt with that question recently.² The second, addressed here, identifies central features of the political, and then explores the manner in which language—as thought, text, speech, and non-verbal expression—discharges those features, and the manner in which concepts carry specifically political import. They do so in two ways: either through special attributes that are plainly political, or through the substantive issues they address. Because human interaction and thought always have a political dimension, whether significant or trivial, some fundamental features of political concepts are universal in a macro sense, while at the same time having particular manifestations at different times and places.

Thinking about Politics

What makes thinking political is its effective engagement in one or more of the following, in no particular order. It *ranks* aims, processes, and structures in order of importance; it *accents* and *justifies* political entities and procedures; it works

out *co-operative* or *conflictual* conceptual arrangements; and it *constructs* and *directs* political plans and, more ambitiously, projects political *visions*. That is what *political* thinking is all about, and it relates to four central features of the political—the distribution of significance, the mobilization of support, the organization of social complexity through which stability or instability are manufactured, and decision-making and option-determining for collectivities. There is of course a fifth political feature, power, but it permeates the other four. Power is evident in the listing of priorities that strives to be incontestable; in the flow of consent or acquiescence towards political systems and leaders; in the regulation of spatial social relationships; and in the attempted control over future social time.

In addition, people apply different levels of *intensity* to these four central areas of the political when talking and writing about politics. The superimposition of intensity endows a political concept, argument, or discourse with additional power. It amplifies the persuasive, rhetorical, and emotional force of the political language through which the first four features are articulated and made public. It is the task of the political theorist of *politics* to identify the everyday as well as professional language through which the now five features—including direct appeals to power—are expressed, and to ponder on how they all reflect and shape social patterns of thinking. While each feature on its own directs us to locating political phenomena, a thick notion of political language will normally contain all five. Due to restrictions of space, I shall focus on the first two features and allude briefly to the third and the fifth. The fourth will have to await separate treatment.

Ranking

Politics is centrally, though not exclusively, about ranking and prioritizing demands, visions and procedures. In political theory that characteristic of politics appears as the distribution of significance: the identification of what is socially and publicly more important and less important, more valuable and less valuable. Significance is a scarce and crucial symbolic resource that is competed over in the various ranking exercises that constitute part of politics. Significance is always distributed unequally and ranked, because of the essential contestability underpinned by ineliminable diversities of opinion and appraisal, and because of the subsequent need to *decontest* those, however contingently and fragilely, in the act of decision-making.³ Consequently, ranking and the conferral of significance are also exercises in power. Ranking may involve the authoritative allocation of values—as in Easton's famous definition of politics⁴—but it may also be non-authoritative, as in the case of a revolution or of looting.

The dual phenomena of expressing ethical as well as interest-related preferences and expressing intensional morphological preferences incur ranking. The first case relates to a particular substantive content, without which the concept and its cashing out are regarded as devalued. Thus, ethical preferences are expected to conform to notions of the good that outweigh and push aside notions of the bad or the indifferent. Interests are expected to conform to what is profitable, satisfy-

ing, or fulfilling to the interest articulator or bearer. Of course, ethical and interest-related preferences may coincide. The second case relates to the range of components that are contained within the domain of the concept, among which a selection has to be made, as some meanings of a concept will always be incompatible with others. Ranking is a fluid and constantly mutating process. It may rely on ethical cosmologies, on the expression of popular cultural preferences, on brute acquisitiveness, or on the conjuring up of teleological visions of the future.

The archetypal *political* concept that discharges the role of ranking (though not the only one to do so) is that of rights. Rights are also *ethical* concepts, but that does not concern us here. The political role of rights is to lock rankings into a particular sequence. They do so by attempting to advance their protected value (be it life, liberty, property, well-being, etc.) to the head of a ranking ladder. A right is a demand to prioritize whatever concept it protects over and above other demands, claims, and wants.⁵ It does so by purporting to confer unchallengeable legitimacy—hence unassailability—on that ranking. That also brings the concept of legitimacy into play. Legitimacy assists in the ranking process indispensable to politics, but furthermore it flows into the second central feature of politics—the need to mobilize support and to generate justification for important political decisions, acts, processes, and institutions. Thus, rights are both ranking devices and successful contenders for the status of legitimacy conferred by the political system. Legitimacy, however, has not only ranking and support dimensions, but also stability and decision-making dimensions—intersecting with the four features of the political enumerated above. Rights plus legitimacy are furthermore exercises of political power in that they ring-fence rankings and endeavour to insulate them from attempts at change or challenge. We may also note the not infrequent failure to discharge that political function, for instance, when some ranking competitions prove inconclusive—that is, when two given rights cannot gain sufficient weight to outclass one another. A typical instance is the standoff between “pro-lifers” and “pro-choicers” with regard to the termination of pregnancy, due to the conceptual contestability of the point at which life begins.

Legitimacy

As a ranking device, legitimacy performs a somewhat different task from rights. Rights rank preferences by ostensibly providing them with the finality that other demands fail to attain. Legitimacy is more broadly an attribute of a political system, though the system can also bestow legitimacy on its constituent parts and practices. Its political aspect is to rank the *systemic* weight given to preferences, demands and practices—including rights. Legitimacy elicits collectively produced approval, irrespective of the basis on which it is claimed—which may be the collective endorsement itself, rule-based or otherwise, or which may emanate from an entity claiming prior authority on the basis of faith, scientific knowledge, experience, cultural superiority, and the like—and that interaction and communal funnelling of “consent” itself constitutes a

vital facet of the political. The concept of rights endorses a vertical ranking from indispensable to dispensable; that of legitimacy endorses a horizontal ranking from acceptable to unacceptable. While ethicists and philosophers of jurisprudence may assert that legitimacy is *morally* linked to what is right, *politically* legitimacy is first and foremost linked to what is regarded as acceptable by whatever grouping is socially, legally or culturally relevant to bestowing that accolade. Hence the political theorist of politics needs to investigate the overlapping and competing languages and discourses of acceptability.

Legitimacy itself is however only *one* member of the family of concepts engaged in identifying and securing political support. Other members are concepts such as commitment, allegiance, loyalty, political obligation, solidarity, and obedience. At an even more specific level of concrete language, we need to identify and understand the terms used to particularize those political concepts, terms such as “uphold,” “pledge,” or “advocate”—indicating a positively engaged agent resolutely supporting, say, a constitution or promoting an ideological stance; and “defend,” “stand together,” or “close ranks”—indicating a negatively engaged agent passionately protecting, say, a way of life from attack.

To appreciate the intricacy of the emerging conceptual map, three further distinctions need to be factored in. The first is between conscious and unconscious forms of support. This in turn intersects with, but is not identical to, another distinction—between rationally reflective and emotional forms of support. Many political philosophers shy away from the unconscious or the unreflective, but those phenomena are endemic to the production and circulation of political ideas and must be included within the remit of the political theory of politics, as part of its relocation within the social sciences. And superimposed on the two distinctions is a third—between speech acts and text acts on the one hand (these can be deliberate as well as contain unintentional surpluses of meaning,⁶ and embrace various mixes of the rational and the emotional, depending on their discursive apparel) and states of mind that serve as background reservoirs from which more targeted political thinking can draw on the other hand (e.g., the strong concept of pride or the somewhat weaker concept of fellow-feeling and belonging).⁷ These latter reservoir concepts are preponderantly emotional but often possess sizeable rational content as well.

Of course, some key political concepts are so through-and-through political that they discharge multiple political functions. As we have seen, legitimacy is one such instance. Its third task, the manufacture of stability, will vary in diverse societies. In more monolithic systems, the legitimizing of dominant ideas and practices, either through tradition and convention or through force, provides stability. What does this mean in terms of a scholarly understanding of political concepts? It means that the multiple meanings that political concepts invariably possess are unable, or not permitted, to thrive, because the contents of some of their conceptions are potentially subversive. Conversely, in pluralist societies, the structural tolerance of multiple discourses and of the essential con-

testability of concepts within a broader, though not infinite, range suggests that morphological flexibility itself, rather than the substantive content of concepts, may contribute to political stability. The political role of managing and ordering social relationships is crucially mirrored in the managing of political language and the ordering of political concepts. Political theorists need to know which conceptual configurations within political discourses contribute to, or detract from, stability in a particular cultural setting. And are there some conceptual combinations that are universally steadying or destabilizing?

Intensity

I now turn briefly to the notion of intensity. The intensity of a concept, as noted above, is a power attribute; in other words, one of the most salient of the political garbs it may don. Often this reflects the rhetorical and illocutionary force of the written or spoken texts in which it is located,⁸ or its rational persuasiveness. But equally it may be an outcome of its direct emotional tone. Discourse analysis and the study of contentious politics have drawn attention in recent years to displays of anger, frustration, and fear on the negative side, and pride, compassion and fellow-feeling on the positive side. But most such studies are either general treatises on the importance of emotion in understanding the human condition,⁹ or explorations of specific emotions in a political setting, in which negative and destabilizing emotions such as anger or fear predominate. There is, however, very little on the direct input of emotion into the forging of political concepts and political thinking. The political aspect of all emotions is to act as an intensifier; that is to say, they superimpose a coating of power on the concepts, conceptual clusters, arguments, and stances to which they are attached. A concept's intensity is a universal aspect of human expression, but the levels and means of intensification may differ.

Commitment and Obligation

The standard political concepts that we employ straddle such distinctions. Habermasian *constitutional patriotism*, for instance, is a notion intended to generate emotional support not for ethnic or national groupings, with their primary appeal to the sentiment of belonging, but for democratic procedures and values—thus producing systemic political allegiance based on the rational construction of an alternative emotional identity.¹⁰ More generally, the notion of *political commitment* is an intense adherence to supportive thought and action based on rational, moral or emotional grounds.¹¹ Many support-mobilizing concepts—for instance allegiance, loyalty, and political obligation—appear in a cluster of distinguishable yet correlated and intersecting concepts. Initially, the introduction of political obligation as a replacement for loyalty and allegiance seemed to offer an ethical breakthrough, because it shifted messy, arbitrary, and violence-ridden notions to a plane of argument devoted mainly to rational and moral dimensions, in which the acts of consent and promising became central, generating conditional obedience. As a consequence, dominant genres of political philosophy have utilized the concept of political obligation for over three hundred years when ana-

lyzing the bases of commitment to the point where that perspective has become part of a dominant, even unassailable, philosophical tradition.

That optimism now seems—regrettably for the aspirations of liberals and consent theorists—both premature and misguided. For political obligation has principally figured in the realms of the conscious, the intentional and the virtuous, as the ethical precondition to justified obedience and compliance. It is optimally expressed through formal acts of public speech and writing. Consequently, when deduced tacitly, as in the Lockean model, it loses its clear status as obligation and becomes famously problematic, precisely because it crosses over into the realm of the unconscious and unarticulated. Moreover, the importance of emotions in eliciting support, acceptance and obligation itself is ignored. All that is a characteristic deflection of the vital issue of mobilizing political support from the methodological field of the political to the methodological field of the philosophical.

The political theory of politics, however, needs to investigate political obligation through identifying the intentional and unintentional discursive roles of “political obligation” within a theory of support mobilization and within its linguistic practices. Concurrently, it should investigate the means by which political language expresses alternative support-mobilizing thoughts and sentiments concerning political commitment, allegiance and loyalty, and explore the political nature of the conceptualizations through which those thoughts and sentiments are contained and shaped.

Alliance and loyalty still play a crucial role in mustering political support. In particular, they fill the gap created when commitment is restricted to the objects of political obligation—a state/constitution/regime, or a specific government—because they offer a linguistic register for commitment to national, supra- and sub-national, and political-culture levels as well. Hence political commitment is directed not only at legal entities with which contracts can be drafted, but at the communal repositories of collective values and memories that define courses of political action and areas of political space. That this may even be channelled through a national soccer team perfectly illustrates the variegated nature of political support mobilization. Loyalty, bonding, pride and security all play a significant role here and their conceptualization and discursive manifestations provide insight into the complex ways in which political thinking is fashioned and accrues meaning.

