

# 1.

## The Political Science Discipline<sup>1</sup>

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Political science is a distinct discipline. What unites its practitioners is a common focus on an interrelated set of public concerns or outcomes—such as order, democracy and community—that have been identified in a rather continuous theoretical discourse of nearly three millennia. Political science has evolved a division of labor institutionalized as a set of fields, each of which tackles different aspects of the common concerns. In the field of political theory, political scientists have placed the major contributors to this discourse in a canon. Political theorists not only extract from this canon the compelling concerns that drive the entire discipline, but reformulate those concerns in a way that keeps them current and vital. Furthermore, political theorists seek to provide reasoned judgment as to which outcomes across a variety of dimensions should be most preferred. In the field of comparative politics, in reference to those concerns in the canon, political scientists ask why different politics arrive at different outcomes. In this field, outcomes of concern get specified as dependent variables, and theoretically derived explanatory factors are specified as independent variables. In the field of political institutions, political scientists take one dependent variable (that of regime type) and address questions of institutional maintenance and adaptation. Up till now, its energies have focused on one value of this dependent variable (that of democracy) and in a single setting (that of the United States). In current practice, the field of political institutions models the inner workings of a few American institutions, but by extension addresses broader questions about the equilibrium conditions supporting democratic regimes. In the field of international relations, political scientists analyze another dependent variable (that of order) in a unique situation (namely, without sovereign authority). In the field of public policy, political scientists seek to get a grip on political change, so that preferred outcomes can be fostered. Finally, there is a field in methodology, where political scientists develop tools to address questions in

each of the substantive fields. With a collective focus on a set of common concerns or outcomes, there is impressive intellectual coherence in the political science discipline.

However, there is institutional chaos. The purpose of this essay is to suggest ways in which the institutional anarchy might adjust itself to the intellectual order. To fulfill this purpose, I describe in Section I (Symptoms of Indiscipline) the institutional status quo, pointing to the signs of indiscipline. In Section II (The Order of Things), I show that lurking behind our institutional chaos there is an implicit order. In Section III (Introducing the Discipline), I provide a discursive outline of a one-year introductory sequence in political science that is designed to make explicit our intellectual order. In Section IV (Alternative Visions of the Discipline), I outline some alternate visions of order, ones that I reject in favor of the vision outlined in Section II. In Section V (Concluding Questions), I address such questions as “whose ox is being gored?” were my proposals to be implemented.

### Symptoms of Indiscipline

The symptoms of institutional incoherence are manifold. Here I highlight four of them.

#### *An Impoverished Curriculum*

There is no standard introductory course in political science across the country. In fact, most departments long ago abandoned the idea of a standard curriculum altogether. Rarely are there prerequisites for advanced courses, meaning that upper-division courses in political science have students who have had no formal introduction to the discipline. In most upper-division courses, professors have no expectations that their students have any common set of tools to address more advanced material. Honors theses are all too often projects that are divorced from specific problems that were encountered in course work.

An examination of college catalogue write-ups in which the political science major is described supports these claims. From a list of all accredited colleges and universities, I selected through a random procedure thirty-four institutions. The introductory sequence ranged from one semester (in two programs) to eight semester-long courses (in one program). The content of these introductory courses was highly varied, with some interesting deviations. One program required a course on the Texas Constitution, another Research and Writing in Public Affairs, and a third Current Political Ideologies. Only nine (26 percent) of the programs made advanced study in political science contingent on having completed the introductory sequence. In nine (26 percent) of the programs, students

have a choice among introductory courses, with no common foundation. In five places, there is no history of political theory required for the major; in seventeen, no statistics; in eight, no formal theory; in five, no international relations; in four, no comparative politics; and in only one is American politics not required for the major. In less than a quarter of the colleges and universities surveyed is there any mention of a survey of the discipline as a whole, as opposed to its component fields. In the sample were four universities that were ranked in the 2001 survey of Ph.D. programs in political science. The undergraduate programs in political science at these ranked universities were less likely to make the introductory course required for upper-division study (three out of four did not) than the nonranked universities and teaching colleges. From this informal survey there is no way to evaluate the intellectual merit of these curricula. The data show clearly, however, that there is no standard undergraduate curriculum for our discipline and that many of our advanced courses are taught with no expectation that the enrolled students have a common disciplinary framework.<sup>2</sup>

A similar tale has been well told in regard to our graduate curricula. Schwartz-Shea (2001) analyzed doctoral requirements in political science for fifty-seven programs in leading American universities. She found great variance in requirements across programs. Sixteen percent of the programs required competency in a foreign language; 66 percent required competency in quantitative analysis; 49 percent have a required course that covers issues in the philosophy of science; 46 percent of the programs have a required course that covers the scope and/or history of the discipline; 9 percent require a course covering qualitative methods; and 30 percent of the programs have no required courses (though some of them require competencies) at all.

In her scrutiny of required courses and Website blurbs, she categorizes three ideal-type graduate programs: one with a strong bias toward statistics and formal training; one with a clear commitment to balance quantitative and qualitative coursework; and one that is an ambiguous mix of the first two. In terms of departmental rank, she shows that there are several leading Ph.D. programs in each of these categories, and none is associated with the preeminent departments. This result shows, and here she is borrowing the imagery of Gabriel Almond (1990), a “separate tables” approach to graduate teaching within the discipline.

#### *Identification of Fields, Courses, and Job Openings through the Use of Proper Nouns*

Rather than consolidating around a common core of questions, the courses we teach and jobs we advertise are typically named with proper nouns. The largest presently existing field in political science (American politics) is named after a

single country. Courses with our highest enrollments are about particular countries or regions. Advertisements for jobs rely heavily on matching ideal applicants with real estate. A recent perusal of the *APSA Personnel Service Newsletter* (June 2001) listed jobs demanding expertise in America, Texas, Eastern Europe, South Asia, post-Soviet Republics, Middle East, and Africa. Our course offerings are replete with proper nouns. At Stanford University, keywords in course titles include "America," "East Asia," "Latin America," "China," "Tropical Africa," "West Europe," "Russia," "Japan," "Justice Marshall," and "Rousseau." It would be useful to compare course offerings in political science departments around the country with the word "Japan" as opposed to those given in astronomy departments with the word "Pluto." (There are no specific planets in the course titles in the astronomy department at Stanford.) Proper nouns are evocative, but they hardly encourage the search for general processes. We therefore organize ourselves in a way that minimizes the chances for scientific discovery.

### *Proliferation of Fields as if They Were Ethnicities*

Rather than consolidating around a division of labor encompassing several well-defined fields, the discipline is fragmenting into an ever-increasing number of self-proclaimed fields. In our "State of the Discipline" (SOD) volumes, it is nearly impossible to find an intellectual order. In the last decadal publication, Schwartz-Shea (2001) sardonically pointed out, there were back-to-back chapters on "Feminist Challenges to Political Science" and "Formal Rational-Choice Theory: A Cumulative Science of Politics." This organization heralded only disarray. And the fragmentation of the discipline continues apace. The number of field papers has increased from nineteen in the 1990s version to thirty in the 2000s version. This is largely a function of interest-group politics rather than intellectual incoherence. The 2000s version of the SOD volume was originally planned with twenty-nine papers, but spokespersons for one group-defined field were outraged that they were neglected, and successfully lobbied for an additional paper. Fragmentation abounds as well in comparative politics, where the ranches of the 1960s became the ranchettes of the 1970s, and we are now facing fields constituted by single-family dwellings. In the last throes of the Social Science Research Council's joint area committees, Koreanists broke from the Asia committee to have their own research community. Universities, with support of political scientists, are creating centers for Afghanistan studies (University of Nebraska, Omaha) and centers for Basque studies (University of Nevada, Reno).<sup>3</sup>

Moreover, we have thirty-four sections each offering panels at the annual convention. A cursory look at the organized sections—see Table 1.1—suggests chaos.

