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Jeffrey Checkel, Jeffrey Friedman, Matthias Matthijs, and
Rogers Smith

ROUNDTABLE ON IDEATIONAL TURNS
IN THE FOUR SUBDISCIPLINES
OF POLITICAL SCIENCE

ABSTRACT: On September 4, 2015, the Political Epistemology/Ideas, Knowledge, and Politics section of the American Political Science Association sponsored a roundtable on ideational turns in the four subdisciplines of political science as part of its annual meetings. Chairing the roundtable was Jeffrey Friedman, Department of Government, University of Texas, Austin. The other participants were Jeffrey Checkel, Department of Political Science, Simon Fraser University; Matthias Matthijs, School of Advanced International Studies, Johns Hopkins University; and Rogers Smith, Department of Political Science, University of Pennsylvania. We thank the participants for permission to republish their remarks, which they were offered the opportunity to edit after the fact.

Keywords: citizenship; civic education; democracy; participation; solidarity; service learning.

JEFFREY FRIEDMAN: My name is Jeffrey Friedman. I'm at the University of Texas at Austin and I'm the editor of *Critical Review*.

This is the second event held by the Political Epistemology/Ideas, Knowledge, and Politics¹ section of APSA. As to what that means, I hope that by the end of this session a certain clarity will come about.

I'm very pleased to introduce people who will talk about each of the four subdisciplines. The three scholars to my left are distinguished students of international relations, comparative politics, and American politics; and I'll cover normative political theory. Jeffrey Checkel of Simon Fraser University will talk about the ideational turn in international

relations. He's the author of several books about ideas and international political change and about Soviet-Russian behavior at the end of the Cold War, and is the co-editor of many books, including *Process Tracing: From Metaphor to Analytic Tool* [Bennett and Checkel 2014]. Matthias Matthijs, of Johns Hopkins University's School of Advanced International Studies, is the author of *Ideas and Economic Crises in Britain from Attlee to Blair* [Matthijs 2011] and co-editor of *The Future of the Euro* [Matthijs and Blyth 2015]. He will be discussing the ideational turn in comparative politics. Rogers Smith of the University of Pennsylvania is the author of many books, including *Civic Ideals: Conflicting Ideas of Citizenship in U.S. History* [Smith 1999]; *Stories of Peoplehood: The Politics and Morals of Political Membership* [Smith 2003]; and *Political Peoplehood: The Roles of Values, Interests, and Identities* [Smith 2015], which is just out from the University of Chicago Press, and is about the study of ideas in American Political Development and American politics. One of my favorite articles of all time in political science, although it was published in *Daedalus* in 1997, is Rogers's "Still Blowing in the Wind: The American Quest for a Democratic, Scientific Political Science" [Smith 1997]. For those who haven't read it, it's a tour de force on the development of political science with a special eye to the absence from political science, in many cases, of ideas as causes of political actors' actions.

What we decided to do in advance is to each speak for one round of five minutes each. Because one of the goals of the Ideas, Knowledge, and Politics section is to encourage people in different subdisciplines who are interested in ideational research to be aware of and talk to each other, we'll then have a second round of five minutes each in which we can react to what the previous speakers said. But if any of the panelists find themselves running terribly over and want to compress their two five-minute talks into a ten-minute talk or one seven and one three, we can be flexible. So I guess we'll go in alphabetical order, beginning with you, Jeff.

JEFFREY T. CHECKEL: My task is to highlight the ideational turn in international relations, so I'll do two things: briefly outline the contours of the ideational turn in international relations over the past 25 years going back to around 1990; and then highlight what I think of as some real cutting-edge issues and challenges for work on the ideational focus in international relations.

So the ideational turn in international relations since the 1990s, that's plural *turns*, because there're several different literatures developing, which don't necessarily talk to each other, or arguably should be subsumed under the rubric of ideational turn. Late 1980s, early 1990s, there was some concern among international relations scholars thinking about cooperation in different ways. Peter Haas, most importantly, with other people, had developed the notion of epistemic communities: groups of individuals who share some collective knowledge and use that knowledge in some cases to promote cooperative outcomes internationally. So a case study of environmental cooperation or lack thereof in the Mediterranean was followed by a special issue of the journal, *International Organization*, on epistemic communities in international relations.

Several years later, quite differently from the epistemic community literature and without much crosstalk, you begin to see the emergence of ideas talk in international relations. And not just international relations, but a whole book published in 1993, *Ideas and Foreign Policy*, coedited by Judith Goldstein and Robert Keohane. And for five to seven years, there was a lot of ideational work; Jeff mentioned my own work on the cold war; there were several books that offered ideational explanations for the end of the cold war. But this ideational turn, and I see you can see that in some of the other panelists, spread well beyond international relations, international relations theory; arguably, I think the bulk of the work was in comparative politics, International Political Economy, even some in American politics: early work by Kathryn Sikkink on development in Argentina and Brazil, her first book; Margaret Weir, looking at jobs in America and making an ideational argument; Peter Paul and others, the power of economic ideas. But the work on ideas and foreign policy in the late 1980s, late 1990s dies off, and what's come along, the new kid on the block, is something called social constructivism, which takes on board the ideational turn but broadens it, or attempts to broaden it in two ways. Conceptually, for a lot of the people talking about international relations in foreign policy, ideas are something up here, between the earlobes; ideas explain, they indicate a preference for something. The constructivist scholars argue, let's think about that more broadly, they tend to talk about norms instead of ideas, shared understanding about different claims, something beyond the earlobes. Related to this sort of meta-theoretically, ontologically, they claim we're not just going to do agents, nor are we going to default over to the structure side, we're going to do both, constitution type stuff.

Instead they end up not doing that kind of stuff, they end up defaulting one way or another. They talk about norms, discourses, practices, narratives, social stuff that plays an important part in shaping much of international politics. So this trans-literature, within international relations, hasn't given us so much new areas to study but instead has given us additional lens for theorizing about, thinking about, old friends in international relations in new ways: so international organizations, international politics are sometimes important actors. Before the ideational turn, we viewed them, in some cases, as cases of providing information, lowering transaction costs, and the like. As a result of the constructivist ideational turn, a lot of work treats international organizations as promoters of social ideas, helping to construct social reality in various sorts of work.

Ditto for the work on norms. Before the ideational turn, there was a lot of work that talked about norms, largely from international law, about equal norms embodied in law. Constructivists came along, argued that these norms sometimes operate outside and behind particular laws and not simply just constraining behavior of actors that may constitute them, which may provide a new understanding of their interest.