Alliance and loyalty may or may not be voluntary, and they may or may not be deliberately adopted. But do they have separate ineliminable cores, while sharing other components? Political systems often display public acts or pledges of allegiance—for instance to a monarch—or parliamentary oaths. The act then is conscious and rational, but the bond is meant to be emotional. Alliance is suspended between obligation and commitment, between rational consent and emotional bonding, between verbalized cognition and unformulated inclinations. The targeted rational attachment of emotional support to a formal political entity appears to be the distinct ineliminable feature of allegiance. To that extent it is a complex but looser

and less forensic political feeling, a form of social binding on the psycho-political level, while obligation is a more specific rationalization of a particular and circumscribed political relationship, and loyalty is a more unspecific and less critical emotional adherence to another, often personified, being. Appreciating those differences may furnish an important key to understanding the thought and language of political support more generally.¹²

We then find that the political role of commitment is largely contained in its intensity, as a passion that creates the relative consistency and immovability vital both to political support and to political stability. In a state of commitment belief in alternative ends, visions or goals is suspended, even though they could be rational as well. That form of decontestation—through ignoring alternative meanings and interpretations—is a usually unconscious attempt to forestall rational deadlock when more than one goal might be justifiable and reasonable.

Clearly, intensity is a weighting feature—a core component of politics—and its outcome is to create an unequal field of concepts in a given utterance or text. Just as the intensity of an emotion may repress other emotions,¹³ the emotional manifestation of a concept displaces other concepts. Intensity assists in the competition over the space and salience allocated to conceptual configurations. Weighting and urgency may indirectly also increase the effective power of the other features of politics and thus cut across them, but intensity is a political feature in its own right. It relates to the attempt to change either rationally or emotionally the verbal, and consequently practical, power relationships and conceptual force of the language it permeates. For whoever controls political language has the firmest of grips on politics itself. While intensity is a form of power, the explicit and implicit means it uses are so singular that it demands separate acknowledgement and categorization by political theorists.

This paper is one exploratory probe in a larger research project. It calls for further propping up through the concrete, empirical analysis of different kinds of political texts, and its account of political concepts requires extending. But its message is this: political concepts primarily convey and mould the ways in which people think in *their own terms* about *core political activities*. Political theorists need to be closely in touch with what the *conceptual* apparatus of politics achieves, or can accomplish, or cannot attain, before moving on to their other concerns.

Notes

¹ This paper is part of a UK Economics and Social Research Council project on “the political theory of politics” on which I am engaged.

² M. Freeden, “What Should the ‘Political’ in Political Theory Explore?”, *Journal of Political Philosophy*, Vol. 13 (2005), 113-134.

³ For a discussion of decontestation, see M. Freeden, *Ideologies and Political Theory: A Conceptual Approach* (Oxford, 1996), Chapter 2, and M. Freeden, “Editorial: Essential Contestability and Effective Contestability,” *Journal of Political Ideologies*, Vol. 9 (2004), 3-11 and 225.

⁴ D. Easton, *A Framework for Political Analysis* (Englewood

Cliffs, NJ, 1965), 50.

⁵ See M. Freedman, *Rights* (Milton Keynes and Minnesota, 1991).

⁶ See P. Ricoeur, *Interpretation Theory: Discourse and the Surplus of Meaning* (Forth Worth, 1976).

⁷ For long- and short-term emotions see Paul E. Griffiths, *What Emotions Really Are: The Problem of Psychological Categories* (Chicago, 1997).

⁸ See Skinner's elaboration of Austin's use of the term to denote the force of an utterance. Q. Skinner, "Motives, intentions and the interpretation of texts," reprinted in J. Tully, ed., *Meaning and Context: Quentin Skinner and his Critics* (Oxford, 1988), 74.

⁹ See the excellent study of M. Nussbaum, *Upheavals of Thought: The Intelligence of Emotions* (Cambridge, 2001).

¹⁰ J. Habermas, "Citizenship and National Identity: Some Reflections on the Future of Europe," *Praxis International*, Vol. 12 (1992), 1-19.

¹¹ See e.g. Howard S. Becker, "Notes on the Concept of Commitment," *American Journal of Sociology*, Vol. 66 (1960), 33; D. Easton, "A Re-Assessment of the Concept of Political Support," *British Journal of Political Science*, Vol. 5 (1975), 451-2; C. Hall, "'Passions and Constraint': The Marginalization of Passion in Liberal Political Theory," *Philosophy and Social Criticism*, Vol. 28 (2002), 736.

¹² An initial attempt in this direction was made by J. Shklar, "Obligation, Loyalty, Exile," *Political Theory*, vol. 21 (1993), 181-197.

¹³ William M. Reddy, "Against Constructionism: The Historical Ethnography of Emotions," *Current Anthropology*, vol. 38 (1997), 333.

Crafting Explanatory Concepts

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"Our discussion will be adequate if it has as much clearness as the subject matter admits of, for precision is not to be sought for alike in all discussions, any more than in all the products of the crafts... It is the mark of an educated man to look for precision in each class of things just so far as the nature of the subject admits; it is evidently equally foolish to accept probable reasoning from a mathematician and to demand from a rhetorician scientific proofs."

Aristotle, *Nicomachean Ethics*, Book I Chapter 3: 2-3¹

Aristotle's basic point about the need to temper methods of inquiry to their subject matter instructs that methodology in the human sciences depends on ontology. Methods of inquiry should be appropriate to the kind of thing the subject of inquiry is. It would be a mistake to hold a single model of knowledge for subjects that are different in kind; in particular, to model all forms of knowledge on that of the natural sciences where the inquiry is typically into concrete objects, causal mechanisms, and law-like regularities. Indeed, many scholars have long recognized that the methods privileged in the natural sciences do not automatically apply within the human sciences.² In the latter case the subjects of study are social inter-

actions that result neither from objective facts about people nor from causal necessities that operate between atomizable units of data.³ To explain phenomena that are social and human in nature we need to insist on methods that are suitable to accounting for the concepts embodied in them. Merely rejecting the logic of naturalism as the appropriate approach to explaining social practices is not enough. We should want to question what makes for the explanatory power of concepts in the first place, and seek to better hone our grasp of the relationship between concepts and practices, meaning, and action.

The first task is to clarify the nature of concepts to understand how they operate within social practices. While concepts essentially constitute social practices, they are vague, by nature, because of the variable ways that people can construe meanings. For this reason, the central occupation of political scientists must be with deciphering the meanings that people attach to their concepts, that is to say, with elucidating how the world appears meaningful or significant to human beings who act for reasons of their own. Hermeneutics is therefore integral to explanation-giving, since the only way to explain social practices is by interpreting the meanings that constitute them. We should therefore reject the methodologist's drive toward formulations of concepts in the abstract along with criterial conditions of rightness that are fixed in advance, which purport to explain their subject matter but end up missing their target altogether. Instead, we should strive to offer good-faith interpretations of culture and meanings and open up the space for critical dialogue about the best accounts on offer. Furthermore, in order to defend the explanatory power of concepts in the face of the rival interpretations they can generate, we ought to address the epistemological question about the status of interpretive claims. In short, while hermeneutic accounts will necessarily be provisional, the automatic disposal of standards of rationality and a repudiation of all forms of objectivity is simply unwarranted. We can ascertain practical truth, or truths of practice, by assessing competing interpretive accounts of culture and meanings comparatively, by debating their rank against one another as better or worse explanatory accounts. Thus, as it happens, knowledge in the social sciences is an inescapably practical and historically situated enterprise, rather than a technical project in abstraction towards objectivity.

The Nature of Concepts

To get a handle on what gives concepts their explanatory power we should want to address the prior question of what they are and how they operate. Quite simply, concepts are ideas perceived by the mind. They are the means by which we think, criticize, argue, desire, intend, act, and in all senses, participate in the world. As such, concepts are the building blocks of human consciousness and knowledge. A concept will be represented by a single word or phrase, but that alone will not automatically give what is meant to be conveyed. Concepts are much more general and abstract than what the proper noun or the term for any one thing might seem to refer to. Since they provide a mental netting for the *perceived at-*

tributes of objects, people, ideas, or phenomena, they collect several, usually many, different things in ambiguous ways. We can speak of the concept of “president,” which brings together shared ideas about the organization of executive power, and makes possible discussion and debate, along with the recognition of many persons as such. We can also speak of a concept of “George W. Bush,” for example, which has to do with the characterizable perceptions about his policies, leadership style, manner of communicating, etc. What is meant in either case will depend on various other concepts we hold about the nation-state, democratic legitimacy, political exigencies, leadership values, and so on. Concepts always rely on other concepts, so none can be singled out to stand on its own.

As such, the meaning of a concept can never be fixed in and of itself. For this reason, although concepts are ubiquitous and fundamental to our being in the world, they are by nature vague in both their intensional and extensional senses.⁴ The vagueness of concepts is attributable to several things, but two points are of particular relevance to political scientists: The first is the fact that meanings do not exist independently of a mind.⁵ There can be words on a page, objects in space, or matter in motion, but what the words, sounds, and actions *mean* is constitutively dependent on the person for whom the meaning is. For example, we might refer to the “meaning” of a novel, but what we are getting at is the particular meanings held by the author, readers of a specific time and culture, a translator, or literary historian, not, strictly speaking, the meaning of the novel in and of itself. The locus of conceptual meanings is the mental processes that go into their perception (as well as their creation) such that meanings exist only in conjunction with a mind. Second, the vagueness of concepts is further attributable to the fact that there are multiple levels at which the world appears meaningful or significant to us as human beings. As a result of the variable ways that people can employ concepts, the same concept, word, object, or action can mean quite different things to different people.

Although meanings are held by individual minds, that could not be possible without the background of shared concepts and intersubjective meanings that are coextensive with social practices. Individuals are fundamentally interdependent on one another, not only for the conditions of their self-preservation but for their identity. Recognition and intersubjective meanings are indispensable dimensions of selfhood and are necessarily transmitted through shared social practices, particularly through the shared linguistic and cultural resources that underpin them.⁶ A social practice is any relatively stable pattern of activity that is reproduced in the course of daily life and enables some established way of doing things by the concepts and meanings that its participants hold in common.⁷ Yet, while a social context does influence people it would be a mistake to try to read off the way people will act from any given rule, norm, standard operating procedures, or institution (however defined). No social context can force actors to adopt the meanings they hold so none can automatically fix the way people will act. Any two people in the same social situation

who exhibit the same behavior can hold very different ideas if their experience of that situation is laden with different prior theories. Therefore, the number of features which any given object or phenomena exhibits is indefinite, and impossible to determine in advance of their perception. We can conclude from these points that concepts neither contain fixed boundaries nor can they dictate exactly how one should or should not apply them. We grasp concepts only through our ability to use them with one another by means of interpretation and analogy.

Explaining a Social Practice

This view of concepts has stark consequences for how political scientists should approach the social practices we seek to explain. The fact that humans are interpretive beings for whom conceptual meanings are essential entails the following about explanation-giving: Human action and social practices cannot be divorced from the meanings people attach to them. For this reason, political science as a knowledge discipline depends on interpreting the concepts embodied in social practices and is therefore inseparable from hermeneutics. We build knowledge not simply by observing or describing human phenomena, but by interpreting the meanings and concepts that inhere in social practices, which are co-extensive with the purposes, behaviors, and attitudes of their participants. Since people’s concepts are intimately entwined with their behavior and not something that can be pushed aside in the quest to explain their actions, there is good reason for political scientists to grant presumptive priority to the concepts of the people involved in the phenomenon we wish to explain—their own ideas, perceptions, beliefs, and purposes. We are always employing concepts to make sense of the world and to participate in it, and so we are always, in this sense, imposing meaning upon the world through the use and construction of our concepts, indeed, through shared concepts that form the loci of intersubjective meanings. And since social practices are fully constituted by the intersubjective meanings that shared concepts embody for agents who act for reasons of their own, we must understand them not as inexorable social processes but as practices that are contingent from the bottom up. Therefore, when we seek to explain social practices, we must allow for the contingency of social life.

It is widely taken for granted that the purpose of political science is to provide explanations of various and particular social phenomena. A common criticism leveled at political scientists, however, is that we spend far too little time thinking about concepts and their implications for explanation-giving, as compared to an ambition to produce good quantitative measures, i.e., issues dealing with indicators, scaling, reliability, and the like. The accusation is more difficult to stick on qualitative methodologists who do exhibit a concern for the explanatory power of concepts beyond the interest also in good numerical measures. Their sensitivity to the more complex ways that concepts actually operate than is accommodated by existing measurement traditions is noteworthy; so too is their seeing the need to “clarify concepts” and fine-tune measurement theories to reflect such complexity. Never-

theless, insofar as these efforts are geared toward seeking better ways and finer means of fixing the definitional and empirical limits of concepts, they are specious and remain in need of rethinking.

