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Mansfield, Edward D., Sisson, Richard

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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. 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). 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 Katzelson 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
central to the field “the various kinds of authority to which men and their associations are subject” (Aristotle 325 B.C.E. [1948], 128). David Easton rejected the state as an orienting concept since “it implies that political science is interested in studying a particular kind of institution or organization of life, not a kind of activity that may express itself through a variety of institutions” (Easton 1953, 113). For Easton, the focus of political science is on activities that influence authoritative policy. “A policy is authoritative,” he declared, “when the people to whom it is intended to apply or who are affected by it consider that they must or ought to obey it” (Easton 1953, 132).

Easton got into trouble when he spoke of policies that are authoritative “for a society,” because for him society required that people have a “sense of belonging together” (Easton 1953, 135). In international relations and severely divided societies, policies may be authoritative even though only a system, rather than a society, exists (Bull 1977). Indeed, the conflicts that political scientists study often derive from the inconsistency between the locus of power, on the one hand, and identities and values, on the other. Yet despite these difficulties, it seems to me that Easton’s formulation is superior to an emphasis on the state. In an increasingly globalized world, societies are linked to each other through a variety of transnational relations (Keohane and Nye 1972; Keck and Sikkink 1998). Political scientists are concerned about how authoritative policies, with wide public implications, are formulated and implemented. These policies may be constructed by firms, religious organizations, or other nongovernmental actors as well as by states.

I believe that a comprehensive definition of the subject matter of political science should include both potential outcomes, as discussed by Laitin, and the formulation and implementation of authoritative policies, as discussed by Easton. Furthermore, power, or influence, is so central to our field that it seems odd to leave it out. The subject matter of political science, as I conceive it, is the processes of influence that generate authoritative policies with wide public implications for potential outcomes such as liberty, equality, and order. Influence can be exercised through coercion, bargaining, persuasion, and emulation.

The study of political science necessarily involves the study of political institutions, which Professor Laitin rightly emphasizes. Indeed, the fields of American politics and comparative politics could both be encompassed within Laitin’s category of “political institutions.” Furthermore, “the boundaries between comparative politics and international relations can hardly be justified” (Laitin, pp. 38–39 in this volume). As he says, anarchy is really a variable (Milner 1991). Some processes in international relations, such as those involving the politics of Europe, are more institutionalized than some domains of comparative politics, such as authoritative policy making in the Congo. Like any division of our discipline, Laitin’s division of political science into four distinct substantive fields probably has a stronger organizational than intellectual rationale.
What Is a Discipline?

Professor Laitin recognizes (Laitin, p. 38 in this volume) that a discipline “puts constraints on the assumptions, the reasoning, and the empirical claims that are permissible.” But he never tells us what these constraints are. Perhaps that is a minefield into which he dares not walk; but the omission does leave the concept of “discipline” undefined. I will try briefly to elucidate some of the constraints that our discipline imposes.

Imposing constraints is never free of costs. It is salutary, therefore, to ask the questions that an intelligent journalist might pose: What is the value of these constraints? Why should some information and some arguments be devalued or even excluded from consideration?

“Discipline” derives from the Latin discere, “to learn” (American Heritage Dictionary, 2d ed., 1982). A discipline, in the sense I am using it here, involves “training that is expected to produce a specific character or pattern of behavior, especially training that produces moral or mental improvement.” Political philosophy at its best, like ethical theory, should produce moral improvement, which provides another reason, in addition to those discussed by Laitin, to maintain its place in our teaching programs at the graduate and undergraduate levels.

As a discipline, political science has standards for the coherence of arguments. To qualify as a serious effort at political science, an argument has to meet four distinctive sets of tests, regarding logic, assumptions, causal mechanisms, and epistemology.

1. **Logic.** Political scientists are justly criticized when their arguments are illogical. A great value of formal theory is to establish whether purportedly logical arguments meet this test. One implication of this demand for logic is that political science propositions are often formulated in terms of abstractions. In our theories, we attempt to move away from proper nouns—although, as Laitin indicates, we do not achieve this goal in our course listings.

