

Should We Make Political Science More of a Science or More about Politics?*

To paraphrase slightly, I have been asked to focus on the apparent tensions between making genuinely scientific contributions by advancing knowledge in rigorous ways within specialized subfields, on the one hand, and addressing substantive political issues of general interest in accessible fashion, on the other. My view is that, though we should try to do both things, we should give priority to the latter—to helping both disciplinary and general public understandings of important substantive political issues become better informed and reasoned. To adopt this priority is still, I believe, to pursue the main tasks of political science as scientifically as possible; but it is true that this course involves significant tradeoffs. I would have us devote fewer resources than we now do to work that achieves greater methodological rigor in addressing minor questions as putative stepping stones toward eventually achieving more scientific work on larger questions. I would

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have us devote more resources to research that may not be methodologically innovative or unusually precise, but that provides evidence and arguments sufficient to judge some positions on important

issues to be more credible than others. I certainly do not advocate abandoning methodologically oriented work; but the promise of achieving substantially greater rigor on most significant political questions is ineradicably limited. Hence it seems appropriate to give a secondary role in the discipline's endeavors to research that is most concerned with advancing the scientific character of political science, rather than advancing and defending substantively important propositions about politics.

This position rests on a certain set of beliefs and my reasoning from them; but space prohibits any elaborate defense of those views here. Instead I simply lay out my beliefs and reasons in ways that may elucidate key points of disagreement with other views of political science. Clarifying differences is often the best way to move discussion forward.

All scientific inquiry is undertaken to serve human interests. Some people believe that the highest calling of all academics, including political scientists, is to pursue science “for its own sake,” or knowledge “for its own sake.”

Mundane human motivations, and certainly political motivations, are deemed unscientific. To the contrary, I do not believe that “science” and “knowledge” have “sakes.” Scientific inquiry is something *people* choose to pursue. They do so for a variety of reasons, but all those reasons are forms of human interest. Some undertake scientific studies because they find satisfaction simply in obtaining and contemplating well-founded knowledge about existence. That is one of my own motivations. Still, this remains knowledge pursued to satisfy a human aspiration or interest. It is not science or knowledge for its own sake.

That is why I think it is a mistake to say that political science should not take its agenda from human interests. It must do so. The only real question is, which interests?

Scientific inquiry into human affairs, especially political affairs, is distinctive because propagation of the results necessarily affects both the studiers and the studied. One may study the motion of the planets, nuclear chemistry, and the behavior of ants and publish the results without significantly affecting the planets, most chemical processes, or most ants. But propagation of the understandings of human beings that result from scientific inquiries inevitably affects the self-understandings that prevail among human beings, at least to some extent. That is a consequence of our activities that we can ignore but not eliminate.

Therefore, it is sensible and responsible for political scientists to treat the consequences of our inquiries for human affairs as central parts of our enterprise, rather than to ignore them or deem them of minor importance. It is also sensible for other people to be concerned about how our work affects or might affect them and their interests.

Quite reasonably, most people who think about it at all believe that political science ought to improve human knowledge on political questions generally thought to be important. When people commit resources to the study of the formation of galaxies and the mating habits of rare species of frogs, they accept that the central payoff is likely to be enhanced knowledge about the natural world, and that any other benefits to them are likely to be quite indirect. They will probably not be surprised or concerned if the results of the research are described in specialized terms not immediately accessible to them (though they may hope for more in regard to the mating frogs). But because they rightly see the study of politics as about them and as potentially at

least significant for their lives, they hope that resources committed to such study will produce results that will speak to their concerns and interests, in ways that they can grasp.

Hence political science is more likely to be seen as important and worth sustaining, and to be sustained, if it illuminates political topics that most people care about in comprehensible ways. I also think that those who provide us support have the right to expect such work from us, although as noted below, they cannot legitimately insist on results that simply advance what they perceive to be their interests.

The subject matter of politics is so causally complex and so normatively infused that honest researchers can only make ordinal probabilistic statements on most important political topics, of the following form: "Our best judgment is that X is less likely to be true than Y." My erstwhile

colleagues Donald Green and Alan Gerber argue persuasively that the most rigorous form of intellectual inquiry is randomized field experiments. Throughout the realms of science, this is the methodology that has the best prospect of identifying causal relationships actually at work in the world, not merely in an artificial laboratory environment, in data sets of statistics subjected to purely mathematical "controls," or in hypothetical thought experiments. But both ethics and logistics prevent conducting field experiments on anything more than a small, relatively minor fraction of the political questions that most interest people. We cannot, alas, randomly assign persons to democratic versus authoritarian regimes. And when we try to knit our rigorous but smaller studies together into accounts of larger, more significant political phenomena, as we should and must do, we inevitably compare some apples and oranges, lose precision, and increase the role of speculative, probabilistic judgments in our work.