Both the classical and the frame-analysis approach to concepts purport to capture some objective or essential features of concepts that support ways of matching them to empirical cases. However, to “build concepts” using the structure of necessary and sufficient conditions is to assume that we can define a concept by giving the conditions for something to fit into a category. It presumes that we can fix the meaning of a concept by reference to a list of general features or essential properties meant to characterize its each and every instance. Methodological tasks such as “structuring concepts” or “constructing multilevel or, multidimensional concepts”⁸ aim to fix the meanings of thick concepts in the abstract where their meanings are, at such a level of generality, at best diffuse and/or essentially contested. As such, they are tantamount to spurious attempts to suppress the untidiness presented by the vagueness of our concepts. If we take seriously the idea that meanings essentially constitute human actions, then we should understand all social practices *including* political science this way, as embodied by concepts that are laden with theories. The call to fix the intension or extension of concepts seems deeply rooted in the drive to make possible accounts of social logics or law-like mechanisms that purport to explain the actions of people. Such analytical tasks rest on more than the goal of enabling sound measurement; the *purpose* of measurement appears to be for atomizing units of data to facilitate accounts of correlations between and among them, and to yield causal explanations. They may steer the researcher to look for the essential or common features of an abstract category that purport to denote some cluster of social practices; they may even lead the researcher to dismiss certain particular cases to pursue these allegedly common features. As a result, analytic rigor may be attained at the costly expense of explanatory power.

No abstract concept, however rigorously constructed, can properly *explain* the actions, ideas, beliefs, or interests of the people associated with the diverse practices describable by the term. Take for example the idea of the “rule of law”⁹: We can talk about the concept as having to do with governance by open and clear laws, which are relatively stable and prospective, rather than retrospective. We might want to emphasize that such laws be brought about by processes governed by rules that are themselves open, stable, clear, and general. We might or might not associate this also with ideas about an independent judiciary, where courts have some review power over implementation, and the courts themselves are open and accessible. We could associate this further with some notions about the observance of standards of justice—ideas about fairness and the absence of bias, etc. We could easily go on further to derive principles from any of these basic ideas about the rule of law. But the *validity* of the general concept, or its explanatory significance, will depend fully on the particular circumstances of different societies that have their diverse practices of democratic governance, according to the particular concepts and meanings its participants hold. Trying to fix

the concept of the rule of law to capture the essential features of democratic governance such that the stipulated properties characterize each and every social practice to which we might apply the term would be self-defeating—were it even possible to do—given the inherent fecundity of concepts and their interdependence on other concepts. Any view of concepts that relies on the false assumption that a concept itself can dictate how it should or should not be applied in practice is likely, therefore, to breed a pernicious way of undertaking explanations. It would be to count any one of the diverse aspects of the rule of law as matching up with a social logic or law-like mechanism at work within a practice, at the expense of invoking the possibly diverse and contested ideas about governance, legal norms, democracy, and equality that the participants of a given practice hold.

Aggregate concepts, such as the rule of law, can only have explanatory power insofar as they are defined in terms of family resemblances that can be used pragmatically to explain a particular practice in question. A family resemblance approach to concepts contains no necessary conditions. Rather, definitions of concepts are couched in terms of the particular meanings embodied in various practices. A family resemblance approach to concepts aims not at comprehensive accounts but points, instead, to diverse accounts that can be rendered by several perspectives on the variable aspects of its subject, none of which needs always to be present. Were we to aggregate from these diverse accounts to analyze the concept itself, so to speak, all we would have is a set of resemblances given by the multiple cases of the concept in practice, but which alone would fall far short of explaining any practice in particular. What is required for explanation-giving is the heavy work of interpretation about the meanings embedded in the practices through which concepts operate. In any particular social practice, what might rightly figure in correlations is never simply given to political scientists; certainly not before the researcher does the interpretative work of fusing the appropriate aspects of concepts to the meanings held by the participants of the social practice in question. A family resemblance approach to concepts allows us to do this precisely because it makes possible hermeneutical accounts that can be used, through interpretation and pragmatic decisions, to explain a particular object under study.¹⁰

Interpretation and Objectivity

Recent calls to more interpretivist approaches to social scientific inquiry signal a growing concern about the ability of conventional research techniques to do adequate explanatory work.¹¹ Although the label “interpretivist” collects a heterogeneous group of ideas and practitioners, there is shared ground among us in seeking to correct for approaches to explanation-giving that continue to resonate with empiricist scientific traditions, that is, with mistaken views of what kinds of explanations are appropriate to giving accounts of practices that are human and social in nature. However, a view of concepts that places interpretation at the very heart of the social scientific enterprise can also embolden the criticism that the argument for proper explanation-giving is circular: A

good explanation makes the best sense of the phenomenon in question, but to appreciate a good explanation one has to agree on what makes good sense, which is squarely a function of one's interpretations. Certain epistemological issues, therefore, need to be considered more closely: If important aspects of the interpreter's own concepts play a constitutive role in how she interprets, where do we get the conceptual resources to translate meanings across different contexts? To what extent does explanation-giving raise issues of truth? Are interpretive accounts doomed to fail in giving us any "right" answers by virtue of their subjectivity?

Two key objections to the argument about concepts presented here can be anticipated and dealt with as follows: First is the reasoning that erroneously attributes to this argument a slide toward the incorrigibility thesis, which contends that in order to understand a social practice we must assume the validity of the judgments and claims made by those whose practices we seek to explain. While the view of explanatory concepts expounded here shows that there is good reason to grant presumptive priority to the self-interpretations of the people whose practices we wish to explain—their own ideas, beliefs, attitudes, and purposes—it does not commend us simply to accept the agent's stated point of view such that the interpreter cannot criticize or correct the agent's self-understanding. There is no compunction to treat the latter as necessarily sovereign, since it can be limited, mistaken, or distorted. Careful interpreters can depart from the explicit self-interpretations of their subjects, and should do so in the direction of a better explanation, provided they can justify each step of that departure by reference to a wider web of concepts and theories that the agent is operating within. The second objection gets at a more fundamental dispute by charging that a slide toward unbridled subjectivism in disciplinary standards is inherent in the approach, built into this argument about concepts. If people's meanings (what political scientists are after) are interpretations all the way down, then any explanatory account will necessarily be provisional and contestable; and it follows that we should expect multiple explanations, indeed, even rival interpretations.¹² It is a mistake, however, to conclude that relinquishing the aspiration for ultimate, definitive, and predictive knowledge of our subject matter is tantamount to flouting the demands of objectivity and the eventual breakdown in disciplinary standards. To the contrary, the argument laid out here about explanatory concepts shows that we must rethink conventional ideas about objectivity that fundamentally misunderstand the relationship between scientific explanation and conceptual analysis.¹³

The approach advocated here does not endorse the conclusion that any explanation goes. We can reject a fixed standard of truth-conferring methods and the possibility for absolute certainty without automatically disposing of all notions of rationality or objectivity. Nevertheless, a deeper argument about the status of interpretive claims is required, for which Gadamer's notion of the fusion of horizons is a useful heuristic.¹⁴ The epistemological model he invokes for understanding historical texts is one of a conversation between interlocutors. As opposed to an interpreter striving to neutralize her ordinary cat-

egories of interpretation in order to explain her subject, interlocutors in a conversation aim for mutual understanding through a dialogical process that relies on, rather than eliminates, the pre-understandings that each side brings to the task. When a person reads a text for the first time or seeks to explain a social practice from without, she encounters a different horizon or zone of meaning. In finding itself challenged through the encounter and in striving to attain a coherent account of the other her horizon of meaning has shifted. In this way, conceptual horizons are in practice and in principle permeable. They can be broadened to take account of other people's concepts, theories, ideas, even without those meanings and beliefs being adopted. For example, making sense of something need not entail showing that it made sense, i.e., giving it a rational explanation. An interpreter can make sense of irrational behavior without re-describing it as rational by advancing an account as to why it was engaged in. Through the encounter a language of perspicuous conceptual contrast can develop, which is not wholly the interpreter's own nor that of the participants of the social practice being explained, but one that enables the differences and convergences between the two contexts to be articulated. Thus, while the interpreter's initial beliefs and values provide an enabling starting point for the inquiry, they are not left to operate unconstrained or unquestioned. The aim of hermeneutical explanations is the *fusion* of horizons not the escaping or slipping on and off of horizons. The metaphor can be contrasted with strictly "fitting" or "organizing" concepts in the sense described by Donald Davidson in his discussion of conceptual schemes.¹⁵

Indeed, seeing explanation-giving as a dialogical process is in keeping with the fact that concepts are not atomizable units of meaning-datum but rather more vague complexes of ideas with no fixed boundaries in and of themselves. And if we were to ask what underpins this process to secure interpretive reliability, the only definitive answer to give would be to point to the nature of human agency itself, which is one of engaged and embodied being in a common world. In our ordinary ways of being, we are all creatures with bodies who find ourselves in a world where we have to act and meet practical demands. Underlying this claim is a conception of the truth of practice that does not require all approaches to reality to be indubitable or incorrigible but which gives those who can find their way around the world on a day to day basis some grounds for measuring interpretations of that reality. No agent's outlook will be thoroughly wrong or totally deluded. Some may be distorted or erroneous in parts, and some more than others, but the fact that knowing is so intimately connected with doing means that one can engage in a dispute with another always on the assumption that the latter's perspective has some validity. The fact that the locus of embodied involvements is a shared world means that parties to disputes about truth have a common reference point for their arguments. This principle of initial respect for another's take on the world extends quite far to provide a foundation for the possibility of mutual understanding across even widely divergent contexts of meanings.

The ultimate criterion for explaining social practices, therefore, is already within our grasp, namely interpretive plausibility. Explanation-giving should be seen as the product of good faith efforts at making sense of meanings, not as an all or nothing endeavor to capture a view from nowhere. Thus, the epistemological model we should hold to ought to be concerned first and foremost with knowing about being or doing, not primarily with knowing about knowing. Knowledge of ontology, rather than of epistemology, ought to assume priority.

Furthermore, establishing the superiority of one interpretation over another need not entail circular reasoning. We can identify better and worse explanations of social life, where the value of an interpretation is assessed relatively and comparatively against alternative interpretations, not by the yardstick of correspondence with some ultimate body of truth or verification method. Indeed, the standards for what counts as good scholarship in the discipline can only rise the more actively we engage in this kind of comparison and debate of the rank of explanations against one another as better or worse interpretive accounts. Of course if there is no consensus among interpreters about what it means to make sense of an action, then it might not be possible to fully persuade one's rivals that one's interpretation is better than theirs, but that is likely to be a rare exception to the rule. We ought to open up the space for greater debate, especially across disciplines and among methodological camps, to argue the merits of one interpretation over another. Comprehensiveness is one crucial basis on which to assess which accounts are more plausible than others. A better interpretation will encompass and explain more of the features of the phenomenon under study than can its alternatives. And a better, more comprehensive interpretation should be able to say why it can accommodate, build on or surpass its rivals' strengths while avoiding their weaknesses.

To be sure, this kind of scholarly knowing can only occur when the practice is underpinned by a cluster of intellectual virtues or values. These would have to include something like a genuine desire and willingness to know what is other, the ability not to dismiss things that seem strange as necessarily irrational, the ability to change, the courage to question one's own assumptions and so on. This kind of disposition is presupposed by a hermeneutical approach to explanation-giving, unlike the instrumental theories of knowledge that is typical of the hard sciences where the aim is to better control or utilize the subject matter. Here, if we were to be persuaded by Foucault's arguments about the implication of the humanities and sciences in the diffusion of the disciplinary society we would charge even the view offered here with a kind of naiveté. However, since there is no given way in which research has to proceed in the social sciences, no dictate of the profession itself apart from the concepts and meanings we ourselves hold as practitioners, we can seek to elevate our practice by deciding and choosing to elucidate the multiple, diverse, and usually very complex ways in which our subjects understand the world and act in it.

Conclusion

As political scientists we must abandon the view of our task as having to do with correlations of observable phenomena that lie unproblematically before our gaze. Reflecting on the various ways that we can think about concepts makes clear that the points of divergence between different ways of understanding concepts are supported by divergent points of view about how social practices are best explained. If concept formation in political science is aimed at fixing the boundaries of concepts to limit their empirical domain and underwrite causal correlations, then, on this view, only a very small range of classifications will yield correlations that have any explanatory force. As a goal of explanation, trying to atomize and isolate units at such a generalized level to measure its affectabilities against other atomized units is radically undermined if we emphasize the primacy of meanings and the contingency of social practices. On a deeper level, the attempt to formulate or structure thick concepts in social scientific research seems to be to maintain that there is no difference in kind between human actions and natural occurrences, which fundamentally misunderstands the relationship between scientific explanation and conceptual analysis.