2. **Testing assumptions.** Like economists, political scientists often make theoretical assumptions that do not mimic reality. For instance, states may be conceptualized as agents, although no one has ever seen a state; people may be imagined to be perfectly rational, although none of us is (Kahneman and Tversky 2000). What is distinctive about political science as compared to economics is that our discipline still demands that colleagues who use such assumptions not merely invent them but show that they are not so inconsistent with actual practice as to be misleading. Political science is resolutely a practical science, focused on understanding political authority and on potential outcomes. Its coherence, insofar as it exists, comes from this focus on
substance and outcomes rather than from any particular method. The implication of our practical focus is that at some point, the theorist has to show how her theory illuminates some aspect of political reality.

3. Specification of mechanisms. Political scientists must spell out the linkages between sequences of variables, whether this takes the form of narrative, statistical associations, comparative analysis, or formal theory. These “social mechanisms” (Hedstrom and Swedberg 1998) or “causal mechanisms” (Elster 1989) enable analysts to test connections at a variety of points, to understand how processes work even if general laws are not available, and to consider possible alternative mechanisms. The implication of our emphasis on causal mechanisms is that we cannot be satisfied merely with descriptions of what happened; we have to try to specify how it happened, even if we do not have a full explanation.

4. Epistemology. Epistemological standards entail methods for making valid inferences. In my view appropriate methods are quite diverse: qualitative and quantitative, cross-sectional and historical, analytic and narrative. Analysts should not choose one method to the exclusion of others but should be encouraged to experiment with their combination (Bates et al. 1998). But for information to have a scientific status it must have been gathered according to publicly known procedures. For inferences to be valid, analysts must adhere to defensible rules of inference (King, Keohane, and Verba 1994, 7–9).

Journalists, and scholars from some other disciplines, sometimes see our search for internal coherence as narrowing our focus, leading us to avoid problems that are important in public life. They are often right. Our emphasis on logic, our willingness to make abstract assumptions, and our demands to elucidate causal mechanisms often lead us toward problems on which we think we can make scientific progress, rather than toward the most pressing issues of the day. Our epistemological constraints preclude the use of certain information—such as information based on impressions and rumors—that might be extremely valuable for political practitioners on a day-to-day basis.

The result, of course, is the familiar tension between scientific rigor and relevance. When the field veers too strongly in the direction of rigor, our audiences in public life turn away in disinterest. We do not have enough valid scientific knowledge to present a comprehensive view of politics. Indeed, our scientific knowledge resembles small pieces of a very large jigsaw puzzle. Inevitably, in our courses we “fill in the gaps” with information of varying quality that does not meet our highest scientific standards. On the other hand, if we fail to hold up
scientific standards at least as an ideal, our work will become indistinguishable from journalism and other popular commentary.

**Conclusion**

I think that political science is *potentially* coherent despite its current state of institutional disarray. Whether our discipline is *actually* coherent is more questionable. If political science were really coherent, it would not be so difficult to demonstrate the fact; and various commentators with the same purpose would not have reached the same conclusion on different grounds. Our efforts to show that the field is intellectually coherent are somewhat suspect because of our obvious institutional interests in reaching such a conclusion.\(^5\)

Nevertheless, there is a core around which coherence can be constructed. For me the core is the subject matter of political science, as I conceive it: processes of influence that generate authoritative policies with wide public implications for potential outcomes such as liberty, equality, and order. Studying that subject matter systematically requires that we accept disciplinary constraints, in particular those implied by standards of logic, testing of assumptions, specification of mechanisms, and epistemology. Although these constraints limit our ability to provide real-time descriptions of political events, they can increase the quality of our knowledge.

I believe that if we keep thoughtfully balancing the resulting tensions between rigor and relevance, we can both improve the quality of political science and continue to speak to audiences outside our own profession. However, our field will always be relatively messy. Contexts change and human beings respond, strategically and often in innovative ways. Those whose psyches demand that they be part of well-ordered programs of scientific progress should find other fields to study!

**Notes**

1. Perhaps Laitin includes liberty under “democracy,” meaning “liberal democracy.” The fact that he assigns readings from Locke and J. S. Mill under the “democracy” heading suggests that he has liberal democracy in mind.
2. Justice is both a procedural attribute and an outcome.
3. Easton explicitly included international relations within his formulation “because the fact of persisting international contact lays down as one of the conditions for its continuation the creation of means for the authoritative settling of differences” (Easton 1953, 139).
4. To illustrate this point, I refer to a classic article in an economics journal on the theory of the firm. In discussing agency problems, Eugene Fama posits (plausibly enough) the existence of a market for managers. But managers could still collude. So
outside directors become crucial guardians of the interests of the principals, the shareholders. Fama then invents “competition among the firm’s top managers,” which the outside directors “referee.” The outside directors are in turn “disciplined by the market for their services.” Fama never even claims to provide evidence of the competition among the firm’s top managers, nor does he explain how the “market for directors” works when they are typically chosen by CEOs and their work takes place entirely in nonpublic settings. Political scientists, I think, are more alert to such inventions than are economists. See Fama 1980, esp. 293–94.

