Whither Political Science?

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The current debate in political science over methods and fundamental theoretical stances recalls similar debates in other fields. Part of the debate focuses on the merits of the use of statistical methods or the use of mathematics and quasi-mathematical reasoning, as in game theory and much of rational choice. Among the critics of those who use these approaches are many who focus more on interpretive approaches to understanding social institutions and behavior. In some ways, the debate seems dated in that the largest and most compelling body of quasi-economic work is broad studies of the relationships between political and economic development. Such work, often with relatively sharply defined statistical models, spans more than two generations of scholars in political science (e.g., Przeworski 2000 and Boix 2001). Such work has given compelling answers to many questions about the workings and workability of democracy. It typically abstracts from culture and it fits congenially with rational choice theory in its focus on microfoundations for various claims.

There was once a debate in or over economics that is strikingly similar to the debate that many political scientists now join. People, including many economists, think that most of the work published in economics journals is either too abstract or too trivial to be of interest to the real world. That debate is at least five decades old, with an early salvo directed at Paul Samuelson's mathematical economics. Just to show that technical sophistication was good, Samuelson (1954) responded with a highly technical, three-page analysis of public goods, an analysis that has been enormously influential. Other people, including the political scientist John Mueller (1999), claim that economic understanding has reached such a state that it might now generally be helpful to governmental economic policymakers. He compares economics today to medicine at about the beginning of the twentieth century. The Flexner Report of the American Medical Association concluded that around the beginning of the twentieth century, going to an American doctor finally was more likely to benefit people who were ill than to harm them (Flexner 1910). So too, Mueller supposes, for governments to follow the advice of economists today is, at last, more likely to benefit than to harm national economic performance.

Economists now advise governments to leave the economy alone to a substantial degree and to let the market work. It is, of course, a corollary of laissez-faire economics that government partially escapes the burden of being judged for the success or failure of its economic planning. Economic performance was once the major dimension on which the major parties fought in many democratic societies: for example, the Republican and Democratic parties in American politics and the Conservative and Labour parties in the United Kingdom. However, economic performance is increasingly a dimension on which these parties concur.

Has the less apparently relevant work of modern economics contributed to that concurrence? A similar question would be worth tackling not only in economics but in many other disciplines. The huge number of people working on economics and economic problems suggests that most of them cannot be doing anything that rises to the level of public awareness, so few of them can have had much direct effect on public debates or policies. That might be too quick and dismissive a claim, however, because someone such as Larry Summers, whose economic advice was centrally important for several years during the Clinton administration, could not likely have achieved his own understanding without the large enterprise of academic, research institute, industrial, and governmental research on economic issues. It is not trivially easy and maybe not analytically possible to ferret out the connections between thousands of journal articles over the past 50 years and the counsel that a Summers has had to offer. Yet it would be absurdly presumptuous to dismiss the relevance of all of that work, as abstract or minutely focused as much of it was. Somehow, the enterprise of economic science has been fundamentally important.

Can we similarly claim that political science is important? Or has it made no advances of great merit? Each of this symposium's participants could probably name a favorite genuine achievement of political science. One of mine is the Downsian theory of democracy that put an end to the APSA-sponsored debates of fifty years ago under the hortatory label "Toward Responsible Two-Party Government." That document was evidently intended to unify political scientists behind a particular vision of the American party system, ideally to change that system. It is perhaps an embarrassment to political scientists that Anthony Downs was an economist working under advisors Julius Margolis and Kenneth Arrow, two other economists, on ideas first put forward by Joseph Schumpeter, yet another economist. Political scientists are still often hortatory, but we do not these days pretend to speak with one professional voice.

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Voting 101. Residents of El Paso, Texas cast their ballot for president of the United States. The author notes it is the lack of incentive to vote that makes the knowledge of how to vote well virtually useless. Printed with permission of Getty Images, photo by Joe Raedle / Newsmakers.

Strangely, one might respond to the recent success of economics with the observation that it is based on understandings that do not go far beyond Adam Smith's (1776) antimercantilist and anti-interventionist views. That would be an exaggeration, but an exaggeration with a large grain of truth. That it has taken us 200 years to become as advanced as Smith is essentially a political, not an economic, failing. Indeed, Smith's chief insights were essentially political, not economic. They were about the likely incapacity of government to control an economy to good effect. Most of the revolutionary and petty dictators of modern times have had the hubris to think that they had the right policy for running an economy. On the seeming presumption that they personally knew how to manage economic development, leaders such as Lenin, Stalin, Peron, Mao, Nehru, Castro, Qaddafi, and countless others have hampered and even ruined the lives of their people.

Alan Blinder[1], another Clinton advisor, recently proposed that we slow down on our policies of free trade until we convince more citizens of the benefits of such policies. He says that 99% of all economists since Adam Smith support free trade but many citizens are adamantly convinced that it harms their interests. His brief editorial remarks suggest what is the central problem of politics: popular incapacity for understanding hard and even not-so-hard issues. What Libyan would have the nerve to stand face-to-face with Qaddafi and say that he is economically illiterate and wrong? And what democratic politician dares to say to the public that they are economically illiterate and wrong? Ignorance pollutes policy debate on free trade and many other issues.

Insofar as it is about political processes and institutions, political science does not need to be reoriented just now. If we are to understand democratic politics, however, we need to deal with the dismal level of popular political knowledge. We can do that well only if we first explain why popular knowledge is so poor and ungrounded. Schumpeter had a compelling answer. He wrote, implicitly invoking a John Deweyan pragmatic-rule definition of knowledge, that "without the initiative that comes from immediate responsibility, ignorance will persist in the face of masses of information however complete and correct" ([1942] 1950, 262). I may have reason to acquire political and policy knowledge because it gives me pleasure, but not because it will be useful in my causing good public effects through my role as citizen.