**TABLE 1.1: APSA Sections**

Comparative Democratization
Comparative Politics
Conflict Processes
Ecological and Transformational Politics
Elections, Public Opinion, and Voting Behavior
Federalism & Intergovernmental Relations
Foreign Policy
Foundations of Political Thought
Human Rights
Information Technology and Politics
International History and Politics
International Security and Arms Control
Law and Courts
Legislative Studies
New Political Science
Political Communication
Political Economy
Political Methodology
Political Organizations and Parties
Political Psychology
Politics and History
Politics and Literature
Politics and Society in Western Europe
Presidency Research
Public Administration
Public Policy
Race, Ethnicity, and Politics
Religion and Politics
Representative and Electoral Systems
Science, Technology, and Environmental Politics
State Politics and Policy
Undergraduate Education
Urban Politics
Women and Politics

To be sure, the organized sections of the American Political Science Association were never planned with a synoptic vision. Any group that writes a constitution, recruits a paying constituency, and forms a leadership group capable of electing officers and organizing panels is listed as an organized section. The list

is therefore unsurprisingly inchoate, and no master narrative can account for the distribution of sections. But there is a message here. Institutionally, as reflected in our organizational structure, we appear to be getting increasingly fragmented, limiting our ability to incorporate findings from related fields, and thus reducing potential gains from our division of labor.

### *Debates over Methods Trumping Reports on Significant Findings*

Our discipline is a contentious one. Some argue that the great debates among us help generate new ideas and approaches. Others argue that these debates degenerate into ideological warfare cloaked in the language of epistemology. But in either case, the fact is that debates among political scientists today are much more likely to address such questions as to whether historical institutionalism is getting enough respect than as to whether opportunity structure or grievances best accounts for the breakdown of civil order. Or to use another example, political scientists seem more engaged with the question whether rational choice is getting too much respect than they are as to whether Brian Barry (2001) successfully purged multiculturalism from liberalism. Methodological posturing seems to trump substantive findings and reasoned political argument about social goals. Debates as to how best to resolve different substantive or theoretical findings reflect a common discipline; debates about epistemology and ontology are debates reflecting the absence of a discipline.

It is therefore not surprising that the *New York Times* reports on political science in its "Arts and Ideas" page where culture wars are fought but not in "Science Times" where findings are reported. In an interchange on February 26, 2000, two prominent colleagues argued over the promise of a particular methodology, and the opportunity costs of training students in that methodology. To be sure, political scientists get quoted often in new articles interpreting elections and catastrophes abroad. But we have no Carl Sagan or Alan Krueger interpreting the scientific knowledge generated by our discipline to interested scholars in other disciplines or to the general public. (I'm reminded of a quip from Nelson Polsby in a debate with Martin Shapiro decades ago on whether our introductory courses should emphasize science or civics. Polsby conceded to Shapiro that the former might be better, but asked if we professors report only on scientific discoveries, what do we do after the first week?) We display ourselves to the outside world as undisciplined.<sup>4</sup>

### **The Order of Things**

There are four substantive fields in political science: political theory, comparative politics, political institutions, and international relations.<sup>5</sup> There is one field

(methodology) that develops tools to address the methodological problems in each of the substantive fields. There is also an applied field (public policy) whose practitioners address policy problems relying on the substantive knowledge and methodological skills developed in the nonapplied part of the discipline. In this section, I will first address the most glaring omission to my characterization of the fields: Where is the field of American politics, the only field almost universally required for majors in American political science departments? I will then show that our professional life is largely (but not universally) conditioned on the six fields that I have identified. Finally in this section, I will reflect on the overall structure of the four substantive fields. It will become clear that I am not, as Harry Eckstein (1973) tried so gallantly and unsuccessfully to do, creating coherence through an abstract deductive exercise, but rather reporting on a coherence that most political scientists feel but rarely articulate.

But where is American politics? The first time I taught the course to be described in Section III, I invited graduate students to sit in on the lectures and meet weekly with me in an informal seminar I called "Teaching Introductory Political Science." One student from Taiwan expressed her amazement on how theoretically foundational the field of American politics (as I still called it) was. She confessed to having as an undergraduate ignored that field and substituted for it Chinese politics, largely a descriptive field. I realized immediately that as our discipline internationalizes, calling the field that has generated a huge amount of theory (its ratio of exports to imports from other fields in our discipline is probably the highest, measured in terms of footnotes) with the parochial name "American politics" does that field a disservice.

As currently practiced, American politics can be bifurcated and then included in two separate fields: comparative politics and political institutions. It makes good sense to have the American case in cross-sectional analysis done in the comparative field. Ignoring the American case in comparative politics in a more internationalized discipline makes no sense. It makes equally good sense to take the part of American politics that is often referred to as positive political economy, and to have it form an essential part of a fourth substantive field, political institutions. In much of today's field of American politics, scholars scrutinize a single institution (democracy) and ask how such a system operates and whether existing institutional elements subvert democratic goals. To be sure, the whole field of political institutions must encompass a far greater set of institutions than democratic ones; but research realities have privileged the study of democracy, and political scientists therefore understand the inner workings of its institutions better than other political institutions.

By including this second part of what was once the American politics field on a wider canvas of political institutions, its practitioners will be induced (and there is already a trend in this direction) to rely on data from institutions far

broader than the U.S. Congress to address their research problems. It should be admitted that the apparently excessive attention to the House of Representatives in this field was justified in that the institution was well structured and long-lived. Thus reasonable data (currently being put on a firmer theoretical foundation) are available on the median voter on the floor and for each party over a large number of congresses. Theory and its testing are ideal with these data. But now it is time (as practitioners recognize) to perform tests of models developed for the U.S. House of Representatives to other democratic legislatures, even outside of the United States. John Londregan's study of Chile's parliamentary committeees (2000) is a model for this expansion of the study of political institutions outside of the U.S. Congress. If my paper brings a single change to our collective practice, it would be to divide the field of "American politics" such that the American case fits naturally into comparative politics and its work on democratic institutions becomes foundational for the newly named field of political institutions.

A true reformer would surely want to tinker more with our field names. Comparative politics includes much case-study work that is hardly comparative. International Relations has dealt far more with interstate than international relations. But these infelicities hardly matter; in comparison, "American politics" sends strong and incorrect parochial signals, and thus reform is called for. With that one change in names and bifurcation of a field, I believe that our current field division reveals a disciplinary coherence far greater than generally acknowledged. Consider the analysis of Ph.D. programs by Schwartz-Shea (2001). She finds that 96.5 percent of the programs provide a "major" in the field of "American"; 95 percent in "comparative"; 95 percent in "international relations"; and 79 percent in "political theory." The only other fields that have majors in any of these fifty-seven departments are "formal theory" and "methodology." And these are rare. Thus my analysis of the four substantive fields reflects the everyday practice of graduate education.

What unites the four substantive fields that I have identified? There is no deductive logic for which political theory, comparative politics, political institutions, and international relations constitute four nodes at the end of a tree. Comparative politics is open to all dependent variables, but international relations only to one. Comparativists tend to privilege macrofactors and students of political institutions tend to favor the micro. But international relations operates at both levels. The division of labor is not neat and clean. But the centripetal force of the enduring questions drawn from the political theory canon allows all practitioners to draw a link between their findings and how they address the common concerns that unite our wider discipline.

The natural question that follows is what does it matter that I can organize our professional life in a small set of categories not linked by a deductive logic?

One answer has to do with the cumulation of research. To the extent that we develop a discipline, we will internalize a set of rules of correspondence that will enable us better to determine whether apparently contradictory findings are in fact anomalies or merely the result of different definitions, assumptions, or variable specifications. In a discipline, therefore, we will learn more from each other. A second answer is that once we all acknowledge the underlying structure of our collective enterprise, we will agree on how best to teach it. To that end, I propose the outline of a common introduction that is both inclusionary and coherent.

### Introducing the Discipline

An introductory course in political science should impart to students a systematic understanding of politics, enable them to enroll with profit in upper-division courses that broaden and deepen that understanding (and this entails the ability to read professional contributions to the discipline), and motivate them in the longer term to make contributions to it (say, in an Honor's thesis). In this section, I shall outline and expand upon an introductory course that I have taught in several iterations. It integrates the four substantive fields and provides initial exposure to methodology, enabling faculty to offer upper-division courses that assume a broad understanding of the scope and methods of our discipline. While public policy is not introduced in this course, upper-division courses in public policy can be designed with the assumption that students have basic analytic skills and a clear sense of the goals that public policy might seek to achieve.

