Politics, Political Science, and Specialization

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Contemporary political science is specialized, deeply concerned with its methods, and politicized. It also remains peripheral to most public debates. But the relationships among these characteristics are ambiguous and each yields advantages as well as costs.

Political Science Knowledge and the General Public. I do not think that political science has had great influence on public discourse and public policy. My guess is that if we were to take a simple indicator like the discipline of authors whose books are reviewed in the *New York Times, New York Review of Books*, or journals of political and social opinion, we would find few political scientists. If we were able to find other measures of the contributions to public debate I do not think we would find many political scientists playing leading roles. Of course there are exceptions like Robert Putnam's analysis of social capital and James Q. Wilson's studies of policing and urban politics—but these indeed are exceptions.

In international politics and foreign policy, work recognized by us as political science but sometimes written by economists (most obviously Thomas Schelling) has influenced elite opinion and policy, but this is somewhat different from contributing to public debate. Many of the people in the high echelons of the foreign policy and national security apparatus have political science Ph.D.s, and academic theories have clearly influenced policy in at least a few areas, the primary example being arms control. But it is not clear how much the policymakers with advanced degrees draw on their academic knowledge, and nuclear targeting policy, which is the operational guts of nuclear strategy, was much more influenced by traditional military thinking that stressed the importance of being able to win a war than it was by academic deterrence theory. Theories of the democratic peace may have had some influence on both the elite and the public levels, but I suspect that this is only because democratic peace deeply resonates with traditional American beliefs. Most Americans "knew" that democracies were benign and did not fight each other long before we told them so.

I am reminded of the lack of our contribution when I visit Aspen in the summer. The Aspen Center for Theoretical Physics has a weekly lecture series in which their researchers explain recent findings and puzzles. Of course they can't go into the details or the math, but they deservedly attract a large audience when they explain the problems, results, and questions they are dealing with. The Aspen Institute also has speakers on public affairs, and many of them are political scientists. But what they talk about are current policy issues, not the theoretical debates or empirical evidence of the discipline. At dinner parties, how often are we asked what our recent findings, models, and arguments mean for politics and public policy, or even how they solve—or raise—intellectual puzzles?

What is more, I think their lack of interest is not entirely unjustified. I can find some areas and questions in which we have things to say that intelligent people don't already know, but I'm not sure we could mount an annual lecture series on them.

Politics and Political Science. For most of us, our subject matter is inherently political and our arguments influence and are influenced by our political preferences. Indeed, I think that the latter is more true than the former. (I cannot believe that most of the profession's members being liberal Democrats does not affect the research that we do, both individually and collectively. This was a topic of a roundtable at the 2001 Annual Meeting.) Most of us were drawn to political science not because of theories, enticing academic analysis, or even gifted teachers, but because we were fascinated by, attracted to, and repelled by the politics that we read about in newspapers or had a chance to practice in school or political campaigns. As I was born in 1940, many of my first memories are of World War II and the beginnings of the Cold War (my nonacademic friends consider me a bit strange for this), and the questions these events raised in my mind have remained the focal point of much of my research ever since.

Almost everything I study, including abstract arguments or detailed analyses of past cases, has fairly direct if debatable implications for current policy. Yet—and this touches on several of Rogers Smith's points—if I am to be an honest social scientist, I must be prepared to entertain arguments that lead to policies I dislike and to be open to the possibility that research will lead me to see that my original preferences were incorrect. (The term "preferences" is ambiguous and can refer to fundamental values and to instrumental beliefs. It is presumably easier to be open to changing the latter than the former, and it is the latter that are more likely to be called into question by research.) I can think of one—but only one—case where my research changed my political views. Studying misperception convinced me

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that cases of spirals of unnecessary conflict were more common than I had previously believed and that there were important instances in which the use of threats had decreased rather than increased the state's security. But I did this research during the Vietnam War and cannot exclude the possibility that my beliefs about this conflict influenced the way I saw other cases and my more favorable evaluation of the spiral model.