This misunderstanding can be corrected if we see that the objects we study as political scientists are themselves interpretations whereby attaining knowledge requires us to grasp the meanings that people attach to their concepts. We would do well to bear in mind how the social sciences is itself a social practice, not unlike those we seek to gain knowledge about. Thus we ought to be sensitive to the theories embodied in our own concepts and the assumptions laden therein. We should reject the drive toward rigorous formulations of concepts in the abstract along with criterial conditions of correctness fixed in advance, which assumes that an abstract concept together with the right operationalization will explain the particular phenomenon under study. If the concepts embodied in our research techniques cannot reasonably be expected to be those of the participants of the practice we are studying, then they cannot rightly be used to explain them. We should be wary of moves toward mechanical explanations of human action that discount how agents act for reasons of their own, based on their conceptions of themselves, their actions, each other, and their world.

Notes

¹ Aristotle (1980). *Nicomachean Ethics*. Ed. D. Ross trans. Oxford: Oxford UP.

² See Almond and Genco (1977), "Clouds, Clocks, and the Study of Politics," *World Politics*, Vol. 29, No. 4 (July) 489-522. Also see C. Taylor (1971), "Interpretation and the Sciences of Man" in *The Review of Metaphysics*, 25: 1 Sept. 1971, 3-51; C. Taylor (1985), *Philosophical Papers I: Human Agency and Language*. Cambridge: Cambridge UP; and C. Taylor (1995). *Philosophical Arguments*. Cambridge, MA: Harvard UP. While Almond and Genco's piece was targeted mainly at grand theories (e.g., game theory/rational choice), their critique can also be considered to apply more broadly. Their main point is that the models of explanation appropriate to the physical sciences will not enable us to come to grips with human and

cultural phenomena because of the latter's creative and emergent properties.

³ We do not think that a black male who voted Democrat in the last election did so because of the color of his skin. Neither do we think that there is a causal relationship at work, for instance, between income bracket and membership in a particular political party.

⁴ Any range of qualities together can constitute generalizable conditions or characteristics of a concept that can be applied to whatever is deemed to comply with them. "Intensional" has to do with the qualities that make up the idea, and "extensional" with the things or objects it is meant to cover.

⁵ I draw from the thinking of John Searle and others who discuss the mind in terms of "intentional states." J. Searle (1983). *Intentionality: An Essay in the Philosophy of Mind*. Cambridge, UK: Cambridge UP.

⁶ I take a holist approach to selfhood and find that solipsistic challenges can only collapse upon themselves, since all meanings are shared, in some minimal sense. Even the self-understandings, identities, and preferences that individuals harbor can only be formed against a background of meanings that are shared in common with others.

⁷ I take from Charles Taylor's view that culture is the key medium of meaning for individuals, which he defines as the broad set of beliefs and understandings about personhood, nature, society, and morality that people share through their practice, even if those remain partly unarticulated. See C. Taylor (1984). "Philosophy and its History" in *Philosophy in History*, R. Rorty, J.B. Schneewind, and Q. Skinner (eds.), Cambridge: Cambridge UP, pp. 17-30. See also A. MacIntyre's very helpful discussion of this in his "A Mistake About Causality" in P. Laslett and W.G. Runciman's *Philosophy, Politics, and Society*. Oxford: Basil Blackwell, 1969, pp.48-70.

⁸ See G. King, R. Keohane, S. Verba (1994). *Designing Social Inquiry: Scientific Inference in Qualitative Research*, Princeton, NJ: Princeton UP, and for a further discussion of issues raised by this book, see H. Brady and D. Collier (eds.) (2004) *Rethinking Social Inquiry: Diverse Tools, Shared Standards*, Rowman & Littlefield. For another recent statement of this ilk, see G. Goertz (2005). *Social Science Concepts: A User's Guide*. Princeton, NJ: Princeton UP.

⁹ H.L.A. Hart's famous formulation of the concept of law (echoing Hayek's writings on the ideal) remains an oft-cited conceptual analysis of the idea, as much for the numerous subsequent attempts to supplant it as for its own merit. See H.L.A. Hart (1961). *The Concept of Law*. Oxford: Oxford UP; in addition see F.A. Hayek (1944), *The Road to Serfdom*, London. For subsequent pronouncements that challenge this view see L. Fuller (1969) *The Morality of Law*. New Haven, CT: Yale UP; J. Raz (1979), "The Rule of Law and its Virtue" in J. Raz (ed.), *The Authority of Law*. Oxford, UK: Clarendon Press.

¹⁰ See L. Wittgenstein (1972), *The Blue and Brown Books*. Oxford: Blackwell.

¹¹ Of course "interpretivism" is not a new phenomenon. Scholars have long been urging a variety of interpretive approaches, since the '60s and '70s (by Charles Taylor, Clifford Geertz, H-G Gadamer), the '80s (by Paul Rabinow and W. Sullivan), and the '90s (by Mark Bevir, and Rod Rhodes, among many others). A useful discussion of current shifts in debates more germane to political science can be found in the Fall 2003 issue of the APSA Qualitative Methods Newsletter, based on its symposium on "Interpretivism." See also C. Geertz (1973), *The Interpretation of Cultures*, New York: Basic; M. Gibbons (ed.), (1987), *Interpreting Politics*, Oxford: Basil Blackwell; P. Rabinow & W. Sullivan (eds.), (1987), *Interpretive Social Science: A Second Look*. Berkeley: University of California; M. Bevir & R.A.W. Rhodes, (2003), *Interpreting British Governance*, London: Routledge. Also, for a more self-conscious look at

the evolution of this school of thought, see J. Scott & D. Keates, (eds.), (2001), *Schools of Thought: Twenty Five Years of Interpretive Social Science*, Princeton, NJ: Princeton UP.

¹² In some sense, this is not entirely unlike the way political science as a discipline has actually already been proceeding, in the Kuhnian sense of scientific progress. See T. Kuhn (1962). *Structure of Scientific Revolutions*, Chicago: University of Chicago.

¹³ What Charles Taylor has famously called "the epistemological model's hold on the theoretical imaginary." For Taylor engaged and embodied identity is prior to, and indeed the precondition of, the sort of disengaged representational model of knowledge that became influential and dominant with the rise of modern science. The great vice of the epistemological tradition is in allowing epistemology to command ontology, by understating and attempting to transcend the role of engaged embodiment in knowing, even while being nested in the latter's wider way of being in and knowing the world. See C. Taylor. *Sources of the Self*, Cambridge, MA: Harvard UP, 1989.

¹⁴ See H-G. Gadamer (1975), *Truth and Method*. New York: Continuum.

¹⁵ See D. Davidson (1984), *Inquiries into Truth and Interpretation*. Oxford: Clarendon Press.

Confounding Conceptual Clarity

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Despite longstanding efforts of leading scholars to standardize usage of the term "democracy" in scholarly practice on the basis of precise operational definitions, David Collier and Steven Levitsky found a proliferation of subtypes, a phenomenon they refer to as "democracy with adjectives." Efforts to define democracy in terms of a "procedural minimum" or those conditions necessary and sufficient for the existence of a democracy have not produced the intended uniformity of usage and high degrees of inter-coder reliability, but rather led to the identification of hundreds of subtypes, of which "authoritarian democracy," "neopatrimonial democracy," "military-dominated democracy," and "protodemocracy," are a few.¹ As they see it, scholars confront a dilemma. On the one hand, there is an impulse to maximize *analytic differentiation* in order to capture the wide variety of democracies that have emerged across the globe. On the other, there is a necessary concern for *conceptual validity* and to avoid what Giovanni Sartori referred to as "conceptual stretching."²

The difficulties associated with defining democracy are not unique. Rather, they are characteristic of most efforts to define and operationalize scientific concepts. The impulse to achieve connotative precision necessarily is frustrated by pervasive diversity. A finite set of concepts will never be able to capture what is in principle an infinite set of possible observations.

In this brief contribution I wish to use the concept of democracy and the controversies surrounding efforts to construct reliable and valid data sets of democratic and non-democratic polities in an exemplary fashion. I aim to highlight prob-

lematic assumptions underlying conventional wisdom regarding the essential characteristics of useful scientific concepts.³

Conventional Understanding of Scientific Concepts

Conventional notions of concept formation and categorization are based on the assumption that useful concepts give rise to clear boundaries established by essential attributes or properties common to members of the category. Clearly defined and precisely operationalized concepts are generally held to be necessary conditions for scientific investigations of the natural or social world. Conventional notions are influential in the social sciences, and the failure of social scientists to adhere to rigorous standards in the formation of concepts is routinely identified as a primary cause of theoretical underdevelopment. Thus, Giovanni Sartori reacted to the widespread practice of what he termed “conceptual stretching,” by arguing the necessity of precision in the specification of the “totality of characteristics any thing must possess” to be considered an example of a given concept.

Conventional wisdom and understandings of science that rely upon it are flawed. First, even the seemingly unproblematic spatial boundaries of physical entities dissolve under the lens of microphysics and can make the specification of necessary attributes or properties problematic. Second, many concepts defy definition in terms that would generate the clear borders Sartori seeks.

Ludwig Wittgenstein pointed out the difficulties of defending the position that concepts can be defined in terms of traits common to all exemplars through a philosophical analysis of the concept of a “game.” His discussion merits citation at length:

Consider for example the proceedings that we call “games”. I mean board-games, card-games, ball-games, Olympic games, and so on. What is common to them all?—Don’t say: “There must be something common, or they would not be called ‘games’”—but *look and see* whether there is anything common to all... And the result of this examination is: we see a complicated network of similarities overlapping and crisscrossing: sometimes overall similarities, sometimes similarities of detail.⁴

What for Wittgenstein connects these various enterprises and constitutes them all as exemplars of the concept of a game are “family resemblances.” Games form a “family” of activities that defy any precise definition or operationalization on the basis of essential attributes, properties, or relationships. Moreover, the boundaries of the family are not fixed:

How should we explain to someone what a game is? I imagine that we should describe games to him, and we might add: “This and similar things are called ‘games’”. And do we know any more about it ourselves? Is it only other people whom we cannot tell exactly what a game is?—But this is not ignorance. We do not know the boundaries because none have been drawn. To repeat, we can draw a boundary—for a special purpose. Does it take that to make the concept usable? Not at all!

Two implications of Wittgenstein’s analysis warrant emphasis. First, the proposition that the boundaries of concepts are usually, perhaps always, extendable. Extendability may result from the emergence of a novel phenomenon that resembles an extant class of phenomena, for example the emergence of video “games” in the 1970s. Or, it may result from the novel employment of a given concept, extending a concept with an established meaning in an unconventional or original fashion to a new domain, perhaps the extension of the concept “game” to the field of international politics.⁵ Second, some instances of a given concept will be better examples than others. That is, concepts or classes are characterized by central and non-central or marginal members, or *graded membership*. Wittgenstein gives the example of dice: “Someone says to me, ‘Show the children a game.’ I teach them gaming with dice, and the other says, ‘I didn’t mean that sort of game.’”⁶

When concepts are generated by means of extension from central or prototypical members, one can imagine a statement: “If anything is a case of x, then this is a case of x.” It is hard to imagine someone saying: “If anything is a game, then throwing dice is a game.” The attributes of dice would not appear to be central to the concept of game, yet, dice can be understood to be a game. A wide range of empirical studies of classification across cultures provides evidence to support Wittgenstein’s philosophical ruminations. Studies of American Indian kinship systems found that kinship categories are generally based on a focal member and then extended to other, non-focal individuals by means of certain rules that generate a set characterized by graded membership and fuzzy borders.⁷ Eleanor Rosch and her associates discovered that categories are generally characterized by the existence of “best examples” or what she termed prototypes, as well as other, less representative, members. For example, when asked to identify the most “birdlike” in a sampling of birds that included robins, chickens, ostriches and penguins, an overwhelming number of subjects will identify the robin. Similarly, a desk chair will be judged to be more representative of the category chair than will be a rocking chair or a barber’s chair.⁸

Rosch’s findings have been replicated across cultures in a number of anthropological studies. For example, in a study of folk classification of pottery in Mexico, Willet Kempton found strong evidence of prototype effects.⁹ And in a now classic cross-cultural study of color categorization, Brent Berlin and Paul Kay discovered strong evidence that conceptual color categories are characterized by focal, best, or purest examples and extended to new observations on the basis of similarity judgments. Although there is strong cross-cultural consensus on what constitutes the “best” or “purest” example of a given color category, individuals differ with regard to the extension of the color category to marginal exemplars. The result is a color category characterized by graded membership and fuzzy borders.¹⁰

The Fuzzy Concept of Democracy

Most contemporary democratic theorists would maintain that something approaching universal suffrage, competitive recruitment of candidates, competitive elections, and an inde-

pendent judiciary are constitutive features of democratic systems. But the dimensions according to which polities constitute themselves are neither constant nor universal. Indeed, scholars are routinely confronted with systems which are clearly more democratic than not, yet do not fit a “strict” operational definition. One need only mention the political system of Great Britain, where the remnants of feudalism persist in the form of a partially hereditary House of Lords.