5. Professor Laitin fears that funders take us less seriously as a result of the outside perception that our field is incoherent, and that “political scientists do themselves a disservice when they represent their discipline as a ‘blob’ or a ‘big umbrella’” (Laitin, p. 37 in this volume). The language of “do themselves a disservice” is the language of interest rather than of truth.
COMMENTARY

Intellectual Diversity in Political Science: A Comment on Laitin

IAN SHAPIRO

David Laitin’s “The political science discipline” makes a number of valid points and proposes a credible introductory political science course. I would not favor its becoming the introductory course in the discipline any more than I would favor instituting the French system of secondary education in American high schools. Whatever benefits might be derived from the Minister of Education’s knowing that the same chapter from the same text is being taught at the same time of day to children from Dieppe to Marseilles come at a considerable cost in terms of diversity and intellectual competition, investment by teachers in what they teach, and other well-known advantages of local knowledge. What holds for secondary education holds more obviously, to my mind, with the teaching of political science to undergraduates. In this field, however much Laitin might wish it were otherwise, there is little agreement about what to study and how to study it—let alone agreement on a body of established findings that can be canned into an introductory course for all. All methods of studying politics have limitations; we should be forthright about this in our teaching as well as in our research.

I will focus my remarks on two areas where I think Laitin’s approach is wrong-headed. The first has to do with his taking the economics profession as a model for what should be done in political science, and his related aspiration to see done for political science what Samuleson did for economics. This strikes me as resting on an erroneous conception of the role of introductory economics courses in the economics discipline. The second concerns his menu for the division of the discipline into political theory, political institutions, comparative politics, and international relations. This is a credible way to organize a course, but it is also challengeable—as is every possible way of doing so—for reasons that can be found in Laitin’s paper. I will conclude with some additional remarks on why fostering a plurality of introductory courses makes better sense for political science than does Laitin’s proposal.

Economics: A Model of What Not to Do

Political scientists are sometimes criticized for breathlessly chasing after ideas that economists are about to abandon. I think there is merit to this critique, but it is not my central point here. Rather it is that an important difference between the
way in which political science and economics are taught to undergraduates is
that political scientists generally try to link introductory teaching to the debates
at the cutting edge of the discipline (as Laitin’s model syllabus reflects),
whereas economists do not. The introductory economics course seems to me to
be a kind of LSAT for aspiring economists: a device to create costly barriers to
entry that will filter out students who do not have mathematical minds, and to
teach a few elementary ideas about price theory. It has virtually nothing to do
with what is going on at the frontiers of the economics discipline.

If one looks through journals such as the *AER* and the others I receive regu-
larly as a paid-up member of the American Economics Association, perhaps the
most striking fact is that the frontiers of economics research, while perhaps not
quite as anarchic as the frontiers of political science research, are a lot more sim-
ilar than political science stereotypes of economists (whether or not envy-based)
would lead you to imagine. The merits of rational choice models are argued over
at foundational levels, developments in psychology are having a large and un-
settling impact on neoclassical orthodoxy, there is renewed interest in the effects
of institutions on economic behavior and preference formation, and there has
been a resurgence of interest in applied and empirical work under the label “be-
avioral economics” of the kind that a decade ago was pooh-poohed as the sort
of thing you did if you weren’t quite up to high theory.1 Even among theorists,
developments in game theory and the study of auctions take it a massive dis-
tance from what is done in introductory economics courses.

One symptom of the gap between research frontiers and introductory—and
indeed much other—undergraduate teaching in economics is that those doing
the cutting-edge research have no interest in doing the undergraduate teaching.
They regard it as a waste of their time, and, at least in the elite departments of
which I am aware, they hire one-year visitors and flunkies to do the great bulk of
it—often to the chagrin of university administrators not to mention the under-
graduates themselves. Just because the introductory economics course is tedious
analytical hoop-jumping that bears scant, if any, relationship to what really in-
terests them, they have no interest in doing it.