This ideational literature is largely developed in international relations as a very American political-science type of literature. You see this in a number of ways, such as a commitment to a sort of quasi-positivist framework. What that does, for constructivists, is it cuts the ties between a rich body of constructivist literature in other parts of the world, in Europe, around interpretivism, and the kind of constructivism that comes to dominate in American international relations. So very quickly, that's the literature that's developed.

What do I see as some of the big challenges? I've got three theories or meta-theories. What's happening in this international relations ideational literature, grand theory is gone, big paradigms are gone, we don't have paradigm wars anymore, hooray, hooray. Well what's replaced it? Little bits of theory everywhere: I've got this argument, I've got that argument, partial theory, middle-range theory—that's a big buzzword in some of this literature. These are very complex, multi-causal, multiple mechanism-based theories that often generalize very little. So is that a problem or an issue, is there a lack of balance now between grand theory and this ideational work?

In terms of method, much of this ideational work in international relations is qualitative in nature, which makes a lot of sense. But it has

not been at the forefront of the current revitalization in methods over the past decade—all this work on case study, discourse, practice, process-basing, whatever, but thinking systematically how to do qualitative methods well; you haven't seen a lot of this filtering into ideational work in international relations.

Finally, meta-theoretically, ideational work in international relations tends—maybe this is just the way it has to be—to gravitate to one epistemological pole or another, quasi-positivist, scientific realist, or interpretivist, and I want to argue that some of the most interesting work straddles that epistemological divide, interesting work that gives us important substantive knowledge about the world. An example of this would be the work of Ted Hopf, an international-relations scholar who actually straddles this positivist-interpretivist divide, who's done interesting work trying to explain international behavior in the former Soviet Russia and post-Soviet Russia, but that work is very much an exception. The norm is that everyone still stays in their comfort zone, in their epistemological home, and I think that's a lost opportunity.

MATTHIAS MATTHIJS: Thank you so much, Jeff, for organizing this roundtable on ideas for APSA's new section on political epistemology. I must immediately say that I am a very poor replacement for Vivien Schmidt today, and thus clarify to the audience that we never intended for this panel to be yet another all-male one. Unfortunately, Vivien had to cancel at the last minute, leaving me with some rather large shoes to fill. I could not possibly do justice to Vivien's likely contribution, but I shall give it my best shot.

My job is to talk about the ideational turn in comparative politics and I am delighted to do so. I will probably disagree with what Jeff Checkel just said, since I think if you look at comparative politics in the last two decades, there has been *much less* of an outspoken "ideational turn" than the one we have seen in international relations. In the subfield of comparative politics, I do not think there has ever been a full-fledged ideational turn, even now. In many ways, of all four sub-disciplines, I believe the study of ideas has received the least attention in comparative politics, and I will try to explain why this is so.

I will focus on political economy, which is the part of the subfield I know best. Think about it for a minute. If you were going to design a graduate course in international relations today, it would probably be structured around the field's broad theoretical paradigms. You would

likely start with realism, then go through liberal institutionalism, followed by constructivist approaches, and so on. But I think there are very few professors who would organize their graduate seminars in comparative politics around theoretical paradigms. They would probably take a thematic approach, focusing on the state, electoral systems, political parties, political culture, etc. Mark Lichbach and Alan Zuckerman [Lichbach and Zuckerman 2009] have tried to do this by dividing up the field based on structure, rationality, and culture. I will return to why that is already problematic, i.e., talking about ideas as *culture*.

There are three points I would like to emphasize in my remarks. First of all, the opening of ideational scholarship in comparative politics in the early to the mid-1990s coincided with a call by top scholars in the field to embrace what they at the time called “analytic eclecticism,” rather than approaching questions by using one particular theoretical lens. The second point is that much ideational scholarship in comparative politics took off at the exact same time as there was an even more dramatic ascent of institutionalist approaches in the 1990s. And here I refer not just to “historical” institutionalism, but also what is often called the “rationalist” institutionalist approach, like the “varieties of capitalism” literature (VoC), for example. VoC was a major research program that was born in the late 1990s and early 2000s that did not really engage all that much with ideational approaches [Kitschelt et al. 1999; Hall and Soskice 2001]. VoC related more directly to rational-choice approaches, like Douglass North’s institutional economics and so on [e.g., North 1990]. This left very little room for ideas. The third and final point is that there remains skepticism even today—and this really is a challenge for all of us who want to take the study of ideas seriously—among probably most comparative-politics scholars about the role, relevance, and usefulness of ideas as an independent variable. Ideas are often confused with culture, and the study of culture more often than not degenerates into highly deterministic approaches. Moreover, comparativists remain much less interested in major theoretical issues compared to international-relations theorists and therefore also produce fewer explicit theoretical statements about ideas.

Let me elaborate a bit more on all three points. First, the real opening in comparative politics to ideational scholarship started in the mid-1980s. The main book was Peter Katzenstein’s seminal work *Small States in World Markets* [Katzenstein 1985]. Katzenstein did not explicitly develop an ideational framework, but clearly argued that what was unique about

the success of small European countries—including my own country, Belgium—in dealing with globalization was a culture or “idea” of consensus. Different societal groups and interests needed to get along with each other if their small, price-taking countries were going to compete successfully in global markets. Several years later Peter Hall published his celebrated edited volume on the role of ideas in economic policymaking, *The Political Power of Economic Ideas* [1989], by tracing the power of Keynesian ideas over time. But I think the ideational literature as we understand it today really took off in comparative politics with Kathy Sikink’s work on ideas and institutions in Argentina and Brazil in 1991, which Jeff [Checkel] mentioned earlier [Sikink 1991]. By the late 1990s, you had a new generation of scholars directly focusing on ideas as their main explanatory variable. Sheri Berman [1998] made the first major effort in comparative politics to operationalize *how* to study ideas, in a revealing comparison of social-democratic parties in early twentieth-century Sweden and Germany. And of course Mark Blyth’s magisterial work *Great Transformations* [2002] compared big shifts in economic policy in the United States and Sweden. But interestingly enough, there were always people who worked both in international relations and in comparative politics who studied ideas; think of Kate McNamara’s influential book *The Currency of Ideas* [1998], on monetary politics in the European Union, but also her most recently published book, *The Politics of Everyday Europe* [2015], which looks at the European Union from the explicit perspective of comparative political development. And of course there was Craig Parsons’s book on the role of ideas and the construction of European integration, *A Certain Idea of Europe* [2003]. McNamara and Parsons always had one foot in international relations as well as one foot in comparative politics, as did Blyth, but they all focused on why ideas mattered in explaining broad patterns of institutional change. Also important was the opening made in Peter Hall’s “Policy Paradigms, Social Learning, and the State” paper for *Comparative Politics* [Hall 1993]. All these scholars explicitly engaged with other approaches, and the ideational work was mainly in response to both rational and historical institutionalism or directly in response, when I think of Craig Parsons’ work, to more conventional rational-choice and interest-based approaches like Andrew Moravcsik’s *The Choice for Europe* [1998].

