Progress and Poverty in Political Science

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Perhaps we do not give ourselves enough credit. Analyzing politics may well be the most difficult of all intellectual endeavors, not only because of the intrinsic complexity of the subject matter, but also because of how deeply intertwined our investigations and our investigators are with the subjects we study. The substantive mysteries of human agency, the need for humane ethical constraints on political investigatory techniques, and our own inescapable concerns about the political implications of what we find, all mean that in some respects it is intellectually and psychologically harder to be a good political scientist than a good chemist, a good geologist, or even a good rocket scientist.

Because analyzing politics is hard, methodological concerns necessarily bulk large in any proper evaluation of every piece of political science research. In the case of the two useful essays offered here by Bennett, Barth, and Rutherford and Schwarz-Shea, for example, I am troubled by the former's decision to omit political theory (except formal theory) from their analysis and to lump together many kinds of qualitative work under the rather misleading heading of "case studies." Schwarz-Shea makes it commendably clear, moreover, that because she counts as a class on "qualitative" methodology any course that contains some mention of any of a number of topics, her results may well overstate the diversity of contemporary methodological training.

But to some degree, these contrasting coding choices may cancel each other out; together the two essays paint a portrait of the contemporary political science discipline that rings true, at least in my experience. Schwarz-Shea shows that most graduate students in most programs are likely to be exposed to requirements and a course structure that many will reasonably interpret as signals that quantitative methods constitute the core of political science. Bennett, Barth, and Rutherford agree that what they call "case study" methods are much more widely practiced than taught. They also supply evidence that many leading journals tend to be methodologically one-sided; and that the *American Political Science Review,* in particular, has long reinforced the message that quantitative methods are what matter most in political science. It has featured such work more than any other sort, and more than the discipline as a whole actually does.[1] We now have three issues suggesting that *APSR* editor Lee Sigelman is changing that pattern by attracting a wider range of submissions as well as through decision-making procedures more open to controversial work. But though I believe change is genuinely underway, the overall patterns of professional training and journal publication are surely still much as depicted here.

Let me acknowledge that, as someone who has long chiefly employed "qualitative" historical, interpretive methods, and verbal theorizing, I have been part of the problem. I was one of the Yale faculty members whose collective failure to offer a course on nonquantitative research methods led graduate students to demand one in 2001, as Schwartz-Shea notes. My attitude for years was that such courses were too abstract to be of practical use: one learned "my" methods by doing, not from methodological textbooks. I also thought a "qualitative methods" course would be less enjoyable to teach than a substantive course, and I like to have fun in my work.

The Perestroika movement and the Yale protest persuaded me, however, that failure to offer such courses sent just the signal these essays depict: quantitative methods appear to be the only "real" way to do political science. That message is deeply misleading, because quantitative methods at their best help with only one aspect of doing good political science. They assist us in descriptive and explanatory inference testing, as King, Keohane, and Verba (1994) have rightly stressed. But we do not always need complex quantitative techniques to make reliable inferences. When Jeffrey Tulis argued that 20th century presidents communicated directly to the public far more often, and as a far higher percentage of their official statements, than 19th century ones, for example, he needed only to classify and count their statements to make his point rather irrefutably (Tulis 1987, 140). It is hard to see what greater confidence concerning his descriptive inferences could be gained by additional statistical analysis.

More fundamentally, inference testing cannot generate good political science if we never have substantively significant hypotheses to test. The formation of such hypotheses, of new and revealing descriptive and explanatory conceptions of politics, generally requires rich contextual knowledge, empathy, acuity, and imagination, much more than mastery of quantitative techniques. Obviously, a graduate education that failed to nourish such skills, and a pattern of journal publications that failed to recognize the scholarship that embodied them, would not represent the best road to intellectual progress and might, indeed, lead to a discipline that was conceptually impoverished.

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I do not think we have gone too far down that road, but these essays give us reason to make sure that younger political scientists feel intellectually and professionally able to explore many paths. For as Schwartz-Shea contends, methodological choices made in ignorance of alternatives, or from fear that only a few choices have a chance of professional success, do not really represent choices at all.