An analysis of social scientific practice suggests that many scholars extend the concept of democracy on the basis of a given system’s resemblance to a ideal- or prototypical core. Having converged on a “procedural minimum” definition of democracy that requires fully contested elections, full suffrage, the absence of widespread fraud, and effective guarantees of civil liberties, scholars were soon confronted with a large number of cases in which one or the other criterion was to some degree compromised. The application of the concept of democracy to the set of observable cases generates a set that is characterized by graded membership and fuzzy borders.

Democracies can be differentiated according to type, with each subtype constituting a full instance of the root definition of democracy. Hence, “parliamentary democracies,” “presidential democracies,” or “federal democracies” all can provide for fully contested elections, universal suffrage, the absence of widespread fraud, and effective guarantees of civil liberties, even as they do so in different ways. The essence of classical subtypes is that they allow one to distinguish amongst exemplars of a given concept along dimensions that are not essential to its definition. However, democracies can also be differentiated in terms directly related to the operational definition. Examples include “limited-suffrage democracy,”¹¹ “male democracy,”¹² and “illiberal democracy.”¹³

In contrast to classical subtypes, these “diminished subtypes” are distinguished by the fact they lack one or more of the defining attributes of democracy, or enjoy these attributes at something less than “standard” or “ideal” levels. The distinctive feature of diminished subtypes is that they identify specific attributes of the case or observation that are *missing*.¹⁴

Once one allows for diminished subtypes, concept extension no longer follows the classical criteria of establishing the presence or absence of a uniform set of necessary and/or sufficient conditions. And because the relevant dimensions along which members of a category vary are not uniform, one cannot construct composite measures or continuous scales. When membership in a conceptual category is graded, coding is not a function of strict measurement but proceeds on the basis of judgments of similarity. The result is usually a set with borders more fuzzy than discrete.

Moreover, because differences amongst individual observations are psychologically more salient than common features, comparisons between diminished subtypes and prototypical exemplars are likely to be characterized by directedness and asymmetry. For example, if United States serves as the conceptual prototype for a democratic system, we would not be surprised to find that significantly more people would agree to the statement “Guatemala’s political system is similar to that

of the United States” than would agree to the assertion “The United States’ political system is similar to that of Guatemala.” If true, this suggests that the borders of conceptual categories, or empirical data sets, will be strongly influenced by the choice of prototype. The set of all polities similar to the United States might be judged to include Guatemala, whereas the set of all polities similar to Guatemala might not be thought to include the United States, a situation inconsistent with orthodox understandings of the nature of scientific measurement and coding.

Collier and Levitsky argue that diminished subtypes “are a useful means to avoid conceptual stretching in cases that are less than fully democratic,” and “provide differentiation by creating new analytic categories.”¹⁵ But recognizing or countenancing the resort to diminished subtypes in scholarly practice does not resolve the supposed dilemma from which their analysis begins. Diminished subtypes do allow for greater differentiation, but questions regarding validity remain, as their reference to the work of Bruce Bagley makes clear. Rejecting the diminished democracy subtypes that have been applied to the case of Columbia in the period 1958-74—e.g. “restricted,” “controlled,” “limited,” “oligarchical,” “elitist,” and “elitist-pluralist”—Bagley characterizes Columbia as a subtype of authoritarianism!¹⁶

Such arguments are precisely the stuff of concepts with graded membership and fuzzy borders. That the dilemma posed by the two values of analytic differentiation and conceptual validity is either false or beside the point is reflected in the response offered to Bagley’s critique: “Scholars should be self-conscious about the analytic and normative implications of choosing to form subtypes in relation to democracy, as opposed to some other concept.”¹⁷ But having argued the virtues of analytic subtypes in terms of their relationship to root definitions, an appeal to the normative implications of “naming” is puzzling to say the least. It does, however, provide further evidence to support my contention that much of the “action” in the social construction of meaning is taking place at the boundaries of our concepts rather than their cores.

One might argue that the problem of fuzzy borders could be resolved through more precise operational definitions. But because scientific concepts are defined in terms of other concepts, the most basic of which are every-day language terms, this has the effect of shifting the borders of concepts but does not deal with the inherent imprecision of any definition. For example, studies of Central and South American “transitional” democracies found that one legacy of authoritarian rule is often the persistence of “reserved domains” where the military exercises power independent of political control.¹⁸ The result has been the revision of the procedural minimum definition of democracy by some scholars to include the attribute of “effective power to rule,” an attribute that many felt was already understood to be implied in the overall meaning of democracy even if it was not explicitly included in the definition.¹⁹ Rather than producing increased inter-coder reliability, efforts to increase operational precision have generated new lines of contention. The example illustrates the futility of efforts to completely specify scientific concepts and the utility of taking

some things for granted or assuming them to be antecedently understood.

With the end of the Cold War, the impact of democracy on patterns of interstate conflict has become one of the dominant themes in the study of international politics. A series of systematic empirical studies have concluded that democratic states do not wage war against other democracies, a finding now routinely referred to as the “democratic peace.”²⁰ The research program is interesting from the standpoint of this analysis because most of these studies have been conducted with the same data.²¹

The exertions to formulate and apply precise definitions of “democracy” and “war” to the history of inter-state conflict, however, have not prevented disputes over the coding of individual cases. Even as critics of the thesis have attacked the inclusion or exclusion of a particular state as democratic and/or a particular interstate conflict as a war, proponents of the democratic peace also confront an increasing number of results, which they contend are best regarded as “unintended implicatures” of the operational specifications. Moreover, a close analysis of scholarly practice in the research program reveals the coding process to be marked by the assimilation of cases to a concept of democracy that is generated by a prototypical exemplar which itself is a product of culture and historical contingency.

As Ido Oren writes: “[I]n all studies America receives virtually perfect scores on the democracy scale. America is the norm against which other polities are measured.”²² Moreover, not only does the contemporary United States achieve perfect democracy scores, but despite manifest differences in the degree to which the population enjoyed the franchise and the state’s protection of basic human rights, the United States receives perfect scores for a period extending back into history for over two hundred years.²³

A review of the data-sets of democratic and autocratic regimes suggests they were not generated by the application of neutral or precise coding protocols. Indeed, on an alternative democracy index constructed by Finnish scholar Tatu Vanhanen, the United States ranked 30th, far behind the West European democracies with Italy at the forefront.²⁴ What accounts for the differences between Vanhanen’s data-set and those of his American colleagues, is not so much the basic definition of democracy—indeed, he adopts the definition offered by the eminent American scholar Robert Dahl—but rather the method of operationalizing competition and participation. Clearly influenced by European models of proportional representation where coalition governments are the rule, the degree to which an election is competitive is measured according to the share of votes the smallest parties receive in presidential or parliamentary elections.

In American debates over the purported existence of a zone of democratic peace, whether the concept of democracy is extended to another state, contemporary or historical, is a function of the “degree of difference” when compared to the United States rather than its meeting a threshold generated by qualitative or index measures that reflect criteria held to be necessary and sufficient for category membership.²⁵ And shift-

ing the analysis to liberal norms does nothing to change the underlying process of assimilation. In American scientific practice, “liberal” norms are merely those generally associated with the contemporary U.S. political system. These are then applied trans-historically to other political systems if not the United States itself.²⁶

Conclusions

A brief analysis of scholars’ efforts to define the concept of democracy in terms of observable attributes that are necessary and sufficient for category membership demonstrates the futility of the effort. The problem, however, is not one of scholarly practice but reflects pervasive variation in the realm of empirical observation.

Any definition establishes borders, and gives rise to “borderline” cases. A large part of what constitutes scientific debate takes place at the borders of scientific concepts, and ruling a particular empirical observation to be “inside” or “outside” of the concept’s borders is an exercise in the social construction of meaning.

Social scientific concepts exhibit prototype effects and fuzzy borders characteristic of everyday language terms. Scholars from different theoretical traditions or culturally determined contexts were shown to disagree both on where to draw the border between democratic and non-democratic forms of rule and on what constitutes the “best” or purest example of a democratic regime. Nonetheless, and the point is not insignificant, debate goes on. Debate, that is communication, is possible, even though scholars are committed to, or guided by, somewhat different concepts of democracy. Whereas many in the scientific community adhere to the proposition that scientific progress demands precision in both the definition and employment of concepts, it strikes me that it is the resemblance, not congruence, of concepts that characterizes fruitful research and indeed makes science possible.

Notes

¹ David Collier and Steven Levitsky, “Democracy with Adjectives: Conceptual Innovation in Comparative Research,” *World Politics* 49 (April 1997), 430-51.

² Giovanni Sartori, “Concept Misinformation in Comparative Politics,” *American Political Science Review* 64 (December 1970), 1033-53. See too David Collier and James E. Mahon, Jr., “Conceptual ‘Stretching’ Revisited: Adapting Categories in Comparative Analysis,” *American Political Science Review* 87 (December 1993), 845-55.

³ For a fuller development of the ideas presented here see James W. Davis, *Terms of Inquiry: On the Theory and Practice of Political Science* (Baltimore: Johns Hopkins University Press, 2005).

⁴ Ludwig Wittgenstein, *Philosophical Investigations* (New York: Macmillan, 1958), 31-2, emphasis in the original.

⁵ Friedrich V. Kratochwil, *Rules, Norms, and Decisions: On the Conditions of Practical and Legal Reasoning in International Relations and Domestic Affairs* (Cambridge: Cambridge University Press, 1989), especially 81-91.

⁶ Wittgenstein, *Philosophical Investigations*, 33.

⁷ Floyd Lounsbury, “A Formal Account of the Crow- and Omaha-Type Kinship Terminologies,” in W. H. Goodenough (ed.), *Explorations in Cultural Anthropology* (New York: McGraw Hill, 1964), 351-94.

⁸ Some of the more important findings are detailed in: Eleanor Rosch, "Natural Categories," *Cognitive Psychology* 4, 3 (1973), 328-50. E. Rosch, "Cognitive Reference Points," *Cognitive Psychology* 7, 4 (1975), 532-47; and E. Rosch, "Prototype Classification and Logical Classification: The Two Systems," in E. Scholnick (ed.), *New Trends in Conceptual Representation: Challenges to Piaget's Theory* (Hillsdale, NJ: Erlbaum, 1983), 73-86.

⁹ Willett Kempton, *The Folk Classification of Ceramics: A Study of Cognitive Prototypes* (New York: Academic Press, 1981).

¹⁰ Brent Berlin and Paul Kay, *Basic Color Terms: Their Universality and Evolution* (Berkeley: University of California Press, 1969).

¹¹ Ronald P. Archer, "Party Strength and Weakness in Colombia's Besieged Democracy," in Scott Mainwaring and Timothy R. Scully (eds.), *Building Democratic Institutions: Party Systems in Latin America* (Stanford: Stanford University Press, 1993), 166.

¹² Georg Sorensen, *Democracy and Democratization: Process and Prospects in a Changing World* (Boulder: Westview Press, 1993), 20.

¹³ Donald Emmerson, "Region and Recalcitrance: Questioning Democracy in Southeast Asia" (Paper presented at the World Congress of the International Political Science Association, Berlin, 1994), 14.

¹⁴ Collier and Levitsky, "Democracy with Adjectives," 435-9. See too Collier and Mahon, "Conceptual 'Stretching' Revisited: Adapting Categories in Comparative Analysis," pp. 850-2.