Later scholarship—here I think of Colin Hay [e.g. Hay 1999], Daniel Wincott [e.g., Hay and Wincott 1998], and especially Vivien Schmidt

[2001, 2008, and 2010]—put ideas much more explicitly at the center of their analysis. This birth of ideational scholarship in comparative politics coincided with a big symposium that *World Politics* organized in 1995, which dealt with the role of theory in comparative politics. In this symposium people like Atul Kohli, Peter Evans, Adam Przeworski, Peter Katzenstein, James Scott, Susan Wolf, and Theda Skocpol all called for more “eclecticism” in the field [Kohli et al. 1995]. Katzenstein argued that, because he is a problem-driven scholar, he is interested in the dependent variable, not in any particular approach to explaining it, i.e., not *per se* in the independent variable. Out of this influential symposium came a renewed emphasis on the significance of causal generalizations, which potentially did point to an engagement with theoretical approaches. But in the end, the embrace of eclecticism proved to be more important than the embrace of big causal generalizations. And it is the embrace of eclecticism that led to less explicit attention to ideas and to the specific issues they raise, because the eclecticists largely instruct us to stop worrying about theoretical divides and use a much broader mix of theories to explain broader empirical phenomena. So at the same time that the ideational turn was gathering steam in comparative politics, a much more prominent group of comparativists was starting to make the case for eclecticism. For better or worse, the latter ended up having more influence. There is actually a great critique of eclecticism that I want to point our audience to, since the author is present here today—Craig Parsons, forthcoming in *International Theory* later this year [Parsons 2015].

Let me now turn to my second point, which is that the ideational turn coincided with two big theoretical breakthroughs in comparative politics and political economy. On the one hand, you saw the “going viral” of historical institutionalism, especially through the work of Paul Pierson [1994, 2000, and 2004], which towers over everything and builds on Hall’s earlier book *Governing the Economy* [1986], and the work of Kathleen Thelen and Sven Steinmo [Steimo et al. 1992; Steinmo 1996; Thelen 1999], James Mahoney [2000, and Thelen and Mahoney 2008], Wolfgang Streeck [Streeck and Thelen 2005], and many others. On the other hand, you had the mushrooming of the research program centered around the idea of different kinds of capitalism, starting with Kitschelt, Lange, Marks, and Stephens’s *Continuity and Change in Contemporary Capitalism* [1999] and Hall and Soskice’s edited volume on *Varieties of Capitalism* [2001], later refined and expanded upon by Bob Hancké, Martin Rhodes and Mark Thatcher [2007]. Much of the historical

institutionalist literature actively engages with rationalist and structuralist approaches, but not all that much with ideas. While it would be obvious to say that ideas and critical junctures play a central role in historical institutionalism, this fact has never been fully exploited by the ideas literature. There is a wonderful chapter written by Mark Blyth and two of his graduate students, Oddný Helgadóttir and William Kring [2016], coming out in the *Oxford Handbook of Historical Institutionalism*, edited by Orfeo Fioretos, Tulia Falleti, and my colleague Adam Sheingate [2016] from the Johns Hopkins political science department. Blyth et al. conducted a rather revealing network analysis where they definitively show that ideational scholars engage very much with historical institutionalism, while historical institutionalists only really engage with rational institutionalism, while rational institutionalists only really engage with each other. So there is a kind of unidirectional arrow where ideational theorists want to dance with historical institutionalists but the favor is not being returned. Those historical institutionalists want to play with the rational-choice people, who rather prefer to exclusively play with one another. Blyth and his colleagues at Brown call this dialogue of the deaf between historical institutionalists and ideational scholars “unconscious uncoupling,” a term they stole without explicit permission from none other than Gwyneth Paltrow. So the direction of citations really goes from constructivism to historical institutionalism, and then from historical institutionalism to rational institutionalism, but not the other way around.

My third point is that there is still—and I think this is especially the case in comparative politics—skepticism about ideas as independent variables. So Mark Lichbach and Alan Zuckerman’s 2009 book, which I mentioned earlier, focuses on rationality, structure, and culture, and I think that is already problematic to some extent. I prefer to think of it as ideas and institutions and the interplay between them, because once you talk about culture, you encounter what I call the Samuel Huntington problem, whose “culturalist” approach in *The Clash of Civilizations* [1996] has given culture a bad name. Such an approach is much too deterministic, and hence you are stuck with continuity and cannot explain change. I think Mark Howard Ross’s approach [Ross 2009] is much more helpful, as it helps explain variation and why culture matters without being overly deterministic.

Ideational scholarship, especially in comparative political economy, has enabled analysts to ask questions that other approaches have not even

considered asking, and in the absence of these questions the comparative reimagining of history usually is avoided. I think that is especially problematic in rational-choice approaches like the work of Ronald Rogowski [1989] and Michael Hiscox [2002]. They say, “here is what the theory tells us, and now we should look for the evidence.” Heckscher-Ohlin models in international trade tell us that capital and labor should be cooperating against land in mid-nineteenth-century Britain to fight the good fight for free trade, and there is some thin evidence proving that fact, but historians will tell you that there was almost nothing capital and labor could agree on in the nineteenth century. After all, Marx wrote *Das Kapital* in London around that time. So you are stuck with trying to get the data to match your theory. Ideational scholars do not have this problem of “reimagining of history,” and are most comfortable when explaining change rather than continuity. Ideas are not overly deterministic. There are shocks and people can change their ideas.

But there remain serious weaknesses that are still hard to overcome. The perennial question is where did these ideas come from in the first place? Ideational approaches also question our ability to generalize and predict, and this is not very popular among our colleagues who have economics and physics envy.

So we probably need to put much more emphasis on questions of ideational change, and to show that ideas matter most when they explicitly go against material interests. That is one of the most obvious times when ideas may matter: when they do not coincide with interests. So I want to flag a forthcoming special issue of the *Journal of European Public Policy*, edited by Leonard Seabrooke, Daniel Béland, and Martin Carstensen, on “Ideas, Political Power, and Public Policy”—we also have a panel with some of those papers at 8 am on Sunday morning if you are still here in San Francisco and have nothing better to do. The second point is that we need to engage much more explicitly with historical institutionalism and rational-choice scholarship on their main concepts, such as power, path dependence, feedback, and whether ideational approaches have anything interesting and important to say about those concepts. Also, comparative political economy focuses on democracy and democratization, while international political economy tends to look at capital flows and international constraints, but there is very little to link these two areas, democracy and international constraint. Think of Greece, for example, the most obvious case: they may have all these things they want to do domestically but they cannot because of international

constraints. We also need to keep engaging with other fields. I think of behavioral economics on the micro level and economics and sociology on the macro level—for example recent work, which is very good, by Vincent Pouliot [2007], and especially Marion Fourcade [2009], and to some extent my own work with Kate McNamara on the discursive construction of northern saints and southern sinners in the Eurozone crisis [Matthijs and McNamara 2015], and on German power and the perverse logic of German ideas [Matthijs 2016]

Okay, I will keep it at that. We can come back to some of these points in the Q&A. Thanks very much.