That is why in the past year I offered my first graduate course blending history of the discipline, philosophy of science, and texts on, and examples of, a considerable variety of qualitative methods. I was still convinced, however, that one largely learns these methods by doing; and one advantage quantitative methods courses have over "qualitative" ones is that they can give students statistical problem sets, where they learn by doing throughout.

Seeking to match that advantage, I called the course "Doing Political Science" and required all enrolled students to undertake research projects using one or more "qualitative" methods, and to write a memo each week exploring how the "qualitative method of the week" could or could not help them complete their projects. By the end of the course, I had come to the humbling recognition that my own methodological thinking had been clarified by the readings, discussions, and reflections on the students' methodological experiments. The students did very good work and were generous to the course in their anonymous evaluations. And it was fun, even for its reluctant instructor. So, now I think such courses are not only useful for sending students the signal that many kinds of work are legitimate parts of political science. They also really help them do many kinds of good political science. And I think, more than ever, that the main regional journals, and certainly the *APSR,* should be genuinely receptive to all kinds of good political science.

Thus, I am very much in sympathy with the spirit of these essays. Let me also call attention, however, to one issue concerning the structure of graduate education in political science and in our disciplinary journals that the essays approach but do not address. That is the question of the conventional structure of subfields in political science, adopted in most departments and reflected to some degree in the editorial foci of various leading journals. As it has done for roughly the last half-century, the political science profession continues to divide itself chiefly into American politics, comparative politics, international relations, and political theory, with older fields like public administration and public law and newer ones like formal theory and political economy added here and there.

I have long heard many political scientists complain that this structure makes no intellectual sense and have yet to hear anyone argue that it does. The criticisms are now familiar: as Schwartz-Shea observes, it is not clear why American politics is a distinct field apart from comparative politics; the barriers between work in comparative politics and international relations are increasingly breaking down; and political scientists in all fields engage in political theorizing, sometimes drawing on the history of political philosophy in order to do so. So why divide up our curriculum and many of our journals in these ways?

The main answer appears to be one that confirms the importance and intractability of institutions: our graduate educational programs are still structured with those fields because we know that most departments still structure hiring accordingly, and we want our students to get jobs. Yet departments still structure hiring in this way chiefly because they want to be staffed to train students in ways that will enable them to be hired in the many departments and published in the many journals that are still structured with those fields. And the journals structure themselves that way because the profession structures itself that way. So on we go, perpetuating a disciplinary division of labor that often works against the development of broad political understanding and insight, and one that often shelters different forms of methodological narrowness. We continue because we are reluctant even to raise, much less to try to resolve, the collective choice problems involved in establishing something different and better.

I feel fortunate to have worked in two departments that have at least sought to improve things. The faculties of Yale and Penn have in recent years seriously discussed what sorts of field structures might make more sense, and they have undertaken innovative hiring processes that focus not on the traditional subfields but on the political problems that seem of greatest contemporary and enduring significance. When one thinks in terms of problems, not subfields, then, as Ian Shapiro has long argued, it soon becomes clear that many methods are needed to form useful concepts concerning the problems and to test those notions rigorously and appropriately. That awareness, in turn, can spark willingness to welcome, and spur desires to learn, a plurality of methods as valuable parts of political science.

In fairness to the current field structure, it represents not something originally imposed from the top but rather something that emerged fairly organically from the mutually reinforcing choices of many political science departments in the modern profession's first half-century of existence. Any changes should come through similar processes. But the prevailing field structure has endured for another half-century in a rapidly changing world. As we enter the modern discipline's third 50 years, it is time for widespread reflection on whether we can in fact collectively converge on a new structure that makes more substantive sense for the discipline. If we can, then we will need to create new types of graduate curricula and revised and reinvigorated journals—ones that are closer to what we preach, and also closer to what we really want.

Note

1. They also report that, after surges in the mid-70s and especially the mid-80s, formal modeling has leveled off or even declined slightly in its representation in the *APSR* and some other journals. I am aware that many formal modelers believe this is due to the fact that most political science journal reviewers are not sufficiently sophisticated to appreciate the cutting edge work of this sort, so that it appears instead in more specialized publications (or in economics journals). Though I hope the trend also reflects some more demanding standards about whether formal analyses really illuminate actual political problems, I have no idea which of these explanations, or some other, may be correct.

References

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