### *Political Theory*

My opening lecture points to three concerns that are drawn from the political theory canon, but retain contemporary relevance. The first concern I address surrounds the question of order. How do states emerge? When do they break down? Can revolution ever be justified? How much freedom ought individuals sacrifice for protection by a state? How is order maintained outside the purview of the state, or between states where no interstate authority governs? A second concern surrounds the question of democracy. How and when do democracies emerge? How are they sustained when there are so many incentives for potential dictators to subvert it? How are they sustained in light of the fact that for a wide set of preference orderings, voting cannot ensure stable group decisions? What are the best institutional safeguards for democratic rule? How are democracies different from the alternatives? A third concern surrounds the question of the community. What are the different forms of communal solidarity? What is the basis for national solidarities? Are solidarities different for nations than they are for other

communities such as classes, or races, or castes? Under what conditions are the boundaries of the state commensurate with the boundaries of the nation? Why do people who share a national consciousness seek to have a polity that is owned by them? Has this always been the case, and if not, why are things different now? Are conflicts within nations pursued differently than those that cross nationality groups, and if so why?

To be sure, there are many other concerns that motivate political theorists. There are concerns about equality, about legitimacy, and about justice. The political theory tradition is replete with these consequential political concerns. In the course that I propose, time and digestibility require no more than three concerns and their subsidiary questions. For reasons that will become clear, order and democracy are perennial. My third concern changes from year to year, but for purposes of exposition, I will discuss community. In other years, I have examined economic well-being, state strength, and equality as focal concerns.

Political theory provides the best entrée to these concerns (and an almost limitless archive of consequential public concerns). I follow my opening lecture with one that provides a *tour d'horizon* of the political theory canon (with quicker glosses than provided in *Sophie's World*), so students will know from where my selections were made that give structure to each of the concerns. For order, I assign from Hobbes, Machiavelli, Marx, and Huntington. For democracy, I assign from Plato, Locke, J. S. Mill, and Dahl. For community, I assign from Rousseau, Hegel, Marx, Weber, Deutsch, Gellner, and B. Anderson. For each of these concerns and connecting to the other branch of political theory, I raise normative issues, for example on Hobbes's justifications for obedience to the Leviathan. I address as well issues of historicity, for example, why the literature on the sources of political community comes so late to the canon, as compared to the question of order. Finally I pay attention to issues of narrative, for example, Weber's analyses of religious communities, to show how narrative plays a role in both description and explanation in this tradition. But the canon's greatest contribution to our discipline is in the setting of an intellectual agenda through raising important questions about politics. Some of the greatest mathematicians earned their fame by setting up conundrums for other mathematicians to solve. In the political theory section of the course, I portray the theoretical tradition in a similar light, as setting up the problems that the wider discipline seeks to solve.

### **Comparative Politics**

The move from political theory to comparative politics is therefore almost seamless. The notion of concerns translates naturally into the notion of outcomes or "dependent variables." Students initially have a difficult time thinking in terms

of variables. To make this transition from concerns to variables, I take a passage from Montesquieu's *Spirit of the Laws* (Montesquieu [1748] 1900, book XIV, chap. 13). In that passage on the "Effects arising from the Climate of England," Montesquieu argues that the rule of law (or democracy) is more likely attained in polities that have inclement weather. Meanwhile tyranny is more likely when the climate is pleasant. I try to get students first to see that the independent and the dependent variables are both dimensions (good climate [bad climate]; rule of law [tyranny]). Because of their dimensionality, variables can have values on an infinite number of points. I ask the students to map Montesquieu's argument from each of the values on the independent variable to each of the values on the dependent variable. I then ask them to specify the mechanism that justifies the mapping. Montesquieu is obscure here, but he suggests that bad weather turns people testy, and testy people are more likely to challenge tyrants. Finally, I ask students to think about evidence in the real world that might lead us to question this theory, for example the long periods of tyrannical rule in frozen Russia. Mapping values on independent variables onto values on dependent variables, theorizing about the mechanisms that drive these mappings, and seeking real-world evidence that the mappings actually describe reality is, I tell the students, what the comparative politics field is about.

I then seek to give more precise specifications to the three chosen concerns in the political theory tradition in terms of dependent variables. Democracy in the context of comparative politics is no longer a concern, but a range of points on a dimension that (to use Schmitter's [1974] nice categorization) goes from monism to pluralism and which I call regime type. Order is an extreme point on a dimension that moves to social revolution (or civil war) at the other end. Here I use a mapping provided by Tilly (1978). Community is more problematic as a dimension, as it can be specified in multifarious ways. I lecture on the different ways community can be dimensionalized, but choose one dimension for purposes of the course. On one end of the dimension are polities that are nationally heterogeneous and at the other end are polities that are nationally homogeneous. For each of these dependent variables, I summarize the current theoretical accounts for variation, leaving the question open as to which explanations are most powerful. Theories differ for each dependent variable, but I include for at least one of the dependent variables class-based theories, historical path theories, modernization theories, and cultural/discursive/postmodern theories.

I then teach the comparative method by employing it. I take students on a tour of five countries, one at a time, in which there is significant variation built in on the values of the dependent variables, both cross-sectionally and diachronically. Because I have relied on Barrington Moore's *Social Origins of Dictatorship and Democracy* (1966) as a text for the dependent variable "regime type," the countries I choose are England, France, Japan, Russia, and India. (It would

fit my pedagogical goals to make the United States one of the countries, since in the third section of the course I will be taking much of America out of the field that is now called American politics. I do have students read the chapter on the U.S. Civil War in Moore's *Social Origins* for one of their assignments, in which I ask them to assess one of the theories in light of a new "case.") Theda Skocpol's *States and Social Revolutions* (1979) not surprisingly serves as the complementary text for the dependent variable "order." For each country, I lecture on the value for that country on each dependent variable (or the changing values over time), and ask which theories are most successful in accounting for the outcome(s). Success is measured in terms of correct prediction, identification of a mechanism that seemed to be present in the case, and the need for the fewest ad hoc adjustments of the theory to make the account correct. As we move from country to country, the number of observations increases, and the demands of theory become greater, as some theories might have succeeded for England (i.e., seemed like a perfect fit), but required extensive modification to account for other cases. After five countries the limits and possibilities of the contending theories are assessed.

This would be the moment to take a methodological excursion. At this point of the course, exposure to elementary applied statistics is invaluable. The goal is to take students to the point that they can understand an expression that has a dependent variable on the left side, and a set of theoretically justified independent variables on the right. They should be able to manipulate a dataset so that they can produce and correctly interpret cross-tabulations. The Polity datasets, for example, code for levels of democracy and autocracy. Students can be given laboratory assignments to test theories about democracy that have been articulated in the theories that they have already examined, for example, comparing findings in Moore (for which a new variable would be needed in the Polity dataset) to those of Przeworski et al. (2000). In this part of the course, the mode of teaching changes from lecture to lab. But the lessons should be consolidated in a lecture that will discuss the trade-offs using qualitative and the quantitative methods to account for variation on our dependent variables.

It should be clear that by comparative politics I do not mean to isolate that set of work that self-consciously employs the "comparative method" relying on justifications going back to J. S. Mill, and brilliantly updated by Collier (1991). From this perspective, the comparative method is one that lies somewhere in between case studies and large-*n* statistical analysis. Rather I see the comparative field to be in search for explanation for interpolity variation, and willing to rely on Millian methods, statistics, or intensive case studies in search of explanatory success. Nonetheless, curious students should be guided to Collier's paper and its bibliography to get a better understanding of methodological issues that are especially troublesome in the comparative field.

Students should come out of the comparative politics section of the course understanding the link between the concerns articulated in the political theory canon and the specification of dependent variables developed in the comparative field. If they can understand that the Hobbesian answer (namely, that a Leviathan is a worthy provider of order) can be empirically and theoretically challenged, with better answers offered, they will have shown understanding of that link.

