

The international relations scholarly community would be well advised to accept this invitation and proceed with a serious investigation of the middle of the third debate. Frank Harvey and Joel Cobb's skepticism in this forum regarding the merits of this invitation derives, in part, from insufficient attention to this author's explicit focus on the communicative—as opposed to epistemological—qualities of the *via media*. From a communicative point of view, and with respect to both intra- and inter-paradigmatic differences, the *via media* should discourage both facile ignoring and dogmatic rejection of other theories and perspectives (see Lapid 2002). Furthermore, the anticipated result of more engaged pluralism should be consistent with both Harvey and Cobb's vision of "multiple middle-grounds across multiple debates" and with Kratochwil's preference for "problem-driven as opposed to approach-driven" analysis.

(4) For some time now, there has been dissatisfaction with the "third debate" characterization of the current state of our intellectual transition. Are we still in the third debate or have we moved to a fourth or, perhaps even, a fifth debate? Were we to engage in a successful implementation of a dialogical turn toward engaged pluralism, it would justify fresh proclamations of a new stage in theorizing. Indeed, the scholarly community is well-situated today to take advantage of new intellectual and communicative opportunities offered by a rehabilitated, enlarged, and more frequently visited median space. "Would it not be refreshing," asks Donald Puchala (2000:142), "if such continuing conversation, and not periodic great debates, became the intellectual mode of International Relations?" Hopefully, the metatheoretical prelude sketched in these comments will help turn Puchala's vision into a reachable goal.

Theory Synthesis in International Relations: Real Not Metaphysical

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Theory synthesis is not only possible and desirable but is constitutive of any coherent understanding of international relations as a progressive and empirical social science. Numerous interesting proposals exist for formulating and empirically testing multitheoretical propositions about concrete problems in world politics. Below the reader will find a set of basic principles that should underlie testable theory syntheses. Yet other contributors to this forum—Friedrich Kratochwil, Yosef Lapid, Iver Neumann, and Steve Smith—do not share this openness to theory synthesis; their views range from deep skepticism to outright rejection. The real issue between us is whether pluralism among existing theories ought to be preserved for its own sake, as these colleagues believe, or whether theories ought to be treated as instruments to be subjected to empirical testing and theory synthesis, as this author maintains.

A Practical Program for Theory Synthesis

Theory synthesis is occurring. Many of the most salient contributions to international relations theory in the past two decades rest on it. Robert Keohane (1984) synthesizes realist (hegemonic stability) and regime theories to explain postwar cooperation. Nearly every member of the recent generation of security theorists, led by Stephen Walt (1987), Jack Snyder (1991), Stephen Van Evera (1990/1991), and Barry Buzan and his colleagues (1993), combine power and intentions to explain alliance formation, imperialism, war, and the global structure. Bruce Russett and John O'Neal (2001) link liberal and institutionalist factors to explain the peace among liberal states. Martha Finnemore and Kathryn Sikkink (1998) summarize much recent work, including many European contributions, combining rationalist and constructivist elements to explain the evolution of international human rights norms. Theory synthesis is particularly important for those who believe, with Kratochwil and this author, that international relations scholarship ought to be problem as well as theory-driven. The complexity of most large events in world politics precludes plausible uncausal explanations. The outbreak of World Wars I and II, the emergence of international human rights norms, and the evolution of the European Union, for example, are surely important enough events to merit comprehensive explanation even at the expense of theoretical parsimony.

Theory synthesis is easier than one might think. There are few limits on its scope. Most syntheses comprise a set of discrete theories, linked by a set of overarching assumptions. The overarching assumptions take various forms, each embedding subtly different formal and substantive assumptions: multivariate regression equations, game theoretical models, explicit models of interactions, decision trees, lexicographical orderings, narrative accounts, multistage sequences (exemplified below), and so on. The major task facing general discussions of theory synthesis—unfortunately one that goes beyond the scope of this short essay—is to clearly elaborate the advantages and disadvantages of these options.

Thus, in contrast to what other contributors to this forum seem to assume, the elements of a synthesis, though necessarily coherent at some fundamental level, need not share a full range of basic ontological assumptions. Although the overarching assumptions embedded in a given model must be minimally coherent and justify the relative position of the elements within a multitheoretical synthesis, there is no need for each subtheory of the synthesis to make identical assumptions about fundamental ontological matters (for example, the identity of the basic actors, the nature of individual motivations, the level of rationality of the actors, the dominant form of social interaction). A multivariate regression, for example, might synthesize socialization and rational choice effects without doing any violence to the statistical assumptions.