My view is that this is not a healthy state of affairs in economics—that a dis-
cipline in which there is so vast a disjunction between what goes on at the re-
search frontiers and what is taught to undergraduates is a discipline in trouble.
Political science is so often portrayed as the scruffy wannabe cousin of econom-
ics (this certainly seems to be Laitin’s image), but I believe that what we do is
better. The reason our introductory courses differ so much from one another is
that we do try to link them to our own research interests—inevitably reflecting
the anarchy at the frontiers of the discipline, where people have competing per-
ceptions of what the basic problems are and how to study them. This struck me
repeatedly while reading Laitin’s syllabus and the rationale for it, but nowhere

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more dramatically than in his eight challenges to democracy (Laitin, pp. 23–26 in this volume). There Laitin clearly wants to get the students up to speed in a set of current debates about democracy that are seen as important in one part of the profession.

I think this is fine, though he includes much that I would not and leaves out things that strike me as important. One reason is that he has a view of democracy in which solving Arrow’s problem is important. On my view, by contrast, preference-aggregation is a comparatively unimportant feature of democracy. Features that are unaffected by the Arrow problem—such as fostering political competition and opposition—seem to me to be more important. This disagreement exemplifies the reality that there is little agreement in the field about what democracy is and why, if at all, it is desirable. Indeed many people who write and teach about democracy probably disagree with one another more than Laitin and I do. In this type of context, trying to suggest that there is an agreed-upon view of the matter 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 that we agree in order to do better at extracting funds from the federal government scarcely seems much better.

I recently completed an introductory book based on the survey course I have been teaching over the past two decades and I found myself grappling with this reality (Shapiro 2003). On the one hand it is meant to give students a grounding in a variety of normative debates that concern political theorists, getting them to understand what the various protagonists think is at stake and why they proceed as they do. On the other hand, it is written from a distinctive point of view in which I frame the issues as they seem most important to me. So I found myself writing in the preface that although the book is aimed at introductory classes it is not a textbook and I will regard myself as having succeeded if other teachers find that they can both use it in introductory courses and argue with it at the same time. A similar acknowledgment of the inherently controversial nature of political science scholarship was implicit in what struck me as the wise instruction Ira Katznelson and Helen Milner gave those of us who were asked to contribute to the 2002 State of the Discipline volume when they asked that we survey a piece of the political science terrain, but to do it in a distinctive voice and from a distinctive point of view (Katznelson and Milner 2002b).

Variables versus Problems

My second comment has to do with Laitin’s remarks about the advisability of organizing teaching and research around dependent rather than independent variables. I fully agree with Laitin’s preference for avoiding the choice of independent variables as the basis for disciplinary organization and training. Aside
from the sheer silliness of trying to develop tools and equip a toolkit before one
knows what one is going to measure, repair, or construct, about which I have
probably written more than I should, there is no faster way to lose the interest of
undergraduates. Unlike graduate students and junior faculty, many of whom un-
derstandably, if sadly, believe their careers depend on investing in the theories
and methods favored by those who they expect to decide their professional fates,
bright undergraduates have a nose for what is important and demand to know
the implications of academic research for some discernible bottom line.²

Yet I find my agreement with Laitin’s instinct to avoid organization of teach-
ing and research around independent variables to be at odds with his recommen-
dations for dividing up the discipline as he does. He does not go far enough in
his discussion of what is wrong with the independent variable focus as the basis
for organizing the discipline (as distinct from researching a problem once it has
been specified, where we all know that the pitfalls of selecting cases on the de-
pendent variable). Something can only be a dependent or an independent vari-
able in relation to a problem for which one is trying to account. Whether or not
it is important to study political institutions, for instance, depends on how im-
portant they are hypothesized to be for what one is seeking to understand. It is
not clear to me whether Laitin thinks we should study institutions because they
are deemed an important independent variable or an important dependent vari-
able. In order to clarify this he would have to give an account of what he takes
the important questions of politics to be which, surprisingly, is nowhere supplied
in his essay.