¹⁵ Collier and Levitsky, "Democracy With Adjectives," 441.

¹⁶ Collier and Levitsky, "Democracy With Adjectives," 441-2; Bagley codes Colombia as an "inclusionary authoritarian regime." See Bruce Bagley, "Colombia: National Front and Economic Development," in Robert Wesson (ed.), *Politics, Policies, and Economic Development in Latin America* (Stanford: Hoover Institution Press, 1984), 124-60. For a discussion of "inclusionary authoritarianism" see Susan Kaufman Purcell, *The Mexican Profit-Sharing Decision: Politics in an Authoritarian Regime* (Berkeley: University of California Press, 1977).

¹⁷ Collier and Levitsky, "Democracy With Adjectives," 442.

¹⁸ J. Samuel Valenzuela, "Democratic Consolidation in Post-Transitional Settings: Notion, Process, and Facilitating Conditions," in Scott Mainwaring, Guillermo O'Donnell, and J. Samuel Valenzuela (eds.), *Issues in Democratic Consolidation: The New South American Democracies in Comparative Perspective* (Notre Dame: University of Notre Dame Press, 1992).

¹⁹ Collier and Levitsky, "Democracy With Adjectives," 442-5.

²⁰ The studies are too numerous to list. For a sampling, see Michael Doyle, "Liberalism and World Politics," *American Political Science Review* 80, 4 (December 1986), 1151-69; Zeev Maoz and Nasrin Abdulali, "Regime Types and International Conflict, 1815-1876," *Journal of Conflict Resolution* 33, 1 (March 1989), 3-35; Zeev Maoz and Bruce M. Russett, "Normative and Structural Causes of Democratic Peace, 1946-1986," *American Political Science Review* 87, 3 (September 1993), 624-38; Bruce M. Russett, *Controlling the Sword: The Democratic Governance of National Security* (Cambridge: Harvard University Press, 1990).

²¹ For the data on interstate wars, see Melvin J. Small and J. David Singer, *Resort to Arms: International and Civil Wars, 1816-1980* (Beverly Hills: Sage, 1982); also J. David Singer and Melvin Small, "Wages of War, 1816-1980: Augmented with Disputes and Civil War Data," Inter-university Consortium for Political and Social Research (ICPSR), Study I9044 (Winter 1984). Most of the above cited studies use data taken from Ted Robert Gurr, "Polity II: Political Structures and Regime Change, 1800-1986," ICPSR, Study 19263 (1990). Data on regime types can be found in Monty G. Marshall and Keith Jaggers, "Polity IV Project: Political Regime

Characteristics and Transitions, 1800-1999," Integrated Network for Societal Conflict Research (INSCR) Program (December 2000), www.bsos.umd.edu/cidcm/inscr/polity.

²² Ido Oren, "The Subjectivity of the 'Democratic' Peace: Changing U.S. Perceptions of Imperial Germany," *International Security* 20, 2 (Fall 1995), 150.

²³ Oren, "The Subjectivity of the 'Democratic' Peace," 266.

²⁴ Tatu Vanhanen, *The Process of Democratization: A Comparative Study of 147 States* (New York: Crane Russak, 1990), 27-8.

²⁵ For a similar conclusion, see David Spiro, "The Insignificance of the Liberal Peace," *International Security* 19, 2 (Fall 1994), 50-86.

²⁶ See the justifications for coding the ancient republics as illiberal in Russett, *Controlling the Sword*, 123; and Doyle, "Kant, Liberal Legacies, and Foreign Affairs," Part I, 212. Clearly rooting his definition of liberalism in the political philosophy of Kant, John Owen comes closest to avoiding this contemporary bias. Nonetheless, in his effort to identify the mechanisms whereby democratic (or liberal) states come to identify one another—a necessary condition for the operation of the mechanisms that are generally held to connect liberalism to pacific resolutions of inter-state crises—Owen employs an a priori concept of liberalism in order to generate a class of cases to study. A truly interpretive research design would have started with the question of "Which states have considered themselves to be liberal?" See John M. Owen, "How Liberalism Produces Democratic Peace," *International Security* 19, 2 (Fall 1994), 87-125. See too Oren's critique of Owen in "The Subjectivity of the 'Democratic' Peace," 150n.

When Can We Apply Our Concepts to the Past?

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Qualitative methods are, more or less by definition, used when political scientists want to acquire a thicker understanding of some phenomena. They are used to generate a more detailed, more textured, more contextualised account of an action, practice, or even institution. No doubt political scientists might have all kinds of reasons for wanting to acquire such textured accounts—it would be nice (but, alas, probably false) to assume the various reasons derived from distinct philosophical analyses of their discipline.¹ Still, one reason for seeking textured accounts might be a concern to understand the reasons actors had for doing what they did. One rationale for qualitative methods is precisely that they can help us to recover the meanings or concepts with which those involved imbued actions and practices.

Yet even when political scientists set out to recover others' concepts, they often proceed to describe these concepts using rather different ones; they describe them using their concepts, not those of the people they are studying. Does it matter that they do so? The answer surely must depend on a philosophical analysis of the general issue of the reasonableness of applying our concepts to other peoples. Alas, this issue has received almost no attention from political scien-

tists. Fortunately, however, the issue has been discussed extensively elsewhere, including in intellectual history where it appears as the problem of anachronism.

What is anachronism? If a production of *Julius Caesar* were set in ancient Rome, it would be an anachronism were a member of the cast to wear a wristwatch on stage. It would be anachronistic because a watch is an object of our time that did not exist in ancient Rome. Anachronisms are perhaps relatively easy to identify when they concern such concrete objects. It is arguable, however, that the main debates about anachronism concern ideas or concepts, not concrete objects. If we use the concept of the state to cover Aristotle's notion of the polis, are we guilty of anachronism? Is it anachronistic to explain the eventual triumph of a Darwinian concept of evolution over a Lamarkian one in terms that assume our current scientific knowledge? Concepts are more troubling than objects here, I believe, because they are more obviously vague. Perhaps, however, we cannot distinguish issues about objects from those about concepts; after all, we can refer to an object, whether it be an anachronistic presence or not, only by deploying concepts. Either way, the main debates about anachronism would appear to revolve around the question, "when can we apply our concepts to the past?"

We would do well, I believe, to distinguish various issues, all of which might lurk within the apparently singular question, "when can we apply our concepts to the past?" One issue is a variant of conceptual relativism. It concerns the legitimacy of treating our ideas as valid for other times and places. A second issue revolves around the nature of textual meaning. It concerns the legitimacy of attributing to texts meanings other than those they could have had at the time they were written. A final issue is that of the nature of reenactment. It concerns the legitimacy of using our concepts to cover very different ideas from the past.

Conceptual Relativism

Various forms of conceptual relativism appear to question the legitimacy of treating our ideas as valid for other times and places. Such relativism seems implausible with respect to our knowledge of the natural world. Surely if we approached the eruption of Mount Versuvius as a historical problem, we would draw freely on our knowledge of volcanic behavior, and we would do so even though this knowledge was not shared by those who lived in Pompei at the time of the eruption. We would draw freely on our knowledge of volcanic behaviour because this knowledge is, in our view, about invariant causal laws that govern physical events.

Conceptual relativism appears more troubling when we are dealing with human activity. If we describe or explain the past using our concepts of action and meaning, do we do violence to cultures that did not have similar concepts? If people did not conceive of themselves as, say, subjects who act for reasons of their own, then can we legitimately discuss their behaviour in such terms? Is it reasonable to apply our conceptual analysis of action to peoples who did not share that analysis? It would seem reasonable to do so, at least in principle, provided that our analysis includes the idea that the

concept applies universally. The relevant idea of universal applicability is, however, one that can not be provided by any amount of empirical evidence. Empirical evidence can not support such universality in part because of the familiar problem of induction: the fact that all the people we had studied possessed a certain type of subjectivity could not conclusively prove that all people did so. Besides, the empirical evidence would embody the analysis to which it was supposed to lend support, so any justification of the analysis by reference to the evidence would be tautological. If we want to give universal applicability to our conceptual analysis of action, therefore, we have to do so by means of philosophical argument. We have to show that the grammar of our concepts is such that this analysis applies across the board. That is to say, we can fend off conceptual relativism by showing that our concept of action entails a related concept of intentionality such that to discuss any action from any era simply is to imply the relevance of that concept of intentionality. Likewise, if our concept of meaning is one that implies that meanings only exist for specific individuals, as I believe it is, then the grammar of our concepts commits us to the universal applicability of what I call a principle of procedural individualism: the grammar of our concepts precludes us from postulating meanings that we cannot attribute to specific people.

Although a conceptual relativist will complain that our conceptual analysis of action, with its related notion of intentionality, might be false, this complaint misses the point. The point is not to defend our concepts as True in any grand metaphysical sense. It is merely to show that our belief in these concepts commits us to certain other positions. Once we grant that we can not conclusively prove the Truth of our concepts—a point to which relativists themselves give great weight—the fact that our beliefs might not be True need concern us very little. What will concern us is that our concepts are ones we deploy for good reasons, and that these concepts entail the reasonableness of applying our conceptual analysis of action whenever we discuss actions, or of invoking a principle of procedural individualism whenever we discuss meanings.

Textual Meaning

An appeal to the grammar of our concepts enables us to defend the universal applicability of some of our concepts. Yet because such appeals involve philosophical arguments rather than empirical ones, the relevant concepts are abstract ones such as those denoting human faculties or capacities rather than more concrete ones prescribing content to these faculties or capacities. The grammar of our concepts might lead us to defend treating people as situated agents even if they do not understand themselves as such. But it does not seem to allow us to argue that situated agents always act in a particular way. The problem of anachronism, in contrast, concerns just such matters of content. It is not anachronistic to discuss people as having a capacity for agency even if they do not have such a concept. But it is arguably anachronistic to discuss Shakespeare's situated agency as expressed in *Julius Caesar* in a manner that involves our ascribing to him our concept of a wristwatch.

Issues of anachronism do not really arise when we apply our concepts to the past. They arise, rather, when we ascribe our concepts to people in the past. We legitimately can use our concepts to discuss and explain the past only provided we have good philosophical reasons for believing those concepts apply universally. Yet when we discuss and explain the past using our concepts, we do not necessarily ascribe them to people in the past; on the contrary, the issue of conceptual relativism arises precisely in so far as we want to describe or explain past actions in terms of concepts that people in the past did not hold. Issues of anachronism arise, in contrast, precisely when we use our concepts to denote the content of actions, texts, or intentions. Anachronism infects cases where we ascribe inappropriate content to actions, beliefs, or intentions.

Anachronism often gets discussed, then, in relation to the legitimacy of our ever attributing meanings to texts other than those they could have had at the time they were written. Because Aristotle's notion of the polis differs from our concept of the state, we would be guilty perhaps of a pernicious anachronism if we were to discuss his text as if it were a treatise upon the state. Is it anachronistic to ascribe to a text a meaning that its author could not have intended it to bear? Are the only legitimate readings of a text those that seek to recover the intentions of the author in writing? Almost all attempts to speak to these questions fall foul, I believe, of what we might call a fallacy of textual meaning. That is to say, they proceed as if texts had meanings, whether these are singular or plural. Much might be gained, in contrast, from an insistence that meanings are always meanings for people. In itself a text consists only of ink on paper, oil on canvas, or whatever. It gains meaning only if one or more individual ascribes a meaning to it. This insistence that meanings are always meanings for specific people is the principle of procedural individualism to which I referred earlier. The grammar of our concepts precludes us from postulating meanings that we can not attribute, at least in principle, to specific people.

The principle of procedural individualism enables us to refine our understanding of the problem of anachronism. Although texts only have meanings for people, these people need not be the authors of the texts. Rather, they might be readers, including us ourselves. Hence we legitimately can attribute to texts meanings that their authors, and even their author's contemporaries, could not have intended them to bear. We can do so provided only that we are clear for whom the text had these meanings. We might say that Aristotle's text meant something to readers in the nineteenth century, or even that it means something to us. We might do so even if that meaning is not one Aristotle or his contemporaries could plausibly have ascribed to the text. In this respect, we can ascribe to texts meanings other than those they could have had at the time they were composed, and we can do so without falling prey to a pernicious anachronism.