ROGERS SMITH: Well, my mandate is to discuss ideational turns in the study of American politics, and it's fair to say that the focus on ideas in American politics has been central to everything that I've done since I started in graduate school 40 years ago this month. But I have to say, in mild dissent to Matthias's remarks—he's worried about comparative politics, but I would say that for most of those four decades, the serious study of ideas has been more marginal in American politics than in any other subfield. What's more, serious reflection and theorizing about *how* to study ideas has been virtually non-existent, and I don't exempt myself from that characterization. I will say, however, that over the last half dozen years, I have seen several different developments in different parts of the study of American politics that I regard as extremely promising. I'm going to focus on the promise of these approaches, not the challenges they face, though we will, I'm sure, discuss the challenges as we go along.

The first thing I want to call attention to is really two related things: First, research in the study of American political thought and, second, self-conscious efforts to connect American political thought with the subfield of American Political Development. I moved from primarily doing public law and political theory to the subfield of American Political Development when it first emerged because it seemed like it might be a good home for the sort of work I wanted to do. But a lot of the historical institutionalist work that has structured American Political Development, work by Theda Skocpol [1992, 1995] and in some ways especially Karen Orren and Steve Skowronek [2004], has not been particularly receptive to including ideas in APD scholarship. And in that period there wasn't that much going on in the study of American political thought either; but recently that has changed.

The resurgence in the study of American political thought is interesting because it takes place across the political spectrum in many ways. There are a lot of younger scholars of American political thought, people like Chip Turner [2012], also known as Jack Turner, Jason Frank [2010 and 2014], Cristina Beltran [2010], Corey Robin [2004 and 2011], Melvin Rogers [2009], Lawrie Balfour [2001 and 2011], and others who are doing studies of American thinkers, and in many cases they're focusing on figures who weren't canonical parts of American political thought, including African-American thinkers, literary writers, sometimes Latino thinkers, women, labor leaders, etc.—they're paying more attention to these voices in American political thought. There is a kind of left tilt in most of this scholarship, which focuses on issues of identity, and identity in relation to class, and contestations over constructions of identity. But there is also a resurgence in conservative scholarship on American political thought. A lot of the modern scholars influenced by students of Leo Strauss have turned to twentieth-century American thought. They used to write mostly about the founding, now they write about what's wrong with the Progressivism and what was good about Calvin Coolidge [e.g., Ceaser 2006 and 2007; Kesler 2012; Johnson 2013]. This renewed interest in American political thought has issued in the creation of the journal *American Political Thought*, which Michael Zuckert edits, and many of its contributors are from this second camp—though many are not. It is proving an excellent vehicle for a wide range of work.

Meanwhile, there is increased concern to connect the study of American political thought to American Political Development, arguing that the interplay of ideas and institutions—theorized in various ways that we can discuss—is vital for understanding American Political Development. George Thomas edited a symposium in *American Political Thought* on connecting American Political Development to American political thought [Thomas 2014]. Ruth O'Brien published an essay in *Clio*, the newsletter of the History and Politics section of APSA (a home for historical institutionalist-American Political Development types), calling for connecting ideas and institutions, particularly in the construction of identities. She cited some of the recent scholarship in American political thought that I mentioned [O'Brien 2012–2013]. This fusion of American political thought scholarship and APD is in its early stages, but this is an area of formative activity that I think is promising.

And the second thing I want to mention, related to this development, is that comparativists such as Vivien Schmidt have responded to the lack of

receptivity among historical institutionalists to the study of ideas by urging that more attention be paid to discourses. Schmidt of course calls for the study of a kind of discursive institutionalism [e.g., Schmidt 2008]. The focus on discourses is meant in part to solve the problem that ideas seem like invisible things inside our heads, but discourses are out there, you can study them empirically. This discursive ideational turn is not something that Americanists have picked up on very much so far, but it is beginning to be picked up more, especially by younger scholars. Even some older guys, like Desmond King and I, published a paper in the *Journal of Politics* last year in which we drew on this literature to provide a theoretical framework to consider how it was that understandings of the *content* of civil-rights issues shifted in the '60s and early '70s from segregation versus integration to color-blind versus race-conscious policies [King and Smith 2014]. And I'm seeing more work by younger scholars that's trying to bring these ideational arguments from comparative politics and international relations more to American politics. Plus it's also true that some of the historical institutionalist scholarship *is* paying attention to the importance of ideas. This afternoon I'll discuss a paper by Verlan Lewis [2015], for example, which argues that party ideologies are reshaped by parties' successes in controlling institutions, controlling the executive branch, for example, which can make them more favorable to executive power. So from the more institutional side there is renewed interest—or new interest, I'd say—in the interplay of ideas and institutions. So I see a little softening of the unwillingness of the historical institutionalists to get out and dance with people doing ideas. Early days, but in my view a promising development.

And I'll note one last development. There is of course in the American politics mainstream literature a lot of attention paid to ideology, as in the Poole and Rosenthal mapping of ideological polarization and congressional voting patterns [Poole and Rosenthal 1985]. But that's not a literature that really takes ideas seriously at all—in this work, ideologies are statistical artifacts of people's voting together; we don't really know or apparently care about the contents of the ideas that drive them to vote together. But some of the quantitative scholars studying ideology in America are trying to take ideas more seriously today. For example, Hans Noel at Georgetown has for some years now been focused on the role of various kinds of public intellectuals in producing new creative syntheses of political positions, forming ideologies in ways that come to shape the positions of major parties; and he's also trying to draw on

some rational-choice theorizing to theorize about how ideological innovation and creativity takes place [Noel 2015]. This work is in its early stages and I'm skeptical about how much we can learn from formal modeling about ideational innovation and agency. But nonetheless, when I look across the landscape at the research and interests of many forms of scholarship on American political thought, at the conscious efforts to connect to the study of institutions, at the way historical institutionalist scholars are increasingly feeling they have to pay some attention to ideas, and at the way that even quantitative analysts of ideology are trying to take ideas more seriously—I think that after 40 years things are looking up.

JEFFREY FRIEDMAN: Can I just ask before I begin whether are there any theorists here? OK, well then I have *carte blanche* to say whatever I want about theory, and I'll take my full ten minutes now.