If, for the sake of discussion, politics is considered to be the study of who
gets what, when, where, and how, then institutions might be important, but they
might not. In the early part of the twentieth century they were thought to be ex-
ceedingly important, but then institutions fell out of favor as independent vari-
able, as Marxists, elite theorists, behaviorists, and early rational choice theorists
deemed such factors as class position, path-dependent access to the instruments
of power, political behavior, and individual self-interest all to be more conse-
quential in determining political outcomes. In recent years institutions have
come back into favor among political scientists as potentially important ex-
planatory variables. I suspect that this is partly because of the failure of these
various approaches to deliver much substantive knowledge and partly because
historians have stopped studying institutions as a by-product of their increased
interest in social history. Whatever the reason, political science has once again
been bitten by the institutions bug, with different methodological camps squab-
bling over who are the “real” new institutionalists.

Without seeking to take sides in these debates here, it seems to me unwise to
reorganize the discipline around whether or not a particular theory or hypothesis
will turn out to be true. Perhaps the next generation of scholars will discover that
institutions matter less than is presently thought. Current scholarship on the subject of democratic stability suggests, for example, that Linz and others were partly mistaken in thinking parliamentary institutional forms contribute as much as they believed was the case to the apparently greater stability of parliamentary than presidential systems. Other factors, such as the structure of coalition politics and the representation of the president’s party in the legislature, may well account for more of the variance ( Fowleraker 1998, 651–76; Cheibub and Limongi 2000). In the study of regime stability generally, it now seems that economic variables are probably more important than institutional ones (Przeworski et al. 2000, chap. 2). Again, without wishing to take sides in these controversies here, surely they are the meat and potatoes of ongoing research rather than the basis for organizing and reorganizing the discipline every time a theory falls in or out of favor.

Comparable points can be made about Laitin’s treatments of international relations and comparative politics. In international relations much innovation over the past decade has revolved around breaking down the obviously artificial barriers between comparative politics and international relations due to the abandonment of the black-box view of national polities among international relations scholars. This resulted from their recognition of what most people would have said all along is obvious: that domestic politics has a large impact on the international behavior of countries. In many circumstances there is, in any event, a degree of arbitrariness to declaring an aspect of politics to be part of international relations rather than domestic politics. Civil wars, for example, are part of domestic politics while intercountry wars belong to the realm of international relations. But in much of Africa national boundaries were drawn on maps by nineteenth- and early-twentieth-century colonial administrators, often with no regard to the politics of identification within and among the countries that were thus minted. The result is that what might appear to outsiders, and coded by international relations scholars, as intercountry wars might be understood by the protagonists as civil wars, and vice versa. Then the political scientist’s disciplinary grid will not correspond to the phenomena under study.

With respect to comparative politics, it again seems odd to assume, as Laitin’s discussion does, that variation among national polities should be the basic unit of analysis in advance of the specification of a problem. Obviously if one wants to understand the consequences of different national electoral systems such an assumption makes sense, but there are many political questions that call for comparative investigation where it is logical to look at subnational or other forms of intrapolity variation. This is most obvious in federal systems, but urban politics is another area in which intrapolity comparative politics might be more illuminating than national comparisons—depending on the question one is trying to answer. Or one might become persuaded that it is important to study
intrapolity regional variation, as does Bob Putnam in his study of the determinants of institutional performance in Italy (Putnam 1993). Then again, there might be good reasons for comparative study of transnational regionalisms when we reflect on the kinds of questions that may work their way onto the political science research agenda due to the creation of regional political-economic entities such as NAFTA and the EU.

Concluding Remarks

As should by now be evident, my view is that the best way to organize both teaching and research is by reference to problems or questions, not variables. Particularly if the goal is to interest bright undergraduates in political science (though I would make this case regardless of that consideration), step one is to articulate a problem and make the case for its importance. This is appropriately followed by moving to more rigorous specification of it, which involves sorting out what the dependent and independent variables might be. Then hypotheses are spelled out and the most appropriate tools for evaluating them are selected, and so the endeavor proceeds. I would argue that, contrary to the current practice in economics, the shape of undergraduate teaching in political science should embody this research structure. For the most part I think it does. The real reason for the multiplicity of introductory courses in political science is that political scientists differ over what they think politics is. As a result, they naturally reach for different specifications of problems and different ways of studying them.