### *Political Institutions*

Political institutions constitute the third section of the introductory course. Comparative politics asks such questions as which countries become democratic and when, with an eye to exogenous factors that yield differential outcomes across polities. In comparison, the field of political institutions takes one value on regime type (that of democracy) and asks the questions of how it works, and how regimes change endogenously. Clearly democracies constitute only a small subset of all public institutions. But they are important, especially inasmuch as issues of democratic governance are key concerns in the canon, and help link this field to the wider discipline.

The section is organized around a set of perplexing theoretical challenges to democratic practice, and has been inspired by the text written by Shepsle and Bonchek (1997), which I assign to the students. (All references in this section are referenced in this elegant and I believe quite easy-to-digest text.) Each of the challenges to democracy as outlined in this text has spawned a vigorous theoretical and empirical literature in the field. Introducing students to these challenges is the purpose of this section of the course. I have found each challenge to merit a full lecture.

The first challenge to democracy concerns the issue of translating people's preferences into policy that reflects the array—or as Arthur Bentley (1908) would have had us say, the vector sum—of those preferences. Here we need to get into issues of vote cycling identified by Condorcet. We need as well to provide the intuitions behind Arrow's theorem on the impossibility of finding a voting rule that meets minimal democratic criteria in translating individual preferences to group policy.

A second challenge to democracy owes its original formulation to Black's median voter theorem (Black 1948), and to Downs's spatial modeling of party competition (Downs 1957). Among many implications of these theories, two are especially troubling. The first is that parties have an incentive (under first-past-the-post voting rules) to converge, reducing the choice given to voters. On questions of the provision of public goods (given some assumptions about the structure of preferences), convergence toward the median voter is socially

optimal. Yet many citizens decry a political world in which there are choices only between Tweedledee and Tweedledum. Is reduced choice in elections, this approach compels us to ask, the price we pay for a well-functioning democracy? We might also want to ask why people seem to be so upset about welfare-optimizing institutions. A second implication of Downs's model is that in equilibrium people have no incentive to vote. Not only will their vote be decisive at a probability next to zero (but above zero, as we saw in Florida in the 2000 presidential election), but given convergence of platforms, it shouldn't make a difference to a voter as to who wins. Here it is worthwhile to introduce the notion of "adaptive rationality" based on a critique of the information and processing assumptions that Downs makes. A recent paper by Bendor, Diermeier, and Ting (2000) develops such a model, with the added value of showing why it is rational for the majority of the electorate to vote in any election, a theoretical finding more in accord with empirical reality. Students might be alerted to this critique in lecture.

A related and third challenge to democracy is the difficulty voters face, to the extent that there is choice, in figuring out what is best for their interest. Given the substantial misinformation provided them about the candidates, how do they sort out useful information from scurrilous attacks? Should voters calculate the difference between the promises made by candidates and their own ideal points, voting for the candidate with the smallest distance between promises and ideal points? But why should the promises be seen as credible? Perhaps voters should (and do) vote retrospectively, voting for incumbents who have fulfilled promises in the earlier period, and voting against them when they have not. If this is the smart way to vote, as theory often suggests, why should voters prefer term limits which undermine their opportunities to vote for incumbents? Worse, term limits give legislators a full term in which they have no interest in representing their constituency. The science of determining how voters vote has perhaps the richest empirical literature in our discipline. Here is the best time to introduce students to the National Election Surveys, and to assign labs where students address questions about voting behavior by analyzing datasets provided to them.

A fourth challenge to democracy concerns the choice of an ideal voting rule. Here we find a wide variety of trade-offs depending on the different electoral systems. Some maximize honest voting (where voters have an incentive to vote for the candidate closest to their ideal points in regard to policy) while others maximize strategic voting (where voters will vote for a candidate far from their ideal points in order to spoil the chances of a likely winner). Some maximize a legislature filled with representatives of small minorities; others maximize the chance that one party will have a majority and therefore will be able to govern coherently. Thinking through the implications for democratic rule of a variety of electoral formulae has become a growth industry in the field of political institutions.

A fifth challenge to democracy turns on the theoretical result reported by Olson (and challenging the basic assumptions in the classic works of Arthur Bentley [1908] and David Truman [1951]). Olson found that with a few basic assumptions about the logic of collective action, groups representing the largest populations in a democracy would be less likely to form and thereby represent those interests less well than groups representing small populations having narrow interests. To the extent that policy is the result of the organizational effectiveness of lobbies and other interest groups, the larger the size of the potential group the smaller would be its influence in a democracy. A literature related to the one spawned by Olson, and relying on similar assumptions, seeks to address the issue of the possible undersupply of public goods in a democracy.

A sixth challenge to democracy concerns oversight. What is to constrain agents of voters to act in their own interests violating the interests of their principals? Once identified, this problem can be seen at all levels of a democracy. Why don't representatives seek reelection (whatever that takes) rather than representing their district, when these goals are in conflict? Why don't committees of a legislature recommend legislation that serves their interest rather than the interest of the median voter on the floor? Why don't bureaucracies created by legislatures subvert the intent of the legislation to fulfill their own goals of growing their organizations and/or capturing rents by making life easier for those whom they are regulating? Why don't judges merely vote their own policy preferences rather than arbitrate neutrally on the best interpretation of the law? The fundamental question underlying all these issues is the degree to which principals in democracies can constrain the incentives of their agents from maximizing their own goals rather than maximizing the goals of their principals.

A seventh challenge to democracy concerns questions of deliberation and debate. If representatives are in a legislature to represent the voters in their districts, why do they engage in debate on the floor? Why do they deliberate, rather than calculate the ideal point of their constituents? And why do they listen to arguments of lobbyists when they should know before any lobbyist opens her mouth that she will be strategically manipulating the data to serve her own client's interest. What is the use of talking at all? Why not state the preferred position and offer a bribe to move the representative toward that position to a degree determined by the size of the bribe? What is the value added to our decisions by acting as if they were the result of deliberation and debate?

An eighth challenge to democracy is the incentive for the executive to abuse her power. At the extreme, such abuse can undermine the democracy itself (or constitutional provisions the executive does not like) through a preemptive coup. It should be simple for a chief executive with a loyal army to promise one part of the population to accept dictatorship in return for policies it favors, thereby dividing potential opposition. Why doesn't a party that loses an election



(especially if it is the incumbent party) simply nullify the election and remain in power? To be sure, in many cases this happens, and this is the sort of question addressed in comparative politics. But the mechanisms restraining rulers from subverting democratic rules are properly studied in the field of democratic institutions. This challenge is not addressed in Shepsle and Bonchek. Its core theoretical issues are developed in Przeworski (1991, chap. 1) and in Weingast (1997). Here the material can be presented in lectures with these readings considered supplementary.

In teaching the literatures that address these challenges, students will necessarily be exposed to formal theory and to comparative statics. I recommend, after taking students through the Arrow theorem as summarized by Shepsle and Bonchek, a second methodological excursion, with an introduction to utility and game theory. Students should be introduced to the concepts of rational choice, maximization, and equilibrium. They should be able to understand the logic of simple games such as prisoners' dilemma and battle of the sexes. (More advanced students might be given an optional section on solving through backward induction subgame perfect equilibria in extensive form games.)

The theoretical models taught in this section have been exposed to many tests, both empirical and experimental. To read journal articles reporting on these tests, students will need to have some understanding of comparative statistics. Since the students had been introduced to statistics in the second section of the course, not much more need be required for them to read tests of the theoretical models presented in this section. But students should be made aware of other avenues of model testing. There is an emerging trend in the field of political institutions in which ethnographic and archival data are being used for tests of models whose actors condition their behavior on the beliefs of others (e.g., Greif 1994).

