

they are situated within environments of asymmetric power and function as hegemonic initiatives. Convergence can be an invitation to fruitful cross fertilization or to paradigmatic invasion, occupation, and conversion. Therefore it's not clear that everyone would cheer at the news of convergence. There is the danger that attempts to systematize interpretive methods would shear them of their thick description, their empathy, their normativity, in the interests of parsimony and impersonal objectivity.

Finally, there is the empirical question: Is it happening? What evidence is there of convergence? Let me return to the question of language, and to the fact that when I learn something conceptual at a conference, it often boils down to language acquisition. Is there any evidence of consequential language exchanges across paradigm boundaries? Terms typical of an approach are the patient mules that bear the burden of whole methodological procedures or perspectives. There has been significant migration of vocabularies across epistemes which we can read as measures of exchange. Game theory has exported the terms "prisoner's dilemma," "free rider," "payoffs," "public goods," "models," for regular use in other epistemic communities. Ethnographic and interpretive methods on their part have exported "narrative," "discourse," "rhetoric," "stories," "hermeneutics." Sometimes a vocabulary item becomes a vehicle of disparagement, as when formalists throw the term "ordinary language" at interpretivists, or exemplars become "anecdotes." Sometimes ownership of a term becomes a *casus belli*, as in the tug of war over who owns the prestige-bearing noun, "theory."

Now where does this leave me? The status of language exchanges helps us see where we are but doesn't tell us where to go. By this measure there is clearly exchange among paradigms. It signals that there is some exchange of valued currency, but no common monetary system, some mutual epistemic legibility, but no community of modes of inquiry. I end on the unsatisfactory note that there is enough communication to provide choices for intentional graduate students to imagine alternatives, but not enough to signal active collaboration. Without fruitful collaboration and convergence, we need at least legibility and civility.

John Mearsheimer: A self-enclosed world?

Let me begin with a word about self-identification, which highlights how complicated the world of political science really is. Susanne divided that world in half, or into two parts. But I find myself on both sides of the divide. Basically I view myself as a rational choice theorist who does not use math. I start with simple assumptions about the nature of the international system and from them I make deductions about state behavior. As

both Lloyd and Susanne Rudolph know, much to their chagrin I might add, I love simple models that explain how the world works. I like to say to students that I know the world is complicated, but please give me a simple theory that tells me how it works. And even if the theory is wrong, if it is simple and elegant, I am likely to be impressed. I don't know why; God just hardwired me that way.

However, because I have used case studies to test and help refine my theories, and because I know a lot of history, I usually am described as a qualitative person or a case study person who is located on the opposite side of the divide from the rational choice people. But again, I am basically a rational choice person who happens to use cases and who thinks that knowing a lot of history is important for developing smart theories. I say all of this to highlight how complicated the world of political science can be.

One other point of self-identification is relevant here: I am something of a methods maven. I have taught a graduate-level methods course at Chicago for about ten years. It is really a philosophy of science/methods course. Moreover, I frequently tell graduate students to take all the methods courses they can, because I think methods are very important, and students should be spun up on a wide variety of them. One can go overboard, but sound methodological training is indispensable for doing first-rate social science.

Having said enough about myself, I now want to assess whether we have dealt with the central issue that Ian Shapiro and Rogers M. Smith asked us to focus on in this volume. In their initial letter to me – and I assume that all the other participants received essentially the same letter – they said that they wanted us to talk about the relationship between methods and problems. By problems it was clear that they were talking about providing answers to important real-world questions, or offering theoretical insights about how the world works. That's what problem-driven research is all about. We all agree, I think, that almost every scholar does both. All of us are concerned about both methods and solving problems. But the really important issue – which Ian and Rogers identified in their letter – has to do with the balance between the two. It was implicit in their letter that they believe that our discipline privileges methods over problems, and that this bias is unhealthy for our discipline. The question is: How well have the contributors to this volume dealt with that important issue?

I have two responses to that question. First, we have barely addressed the issue that Ian and Rogers laid out in their letter. Second, many of the essays have borne out their point that our discipline privileges methods over problem-solving.

Except for Rogers Smith, I do not think that a single person explicitly addressed the question of the relationship between methods and problem-solving in the way that was set out in the invitation letter. What we have done instead, and I think this is reflected in Susanne's comments, is focus on methodological – or epistemological if you like – differences among us. We have had the so-called formalists on one side of the debate and the qualitative scholars or critical theorists on the other side of the divide. Inside the formalist camp there has been a debate between rational choice advocates and large-N scholars like Donald Green that has been every bit as adversarial as the debates between formalists and qualitativists.

In all of these debates among the panelists, however, the focus has been almost exclusively on methodological issues. There has been little sense in the discussions that there is a fascinating world out there, that fundamental changes are taking place as we enter the twenty-first century, and that we should be committed to trying to understand that emerging world. It seems to me that there are all sorts of important problems in the world that we should be excited about studying, and that we ought to think about methods in terms of how useful they are for helping us address those concrete problems. Scholars should ask: Does my theory or my methodological approach provide important insights about important issues? But I did not hear participants talking in those terms. Instead, we seemed to be obsessed with methods.