It's striking that it so happens that the order we chose by alphabetical lot begins with Jeff Checkel saying things aren't all that great for ideas in international relations, and Matthias Matthijs saying things are worse in comparative politics, and Rogers Smith saying they're even worse in American politics—and I'll say they're even worse in theory, although they're looking up a bit, as Rogers also said about American politics. Now in one sense, theorists are always talking about ideas, at least historians of thought: ideas in Hobbes, Locke, Arendt, and so on. And if you are writing theory you are engaged with ideas even if you aren't treating them as objects of interpretation. If you're doing normative theorizing, by definition you are engaged in the analysis and creation of ideas. But that's true of all scholars. So it is odd that all scholars don't take ideas seriously in principle since in practice they do—because they're constantly engaged in producing ideas. You would think it might also occur to them that the particular research that they're pursuing, the particular hypotheses that occur to them, might themselves be products of ideas to which they've been exposed. The absence of scholars who explicitly treat ideas as determinants of behavior isn't unique to political theory.

On the other hand, ideas are treated in historical political theory in a way that I think perhaps could be a model, in a sense, for all ideational treatments, because it's pretty clear that when you're dealing with an individual, when you're asking why he took certain actions, why he wrote certain things in the text you're interpreting, that you have to look to the cultural context, by which I mean the ideational influences on him

—biographical, pedagogical, and so on. And even if you're talking about mass behavior you're still talking about human beings, so ideas should still be at work, you would think, in determining their behavior. Yet the consideration of the role of ideas by political theorists has been under-theorized and largely ignored, with the promising exception, which I'll go into, of recent theorizing about "epistemic democracy."

I hope that when we turn to the roundtable part and the audience-participation part, that an obvious question we can address is why it's so common in all four subdisciplines to ignore the role of ideas. In political theory we share in the common political-science view that politics is all about power and that political power is used to advance conflicting interests or values. Now values are ideas, you might think, so political theorists might be interested in where values come from. That's certainly an empirical question, but it's not one that necessarily requires statistical high-tech treatments, so in principle political theorists could engage with it. As for interests, political theorists seem to share the unfortunate view that people's political interests are self-evident to them, rather than being ideas that are fallible. Once you recognize that our "interests" are actually ideas about interests, and that these ideas, like all ideas, are fallible, the question arises as to how *reliable* are the ideas that political actors have. How "accurate" are they, for lack of a better term? That's a quasi-normative question that political epistemologists, such as epistemic democrats, have taken up, but I think that they face resistance from the fact that their fellow political theorists tend to overlook that when people act in politics, their actions are mediated by fallible interpretations of their situations and of the causes of the problems they're trying to solve through political action. Therefore the question of political actors' *knowledgeability* doesn't seem very relevant to many political theorists—and by knowledgeability I mean the reliability of the actors' ideas and interpretations. So you can see how epistemic democrats, who *do* care very much about political actors' knowledgeability, are anomalous within political theory and represent a very promising opening for the normative consideration of the role of political actors' fallible ideas.

Now epistemic democrats also faced a barrier in the form of a general commitment among political theorists to democracy as inherently valuable in one sense or another—for example, as an expression of equality or autonomy. If it's inherently valuable we're not going to be that interested in assessing what citizens do with it, or assessing whether the policies implemented by real-world democracies or imagined ideal

democracies are beneficial—because democracy is just inherently good, whatever the policy outcomes it produces, regardless of the consequences it produces.

This view of democracy as inherently valuable began to break down a bit in 2008 when a philosopher, David Estlund, published *Democratic Authority* and pointed out that if our concern is just expressing or doing justice to equality, we could pick policies or officials randomly, or pick among voters randomly and give them all the power [Estlund 2008]. Since everyone would have an equal chance to be picked, that would be a perfectly fine expression of equality and probably better than anything we have in real-world elections, where there are all sorts of inequality, despite the formal equality of one-person-one-vote. Even before that, in 2006, a paper by José Martí pointed out that in deliberative democracy—which was and is arguably the biggest thing going in democratic theory—that within a deliberation, all the deliberators are trying to achieve some objective or another, although not necessarily a self-interested objective: it can be a sociotropic objective or a value of some other kind [Martí 2006]. In trying to achieve objectives they are presupposing some standard of evaluation of what is a good objective and what is a bad objective, and that normative standard has to be independent of the process, the deliberative process, which theorists treat as valuable in itself. I would extend this point by saying that it applies to virtually all political action, not just deliberation. A voter goes into a voting booth. Let's say the voter is a political theorist and he goes there because he thinks it's good for people to participate in politics. Once he gets into the voting booth, though, how is he supposed to vote? Well, there has to be some criterion for choosing among the options on the ballot; otherwise he'd have to vote randomly. We can say that in such cases, democracy is, for the voter, instrumental to achieving the independent criterion; its goodness is not inherent but depends on whether it achieves good ends as judged by independent criteria. An exception is when democracy itself is at stake, or when its twin value, equality, is at stake. Then you can say that the procedures of the institutions are the same as the goals of some of the deliberators. But there are many political issues where democracy itself isn't an issue. And we have plenty of people with strong opinions on these issues, yet political theorists tend not to care about whether these opinions are liable to be accurate and where the opinions come from.

I think the gut-level impetus driving a lot of epistemic democrats is the question whether, if democracy led regularly to disastrous outcomes on

these issues—like every third year there was a depression in every democracy in the world, or there was mass poverty in every democracy in the world that was only getting worse, or whatever else would count as a terrible outcome—then would we really still be committed to democracy just because it manifests equality or some other value inherently, no matter what its outcomes are? Epistemic democrats tend to say no. And so, from that perspective, they want to understand why democracy *doesn't* produce these bad outcomes—what quality is it, what institutional quality it is, that accounts for good outcomes? There are also those who are more critical of democracy and are inclined to think it actually doesn't do such a great job at producing good outcomes. They want to account for these bad results, while the epistemic democrats want to account for what they take to be good results.

The next step in the development of this literature was the complication introduced by H el ene Landemore in *Democratic Reason*, which proposed two mechanisms that could account for the putative superiority of democratic outcomes [Landemore 2013]. In addition to the Condorcet Jury Theorem, she borrowed the Diversity Trumps Ability Theorem from Scott Page and Lu Hong [Hong and Page 2004] to argue that a large, diverse group of people trying to solve problems might do better than a small group of experts. But I think the more important step that H el ene took, other than proposing these two mechanisms, or putting them together, is that she very carefully bracketed first-order considerations about outcomes altogether. Even though she thinks that democracy produces good outcomes and she's trying to explain why that is so in her book, she doesn't rely on saying, here are a bunch of good outcomes that democracy has produced, so we need to identify the mechanisms that would explain them, and here they are—the Jury Theorem and Diversity Trumps Ability. Instead she appeals only to second-order arguments about the positive outcomes we can expect of democracy because of the epistemic effects that are likely to be produced by those two mechanisms.