Because people resist facing value trade-offs, arguments and political preferences tend to be consistent and reinforcing. Morton Kaplan's *System and Process in International Politics* is an abstract and theoretical book, but its arguments also point to a "hard-line" foreign policy, which happened to be Professor Kaplan's policy orientation. Kenneth Waltz's *Theory of International Politics* put forth a general analysis of the stability of bipolar systems that implied, among other things, that the U.S. did not have to fight in Vietnam, a position that also happened to correspond to the author's views. One of the most contentious areas of domestic research is the effect of school choice and vouchers and I will say a bit more about it later. Here I just want to note that one can do a good job of predicting what a study will find by knowing the preferences of the scholars who undertook it.

Current foreign policy debates about national missile defense and the expansion of NATO illustrate these points. Although many IR scholars have strong views on these matters, they have played almost no role in the public debate, both because they have not been sought by the media and because they have not gone out of their way to take their arguments to the public. I believe that there is close to a consensus among academics that these policies are foolish, but this is not widely known, and probably would have little impact if it were. Someone who favors these policies could look at my previous paragraphs and retort that since most social science arguments reflect policy preferences, the near-consensus merely tells us of the academy's liberal bias. I would not completely deny this, but cannot help thinking that my years of studying international politics inform if not determine my conclusions.

Specialization. Specialization is both necessary and troublesome. It is necessary because it allows us to study problems and theories closely; it is a problem because it takes our minds off broader questions and cuts us off from findings and perspectives in other areas. It also encourages the development of arcane concepts and terms that make communication difficult within social science and impossible outside it. But these problems are not unique to science. The reigning ideology in the humanities is both "antiscientific" and involves jargons that seal off most of the discourse from the outer world, which is particularly ironic considering that for many people in these fields the goal of their study is to change society.

Specialization within political science has greatly increased during my lifetime. When our graduate students complain about how much they have to learn for their comprehensive exams, we tell them: "I did it, so you can too." But we didn't do it. When I was a first-year graduate student, I wrote a seminar paper reviewing the literature on voting behavior. All of it. There simply wasn't that much. Now there are more IR journals than there were political science journals 40 years ago. This proliferation both causes and reflects greater knowledge. Without falling prey to the Whig view of intellectual history, I think we know a great deal more than we did when I was in graduate school, although there have also been losses in terms of concepts and arguments now forgotten.

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An obvious cost of specialization is that we fail to learn from each other and waste effort reinventing the wheel. To take an example that is sure to be seen as subfield bragging, over the past 20 years or so American and comparative politics have moved ahead (or at least moved) on the basis of seeing that it is difficult for the actors they study to make binding commitments to each other. There is a great deal to this, but the arguments are familiar to me because this has been the standing assumption in IR from the beginning (and is questioned by many in the field). My colleagues could have moved more quickly had they been aware of the IR work on bargaining, credibility, and cooperation under anarchy.

The need to ameliorate the unfortunate effects of specialization is the main reason APSA decided to establish a new journal of review essays, essays that apply political science knowledge to public policy questions, and "cross-cutting" essays that are designed to interest people in more than one subfield. Contrary to the widespread impression, the development of *Perspectives on Politics* was not a response to the feeling that *APSR* did not publish research in all parts of the discipline. Instead, it grew out of the deliberations of the Strategic Planning Committee, which articulated our need for a journal that could deal with the problems created by specialization, that could help us learn about and be stimulated by what was being done in areas outside our particular interests.

For all the problems it entails, specialization is necessary not only for academic progress, but for contributions to public policy and discourse. Some of what we have to say comes from our general political science perspectives, but much is generated by careful and detailed study of particular, narrow topics. Thus while I have a lot to say about the Florida presidential election and the Supreme Court decision that settled it, most of this comes from reading the *New York Times* and opinion pieces. I bring to the topic some expertise derived from political science knowledge, but this is the smaller part. When I talk about current problems of U.S. foreign policy and international politics, however, the proportions are reversed: I draw on newspaper stories and journals of opinion, but most of what I make of these comes from my specialized research. So I do not think it would be correct to pose a principled conflict between specialized knowledge and contributions to public discussion.