Most of the research and debate on voting since Downs has focused primarily on the incentive to vote rather than the incentive to know enough to vote intelligently.[2] The latter is at least logically derivative from the former, because it is the lack of incentive to vote that makes the knowledge of how to vote well virtually useless, so that mastering that knowledge violates Schumpeter's pragmatic rule. Just because my vote has miniscule causal effect on democratically determined outcomes, there is no compelling reason for me to determine how to vote by assessing the causal effect of my vote on such outcomes. Or, to put this the other way around: although I would benefit from policy X, I do not have reason or incentive to know about or to understand the implications of policy X unless, by the pragmatic rule, I can somehow affect whether policy X is to be adopted.

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We could make this a moral issue, as many editorial writers do, and say that the people are in default. One of Milan Kundera's (1974, 97) characters says, "If a man were responsible only for what he is aware of, blockheads would be absolved in advance from any guilt whatever. Only, my dear Flaishman, man is obliged to know. A man is responsible for his ignorance. Ignorance is a fault." I think that is not the way to go; it is a variant of APSA's hortatory stance of 50 years ago. Knowledge has costs and it therefore trades off against other, sometimes more important, things. There are times and roles in which we should opt for knowledge rather than other things, and other times when we should not.

If the citizen has no interest in voting, then the citizen has no interest in making the effort to learn enough to vote well. Something that is not worth doing at all is surely not worth doing well. If the problem of knowing enough to judge government officials is already hard, the lack of incentive to correct that problem is devastating. Indeed, the costs of knowing enough about government to be able to vote intelligently in one's own interest surely swamp the modest costs for most people in the United States of actually casting a vote, at least on commonplace issues of public policy outside moments of great crisis. An economic theory of knowledge or what I call street-level epistemology therefore weighs against knowing enough to vote well because the incentives

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heavily cut against investing in the relevant knowledge. The typical voter will not be able to put the relevant knowledge to beneficial use and should therefore invest in other things. Robert Jervis (this symposium) gives reasons why the knowledge of political science per se has little currency in popular debate.

The problem of the inutility of political knowledge may actually have been exacerbated in our time because, increasingly, we let the economy run itself rather than try to run it from Washington. We do that because, as Mueller says, we now know enough to know we cannot do it better. Unfortunately for popular political knowledge, that means we do not have huge stakes in determining economic policy and we no longer have major parties that are divided on lines of economic policy. Indeed, it is not yet clear on what lines our parties will divide in the near future. Perhaps this means that we, as political scientists, should focus more sharply on politics and political issues than we do, taking up abortion, stem-cell research, diversity, and so forth. If so, that is only because these issues are where the noise is, not because they are more important to our lives than economic matters such as prosperity and opportunity for achieving it.

Finally, I think it would be a big mistake to try to say what individual political scientists should do in their own research. We can judge the quality of research within its field and we can take little or no interest in much of what is done, but we should not try to channel efforts. Talcott Parsons notoriously tried to get the Harvard sociology department to promulgate a statement of its general position on sociology. That effort foundered on the disbelief of such colleagues as Samuel Stouffer and George Homans that the members of the department shared any general position (Homans 1984, 301–3). The advance of knowledge in the social sciences is similar to technological and other kinds of innovation. Some of the results that come from individually driven projects often prove to be of little wider interest, but other results turn out to be things that would not have occurred to any central planner. Perhaps the work by Downs and other economists had such great impact on political science because such work came from outside and was not part of a large program that would have suited APSA and its leadership.

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Where should our current debate take us? One response is to try what Parsons tried. Any such move at this time must fail. There is genuine value in many different approaches to understanding politics and highly qualified people are committed to each of these. None of them will take instruction from the other camps any more than Homans or Stouffer were willing to accept instruction from Parsons. It is essentially absurd for anyone now to say, for meta-theoretical reasons, that we should go one way rather than another. Meta-theory has no ground on which to stand until we develop simple theory well enough to answer major questions definitively. Therefore, Marion Smiley pleads for continuing pluralism on how we do our work. Susanne Rudolphargues not merely for pluralism but for crossing disciplinary and social borders. Elinor Ostrom focuses on particular classes of problems and welcomes work from many perspectives. Rogers Smith concludes that we should focus more on politics and less on "precision," and he gives his reasons for this focus. His sly observation that his reasons work for him seems to grant the claims for pluralism: his reasons will not work for many other political scientists.

Fifty years from now, scholars might look back at our time as one of incompetence or underdevelopment, but they might look back and note that their time is quite similar. No matter how we would be viewed, new scholars in political science seem to bet that theirs will be a pluralist world. If I were advising them, I would give no advice relevant to our topic in this symposium. I would say only do good work and be serious about getting something right. I would count good methodology as much as good institutional or individual-level analysis. Indeed, I think it is evident that solid methodologies and theories from other contexts often enlighten work in our fields. Beyond that, we have little advice to give because we know no better than APSA knew 50 years ago where our own inquiries will lead us.

Notes

1. Alan Blinder, "Free Trade Needs a Chance to Sell Itself," New York Times, 29 July 2001, sec. A.
2. Knowledge problems are not ignored. For example, knowledge is often assumed to be correlated with class, status, and other individual attributes that might characterize objective interests.

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