Re-enactment

Anachronistic fallacies do not arise simply because we apply our concepts to the past or to texts. Rather, they arise when we ascribe to people beliefs or intentions that they could not have held. It is arguable, however, that we have no philo-

sophical grounds for ruling out the very possibility of someone having held a concept as opposed to our regarding it as highly unlikely that they did so. Whether or not people held any given concept is, after all, an empirical issue. Properly speaking, then, anachronistic fallacies occur when we ascribe to people beliefs or meanings that empirically we believe it to be extraordinarily unlikely that they held. Issues of anachronism are particularly pertinent, in other words, when we try to use our concepts to discuss the rather different beliefs or concepts that we ascribe to people in the past. Anachronism is a danger for our attempts to re-enact beliefs, ideas, or intentions.

The analysis of anachronism remains complex even after we thus narrow it down to attempts to re-enact past beliefs, ideas, or intentions. Some complexities surface once we recall that past beliefs or intentions are typically objects we postulate on the basis of textual evidence rather than objects to which we have any direct access. So, we postulate Aristotle's beliefs based on his books, his correspondence, and other types of evidence: the *Politics* does not simply present us with his beliefs. Even more complexities surface once we realize that the process of postulating beliefs inevitably creates a gap between these beliefs and any particular set of words in which they might be expressed. Although we could simply record the words Aristotle used in his *Politics*, our doing so would result only in a transcription rather than an attempt to identify his beliefs, ideas, or intentions: when we postulate his beliefs, we select the mode of expression by which we do so. In this respect, re-enactment is better described as ascription. We ascribe beliefs to people, but we do not do so on the grounds that we relive their mental lives or that we are using their modes of expression. Attempts to re-enact the beliefs of people in the past are matters of "making" rather than "matching." We do not compare or match our linguistic description with a past belief that is given prior to our so describing it. Rather, we make a phrase as a means of conveying the belief we are ascribing to someone on the basis of the historical evidence.

We might distinguish, then, between anachronism in the beliefs we ascribe and anachronism in the phrases we use to invoke these beliefs. Obviously anachronistic errors can arise when we ascribe beliefs to people about objects or concepts that had not been invented at the time they lived. Yet even these errors are at times better understood as cases in which we have not been sufficiently clear about the level of abstraction at which we are describing the relevant beliefs. If we write about Aristotle's view of the separation of powers, for example, a critic might object that Aristotle could not have held any beliefs about the separation of powers because the concept of the separation of powers only makes sense against a background of beliefs Aristotle did not hold. If we were using the phrase "separation of powers" in a narrow sense to refer to the executive and judicial branches of government being institutionally distinct from the legislature, the critic would no doubt have an excellent point. However, if we were using the phrase in a broader, more abstract sense to refer to, say, constitutional theories in which no single person or body had the final say on

all collective decisions, then it would seem far less clear that Aristotle did not hold the beliefs needed to consider such matters.

Conclusion

The observant reader might have noticed that I have almost defined anachronism out of existence. How did that happen? One way it happened was an implicit rejection of a fairly common way of thinking about anachronism. Often anachronism appears as a placing of an object in an inappropriate context; the appearance of a wrist-watch in a play about ancient Rome might stand as a paradigmatic case of anachronism so conceived. In contrast, I am dubious that we have objects and contexts with the degree of fixity that this analysis of anachronism appears to presuppose. Hence I have approached anachronism as an issue that arises when we map two narratives, webs of beliefs, or sets of concepts on to one another; one is our own, the other is that we ascribe to people in the past. Three issues stand out when we think about anachronism in this way. To begin, we need to ask about the conceptual reach of our own narratives, beliefs, or concepts. I have argued here that we can rebut conceptual relativism by philosophical arguments about the grammar of our concepts. It is, I believe, the apparently clear lack of universality of our concept of a wrist-watch that would make the appearance of one in *Julius Caesar* liable to charges of pernicious anachronism. Next we need to ask whose narratives, beliefs or concepts we want to map our own onto. I argued here that we legitimately can ascribe to actions or texts meanings that they did not have at the time they were performed or written; we can do so provided only that we are clear that the relevant meanings existed for later readers or even for ourselves. A producer of *Julius Caesar* thus might defend the appearance of a wrist-watch in the play by saying that his production was about the meaning the play had for him as a story about absolute power. Finally we need to ask how accurately our narratives, beliefs, or concepts capture those onto which we seek to map them. I argued here the degree of accuracy we require varies with the purpose and level of abstraction of our own narratives. The producer thus might argue, rather more ambitiously, that his production attempted to address questions of power at a sufficiently abstract level to translate what Shakespeare believed into our terms.

Notes

¹ In the philosophy of social science, there has been a high-on ubiquitous turn from positivist concerns with behavior, institutions, and general theories to analyses of meaning, practices, and interpretation. This turn challenges much of what passes for qualitative methodology just as much as it does quantitative methods or deductive models. Even a decade ago, this turn had become such a commonplace that it appeared only as the brief starting point for a textbook on the philosophy of social science: see Brian Fay, *Contemporary Philosophy of Social Science* (Oxford: Blackwell Publishers, 1996). It is unfortunate (but sadly not at all surprising) that

political scientists appear to be almost wholly uninterested in such questions or, more generally, in the philosophical status of their concepts and practices.

Book Notes

Book descriptions are excerpted from publisher's websites.

If you would like to recommend a book to be included in this section, email Joshua C. Yesnowitz, the assistant editor of QM, at jcyesnow@bu.edu

Anderson, Lisa. 2003. *Pursuing Truth, Exercising Power: Social Science and Public Policy in the Twenty-First Century*. New York: Columbia University Press. \$27.50 hardcover.

The conditions that shaped the rise and expansion of American social science are rapidly changing, and with them the terms of its relationship with power and policy. As globalization has diminished the role of the state as the locus of public policy in favor of NGOs, multinational corporations, and other private entities, it has raised important questions about the future of the social sciences and their universalist pretensions. As dean of Columbia University's School of International and Public Affairs, Lisa Anderson has a unique vantage point on the intersection of the social sciences, particularly political science, and the formation and implementation of public policy. How do, or should, the research and findings of the academy affect foreign or domestic policy today? Why are politicians often quick to dismiss professors as irrelevant, their undertakings purely "academic," while scholars often shrink from engagement as agents of social or political change? There is a tension at work here, and it reveals a deeper compromise that arose as the modern social sciences were born in the nursery of late nineteenth-century American liberalism: social scientists would dedicate themselves to the pursuit of objective, empirically verifiable truth, while relinquishing the exercise of power to governments and their agents. Anderson argues that this compromise helped underwrite the expansion of American influence in the twentieth century, and that it needs serious reexamination at the dawn of the twenty-first.

Collins, John, Ned Hall, and L. A. Paul (eds). 2004. *Causation and Counterfactuals*. Cambridge: MIT Press. \$40.00 paperback.

One philosophical approach to causation sees counterfactual dependence as the key to the explanation of causal facts: for example, events *c* (the cause) and *e* (the effect) both occur, but had *c* not occurred, *e* would not have occurred either. The counterfactual analysis of causation became a focus of philosophical debate after the 1973 publication of the late David Lewis's groundbreaking paper, "Causation," which argues against the previously accepted "regularity" analysis and in favor of what he called the "promising alternative" of the counterfactual analysis. Thirty years after Lewis's paper, this book brings together some of the most important recent work connecting—or, in some cases, disputing the connection between—counterfactuals and causation, including the complete version of Lewis's Whitehead lectures, "Causation as Influence," a major reworking of his original paper. Also included is a more recent essay by Lewis, "Void and Object," on causation by omission. Several of the essays first appeared in a special issue of the *Journal of Philosophy*, but most, including the unabridged version of "Causation as Influence," are published for the first time or in updated forms. Other topics considered include the "trumping" of one event over another in determining causation; de facto dependence; challenges to the transitivity of causation; the possibility that entities other than events are the

fundamental causal relations; the distinction between dependence and production in accounts of causation; the distinction between causation and causal explanation; the context-dependence of causation; probabilistic analyses of causation; and a singularist theory of causation.

Davis, James W. 2005. *Terms of Inquiry: On the Theory and Practice of Political Science*. Baltimore: Johns Hopkins University Press. \$19.95 paperback.

Following World War II, American social science research became increasingly characterized by its adherence to the scientific method. The application of the scientific method and efforts to achieve precision in the social sciences opened the doors to large research programs, particularly in political science and international relations. However, as research became less accessible to the general public, policymakers began to ignore the findings and advice given to them by political scientists. The disconnect between policy and academic debates was exacerbated by the failure of empirical research to predict important developments, such as the fall of the Soviet Union and the peaceful resolution of the Cold War. James W. Davis engages with the widespread dissatisfaction within the social sciences applying the scientific method to the study of social outcomes. *Terms of Inquiry* critically examines central claims and assumptions made by proponents of the scientific method in general, as well as the specific problems confronting the social sciences in particular. Davis seeks to develop a middle ground between the uncritical application of the scientific method in pursuit of empirical truths and the postmodernist assertion that there is no foundation upon which to build an edifice of social science. Although interested in fundamental questions of scientific inquiry, Davis is nonetheless concerned by the increasing irrelevance of the field of political science to the actual practice of politics. In an effort to re-link empirical research to pressing questions of public policy, *Terms of Inquiry* provides a much needed discussion of practical research methods in a critically important discipline.

Franzosi, Roberto. 2004. *From Words to Numbers: Narrative, Data, and Social Science*. Cambridge: Cambridge University Press. \$35.00 paperback.

Departing radically from traditional content analysis approaches to the quantitative study of texts, this work is based on a linguistic theory of narrative, rather than the ad hoc approaches of context analysis. This book illustrates a set of tools—story grammars, relational data models, and network models—that can be profitably used for the collection, organization, and analysis of narrative data in socio-historical research. A story grammar, or Subject-Action-Object and their modifiers, is the linguistic tool the author uses to structure narrative for the purpose of collecting event data. Relational database models make such complex data collection schemes practically feasible in a computer environment. Finally, network models are a statistical tool best suited to analyze this type of data. Driven by the metaphors of the journal (from ... to) and the alchemy (words into numbers), the book leads the reader throughout a number of paths, from substantive to methodological issues, across time and disciplines: sociology, linguistics, literary criticism, history, statistics, computer science, philosophy, cognitive psychology, and political science.

Katznelson, Ira, and Barry R. Weingast (eds). 2005. *Preferences and Situations: Points of Intersection between Historical and Rational Choice Institutionalism*. New York: Russell Sage. \$45.00 hardcover.

A scholarly gulf has tended to divide historians, political scientists, and social movement theorists on how people develop and act on their preferences. Rational choice scholars assumed that people—regardless of the time and place in which they live—try to achieve certain goals, like maximizing their personal wealth or power. In contrast, comparative historical scholars have emphasized historical context in explaining people's behavior. Recently, a common emphasis on how institutions—such as unions or governments—influence people's preferences in particular situations has emerged, promising to narrow the divide between the two intellectual camps. In *Preferences and Situations*, editors Ira Katznelson and Barry Weingast seek to expand that common ground by bringing together an esteemed group of contributors to address the ways in which institutions, in their wider historical setting, induce people to behave in certain ways and steer the course of history. The contributors examine a diverse group of topics to assess the role that institutions play in shaping people's preferences and decision-making. For example, Margaret Levi studies two labor unions to determine how organizational preferences are established. She discusses how the individual preferences of leaders crystallize and become cemented into an institutional culture through formal rules and informal communication. To explore how preferences alter with time, David Brady, John Ferejohn, and Jeremy Pope examine why civil rights legislation that failed to garner sufficient support in previous decades came to pass Congress in 1964. The essays in this book focus on preference formation and change, revealing a great deal of overlap between two schools of thought that were previously considered mutually exclusive. Though the scholarly debate over the merits of historical versus rational choice institutionalism will surely rage on, *Preferences and Situations* reveals how each field can be enriched by the other.

Morris, Irwin L., Joe A. Oppenheimer, and Karol Edward Soltan (eds). 2004. *Politics from Anarchy to Democracy: Rational Choice in Political Science*. Stanford: Stanford University Press. \$21.95 paperback.

Although the study of politics dates to ancient Greece, the basic questions that interested those earliest political scientists still linger with us today: What are the origins of government? What should government do? What conditions foster effective governance? Rational choice theory offers a new means for developing correctable answers to these questions. This volume illustrates the promise of rational choice theory and demonstrates how theory can help us develop interesting, fresh conclusions about the fundamental processes of politics. Each of the book's three sections begins with a pedagogical overview that is accessible to those with little knowledge of rational choice theory. The first group of essays then discusses various ways in which rational choice contributes to our understanding of the foundations of government. The second set focuses on the contributions of rational choice theory to institutional analysis. The final group demonstrates ways in which rational choice theory helps to understand the character of popular government.