I think the first-order/second-order distinction is crucial because most epistemic critics of democracy, like the economist Bryan Caplan [2007], the philosopher Jason Brennan [2008 and 2011], the former political scientist Ilya Somin [1998, 2013, 2014, and 2015], and a lot of public-opinion scholars [e.g., Achen and Bartels 2016], think that actually democracy produces lousy outcomes. A second-order reason for this is public ignorance: How can voters who don't know anything about

politics be expected to pick policies that would produce good outcomes? In contrast, a first-order reason would be to point at what one views as bad policies and work backwards, perhaps to public ignorance, to explain them. Jennifer Hochschild just published a book taking this approach [Hochschild and Einstein 2015; cf. Hochschild 2012]. The danger with first-order approaches, though, is that they're entirely dependent on the political convictions of the theorists who develop the approaches. If you don't already think democracy produces lousy results, you won't be interested in a scholar's explanation for these alleged results. So first-order epistemic scholarship threatens to become little more than various theorists' politics being furthered through theoretical means. There is already a lot of that, too much of that in political theory, and it would be a shame if that's what happened to epistemic democracy. But because of the scrupulousness of the epistemic democrats in appealing to second-order considerations, for the most part, it doesn't look like that danger is imminent.

Let me relate two last steps in the evolution of epistemic democracy that may have wider lessons for the study of ideas in other subdisciplines. A paper that Landemore and Scott Page [2015] just published in *Politics, Philosophy, and Economics* distinguished between political issues that are so simple that when deliberators hear the answer, it's like hearing an "oracle"—they all have a eureka moment: "Ah, of course, that's the answer!" Now that's actually the model H el ene used in her book, in inferring from the Diversity Trumps Ability Theorem the conclusion that democratic deliberation is likely to lead to better outcomes with bigger groups of deliberators than a smaller group of experts. If the bigger group of deliberators is cognitively diverse it will likely have more ideas about how to solve a problem, and if the problem is so simple that once the best answer is suggested everyone will see that that's the best answer, then the Theorem works. But in this new paper, she and Page distinguish between simple problems and more complex problems, which they call predictive problems. I'll quote two sentences from the paper, about economists faced with complex problems: "In these problems, like macroeconomic problems, the deliberators make different assumptions about the world, they use different formal constructions, and they rely on distinct variables and behavioral assumptions. All of these are incommensurable, hence economists tend to disagree" [Landemore and Page 2015, 14]. That's crucial because in the real world of politics, I would argue, we are in the position of economists

in this respect, whether we're aware of it or not. We usually aren't facing eureka moments and there usually isn't a self-evident, obvious solution to the problem we're trying to solve. And if we think that an oracle has just announced a self-evident solution, we'd probably be well advised to unmask that oracle (such as Donald Trump). Generally we're facing complex problems where there are genuine considerations on both sides of the issue, or where there are several sides of the issue, not just two. And epistemic democrats need to consider this much more treacherous terrain—treacherous for democracy, because the intuitive solution to the problem of complexity would be technocracy: let democracy deal with values, because nobody is more expert than anyone else on matters of value, but we turn to experts when it comes to the complex task of figuring out which policies will best achieve our values. For this reason epistemic democracy might actually culminate in epistemic technocracy. But as the quoted sentences wisely point out, even the experts disagree very often, which may call into question the reliability of the experts' "knowledge" even when there is a consensus among them.

To me the problem of complexity indicates a large potential for common ground between empirical researchers who are interested in ideas and normative theorists who are interested in epistemics, and exploring this ground together might have the side benefit of helping to bridge the pretty awful and destructive division in political science between theorists and empiricists. Before we turn to technocracy as an alternative to democracy, we have to know how reliable technocrats' knowledge is or is likely to be. So we have to ask what kind of knowledge technocrats should acquire, ideally, and how likely technocratic knowledge is, in reality, to be good, accurate knowledge. This will obviously lead us to the philosophy of the social sciences. But we'll also want empiricists to investigate what it is that real-world technocrats, such as people at the I.M.F., actually know or think that they know. What do they learn in graduate school, what do they learn in college, what do they learn in elementary school, what do they pick up from popular culture, what do they learn from the news sources they consult and other forms of political information? These are empirical questions that normative theorists aren't equipped to answer, but it would certainly be helpful to normative theorists if they got some answers from their empirical colleagues; otherwise they won't be able to assess how desirable technocracy is as a solution to the problem of complexity. Similarly, epistemic democrats would benefit from knowing about the ideas of ordinary citizens: what are the

various forms of education and political socialization that shape their ideas? How likely is it that ideas coming from these sources are epistemically reliable? I'll conclude with that.

To begin our round of 3-minute or 5-minute statements, I wanted to ask a question of each of my fellow panelists. I wanted to ask Jeff, how can you have qualitative methods that aren't ideational? This is something I've been trying to figure out from the outside. I can see how quantitative methods, by reducing behavior to homogeneous units, may leach out the inherently qualitative aspects of ideas, but what other type of qualitative research might there be that isn't ideational? And also, if you could say something more about the positivist versus interpretivist divide. What I'm curious about is how can ideational theorists be positivists—what does that mean? I can much more easily grasp how they can be interpretivists. And Matthias, I just wanted to ask you to elaborate, if you would, on the problem of the unpredictability of ideas as a barrier to the wider acceptance of ideational research in comparative politics. What makes ideas unpredictable, and what can we do about that barrier, if anything? And Rogers, you didn't really talk about why most Americanists ignore ideas; maybe you could say something.

ROGERS SMITH: I can answer very quickly. Most Americanists ignore ideas because they think politics is driven by interests and they think that interests are relatively self-evident and they underestimate the role of ideas in defining what interests are.

JEFFREY T. CHECKEL: I'll take three or four minutes to respond to you and to some other things that have been said.