Let me come at this matter from a different angle to drive my point home. Rudra Sil asked: "Who is our audience?" This is a great question. When we write a book or an article, who are we appealing to? Who is going to read it? Who is going to care about it? How are we going to spread our ideas to others? Who are those others? I do not think anybody answered Professor Sil's important question, and I think the reason nobody answered his question is that our discipline operates on the assumption that we only talk to each other. We operate in a self-enclosed world. The fact is that political scientists in recent years have tended to marginalize themselves from the wider world. We do not engage those outside our discipline in important ways. To use the word "policy" – which is actually a synonym for politics – to describe the work of a prospective hire, is to doom that person. You never want to say that a job candidate does policy-oriented work, even if it is first-rate, because it deals mainly with real-world issues. Instead, we prefer to hire individuals on the basis of their methodological proclivities and skills. In short, discussions about hiring always have a heavy focus on methodology; usually little attention is paid to what scholars have to say about the real world.

Let me take this point a step further by considering the subject of writing op-eds for newspapers, or articles for popular journals like

the *American Prospect* and *Foreign Affairs*, or appearing on the *Lehrer NewsHour*. If you do such things, they are likely to be held against you in a hiring meeting or a promotion meeting. It is just not the kind of work we academics are supposed to do. That kind of thinking, I might add, applies to distinguished senior scholars as well as young scholars.

I remarked to Alan Ryan that over the course of my twenty years at Chicago, the group of political science colleagues who I have found to be the most interesting to talk with about politics is the political theorists – and here I am talking about the political philosophers. The reason is that they have tended to be much more interested in the real world than most of my other colleagues. For example, Stephen Holmes, Bernard Manin, and Nathan Tarcov, all political theorists in my department at one time, were remarkably knowledgeable about nitty-gritty political issues. If one wanted to talk about NATO expansion or the deployment of SS-20s to Europe, talking with them made for great conversation. They were all deeply interested in the real world.

In my conversation with Alan, I asked him why he writes for the *New York Review of Books*. He said that he is trying to communicate to a wider audience than just his academic colleagues. I know few other academics who write for the *New York Review of Books*, or would consider doing so. Why? Because we live in our own little world where we talk mainly to each other. Of course, not every political scientist fits that description, so I am obviously overstating the case somewhat. But I do believe that there is a lot of truth in my claim, and that it has been reflected in what we read in this volume, where, again, the emphasis has been almost exclusively on methods. And this happened despite the marching orders that Rogers and Ian gave us to write about the relationship between methods and problems.

I would like to say a few words about my own views on the relationship between methods and problem-solving. First, I think we should privilege problem-solving over methods. I do not believe that methods are unimportant, which is why I said earlier that I teach a methods course and I encourage students to take methods courses. Nevertheless, methods are merely tools for answering important questions. Second, I think we should reach out to a wider audience than our fellow political scientists. Of course, we should speak to each other, but there is a wider world out there which should care about what we have to say and we should care about communicating with it. We should do this because we have a social responsibility to our fellow citizens to help them understand how the world works.

I do not think I have ever met a political scientist who has wrestled with the question of what he or she is doing in this business. In other words,

assigned Wohlstetter and a handful of other analysts to study the issue and come up with recommendations. In the process of doing this basing study, Wohlstetter and his colleagues invented a framework or theory for thinking about the problem. Specifically, they came up with the distinction between first-strike and second-strike, as well as concepts like crisis stability, vulnerability, and survivability. All of these notions, as well as the theory of deterrence put forth in the article, were path-breaking and truly important for helping us think about nuclear deterrence. But they were not developed by isolated academics locked away in an ivy tower looking to invent a theory of deterrence. Instead, they were developed by first-rate minds engaged with a truly important real-world problem.

Another important scholar whose work fits the same mold is Thomas Schelling, who is something of a god for most rational choice scholars, including me. I think one of the great crimes of academia is that Schelling has not gotten a Nobel Prize for his work on deterrence theory. I say that as someone who disagrees with some of Schelling's key ideas. The point I want to make here, however, is that his seminal writings on deterrence during the 1950s and 1960s came out of policy-oriented research that he did at Rand, where he was examining many of the same issues that concerned Wohlstetter, who, of course, was his Rand colleague. All of this goes to show that the claim that focusing on real-world problems will lead to journalism and not serious scholarship is mistaken.

I want to conclude with a final point about math. I have no problem with scholars who use math in their work. I think it sometimes facilitates the production of elegant theories, and as I said before, I like elegant theories. But I agree with Truman Bewley that the use of math is not a necessary condition for coming up with good theories. John Roemer and Bruce Bueno de Mesquita have said, in effect, that if you do not use math you are not doing real social science and that the resulting theories will not be worth much. I would note that both Schelling and Wohlstetter used little math in their work, yet they both produced seminal theoretical works.

We actually tried to hire Thomas Schelling in the political science department at Chicago many years ago, but the appointment was resisted by the economic department, where there was a strong feeling that his work was not rigorous enough, which means that he was not an applied mathematician. But who cares whether he uses math? The key point is that he invented a body of important theoretical ideas that have profoundly influenced how huge numbers of scholars and practitioners think about deterrence, and how we think about international relations more broadly. For a certain body of economists, however, and I think for Bueno de Mesquita and Roemer as well, if you are not using sophisticated math

to develop and articulate your theories, there is something wrong with your work. Again, I am not saying that there is anything wrong with using math, but I do think it is wrong to argue that math is necessary for developing good theory.

My bottom line is that I thank Ian and Rogers, who to their great credit, gave us a very important issue to deal with in this volume. The discipline of political science needs to think about the audiences it seeks to reach and the importance of solving problems versus focusing on methods. We failed, however, to respond to their directive and address these weighty issues.