This strikes me as a healthy state of affairs. A significant danger for political scientists to guard against seems to me to be their penchant for confusing their discipline with their object of study. Most people are attracted to political science because they want to understand more about, and perhaps some because they want to influence, politics—not the political science discipline. It is easy to lose sight of this fact, and there are many professional incentives pushing political scientists to do so. But the incentives should be resisted in favor of the problem-driven approach proposed here, where the tools of the discipline are brought to bear on problems that have been independently identified rather than being artifacts of those tools. If the cost of this is a plethora of curricula that embodies the conflicting views of political problems in the discipline, so be it. In my judgment these are better costs to pay than those of mimicking the last generation of economists. We are less likely to produce an inward-looking discipline designed primarily for the benefit of its practitioners and more likely to say things that are worthwhile. In any case there are signs that economists have begun to realize the limitations of their disciplinary orthodoxy, in their research if not their introductory teaching.
Notes

1. This is an impressionistic claim, to be sure, based on browsing and conversations with economists. For three anecdotal illustrations of high-powered technical economists who are moving away from orthodox economic models of human behavior, see Daniel McFadden’s Nobel lecture, “Economic Choices” (McFadden 2001), on the empirical validity of rational actor models and alternatives to them (Bewley 1999), whose analysis of labor markets eschews conventional rational actor models in favor of behavioral analysis and field research, and Robert Schiller’s reliance on psychological theories of herd behavior, among other theories, to explain buying and selling in U.S. equities markets in his widely acclaimed Irrational Exuberance (Schiller 2000, 135–202).

2. In this connection it may be of interest that a few years ago one of my colleagues in the Yale economics department taught Pathologies of Rational Choice Theory (Green and Shapiro 1994) to a joint seminar of economics seniors and graduate students. He reported the seniors to be fully persuaded by our argument while the graduate students dismissed it as irrelevant to their concerns.

3. For a more thoroughly reasoned defense of this view, see Shapiro (2002).
In several senses, this is an exciting moment for our discipline. One manifestation is the contention raised by rational choice theory and its critics. A more important if quieter one is the fact that the barriers among fields are everywhere coming down. If at mid-twentieth century political scientists had an intellectual defense to the organization of the discipline into four fields—American politics, comparative politics, international relations, and political theory—one no longer exists. The problem facing the discipline is that there is no consensus over what ought to replace the current organization.

In this setting, the time is right to rethink the organization of the discipline, and David Laitin has cleverly used the organization of a model “Introduction to Political Science” as his rhetorical vehicle for suggesting a reorganization and synthesis of the field. There is much to criticize of this statement. Yet to focus on the flaws in Laitin’s ideas is to miss the point. No proposed reorganization of the discipline is likely at this time to satisfy even a majority of the discipline. Instead, the principal value of Laitin’s proposal is its provocativeness—that it urges us to participate in a dialogue about a set of issues central to the future of our field.

In three senses, however, Laitin’s focus on an introductory course is more than a rhetorical device to discuss larger issues. First, it demonstrates the disparate nature of the discipline and the fact that there is stark absence of any consensus about how we should introduce undergraduates to our discipline. This contrasts with how to structure a first-year Ph.D. program in political science. Second, Laitin’s approach demonstrates the strengths and weaknesses of any attempt to provide more coherence. Third, the problem with devising a satisfying “introduction to political science” goes beyond the absence of consensus. As Laitin’s quick survey of practice at major universities suggests, we do not take our introduction seriously: at most colleges and universities, few other courses in the major treat this course as a true introduction in the sense that they build on its contents. A major weakness with the structure of a typical undergraduate curriculum is our inability to bring undergraduates to the forefront of our field. Instead, we present a hodgepodge of courses that are more like introductions to a wide range of pieces of the field.
COMMENTARY ON LAITIN

In what follows, I focus on Laitin’s suggestions for reorganizing the discipline. I first emphasize some of the exciting aspects of the fall of barriers among our fields and how this inevitably implies a different structure of the discipline—even if that structure is not apparent today. I then discuss some aspects of Laitin’s suggestions for reorganization. Finally, I discuss some of the barriers to reorganization that not only work against it now, but are likely to continue to do so even if a compelling and widely shared intellectual justification for reorganization emerges.

Falling Barriers among Fields

If in an earlier era political scientists agreed how to organize our discipline, a series of intellectual developments have undermined the basis for this agreement. Consider:

First, many comparativists have long argued that there is no compelling reason for why American politics should be a separate enquiry from comparative politics. Increasing, American politics scholars have come to agree. As Laitin suggests, American nationalism remains a large part of why this field remains separate.