If syntheses can be theoretically diverse, what limits their scope? The answer is: Data. Proposed syntheses, like individual theories, can and should be subjected to empirical testing. Testing along with exploring the challenges to internal consistency of the constituent parts are the primary means of imposing intersubjectively valid constraints on theoretical conjectures. In this regard, the epistemological status of a theoretical synthesis is no different than that of a single theory; in both cases, our confidence is a function of plausibly objective empirical support.

As a practical matter, however, the testing of broad and complex syntheses may raise greater methodological difficulties than the testing of simple conjectures. Problems stem both from the difficulty of finding relevant comparative cases and the increasingly chaotic aspects of complex interactive processes (see Fearon 1996).

One way to work around these issues is to break down the elements of the synthesis and separately test each one as well as the joining assumptions. Such disaggregation is often the key to reliable theory testing—particularly using qualitative means. For an illustration of this process, see Moravcsik (1998:Ch. 1).

The Reflectivist Rejection of Theory Synthesis

This account of the nature and virtues of theory synthesis differs from the skeptical and critical accounts of the other contributors to this forum. Why is this so? Underlying our disagreement lays a divergence in philosophy of science. Unlike the others, this author believes that social science can only be justified as a tool to generate empirical knowledge about cause-and-effect relationships. The health of international relations as a social scientific endeavor is best assessed in terms of its ability to encourage a deeper and broader understanding of the existing empirical support for competing causal conjectures about world politics. (This statement is not intended to imply that a single orthodoxy will emerge, only that there will be greater consensus concerning the range of viable arguments and the nature and weight of the evidence supporting each.) The broader the range of plausible conjectures tested, the wider the sources of data employed, and the more precise, rigorous, and reliable our understanding of the relationship between these two, the more satisfying the state of international relations. We cannot, of course, rule out of social scientific debates discussions of intellectual history, fundamental social theory, ontology and epistemology, the motivations or constraints on individual researchers, policy purposes, and normative values. Thus, Smith is correct in his contribution to this forum when he observes “any discussion of the possibilities of dialogue and synthesis must reflect underlying, and usually implicit, metatheoretical commitments.” But considerations of the latter cannot supplant the former. In other words, such digressions must be justified, ultimately, as efficient means to promote a wider and deeper understanding of the relationship between causal theory and the empirical record of world politics. Any social scientific debate that is permanently sidetracked into such metatheoretical discussions should be treated with suspicion.

The position underlying this view is essentially Weberian. In the modern world of plural discourses, specific modes of inquiry must be justified in terms of their distinct form and purpose. As regards form, social scientific discourse is distinguished from other prominent modes of discussing politics (for example, symbolic art, philosophy, rhetoric, journalism, and historical description) by its explicit emphases on theory, method, and empirical explanation. Unlike political art and symbolism, social scientific discourse is not beautiful. Unlike normative philosophy, social science does not directly interrogate our deepest moral intuitions and ideals about politics. Unlike positive philosophy, it does not explore the basic epistemological or metaphysical bases of our apprehension of reality. Unlike the political rhetoric employed by leaders and entrepreneurs, social scientific language is not an optimal discourse for inspiring and mobilizing broad support for social change. Unlike journalism and certain sorts of historical analysis, social science does not focus on compiling a precise chronicle of the immediate flux or subjective experience of political life. The primacy accorded by social scientific discourse to theory, method, and empirics makes sense only if we assume that its distinctive purpose is to illuminate patterns of cause-and-effect relations in the concrete empirical world of politics. Theory and method are, therefore, means not ends; they exist to promote our understanding of empirical causes by encouraging theoretical breadth, logical coherence, and empirical objectivity.

The standard rules of social scientific discourse require that any debate should address a broad range of plausible alternative conjectures about empirical cause and effect, that competing conjectures be rendered in as coherent and general a form as appropriate, and that empirical claims be constrained by objective methodological procedures specifying what constitutes confirming and (more importantly) disconfirming evidence. In other words, social scientific discourse is useful not because it assumes certainty, but because it imposes skepticism. Standardized theoretical and methodological constraints of this kind are designed to render all claims provisional and to structure the intersubjective evaluation of such claims. Theory and method make it easier for any trained person—sometimes even those without a great deal of knowledge or investment in a debate—to challenge the empirical validity, both internal and external, of any claim. Accordingly, the greater theoretical and methodological constraints social scientists impose on themselves—that is, the greater the range of alternative explanations, the more logically coherent the favored account, and the more difficult the methodological hurdles—the greater the resulting confidence skeptics should have in any positive result. Even Smith, the contributor to this forum who is highly critical of any form of positivism, concedes that such standards have a plausible claim to “perform the function of disciplining the discipline.” The standards promote pluralist debate while also, as is often forgotten, providing intersubjectively valid reasons for focusing intellectual energy and limiting debate. These discursive constraints distinguish social scientists from artists, philosophers, journalists, historians, and political activists, who, no doubt for good reason, greatly outnumber them in the modern world.