The field of political institutions is in intellectual ferment. The course outline will therefore be subject to change. For one, the study of political institutions goes far beyond democratic ones. In future iterations of the course, other institutions, such as markets, oligarchies, and institutions regulating relations between business organizations and the state, might be examined. Models describing the mechanisms that sustain these institutions are increasingly available. Second, as the information and processing assumptions of present models get further relaxed, the driving questions in the study of democratic institutions are sure to change. Third, there is increasing attention in this field to institutional dynamics that takes focus away from the question of self-maintenance, and directs attention to the endogenous sources of change. The challenge here is to model change in such a way as to restrict a variety of paths of play as off the equilibrium path. Fourth, as practitioners of historical institutionalism move from diachronic comparisons to mechanisms sustaining path dependency (a trend already observable),

their work will branch into political institutions. The merger of dynamic game theory and historical institutionalism, as foreshadowed by the work of Greif (forthcoming), is likely to transform the field of political institutions. Students who have gone through the section I have outlined will be able to appreciate this new work. More importantly, they will, as a complement to the macro orientation of comparative politics, get a micro perspective on the inner workings of one value (democracy) for a consequential political outcome (regime type).

### *International Relations*

International relations constitutes the final section of the course. This field is consumed with the fundamental question in the political theory canon of order—how it is maintained under a single parameter value (anarchy) and how it breaks down. International relations also addresses the creation of regimes and normative orders which create public goods but do not have the sovereign authority that states claim. And more so than the other three fields, international relations is intimately connected to the field of public policy, as students of international relations address issues of foreign policy.

The study of the outbreak of war (constituting a subfield) is quintessential to the field of international relations. International relations experts who address this question examine it from a macroperspective (the general condition of the security dilemma, the structure of the international system, ideology, the nature of the regimes in the combatant states, the economic differentials between the states) and a microperspective (the dynamics of the war of attrition, the issue of private information about each other's resolve, the problem of credible commitments) as well. In whatever combination of micro- and macroperspectives, having a unified theory that can explain such different outcomes as the one hundred years of Great Power peace after the Napoleonic wars and the carnage of the first half of the twentieth century is a major challenge to international relations theorists. In the 1990s, practitioners of this field have begun using their theories and techniques to analyze internationality (within state) wars, and have thereby begun to expand their domain of concern, with the same specification (peace and its breakdown) on the dependent variable.

The study of cooperation under conditions of quasi-anarchy, and the development of regimes and normative orders that ameliorate anarchy through the joint production of public goods, shares the spotlight in this field with the study of war. This subfield has been dominated in the literature by those studying International Political Economy (IPE). IPE scholars ask how states cooperate under conditions of anarchy, and especially under conditions of intense economic competition. Many IPE scholars, like their compatriots who study war, analyze the reasons for the escalation of economic conflicts, jeopardizing efficient

outcomes. Yet the distinctive contribution of IPE scholars is their concern for why orders (or “regimes”) get created and how they get sustained (Krasner 1983). International contracts, international regulatory agencies, regional pacts, and treaties of all sorts are not—as might be expected from a traditional perspective on sovereignty—nonenforceable. International agreements do get made; parties to them do overcome free-rider problems; and in the case of some international regimes agreements help promote economic growth, individual freedom to travel and work beyond one’s borders, and state security. The effective authority of international regimes that lack any claim to monopoly of legitimate violence is a fundamental puzzle in political science and is studied systematically within the international relations field. Seeking to understand how such agreements get made and enforced (and as with the case of students of the European community, how some agreements under conditions of anarchy can lead to the creation of quasi-sovereignty) is a central concern in IPE. Like their brethren studying war and peace, some IPE scholars macrofocus on economic, bureaucratic, and state-regime variables while others microfocus on the solution to a variety of collective action problems. In recent years, a new generation of scholars has focused less on the microdynamics of anarchy reduction and more on the cultural and normative sources of interstate order (Katzenstein 1996).

Integrating the field of international relations into the introductory framework developed here requires the downplaying of what practitioners call the “axes of debate” (Fearon and Wendt 2002). For a generation, the principal axis of debate was that between realists and liberals. This axis divided practitioners based on their preferred independent variables. Realists favored arguments that focused on the relative military power of the states within the international system, while liberals focused more on the economic and political interests of governments and their ruling coalitions. Realists ignored international institutions as ephemeral and derivative, while liberals saw these institutions as both enabling and constraining. This axis, as suggested by a retrospective article published by Katzenstein, Keohane, and Krasner (1999), is now being superseded by one pitting rationalists against constructivists. This second axis of debate divides the field based on ontology. Rationalists tend to privilege choice, and seek to model international relations in terms of actors maximizing their goals given constraints having to do with information and resources. Constructivists tend to privilege a logic of appropriate action which heavily constrains choice. The historical and social construction of actor identities, for example, provides information to actors as to who are their natural allies and who are their possible foes. Change is possible, but this requires some reconstruction of identities. These debates on axes or “isms,” as I shall elaborate in the next section, seem divorced from the substantive problems we are collectively seeking to solve. To be sure, realist and liberal theories need to be elucidated, and rationalist and construc-

tivist approaches need to be presented. But the focus of this fourth section, to be consistent with the logic of the course, must be on the value of order, its breakdown, and how institutions and/or normative orders get constructed under conditions of anarchy.

The section, after laying out the special preserve of international relations, explores theories of the causes of war. Reconnecting with political theory and how the question of order was originally presented, Waltz’s *Man, the State and War* (1959) is an ideal text, outlining three traditions of explanation. The “first image” posits individualist sources of war focusing on psychological independent variables. The “second image” examines variations in type of state to explain the likelihood of war. Today, Waltz’s text needs to be supplemented by readings on the democratic peace, including those that focus on audience costs that differ across regime types in the handling of crises. The “third image” examines systemic explanations for war. The text here might be supplemented by more recent work on the security dilemma (developed by Jervis 1978) and by the full development of the “third image” in Waltz’s *Theory of International Relations* (1979). I think introductory students can be exposed to Jervis’s article, but Waltz’s *Theory* is best deferred for an upper-division course.

From the dimension “peace/war” this section moves to the dimension of “anarchy/regime.” The fundamental questions here are when states cooperate (the dynamic question of causation) and how states cooperate (the static question of equilibrium). To address these questions, I assign Axelrod’s *Evolution of Cooperation*. This book provides inter alia a microanalytic perspective on how the security dilemma can be overcome with a sufficient shadow of the future. I also lecture from material associated with the neofunctionalist school (Haas’s preface and opening chapter to *Uniting of Europe* [1958] is exemplary) that posits a mechanism of “spill-over” to account for regime creation. Finally, to address the “when” question, I lecture on Keohane’s *After Hegemony* (1984) where he addresses the systemic conditions when regimes are more easily constructed.

Once the dimensions are specified and theories outlined to account for variation, the course seeks to test these theories in a way similar to the empirical part of the comparative politics section. Given that it is logically impossible to test third-image theories cross-sectionally, I put this perspective to test from a historical perspective. I take four historical eras: the Congress of Vienna and the Hundred Years’ Peace; War and Depression from 1914 through 1945; the Cold War; and the Post-Cold War World. For each era I focus on the level of warfare and the level of trade (as a proxy for the international public good of a free-trade regime). Here I look mostly at whether systemic or state-level factors play a role in accounting for the values on both specifications of the dependent variable. In teaching this part of the course, I have found Kissinger’s *A World Restored* (1957b) and Gilpin’s *U.S. Power and the Multinational Corporation* (1975) nice

complementary texts for the nineteenth-century material. Gourevitch's *Politics in Hard Times* (1986) helps develop the argument from the perspective of economic policy in the interwar years. Van Evera's book *Causes of War* (1999) develops theory and the case of World War I. For the post-Cold War years, there is much exciting work accounting for the creation of regimes (perhaps the consolidation of the European Union has the largest literature), and the relative rise of civil wars in comparison to interstate wars.

The international relations section can conclude, as did comparative politics, with statistical testing of a variety of theories of war. Here students can be exposed to the *Correlates of War* and the Militarized Interstate Disputes datasets, and some of the literature that relies on these data. Work that integrates formal theory and statistical tests (such as Bueno de Mesquita and Lalman 1992) might be presented in lecture in this connection. A final lab assignment, asking students to do their own data analysis testing their own conjectures, can help consolidate skills taught earlier. The focus should be on the sources of interstate order, when it succeeds and when it breaks down. This is the key link to the consequential public concerns that drive the entire course.