Reinforcing this historic division between American and comparative is the tradition of American exceptionalism in comparative politics. Although the United States was commonly a case in the immediate postwar generation’s studies—witness the work of Almond and Verba, Dahl, and Lipset—comparativists have not maintained this tradition.

Over the past two decades, considerable interaction between American and comparative has emerged as, typically, tools for studying elections and institutions in American politics have been adapted and applied in comparative contexts. As Laitin emphasizes, American politics enjoys the highest export-to-import ratio of tools and techniques.

Two aspects of this high export-import ratio are worth remarking. First, on the positive side, the high ratio reflects American politics as a fertile ground for the development of new tools and techniques. Second, on a more negative note this greater reliance on sophisticated tools combines with the tradition of American exceptionalism in comparative to reduce the importation of tools and concepts from comparative critical to understanding aspects of American politics missed by the dominant approaches and paradigms.

One illustration of this last point is worth pursuing. When I first began to study comparative politics and encountered the literature on democratic stability, it astonished me that not only do we lack a good theory in American politics to account for the stability of American democracy, but we do not even ask this question.\footnote{Put simply, American politics would be enriched by confronting many of the standard questions and concepts in comparative politics.}
The second compelling challenge to the standard divisions among fields concerns the division between comparative and International Relations (IR). As is widely recognized, it is no longer possible to tell where one ends and the other begins. Although we can identify questions that are squarely in one field or the other, the answers that have emerged in both fields typically draw on tools, techniques, concepts, and factors traditionally in the other field. The very different approaches of Katzenstein and Rogowski to the internal political organization of small European trading states both sit squarely across the divide without easily being classified as from one field or the other. Skocpol has taught us that we cannot understand social revolutions—traditionally a comparative question—divorced from the international environment. And a large literature emphasizes how the “second image reversed” (Gourevitch 1978) results in dramatic effects of the international system on state building, as Ferguson’s Cash Nexus (2001) and Bates’s Prosperity and Violence (2001) emphasize. Finally, the emergence in the last twenty years of the literatures on domestic effects of foreign policy making and on the so-called democratic peace compellingly demonstrates the value of going beyond the traditional assumption that international relations can be studied without modeling the internal workings of particular states.

The final compelling challenge to discipline walls concerns a traditional rationalization separating international relations from American and comparative politics, namely, the assumption of anarchy. The absence of an external enforcement agent in the international realm limits the ability of states to make agreements concerning security or economic relations. International relations specialists traditionally predicated theories of war on anarchy.

Yet here, too, the distinction is no longer easy to sustain. First, it is now apparent that similar questions arise in domestic politics—what, after all, compels individuals, groups, politicians, or the military to observe constitutional rules? Here, too, the absence of an external enforcement agent plagues the ability of citizens and political officials within states to adhere to constitutional rules. Indeed, the observation that most countries in the world cannot sustain democracy, constitutions, and markets suggests that the problems of anarchy within states are considerable. In other words, the very different problems of sustaining peace and domestic constitutions are in important respects similar.

Second, recent tools have emerged to make some headway on both problems, focusing on issues of self-enforcing agreements and credible commitments, as the work of Axelrod and Fearon in international relations and Przeworski in comparative demonstrate. Similarly, the rise of international agreements, such as GATT, WTO, and NAFTA, all suggest that institutions alter the interests of states and hence their behavior in ways that parallel how domestic institutions alter the interest and hence behavior of actors within states.

The fourth aspect of integration concerns the rise of the methods field. I am a
methodological pluralist, and I believe this pluralism is important for the long-term integration of our discipline. The label “methods” is largely associated with statistics, data analysis, and the techniques of scientific inquiry (e.g., King, Keohane, and Verba 1994). Yet systematic methodological issues arise in many areas of inquiry. There is a long tradition discussing case studies and case selection. Similarly, methodological issues arise in historical institutionalism and in applying game theory.

The final aspect of integration is perhaps the most difficult—the divide between the three traditional substantive fields and political theory. Here I return to Laitin’s introductory course. Political theory has tended to develop separately by focusing exclusively on ideas, often divorcing the study of ideas from the everyday context of politics. Laitin’s focus on problems of order, democracy, and community emphasize, as many others emphasize, that there is a seamlessness to the normative and positive aspects of these and related questions. Indeed, many of the great texts in the canon were written by writers struggling to understand the difficult dilemmas confronting their societies, as the Anglo-American writers Hobbes, Locke, and Madison demonstrate. The discipline would be greatly enriched if this boundary were more commonly breached.