Additionally, human beings are affected by the results of social inquiry in ways that nondiscursive entities are not, so that political generalizations that are true when first derived may be made obsolete by their very propagation (e.g., if we publicize in schools that X% of eighth graders cannot identify the president, that number will probably decline). Then consider that every stage of human scientific inquiry, from our definitions of the objects of study (individuals? families? economic classes?), to decisions about whether questions are "trivial" or "important" (are local elections worth studying?), to our interpretations of data (is racism of declining significance?) are all inextricably normatively infused. Acknowledge, too, that we have no sure, agreed-upon normative template to guide us in making these inescapable judgments. It then becomes clear that while we can make parts of the evidence and arguments pertinent to important political questions more empirically and logically rigorous, we cannot hope to make our overall analyses more than roughly, probabilistically true, empirically, and more than coherent and credible, normatively. Field experiments may enable us to say with confidence that in parts of the U.S. today, personal canvassing raises voter turnout by somewhere between 10 and 13 percentage points while mailings do less and telephone calls have no impact (Gerber and Green 2000). Yet knowing this along with other,

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often less rigorously obtained empirical findings about contemporary politics still leaves us able only to speculate, though now with better information, about whether the modern decline in American voter turnout predominantly expresses alienation, satisfaction, or comparatively ineffective mobilization techniques. We can only say that more personal mobilization techniques would probably help. This statement also implies judgments that increased voter turnout matters and is a good thing—normative propositions for which we can make reasonable cases but which we cannot prove.

Political science, then, is inherently limited in how far it can reach very precise results on an enormous range of significant political questions. On most major issues we cannot expect even cardinal orderings of the likelihood of alternative propositions or causal explanations, though we can achieve such orderings on many smaller topics and it is desirable to do so. The notion that we can ever accumulate those more precise findings into research that achieves similar rigor on larger political matters is, however, less likely to be correct than the notion that we cannot (or: my judgment is that this particular "X" is less likely than that "Y").

The core value of all genuinely scientific inquiry is intellectual honesty. More fundamental than any particular doctrine of scientific method is the standard by which methods should be judged. Whether "truth" really exists or is obtainable in scientific inquiry is legitimately controversial. Instead of the attainment of truth, then, I believe our methodological standard should be intellectual honesty. We should strive to achieve thoroughly honest presentations of what we think we have learned through inquiry and analysis. We should be honest about what we have actually done, honest about everything we believe we have found, and honest about why and how far we think our methods and interpretations appropriate and our findings substantiated, in light of any plausible objections that might be made. Though little in human existence is free of ambiguity, most of the time it is not hard to see what such honesty involves. It certainly forbids presenting our results in ways that tell those who support us what they want to hear when we think that what they want to hear is probably wrong. I believe that we make our most significant contributions and fulfill our highest disciplinary calling when we offer well-founded challenges to misguided conventional wisdom or inaccurate, self-serving beliefs prevalent among our societal sponsors. Still, intellectual honesty compels us to make clear whether and how our inquiries relate to their concerns at all.

It is in fact intrinsic to the logic of all scientific endeavors, including political science, to be honest about the levels of rigor we can hope to achieve on different sorts of questions. It is equally intrinsic to be honest about how the questions we examine relate to topics that other people regard as important. It is not very honest or scientific to imply that we are studying things that they regard as important, or should regard as important, when we are not. It is also not honest or scientific to imply that we are likely to achieve empirical findings on many significant political questions of enough precision to be expressed in credible cardinal orderings. It is more intellectually honest *and therefore more scientific* to bring to bear on those questions any and all evidence and arguments that can credibly support at least ordinal claims that "X is less likely than Y." Research should therefore not be evaluated chiefly by whether its methods most closely approximate the best method in some

other scientific endeavor. We should ask instead whether its methods have been honestly pursued and presented and whether they really enable us to make better-founded statements about politically important matters than we could otherwise do.

Appealing to these beliefs and reasoning, I reach the broader conclusions with which I began. Because political science, like all science, is pursued out of human interests; because its results are inherently linked to people's self-understandings and interests in ways about which they may be sensibly concerned; and because people reasonably expect political science to address and affect questions that they see as important for their interests, I conclude that political science should do so. It is both prudent and responsible to make political science centrally concerned with political questions that command wide interest, to do this work honestly, and to present our results in ways

that many can understand. But on most such questions, political science cannot hope to achieve highly precise findings; its most scientifically defensible findings will be only rough probabilities. Put more exactly, then, I conclude that the main endeavor of political science should be to make roughly probable empirical and logical cases for and against claims about political questions that many people can be persuaded to regard as substantively important. Because the achievement of more precise knowledge only comes on narrower topics, generally studies structured to achieve a very high degree of precision in their findings can at best feed into the sorts of work that should be central to our discipline. They should not be the center of our discipline.

Quad erat demonstrandum? Admittedly, not quite. But the above reasoning works for me.

Note

A modified version of these arguments appeared as Rogers M. Smith, "Putting the Substance Back in Political Science," *Chronicle of Higher Education Sec. 2: The Chronicle Review* 48: B10–B11 (April 5, 2002).

Reference

Gerber, A.S., and D.P. Green. 2000. "The Effects of Canvassing, Telephone Calls, and Direct Mail on Voter Turnout: A Field Experiment." *American Political Science Review* 94:653–64.