A final lecture, seeking to account for democracy, community, and order—focusing more on what needs to be done rather than on what has been accomplished—would fulfill two goals. First, it would consolidate the notion that political science is a discipline organized around consequential public concerns. Second, it would invite our best students to make future substantive contributions to its development.

This is a brutal course, and it has yet to be made user-friendly. That is a task that awaits a Samuelson. My purpose here is to demonstrate that the substantive fields can be presented in a coherent manner, and to show that we are in a discipline awaiting its canonical text.

### Alternate Visions of the Discipline

Many colleagues, as exemplified in the discussion when an earlier draft of this essay was presented at the 2001 annual meeting of the APSA, are enthused by their membership in a discipline that has no core, no set of standardized procedures, no agreed-upon set of problems that require solution, and no agreement as to what constitutes a solution. Indiscipline for them represents a pluralistic utopia. But other colleagues who share my view that we ought to aspire to disciplinary status differ with me on how best to organize political science. Some organize the discipline according to preferred independent variables; others along lines of places and actors; others along lines of the grand debates or “isms”; and still others (including myself, in my more radical moments) along lines of strategic interactions. In this section I shall offer some remarks about these alternate

visions, and why I do not see them as sufficiently attractive for present-day reform.

### Focus on Independent Variables

Instead of asking, for example, “What are the causes of revolution?” with a focus on the dependent variable, many political scientists ask, “What is the magnitude of the effect of class structure on revolution?” These students can use their data on class structure to address other dependent variables, for example, the size of the welfare state. In my perusal of the sections in the APSA (from Table I), eight have a heavy emphasis on consequential independent (or causal) variables.<sup>6</sup> It is my sense that the sections categorized here ask similar questions: What are the implications of gender for social mobility in society or power in institutions? What are the implications of new information technologies for the maintenance of autocratic rule? How does religious heterogeneity impact on democratic stability? How have ecological issues transformed political parties?

While the two sections that focus on history have a strong methodological component (e.g., in working out such problems as the selection bias in relying upon secondary sources, as pointed out by Lustick 1996), its main concerns lie in innovative ways of providing historical explanations for important political outcomes. Consider the paper written by Paul Pierson and Theda Skocpol (2002). It is a mission statement for a field they call historical institutionalism, and scholars who write in this field are leaders in the politics and history section. In terms of the dependent variables that organize our discipline, historical institutionalists feel they have something to say about all of them. “More broadly,” Pierson and Skocpol (2002, 694) write (and I omit their extensive references), “historical institutionalist studies have recently cumulated to provide wide ranging as well as causally precise understandings of such important matters as transitions to democracy; the emergence and demise of authoritarian regimes; the intersection of domestic and international politics; the origins and development of welfare states; social identities in politics; the roots and development of economic regimes; and the causes and consequences of social movements and revolutions.” What unites them is not substantive concerns, but rather powerful intuitions about the sources of variation and how to specify them. “Despite variability on many key dimensions,” they write, “historical institutionalists share distinctive and complementary strategies for framing research and developing explanations. . . . They focus on *institutional origins and development and take macro contexts seriously*, dissecting causal configurations that shape social and political processes. And most important of all, historical institutionalists *develop explanations by specifying sequences and tracing processes over time*. Taken together, these three features add up to a recognizable historical

institutional approach that makes powerful contributions to our discipline's understandings of government, politics, and public policies."

In my reading, this is not a "field" but rather a conjecture on the influence of an independent variable (historical context) and how best to specify it (paying attention to sequencing). This implies, say, for the study of democracy, that we should be attentive to factors such as a country's past political institutions, the international context in the period of democratization, and whether it is preceding the welfare state or following it. These conjectures are all oriented toward specification on the right (or independent) side of the expression rather than the left (or dependent variable) side.

There is a good reason to consider organizing the discipline according to independent rather than dependent variables. Research becomes more tractable. Trying to put together a complete story on the causes of revolution is Sisyphean in scope; but asking if there is a measurable effect of class structure (controlling for other independent variables) and accounting for that effect is quite feasible in the context of a single research project. Geddes's appeal (1991) to focus on processes makes a similar point. It is possible, she shows, to demonstrate the impact of regime type on regime breakdown; but far less possible to nail down a model of regime breakdown in general.<sup>7</sup>

But there are two powerful reasons that make me a "leftist" in supporting a focus on dependent variables. First, independent variables are alive only to the extent that they have explanatory power. In the medieval period, astrologists believed (reasonably) that the ordering of the planets at the time of a person's birth would impact on her personality. By organizing themselves scientifically in terms of an independent variable, astrologists far outlasted their scientific usefulness. The magnitude of the planet effect was zero, but the organizational strength of astrologists was nonetheless hard to undermine. Similarly with Marxists. Class structure is a powerful specification of an independent variable. In a certain configuration, Marxists theorized, it would induce revolution. This has proved to be incorrect. To organize a subdiscipline in political science around such apparently powerful independent variables as class structure would be dangerous. If their magnitude turns out to be small, the discipline would be faced with all-of-a-sudden defunct fields. Dependent variables remain on the agenda. Independent variables, meanwhile, when they are unable to explain variation, ought to be excised from future analysis.

Second, political scientists share a set of public concerns deriving from the canon. We remain eclectic and interdisciplinary in our choice of independent variables. We see the study of civil war, for example, as political. But if "mountainous terrain" is a key independent variable that is associated with the likelihood of civil war, we would be reluctant to include the study of terrain as a field in our discipline.

For these two reasons—independent variables are necessarily discardable and they are often not public—I recommend a common disciplinary focus on dependent variables. This does not mean that I would argue against Geddes's advice about scientific procedure. It may turn out that methodologically and practically, tests of theory are best done with an eye on a particular specification of an independent variable. But, to take her example on regime types, it is best to think of it as a contribution within the field of comparative politics and as a partial account of the breakdown of order. From a disciplinary perspective, then, sections that focus on independent variables are scientifically merited; but the discipline should not be organized around those variables.

### *Focus on Places and Actors*

Many of our upper-division courses in political science and the organization of our research is based on places, actors, and specific groups. In comparative politics, we offer courses and submit to journals based on regional expertise. Experts identify themselves as "Africanists" or "Latin Americanists" or "South Asia experts." Furthermore, political science departments offer courses and faculty do research on questions of ethnicity, race, class, and gender. Scholars with such expertise claim that their fields are "race and politics" or "gender and politics." These are all examples of important areas of research and teaching that have not merited, in my scheme, field status. Why, it should be asked, shouldn't the discipline be organized along these lines rather than by the four fields I have identified?

My proposal does not ask for the abandonment of research and teaching into specific places and of specific actors, but rather a reorientation of that research and teaching such that they speak to more broadly articulated public concerns.

Consider the fate of area studies. Area studies in their heyday of the 1960s transformed American political science from a discipline that was narrowly American (with a touch of Europe) into a global enterprise. Questions of comparative economic development, national formation, political stability, and the creation of political order—virtually ignored in early post-WWII political science—came to the fore in a powerful way. To a certain extent, comparative politics was empty until we had cross-sectional data for a wide variety of countries with different values on core dependent variables. Thus, area studies provided the framework for the contemporary discipline.

By the 1990s area studies had lost its glamour within political science, and readers of this essay will see that area expertise is given no special prominence in my categorization of the discipline. One reason for this is that knowledge that country  $x$  is in region  $y$  has not provided enlightening information in explaining variation on key dependent variables. To understand disorder, democracy, or

poverty, it doesn't help very much to know that a country is in Africa, or South Asia, or Latin America. In our attempt to account for specific values on key dependent variables, our disciplinary training compels us to look for independent variables that do not start with uppercase letters (Przeworski and Teune 1970). A past history of colonialism with indirect rule is a variable for which we can theorize; meanwhile, "British colony" is insufficiently informative about what might be driving the outcome of interest. As the discipline moves away from "uppercase" variables, the premise of area studies—viz., that there is a regional context that serves as a natural control for cross-sectional comparisons—is severely challenged.