So let me clarify because maybe it wasn't that clear from my remarks: I actually don't think the status of ideational constructivist work in international relations is *that* bad. Matthias and I talked about how a very typical international relations syllabus will divide the world up into, oh, there's that realism stuff and there's constructivism. Constructivism just wasn't there 15 years ago. To put it in context, I beat up on ideational constructivist work in international relations, so that has some problems, but what about the other big paradigms—*realism*? Jeffrey Legro and Andrew Moravcsik wrote a piece over a decade ago, "Is Anybody Still a Realist?" I mean realism was just all over the place. So I picked on constructivist work in international relations. I think there are big paradigms in international relations that have similar problems. I don't think the

status, the role of constructivist stuff in international relations is that bad. In fact, I'd further argue that the problems I touched upon are problems not caused so much by the discipline—by the fact that political scientists just don't *do that*. Rather, I see the problems as coming more internally, from a lack of critical self-reflection by positivist ideational scholars. Just to give you a couple of examples. When constructivism first came on scene about a decade and a half ago, a big trump card for some of the constructivists was socialization. This made them different from realists and political institutionalists or whatever. But they ended up—I'm a part of this, right?—picking up a narrow slice of socialization, which made the world nice and cooperative. They made zero connections to work on political socialization, I don't know why, they made zero connections to work by comparativists from early generations, sociologists who studied socialization in connection to violence, producing death, and genocide, and all these kinds of lovely things. Why did they do that? I think there's been some move to try to correct that. But come on guys, let's be serious.

Another example of a lack of critical self-reflection that I see in the work of constructivist work in international relations is the option of pluralist analytic eclecticism as sort of the master narrative in thinking about how we should be thinking about redevelopment. Some very powerful people, such as Peter Katzenstein, are behind this. Peter is a dear friend but I do disagree with him on this stuff because analytic eclecticism is a prescription for—I'm not sure what it's a prescription for, but it's not for—I mean it sort of opens up space to do some interesting creative theorizing, but I'm not clear how it coheres, how you can avoid the problem of having little bits of theory everywhere that don't talk to each other, including within international relations. And I think that's a bit of a problem.

And then just quickly, to something Jeff just asked about qualitative methods. I think I need to clarify. You said, aren't all qualitative methods ideational? Yes, actually. I don't disagree. My concern is a lot of the constructivist ideational work I see in international relations is not self-conscious enough about how it uses its methods to make its arguments. Because this work is typically very empirically grounded, which is great, but empirics require methods, and often the methods are a bit opaque or non-transparent about how they're being used. So that's my concern, these folks aren't actually getting on board with the revitalization

of qualitative method that we've seen across a number of positivist or post-positivist methods.

MATTHIAS MATTHIJS: Just 30 seconds before we get the audience involved. The problem of what makes ideas unpredictable. I think Peter Hall's 1993 paradigms piece in *Comparative Politics* is responsible for this to some extent [Hall 1993]. Following Hall, we neatly divided up the world, with a Keynesian period from the mid-1940s to the late 1970s in political economy, followed by a neoliberal period starting in the early 1980s. But if you look at Britain, for example, the neoliberalism of Thatcher versus the neoliberalism of Blair versus the neoliberalism of Cameron are very different kinds of "neoliberalism." There has been a gradual change within the neoliberal paradigm and you could not have easily predicted which one would take hold and which one would not. Or take the debates on fiscal deficits that we now have, as another example. Does the emphasis on the evils of deficits really coincide with certain powerful interests? Yes, sure, but only *certain* interests, and not others. There are plenty of ways in which big businesses get their way, but there are plenty of times when they do not get their way, like on immigration, for example, in the American context, or with infrastructure spending, and so on. I think that is where we need to be much more specific about under what conditions certain ideas take hold or not. But it is not as straightforward as businesses have interests, workers have interests, states have interests. Institutions constrain choices but ideas can change much more quickly or they can change slowly and we do not have a good mechanism to think about that.

JEFFREY FRIEDMAN: I'm going to publish these proceedings in *Critical Review*, so the Managing Editor has a recording device that she'll pass around. So just hold it up while you talk and please identify yourself and your institution very clearly so we can contact you for permission to print your words in the issue of *Critical Review* that we'll publish.

CRAIG PARSONS: I'm Craig Parsons from the University of Oregon. Thanks, Matthias, for mentioning my work. Rogers's last comment attracted my interest. The answer you gave in response to why Americanists don't deal with ideas resonated with me; I think most non-ideational scholars would agree that interests are what drive political action, not ideas. But if that were really the reason, in principle if these people

were remotely open minded, then at some point they would take seriously good qualitative work that showed that interests aren't clear to actors. Which suggests to me that the real problem is in the philosophy of science regarding what constitutes a worthwhile theory. Certain kinds of political scientists just have poor epistemological training and have assumed that only a generalizing rationalist model can ever produce something called knowledge. So even if you can demonstrate repeatedly with good qualitative work that different actors with different stories didn't actually know what their interests were, they wouldn't listen to that.

ROGERS SMITH: There's a version of what you're arguing that I think is clearly partly correct and may be entirely correct. I do think that one reason that a lot of quantitative Americanists simply accept the notion that interests that are fairly self-evident drive politics, is because they are looking for models that they can operationalize empirically, and it's just easier to say that actors have these interests that define their goals; then they get on with the kind of work they want to do: How do actors strategize, how can we measure the distribution of preferences, and so forth. So I do think there's a kind of methodological bias to take interests as given.

But I'd also say that those of us who understand interests to be constituted by ideas, and in ways that are contested and changing and are often uncertain, and who have all these more constructivist kinds of understandings of interests, haven't done as well as we could to show that interests are more complicated than our colleagues think. It's not like we produce these brilliant demonstrations and they just ignore them. I think the people who study ideas, including myself, haven't been self-conscious enough in the task of making those arguments. So I think we can do that better. I'll even throw in that there's this movement towards digital humanities today, this effort using new computer techniques, big data software to analyze the distribution of terms in discourse; I think there's a potential there to show to positivists that—rather than competing ideas being there and then institutions and coalitions determine which ones prevail—that there is such a thing as ideational agency over time, that you can see a variety of efforts to formulate new ideas and then some come to the fore in ways we can begin to understand. So I think that yes, there's a kind of methodological bias there, but there are also challenges and opportunities for people who study ideas to persuade people of their importance despite those biases.

ALBERTO SPEKTOROWSKI: Alberto Spektorowski from Tel-Aviv University. Just to point out the importance of ideas, we can make a real comparison between how democracy was perceived and understood and debated at various points in the twentieth century. In the 1920s there was no serious intellectual who didn't come down hard on democracy. I would say that you cannot understand fascism without understanding this. The anti-democratic spirit of the times in the '20s may not be quantifiable, but the importance of ideas seems to be quite clear. The assault on the ideas of the Enlightenment led to the intellectual current that set the basis of fascist ideology. For a long period of time scholars avoided dealing with the deep ideological sources of fascism, but scholars such as Zeev Sternhell [1986] have now returned attention to fascist ideology as a factor in political debate [Spektorowski 2016, 124]. In current times, we might be approaching a new dark age, maybe without the results of the past, but still we should be attentive to it. We should question the nature of the current backlash against political correctness, multiculturalism, etc. We should deal critically with the construction of "political correctness," with the ideologization of economic sciences, and detect what ideas are behind the current populist outbreak. Is populism devoid of ideological background? We might be extremely naïve if we disregard the ideational background of the current populist reaction as well as the ideational background of the worldviews, attitudes, and practices that set the stage for populist outrage.