**Reorganizing the Field**

Although this remains the most difficult question with, as yet, no satisfactory answer, it remains worth debating. Laitin’s suggestions are both intriguing and problematic. The six new fields are logical to a degree. American politics is too similar to comparative to be separate; yet Laitin parcels it not to comparative politics, which remains a separate subfield, but to political institutions and public policy.

More problematic is the internal tension between this division and Laitin’s intriguing remarks about transforming the organization of the field from one around dependent variables to independent variables. The advantage of this approach is that it forces attention to relationships and begs the analyst to understand the mechanisms underlying that relationship. The disadvantage is that many of the most interesting variables are not readily classified as independent or dependent. As Laitin suggests, we can study the effect of gender, religion, and race on various aspects of politics; that is, as independent variables. The other side of the coin, however, is that each of these issues can also be considered a dependent variable: for example, in a given country why is gender, race, or religion a critical subdivision of politics at only some times, and why does the importance of these variables change over time?

Finally, Laitin mentions a most intriguing suggestion, that the discipline might usefully be organized around sets of problems. As I suggested above, central
problems of both international relations and comparative and American politics involve the absence of an external enforcement and hence the reliance on institutions (when possible) to sustain peace, economic agreements across international boundaries, constitutions, and democracy. A range of these types of mechanisms have emerged, as the widespread application of the prisoners’ dilemma and, more recently, coordination games suggests. Inevitably, understanding common mechanisms underlying diverse problems at once improves our understanding and forces previously unnoticed connections.

Institutional Barriers to Reorganization

Although impressive intellectual forces press for reorganization of the discipline, strong forces support the current “equilibrium” division of the field. These forces reflect self-interest and the allocation of resources within universities. I briefly mention three forces that support the status quo. First, consider the coordination problem of the job market: If Stanford starts selling students who, following Laitin, advertise themselves as experts in some mechanism with applications across two or three traditional fields while all other universities search for new faculty using the traditional four fields, Stanford students will be at a competitive disadvantage simply because they no longer quite walk and talk like ducks. To avoid being at a disadvantage in obtaining jobs, students of such mechanisms are compelled to focus predominately on one field and to advertise their research and teaching expertise within that field. Moreover, this problem remains even if all major universities agree that the current organization is antiquated and inadequate, as long as there is no consensus—or focal solution—about what the new organization ought to be.

Second, departments all struggle with their administrations over resources. In recent decades, university fund-raisers have been great at raising money for centers organized around—as Laitin emphasizes—proper nouns: Latin America, Europe, and Asia; but also international affairs, arms control, Congress, or the presidency. Similarly, it is often useful to argue with deans that Harvard and Yale are both developing strength in region X—say, South Asia—and we have no one. The ability to raise funds in this way represents an obvious force supporting the status quo.

Finally, and most subtly, it concerns an implication of departments qua majority rule institutions. Majorities are fickle and unstable. Given the nature of our careers—investment in particular specialties—new departmental majorities can disenfranchise particular areas, discounting costly investments made by other faculty. The common quip about academics has it that the fights are so bad because the stakes are so low. This quip may be true with respect to external effects of academic politics, but the stakes in terms of individual careers are large.
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To reduce the uncertainty and potential chaos in departments, most rely on a range of mechanisms to prevent chaos from erupting. The most common mechanism is the slot-mentality associated with the fields. Most departments have an implicit agreement about the rough balance among fields. Although this may be adjusted at the margin, in the long run it tends to be sustained. These mechanisms in part prevent huge fights; but they reinforce the status quo.

Conclusions

As Laitin’s proposed reorganization suggests, this is an exciting time for the discipline. The intellectual arguments for the current organization of the field have been greatly weakened as the barriers among the fields have fallen dramatically.

Because no obvious reorganization exists, it is useful to debate different visions of how to do so. Laitin has provided one. Others should do so as well. In the end, the intellectual debates around these proposals will enrich our discipline.

Notes

1. Of course, comparative politics provides no compelling explanation of American democratic stability. But at least they have formulated the question and provided the beginnings of an approach to address this issue.
2. The emergence of the European Common Market after World War II and its evolution into the Europe Union underscore this point.