By no means do I support a discipline without expertise about places and regions. Rather, I support teaching and research that encourages expertise about places and regions. Without such expertise, there is no way we will capture intervening variables, sequencing, and interaction terms (the threads that woven together become "context") that help differentiate the political paths of different polities. My point is not to decry such expertise, but to make such expertise work in the service of cross-polity analysis, to account for differences systematically. Area studies has long been closely associated with comparative politics; here I am arguing that the relationship requires a somewhat different format.

What about research on race, gender, and class? Consider Michael Dawson's *Behind the Mule* (1994). I could not agree more with Hanes Walton's (back cover) evaluation that this "is a pioneering and painstaking exploration of African American politics from the standpoint of the people in that community, using their opinions, attitudes, and behaviors as guides." Yet the book also makes at least two contributions that speak to the core of the discipline, and transcend any interest in African American politics. First, Dawson contributes to our understanding of democratic institutions as he develops the theme of the "black utility heuristic." His data show that African Americans along a wide spectrum of economic interest converge on the left side of the political spectrum (in fact, to the left of the Michigan extreme, because former researchers truncated that spectrum) due to a collective understanding that individual interests for blacks are best induced from the common situation of African Americans. Learning the general conditions under which individuals adopt the political orientation of the modal member of their group would play an important role in a political science seeking to learn the principles of preference aggregation in democratic politics. Second, Dawson's book cannot be ignored by scholars who have a general interest in the dependent variable of equality. The nonpoliticization of class differences within the African American community is a fascinating outcome, and the conditions under which inequality within marginalized groups remains off the political agenda is an equally fascinating question, for which

Dawson provides well-considered answers. In this regard, Dawson's book is a major contribution in comparative politics as well.

In both advanced disciplinary courses and in publications, I support the continued study of places and actors. But my disciplinary suggestion is that scholars who labor in these fields should present their findings to students and colleagues as partial answers to the enduring questions that drive the wider discipline. This is best done if presented in the context of the field-based division of labor that I have proposed.

### *Focus on Grand Debates or "Isms"*

In Pierson and Skocpol (2002), it is suggested that political science might be divided into three categories: behavioralism, rationalism, and institutionalism. The field of international relations characteristically divides itself up by paradigmatic arguments in a similar fashion. These divisions remind me of an acidic comment by Pitkin (1972, 219–20) in regard to dichotomizations, that while it is possible (more or less) to divide the world up into herrings and fruits, not much would be gained by so doing. It is heartening that in the development of APSA sections that these divisions have not been reified into organized research communities.

But Pierson and Skocpol only reported the tip of the "ism" iceberg. Our discipline is rife with all sorts of "isms." Some "isms" are now practically defunct: marxism, structuralism, and functionalism. Others have become so hegemonic that they hardly need a defense: liberalism in the context of American political theory. Still others are knocking at our disciplinary gates: postmodernism, poststructuralism, feminism, postfeminism, and constructivism. What these "isms" share is an orientation to politics that is offered as a challenge to the status quo. Adherents to these "isms" seek to fight status quo approaches in our discipline with a war of position on several fronts, from methodology, to subject matter, to theory.

But consider poststructuralism and its kin, postmodernism. The intellectual heavyweight in clearing this field is Michel Foucault. His work is in part on the sources of political and social order, as developed in *Discipline and Punish* (1979). Among his insights is that social order is maintained in the everyday social practices within the capillaries of the body politic rather than in its arteries, which are controlled by armies and police forces. By studying the micro-processes of domination, often sustained by discourse, one gets an appreciation for both power and obedience. For this and other insights, his oeuvre merits inclusion in the political theory canon. Furthermore, his insights not only on discipline but also on the sources of and defense of categories that define social and political life merit systematic empirical investigation in the comparative field. Thus, the ordering of the discipline as I have conceived it does not rule out the

study of Foucault's work, or of postmodernism. It is just that I do not think there should be a special field devoted to this "ism" any more than I would want a field of political science to be oriented around "Hobbesianism."

"Isms" carry with them great energy and vision; but as political scientists we ought to discipline them in the hope of raising, reformulating, and answering consequential questions about political life.

### *A Focus on Strategic Dilemmas*

Political science, as the study of "who gets what, when and how" (Lasswell 1936), can be thought of as a discipline focused on strategic interaction in the production and allocation of public goods. Lasswell's gigantic questions have been analytically broken down into specific game situations, such as that of "commitment" or "reputation." Each of these game situations has wide applications in the study of democratic institutions, interstate war, and ethnic conflict. Political science could thus become organized around a limited set of specific strategic situations or fundamental political mechanisms—conflict, security, delegation, agency, coordination, allocation, representation—rather than by dependent variables.

I cannot rule out the possibility that this analytic framework might one day replace the disciplinary ordering proposed herein. It allows for the systematic study of parallel processes that occur in very different environments. Solving problems of commitment in general allows for applications in a wide variety of political arenas, and the separate study of arenas (e.g., ethnic politics, international relations, political economy) takes the focus off the fundamental strategic or processual problems faced in all of these arenas. In this sense the strategic situations are more foundational than the dependent variables culled from the political theory tradition.

Despite the powerful analytic reasons to adopt the strategic/processual organization of the discipline, there are more weighty substantive concerns that diminish the attractiveness of this alternative. Politics are broader than issues of production and distribution of public goods. Fundamental values such as freedom stand outside the Lasswellian framework. While it is true that to assure individual freedom it is necessary to assure the commitment by leadership that it won't abuse the trust of its constituency (and thus back to a core strategic issue), it is also the case that the maximization of freedom involves a host of strategic issues (such as reputation, aggregation of preferences, information symmetry) in a particular configuration. It is the configuration of political activities that leads to human freedom or its denial that gives special prominence to political science. In the field of political institutions, I advocate the systematic study of strategic situations and processes. But the field of political theory organizes

them into compelling questions; and comparative politics provides a field where macrovariables combine with strategic interaction to explain different strategic outcomes. Strategic interactions are fundamental to politics, but give way to the conglomerate issues articulated in the canon as foundational for the discipline.

### **Concluding Questions**

Rather than summarize my answer to the question of what constitutes our discipline, I shall conclude by raising (and giving responses to) three new questions.

Why seek disciplinary coherence? A first answer is if we abandon responsibility for charting the discipline, we leave it to our students, who are less equipped than their professors to figure out what is foundational in their major. If we take that responsibility, we build for our majors a strong foundation for advanced study in our discipline.

There are other reasons to support a coherent discipline. Political scientists do themselves a disservice when they represent their discipline as a "blob" or a "big umbrella." Funders take us less seriously. The National Science Foundation a few years ago increased funding for psychology and economics at the expense of political science. This decision in my judgment reflects more on our presentation of selves than our scholarly accomplishments. As I complained earlier, other outsiders have made a similar conclusion: we are consigned to the pages of "Arts and Ideas" rather than "Science Times." Undergraduates perceive us as a "soft" major, lowering the mean quality of our students. Worse, they are ill prepared to make significant contributions to the discipline in their Honor's work. And finally, scientific progress is slowed to the extent that we refrain from organizing ourselves such that findings in theory and empirical work, and findings in statics and dynamics, impact upon one another in a more coherent way. My overall answer to the first question is that there are unrealized gains for organizing ourselves institutionally in a manner that reflects our scholarly successes.

A second question: Whose ox is being gored? A few years ago in an APSR symposium, I heralded King, Keohane, and Verba (1994) for its efforts in "disciplining political science." In that review (Laitin 1995), I carried a brief for cultivating a discipline. Referencing Foucault, however, I acknowledged that a discipline implies rules of inadmissibility and exclusion. From Foucauldians among us, and from other quarters as well, I therefore anticipate virulent accusations that this paper has an implicit agenda to exclude approaches that I do not approve of, or ideologies that I find distasteful. My goal here, I protest in advance, is not to exclude but to create a public good (the discipline) that will enable us collectively to advance political knowledge. Fearing association with Iago, I will keep my protest succinct, and limited to four points.