Both specialization and political preferences explain why we often ignore relevant work. People tend not to know of scholarship in areas outside their own; they also do not like to give credit to work that has political implications they reject. Thus many students of politics interested in questions of compliance with agreements do not realize that this question preoccupied studies of arms control during the Cold War, and the recent resurgence of interest in the "power of ideas" in international and comparative politics (generally written by scholars committed to extensive social change) rarely cites or draws on earlier work that happens to be written by conservatives such as Edward Banfield, James Q. Wilson, Samuel Huntington, Lawrence Harrison, and the anthropologist Oscar Lewis.

Significance and Validity. There may be a trade-off between the significance of a subject and the degree of confidence we

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can have in our results (although we may stop exploring even small questions not when we have answered them, but when we get bored). I remember one of my college professors saying that political scientists were divided into two camps, one gathered under the flag "it's true, even if it's trivial," and the other under the banner "it may not be true, but at least it's important." The problem then is not a new one, although the former camp may be larger than it was in the past. Regardless of the validity of this claim, the dilemma is real: while some unimportant questions are hard, few important ones are easy and many may be intractable.

Questions and Methods, Not Answers. We may have more to offer by asking the right questions than by getting the right answers. Similarly, we may have more to contribute by criticizing others' arguments than by coming to firm conclusions. This is consistent with Smith's stress on the importance of honesty, and it often involves being self-conscious about our methodologies. As usual, I can best explain what I mean through examples. One important case is school choice and vouchers, an area where political research and analysis has received public attention. Most of the studies are designed to tell us whether children who move from public to private or parochial schools fare better than matched students who stay in the public system. But while intriguing, this is not the question of primary interest if we care about improving the education of large numbers of children. To the extent that schools make a difference (and if the famous Coleman report is to be believed, the difference is rarely great), the "experiments" are largely irrelevant, or at best a first step. As Mark Schneider, Paul Teske, and Melissa Marschall assert in their *Choosing Schools*, large-scale improvements can only come through the impact of the possibilities of choice on the bulk of the public school system. Other schools must be able to emulate or learn from the more successful ones. The fact—if it is a fact—that students who move into successful schools do better than their counterparts who remain cannot tell us how or indeed whether this can be done. (A minor methodological point is that relying on standardized tests to judge students and schools is likely to lead to behavior that degrades the tests as useful measures of knowledge.)

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Thus a famous marine biologist who normally thinks in terms of interconnected systems argues that mandating double-hulls on oil tankers would be certain to decrease oil spills. But, as readers of my *System Effects* know (please excuse the advertisement), this common sense statement is incorrect. To take an IR example, comparisons between the "success rate" of the use of force and the application of economic sanctions would be interesting, but could not provide guidance for what should be done in the future. Part of the reason is that this knowledge could change the behavior of actors (a point Smith notes); another part is that, in a form of selection effects, decisionmakers chose to apply one instrument in one set of cases and the other instrument in other cases on the basis of their estimates of the likely effects. Attempts by later analysts to compare the sets of cases holding everything else constant are not likely to catch all the relevant differences. To take an actual case where bad methodology mattered, the U.S. might have caught the spy Aldrich Ames much sooner if the counterintelligence officers had gone beyond asking who knew the identities of the agents who had been eliminated by the Soviets to also determining who did not know the identities of those agents who were still operating. I could extend this list, but the point of the exercise is simply to remind us that concern with methodology is not antithetical to being able to contribute to public debate.

The last word has some significance. Often public policies are indeed topics of debate, with polarized opinions. Perhaps good political science can contribute both complexity and civility. I would like to think that political scientists are trained to see multiple facets of issues, to detect trade-offs, to see that truth and justice are rarely all on one side (and that truth and justice may conflict with each other), and that being open to new evidence and arguments is likely to produce better ideas and policy. But it is an interesting question whether political scientists or politicians better exemplify these values.

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