Shapiro, Ian. 2005. *The Flight from Reality in the Human Sciences*. Princeton: Princeton University Press. \$24.95 hardcover.

In this captivating yet troubling book, Ian Shapiro offers a searing indictment of many influential practices in the social sciences and humanities today. Perhaps best known for his critique of rational choice theory, Shapiro expands his purview here. In discipline after discipline, he argues, scholars have fallen prey to inward-looking myopia that results from—and perpetuates—a flight from reality. In the method-driven academic culture we inhabit, argues Shapiro,

researchers too often make display and refinement of their techniques the principal scholarly activity. The result is that they lose sight of the objects of their study. Pet theories and methodological blinders lead unwelcome facts to be ignored, sometimes not even perceived. The targets of Shapiro's critique include the law and economics movement, overzealous formal and statistical modeling, various re-ductive theories of human behavior, misguided conceptual analysis in political theory, and the Cambridge school of intellectual history. As an alternative to all of these, Shapiro makes a compelling case for problem-driven social research, rooted in a realist philosophy of science and an antireductionist view of social explanation. In the lucid—if biting—prose for which Shapiro is renowned, he explains why this requires greater critical attention to how problems are specified than is usually undertaken. He illustrates what is at stake for the study of power, democracy, law, and ideology, as well as in normative debates over rights, justice, freedom, virtue, and community. Shapiro answers many critics of his views along the way, securing his position as one of the distinctive social and political theorists of our time.

Woodward, James. 2005. *Making Things Happen: A Theory of Causal Explanation*. Oxford: Oxford University Press. \$24.95 paperback.

This book develops a manipulationist theory of causation and explanation: causal and explanatory relationships are relationships that are potentially exploitable for purposes of manipulation and control. The resulting theory is a species of counterfactual theory that avoids the difficulties and counterexamples that have infected alternative accounts of causation and explanation, from the Deductive-Nomological model onwards. One of the key concepts in this theory is the notion of an intervention, which is an idealization of the notion of an experimental manipulation that is stripped of its anthropocentric elements. This notion is used to provide a characterization of causal relationships that is non-reductive but also not viciously circular. Relationships that correctly tell us how the value of one variable Y would change under interventions on a second variable X are invariant. The notion of an invariant relationship is more helpful than the notion of a law of nature (the notion on which philosophers have traditionally relied) in understanding how explanation and causal attribution work in the special sciences.

Announcements

Giovanni Sartori Award

This award is granted to the best book developing or applying qualitative methods, published in the previous calendar year. This award honors Giovanni Sartori's work on concept formation and qualitative methods, and especially his contribution to helping scholars think about problems of context as they refine concepts and apply them to new spatial and temporal settings.

Recipients: Henry E. Brady and David Collier, eds. *Rethinking Social Inquiry: Diverse Tools, Shared Standards*. (Lanham, MD: Rowman & Littlefield, 2004).

Committee: Deborah Avant, George Washington University; David Waldner, University of Virginia; and Kurt Weyland, University of Texas, Austin (Chair).

Citation: The selection committee read 34 very good books,

among them a number of truly excellent volumes. After careful deliberation, the committee decided to award the prize to *Rethinking Social Inquiry: Diverse Tools, Shared Standards*. This fundamentally important collection was edited by Henry Brady and David Collier; Jason Seawright also made central contributions by co-authoring six chapters. The committee was impressed by the wide range of high-caliber contributors assembled by the editors. Scholars of diverse methodological persuasions reflect on the contributions that qualitative methods can make and on their relationship to quantitative approaches. Guided by the quest for shared standards, they demonstrate that the distinctive tools used by qualitative researchers have their own logic and value. By analyzing a variety of crucial issues and problems, they convincingly establish the validity of qualitative methods and advance powerful counter-arguments against the critics of qualitative approaches.

The collection shows, for instance, that issues of case selection need not cripple small-N research and that the often-heard recommendation to avoid selection bias by increasing the number of observations has its own downsides. For instance, it can lead to conceptual stretching and exacerbate causal heterogeneity, which makes valid causal inference difficult. By offering guidance on proper concept formation and causal inference, Brady, Collier, and their contributors highlight crucial aspects of the research process that are sometimes neglected by quantitatively oriented methodologists. In fact, they demonstrate that rigorous reasoning based on causal process observations can correct the findings that a statistical analysis may suggest. In these and many other ways, *Rethinking Social Inquiry* offers foundational insights for qualitative methodology.

The volume provides a powerful yet measured response to the critics of small-N approaches. In the 1990s, qualitative researchers were asked to change their ways and take much more inspiration from quantitative research. The Brady and Collier collection constructively engages these arguments: It encourages dialogue between qualitative and quantitative researchers, but emphasizes the full legitimacy and high potential payoffs of the qualitative approach. In this way, it leads a way out of the methodological battles that our quarrelsome discipline has seen. The committee hopes that *Rethinking Social Inquiry* will be read widely and assigned in many courses.

Honorable mention: Kristen R. Monroe, *The Hand of Compassion: Portraits of Moral Choice During the Holocaust*. (Princeton, NJ: Princeton University Press, 2004).

Citation: The committee also wants to honor the deeply moving book by Kristen Monroe, *The Hand of Compassion: Portraits of Moral Choice during the Holocaust*, which provides an exemplary application of qualitative methods. Through in-depth interviews with people who saved Jews from the Nazis, she establishes a very important theoretical point: Their identity compelled these rescuers to act as they did; they did not see themselves as having a choice. This important conclusion raises the crucial question whether our actions necessarily are the product of choice, as some theoretical approaches assume. Due to its great methodological and theoretical importance, we

awarded Prof. Monroe's book an "honorable mention."

Alexander George Award

This award is granted to the best article or book chapter developing or applying qualitative methods, published in the previous calendar year. This award honors Alexander George's prominent role in developing and teaching qualitative methodology, in particular the comparative case-study method.

Recipients: Henry E. Hale, "Divided We Stand: Institutional Sources of Ethnofederal State Survival and Collapse." *World Politics* 56, no. 2 (January 2004): 165-193; and James Mahoney and Gary Goertz, "The Possibility Principle: Choosing Negative Cases in Comparative Research." *American Political Science Review* 98, no. 4 (November 2004): 653-669.

Committee: Miriam Fendius Elman, Arizona State University; James Goldgeier, George Washington University; and Emily Goldman (Chair), University of California at Davis.

Citation: Why do so many ethnofederal states collapse? In "Divided We Stand: Institutional Sources of Ethnofederal State Survival and Collapse," Henry Hale addresses this important question, offering not only an elegant argument for ethnofederal state stability and failure, but also a new way of understanding both historical and contemporary cases of ethnofederalism. Starting from a well-crafted theoretical proposition—if ethnofederal states lack core ethnic units, then they are less likely to collapse—Hale expertly uses positive and negative cases to reinterpret state survival and collapse in Nigeria, the Soviet Union and Russia, and over a dozen other cases of ethnofederalism. Hale argues that ethnofederal states are more likely to collapse when they contain a core ethnic region, or a single ethnic federal region that enjoys dramatic superiority in population. Conversely, ethnofederal states where a dominant group is divided into a number of federal regions are more stable because the dominant group faces major obstacles to collective action that might challenge the union of the state, minority ethnic regions perceive fewer threats, and political entrepreneurs cannot easily promote collective imagining of an independent core nation-state. At a time when we are wondering whether or not Iraq will hold together under its new constitution, this study of previous cases of ethnofederal states provides important lessons for the conditions under which we can expect ethnofederal states to survive. As Hale's findings show, it is not so much a question of whether there is a dominant ethnic or religious group in a country but whether or not that group is divided into different administrative units.

Citation: In their article, James Mahoney and Gary Goertz take on an important problem in qualitative research: the nonevent. The Possibility Principle advises researchers to select only negative cases where the outcome of interest is possible. The challenge of selecting an appropriate sample of cases is usually cast in terms of distinguishing positive from negative cases. But a prior issue involves the construction of the relevant population, and this involves distinguishing negative cases (or nonpositive cases that are relevant because the outcome

of interest is possible) from irrelevant cases (or nonpositive cases where the outcome of interest is impossible). Mahoney and Goertz demonstrate why including irrelevant cases can produce erroneous causal inferences, highlighting another way in which the methodological imperatives of small-N qualitative research differ from those of large-N statistical analysis. In a masterful discussion of the challenge that negative cases pose for qualitative research, Mahoney and Goertz offer a clear set of user-friendly guidelines for differentiating relevant from irrelevant cases. Political scientists who reference negative cases in their work now know better how to select non-events for study, and how we might justify our choices.

Sage Award

This award is given to the best paper developing or applying qualitative methods, presented at the previous Annual Meeting of the American Political Science Association. This award honors the contribution of Sara and George McCune to the field of qualitative methods, through their role in founding Sage Publications and developing it into a leading publisher in the field of social science methodology.

Recipient: Colin Elman, for "Theoretical Typologies in the Qualitative Study of International Politics." [Editor's Note: The Sage Paper Award committee reviewed the version of the paper presented at APSA. A substantially revised essay was subsequently published as "Explanatory Typologies in Qualitative Studies of International Politics," *International Organization*, Vol. 59, No. 2 (Spring 2005): 293-326.]

Committee: Jane Cramer, University of Oregon; Marc Morjé Howard (chair), Georgetown University; and Meredith Weiss, DePaul University.

Citation: The unanimous winner of the 2005 Sage Paper Award is Colin Elman, for his paper titled "Theoretical Typologies in the Qualitative Study of International Politics." The committee found this paper to be conceptually innovative, theoretically rich, and methodologically ground-breaking. The paper begins with an impressive synthesis of the extensive but largely arcane literature on typologies, greatly aiding in the cumulation of knowledge. Then it innovatively describes analytic moves to better manipulate typologies, including common pitfalls in the handling of typologies. The straightforward application of these methodological techniques to prominent international relations theories of offensive, defensive, and neo-classical realism is exemplary, and plainly brings to life the power of self-conscious application of these techniques.

The paper's analysis of the different ways in which typologies are used, and especially its constructive suggestions for a more self-conscious and cumulative approach to developing typological procedures, will have a tremendous impact on the field of international relations. Because this paper cumulates knowledge, is accessible to a broad audience, is substantively interesting and methodologically innovative, it should also serve as a model to scholars in other subfields who work with similar methodological frameworks in quite different theoretical and empirical contexts. As such, it exempli-

fies the breadth and strength of the “field” of qualitative methods, and is a deserving winner of this year’s Sage award.

**Call for Panel Proposals and Papers:
Qualitative Methods Section at 2006 APSA conference**

The Qualitative Methods Division welcomes panel proposals and papers focused on the broad spectrum of research tools associated with qualitative methodology. These include but are not limited to: the case-study method; small-N analysis; concept analysis; the logic of inquiry; comparative and historical methods; constructivism; and interpretive methods. We welcome submissions from all subfields of political science as well as proposals for complete panels.

The overall theme for APSA 2006 is “Power Reconsidered.” We encourage proposals that deal with the methodology of conceptualizing or measuring “power” and encourage submissions to explore the array of usages of the term and the methodological implications of this diversity. We welcome investigations into how methodological choices relate to wider power structures, and invite submissions that explore power within the discipline of political science itself, particularly with respect to methodological issues. In recent years, scholars

have called for greater attention to methodological approaches beyond the large-N, quantitative, and/or rational choice institutionalist approaches that have tended to dominate in the last few decades. Indeed, the Organized Section on Qualitative Methods is one expression of diverse calls for greater recognition of methodological diversity. Thus, papers that address forms of power within the methodology of political science as well as conceptualizing the nature of power are particularly welcome.

To submit a proposal, go to <http://www.apsanet.org>. The deadline to submit a proposal is November 15, 2005.

**Call for Participants:
Short Course at 2006 APSA conference**

Organizers are seeking presenters for a Wednesday short course on methods of studying public opinion before public opinion polling. If you yourself are doing work on this question or you want to recommend someone who is, please contact Brian J. Glenn at bjglenn@hamilton.edu. Professor Glenn will be putting together a participant list early in 2006 and the goal is to assemble a variety of cutting-edge approaches.

**Qualitative
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