First, I am not a positivist blind to the value implications of what we study. In

my disciplinary outline, I have not hypocritically paid obeisance to political theory with a set of humanistic platitudes. Rather, I have argued that political theory is foundational for all of political science, and continues to set its agenda. Second, I have not privileged any one methodological approach. Rather, I have emphasized the importance of narrative, statistics, and formal theory, each playing a crucial role in the disciplinary division of labor. The course I outlined privileges narrative and the macrotheories associated with narrative approaches in the comparative politics section, formal theory in the Political Institutions section, and gives statistics an important role in all but the political theory section. Therefore, there are no methodological approaches that are excluded. Third, I am not trying to squelch vigorous debate. Rather, I am trying to refocus debate on what draws us together as political scientists—accounting for and justifying different public outcomes—rather than on diversionary tacks inducing us to grapple with ontologies or epistemologies.

To be sure, someone's ox will be gored, and this is my fourth point. Political scientists who write that ethnic war is common (in the spirit of travel writer Robert Kaplan) will need to show colleagues precisely what the metric is, and what statistical methods were used to make such a claim. Political scientists who write that under conditions of violent ethnic conflict partition is an attractive antidote will need to show that they thought about the effects of partition in one region on the political dynamics of a neighboring region of the country, or in other countries. Formal models are designed to alert researchers to potential reaction functions, and therefore arguments about causation that do not address issues of endogeneity are not adequate. Political scientists who assume that a shared ethnicity implies common knowledge about the history of all interactions among one's own group will need to show from ethnographic or other forms of data that this assumption approximates the real world. Our discipline puts constraints on the assumptions, the reasoning, and the empirical claims that are permissible. These constraints are not trivial, but they do not privilege any particular subgroup within our discipline. In this sense, for the creation of a public good, all our oxen are being gored.

A final question: How stable is the discipline that is organized in the proposed introductory course? My intuition is that it will from the get-go teeter on obsolescence, and never be fully commensurate with the activities within the community of researchers. Science is moving too quickly to be properly wrapped up between hard covers of a long-lived text. The boundaries between comparative politics and international relations can hardly be justified, as the parameter "anarchy" is really a variable. The study of tax policy in the European Union and that of regional warfare in the Congo are core questions in both fields. Keeping up a boundary based on an arbitrary point on a variable seems undisciplined. Furthermore, the boundaries between comparative politics and

political institutions are breaking down. Comparativists are increasingly asking mechanistic questions about nondemocratic processes, such as ethnic organization and authoritarian rule, for which game theory is appropriate. And there is more ambiguity still. As suggested earlier, political science may one day become organized around a limited set of specific strategic situations or fundamental political mechanisms—conflict, security, delegation, agency, coordination, allocation, and representation—rather than by dependent variables. Or it may be the case that the field I currently call political institutions will give way to one called fundamental political mechanisms, while the other three fields continue along present paths. Another likely scenario is that the discipline will become reorganized separating those who study macro-political phenomena and those whose orientation is micro-, as the field of economics is presently organized.

However attractive these futures might seem, I believe that the political theory canon, and the concerns it has cast upon our discipline, will for a long time give political science a distinct mission. The fields of comparative, institutions, and international relations will continue to evolve and boundaries will shift among them. This is to be celebrated. But change should be guided by a vision that we are asking the same questions, and that we are cultivating a dynamic division of labor to answer these questions. This is our mission. The disciplinary organization that I am promoting is both true to that mission and as such motivated by the goals of science. If adopted, it will do us all a service before it dies a slow death.

## Notes

1. This essay was originally a paper presented at the Annual Convention of the American Political Science Association (September 2001), San Francisco, California. It has benefited from spirited criticisms at the panel by Robert Keohane, Theda Skocpol, Ian Shapiro, and Barry Weingast. After the conference, comments by Gerardo L. Munck were especially helpful. Matthew Kocher twice served as teaching assistant for the course to be described in Section III. Several of the innovations in the course are due to his suggestions. He also commented usefully on an early draft of this essay. The Thursday reading group at Stanford read and commented helpfully on a later draft. Especially useful were comments by Richard Brody, James Fearon, John Ferejohn, Isabela Mares, Rob Reich, Paul Sniderman, Michael Tomz, and Anne Wren. The anticipated comments from Peter Katzenstein, who had not yet read the paper, altered the tone of this essay considerably, though I remain sure not to his satisfaction.
2. Anna Laitin performed the research that went into this paragraph; Todd Heitlenbach provided technical assistance. Fifty-five colleges and universities were randomly selected for this study. They were chosen from a list of 1,600 colleges and universities maintained at the University of Florida (<http://www.clas.ufl.edu/CLAS/american-universities.html>).

The list includes accredited schools in the United States that offer bachelors' degrees and/or advanced degrees and maintain websites. A Perl database was used to select the list of fifty-five schools. Institutions were not included in the final sample of thirty-four schools if they did not offer a major in political science (15), if the school's website did not include adequate information about the requirements for the major (5), or if the institution is an Internet university (1).

3. On Nebraska, see <http://www.unomaha.edu/~world/cas/cas.html>. On Nevada, see <http://basque.unr.edu/>.

4. Since I wrote this paragraph in draft format, several instances of reporting disciplinary findings in the context of science have come to my attention. See, e.g., Robert Adler, "The Crystal Ball of Chaos," reporting on the research of the State Failure Project involving inter alia Jack Goldstone and Ted Gurr, and the scientific critique of it by Gary King and Langehe Zeng, published in the journal *Nature* 414 (November 29, 2001): 480–81. This was not a clash of opinions, but one of proper model specification and control. Readers were given the impression that through this research, we were developing better understanding of the conditions that are most propitious for civil war.

5. In earlier papers (Laitin 1998b and 2002), my field division was somewhat different. What I called in those papers "Democratic Institutions," I now broaden to include all "Political Institutions."

6. Politics and History, International History and Politics, Religion and Politics, Race, Ethnicity and Politics, Women and Politics, Ecological and Transformational Politics, Political Communication, Information Technology and Politics.

7. On her general point concerning the dangers of choosing cases based on similar values on a dependent variable, I am in thorough agreement.

## COMMENTARY

### Political Science: Subject Matter and Discipline

ROBERT O. KEOHANE

David Laitin has written a masterful essay demonstrating the institutional incoherence of political science and heroically attempting to show that our discipline is intellectually coherent. However, he does not address two important questions. First, what are the behavioral processes on which political science should concentrate? Second, what are the defining features of a discipline? In this short comment, I seek to fill these gaps in his argument.

#### The Subject Matter of Political Science

Professor Laitin's reconstruction of a coherent discipline centers on concerns that motivate political scientists, which he also interprets as dependent variables. For him, the chief concerns are order and democracy; others include community, equality, legitimacy, and justice. Oddly, he does not mention liberty.<sup>1</sup> In the canon of political theory, these concerns could be viewed as a combination of procedural attributes of a political system (such as democracy and legitimacy), and potential outcomes (such as order, liberty, equality, and community).<sup>2</sup> Most or all of these attributes and potential outcomes are valued in contemporary political philosophy, although to different degrees in different traditions. Indeed, one of the great virtues of Laitin's reconstruction of political science is that it retains the centrality of classical political philosophy, with its cogent and eloquent expressions of the core concerns of the field.

In my view, Laitin's solution to the "coherence problem" of political science is sensible. However, it would be equally sensible to argue that political science is coherent because it focuses on a distinctive set of phenomena. In their introduction to the current edition of *Political Science: the State of the Discipline* (2002), Ira Katznelson and Helen V. Milner adopt this perspective. For them, as for Laitin, political science is coherent. But for Katznelson and Milner its coherence comes from its consistent focus, since its founding a century ago, on understanding the modern liberal state. Whereas Laitin looks to classical political philosophy for sources of coherence, Katznelson and Milner look to the history of our discipline in the United States.

Other political scientists have focused, like Katznelson and Milner, on the core phenomena studied by political science rather than on potential outcomes. But to many of them, a focus on "the state" has seemed too narrow. Aristotle viewed as