So I approve totally of what you say here, and thank you.

ROGERS SMITH: Well that's what I'm trying to say. You might be able to quantify them, you might be able to show that democracy as a negative term surges in the '20s and helps set the stage for fascism.

ADAM HANNAH: Adam Hannah from the University of Melbourne. My question is for Matthias. You talked about how comparative politics has related to historical institutionalism and rational-choice work. Do you think there's scope for reorienting some other literatures that are more explicitly friendly towards ideas? I'm thinking about public-policy literature and agenda setting by John Kingdon [Kingdon 1984], that sort of thing.

MATTHIAS MATTHIJS: "Absolutely" is the answer to your question. I think the most useful thing is economic sociology, but in an American context

there is always this professional constraint about where to publish and who to engage with. So that is part of the problem as well, you are not really encouraged as a junior faculty member to publish in these “weird” journals all that much, even though it may be very useful in pushing forward an ideational agenda. Such publications simply do not fit into the standard agenda of what search committees will be looking for when they are evaluating your record for promotion or tenure.

JEFFREY T. CHECKEL: Just a comment on this. In my first book in 1997, I used Kingdon and others. And one critique of that book by my fellow international relations people is that it was too much comparative politics and too much public policy.

NICOLA NYMALM: Nicola Nymalm from the German Institute of Global and Area Studies in Hamburg. Could each of the panelists say a few words on core methodological debates in the study of ideas? This is linked to the question of how you actually conceptualize “ideas.” Now it comes across as if we are all on the same page when we talk about ideas. But I wonder, especially taking differences in the social sciences as disciplines in Europe and the U.S. into account, whether in the end we’re all talking about the same things once we start to unpack the concept. We might end up with norms, values, discourses, identities, etc., which here might all go under the notion of “ideational factors,” but they are not all approached and studied in the same way after all.

MATTHIAS MATTHIJS: I just very briefly want to flag two. First, there is the great Cornell Ban’s work at Boston University [Ban 2016]. What he has done is taken Jeffrey Chwieroth’s work much further [Chwieroth 2009], by looking at the IMF and the ECB, and how their staff members were socialized—not just where they got their Ph.D.s, Chicago vs. MIT or something like that—but also who cites whom in these IMF and ECB publications, and then looking at who actually got systematically promoted. Ban ends up with a kind of dramatic story of those at the IMF under Olivier Blanchard who promoted new-Keynesian types and kept the old neoliberals down, while at the ECB you end up seeing exactly the opposite. Ban has a really cool way of quantifying how and why all this matters for policy outcomes. Second, I want to echo Vivien Schmidt’s call for more “discursive” institutionalism [Schmidt 2008]: the great thing about discourse is that it is out there.

You can do discourse analysis, compile all this stuff, and not just purely say this is elites, like if I interviewed Angela Merkel and she told me she was doing something for a specific reason, which often leads to a dramatic re-writing of history to serve the interviewee's purpose.

ROGERS SMITH: I don't have a lot to add, partly because even though there are clearly different conceptions of ideas at work in American politics scholarship and there is some contestation over different approaches, these debates are not well developed and can't be easily mapped or summarized. I do think that the prevailing approaches are to understand ideas chiefly as discourses that provide certain kinds of empirical portraits and certain kinds of normative prescriptions about political life. And the debates are over whether we understand those chiefly as rationalizations of interests, or chiefly as discoveries of perfect truth, or whether we understand them as something more complicated that represents an effort both to make sense of experience but also to craft conceptions that can serve political projects and can also provide senses of meaning. But it's part of the challenge to the study of ideas that we need to think about these issues more fully.

JEFFREY T. CHECKEL: In terms of the core methodological debates for constructivism in IR, ideational work, there are two playing out right now: one is internally generated by a set of constructivist scholars, the other is coming from the outside. The internally generated issue I've already alluded to is how or whether you can or should integrate methods from, to put it crudely, the American toolkit, process-tracing case studies, or the European toolkit, discourse-practice. Some people say that's impossible, it can't be done. Others I think are onto something in thinking in a more grounded way about how do you develop community standards, cutting across these different methods—people like Fritzie Loeb and, as I already mentioned, Ted Hopf. That's a debate roiling within constructivist work in IR. The issue coming from outside, raising a lot of concern, is the broader DART initiative within Political Science—Data Access Research Transparency—which has a very big head of steam. A number of journals now have a very explicit requirement about publishing only *replicable* research, including the *American Political Science Review*, which has a replicability requirement that I find very troubling, at least in some of the language on their website. *International Organization* is trying to come up with a policy here too, but there are a lot of

very hairy difficult questions that haven't been fully thought through, implying that the qualitative work on ideational stuff is unscientific. That's something coming from the outside.

JEFFREY FRIEDMAN: In normative political theory there's no debate of this sort, which I think is a sign of the problem. Even among epistemic democrats there's a neglect of the very idea of ideas or interpretations as opposed to information or knowledge. We initially called this section of the APSA "Political Epistemology," perhaps mistakenly, because what the epistemic democrats do is evaluate two things. First, they compare democracy to other types of political arrangements in terms of how much knowledge they can be expected to aggregate or generate through deliberation or voting; but the assumption, which is brought out by the oracle model, seems to be that information is just unmediated, knowledge is just unmediated, except by the deliberation itself, so the only question is whether some people have it and others don't. So deliberation is supposed to bring together the diverse pieces of information scattered out there.

I think an alternative to this very simple view of "knowledge" can come from interpretivism. Interpretivism could be to political theory what constructivism was to international relations: a sophisticated way of understanding people's political ideas, one that doesn't treat these ideas as innate or as self-evident. Mark Bevir laid the groundwork for political theorists to take up interpretivism in his *Logic of the History of Ideas* [Bevir 1999] and in the introduction to his *Democratic Governance* [Bevir 2010]. He and Jason Blakely have an important book forthcoming from Oxford University Press, *Interpretive Social Science: A Theoretical Perspective*, which discusses the application of interpretivism in the empirical subdisciplines; on that front I should also mention the wonderful collection edited by Dvora Yanow and Peregrine Schwartz-Shea [2006], *Interpretation and Method: Empirical Research Methods and the Interpretive Turn*. Finally, I develop of version of interpretivism that I call "ideational determinism" in my own forthcoming book, *No Exit: The Problem with Technocracy*.

NOTE

1. At the executive council meeting of the Political Epistemology section of APSA, held the next day, the name of the Political Epistemology section was changed to "Ideas, Knowledge, and Politics."

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