Because the other contributors to this forum take a less optimistic view about the prospects for progressive social science, their discussion is sidetracked into abstract philosophizing, where it remains. To be sure, Smith concedes the potential advantages of a positivist approach, notably its ability to offer a means for structuring a skeptical and pluralistic debate among diverse participants. Kratochwil’s model of legal advocacy, too, is very close to the soft social scientific position to which this author adheres. Even Neumann, starting from seemingly radical premises, circles around to a defense of empirical problem-oriented research. Yet each ultimately recommends that we forego these positivist virtues in favor of theoretical pluralism. It is hardly coincidental, therefore, that they offer no detailed, empirically grounded proposals for theory synthesis of use to concrete researchers. Indeed, their essays barely mention empirical theory or examples drawn from the real world of international politics. For them, the central issue is not theory synthesis *per se* but whether we should encourage serious empirical testing at all. The mere hint that a concern about synthesis might expose reflectivist hypotheses to external challenges—and, dare we say it, disconfirmation—seems enough to provoke outright rejection. Indeed, the other contributors make little effort to discuss theory synthesis, treating this activity instead as just the latest disguise of the positivist threat even though, as noted above, it is quite unclear whether synthesis imposes much constraint on the scope of actual theorizing.

The tendency for the abstract and philosophical to push aside the concrete and empirical is not a coincidental characteristic of this particular forum. It is a near universal tendency among postpositivist writing. As we have moved through the first, second, and third debates—and now seem fated for a fourth—the terms grow ever more abstract. Perhaps the underlying premise is that deeper philosophical understanding or, as Neumann proposes, more attention to the intellectual history of international relations will eventually facilitate a richer empirical understanding of world politics. But the payoff always remains just one more debate away. This

discouraging trend signals that we should be somewhat suspicious of the opposition of critical theorists to theory synthesis.

Underlying Sources of Philosophical Disagreement

Still, it is worth noting precisely why we differ on abstract issues. Smith's essay, which concedes the ideal benefits of positivist discourse yet ultimately discounts them, illustrates the reasons most clearly. The primary implication of what he is saying is that nothing, in particular neither empirical disconfirmation nor the need to combine theories to explain complex real-world events, should be permitted to dampen theoretical pluralism. Smith's belief appears to rest on his view that a positivist philosophy of science biases the debate in favor of certain rationalist theory, a tendency that is most evident in the narrowness of US debate. Thus, we should encourage theoretical pluralism. But there is no necessary link between positivism and rationalism. In part for this very reason, the US debate regarding international relations is exceptionally broad. Indeed, Smith's alternative of greater theoretical pluralism is arbitrary and, ironically, a conservative plea for disciplinary stasis. Let us consider each of these ideas in turn.

First, Smith incorrectly assumes a necessary connection between positivist epistemology and rationalist theory. Positivists, he and others in this forum imply, cannot presume nonrational behavior. Yet there is no reason why the sort of theoretical and empirical claims constructivists and reflectivists advance—in particular, that the interests and values of powerful countries reflexively evolved from prior practice influence world politics more than raw power or material interests—cannot be subjected to positivist evaluation. There are many examples of such investigations in contemporary international relations; indeed, postmodernists and reflectivists have recently been placed on the defensive by the increasing number of constructivists who share their ontology and theory but accept a positivist philosophy of science. Their defense appears to be that positivist debates cannot function well if a full range of theories are not considered.

Second, Smith's view of North American international relations theory as narrowly realist, rationalist, and hegemonic is a caricature; it ignores the diversity of real empirical research occurring on the continent. Consider as an example international monetary cooperation and conflict, an area, given Smith's own worldview, that ought to be as dominated by rational materialists (not to mention US unilateralists and apologists for modern global capitalism!) as any. An examination of prominent monographs published in the past fifteen years by North American political economists on this topic—all positivists, some quantitative, and many engaging in theory synthesis—reveals an exceptional diversity. This research stresses such factors as concentrated economic interests (Jeffrey Frieden and Lawrence Broz), particular economic beliefs stemming from past economic experience and partisan conflict, capital mobility (Barry Eichengreen), geopolitical ideology (Martin Feldstein), path-dependent institutional and ideational legacies of past decisions (Wayne Sandholtz), partisan and institutional characteristics (Beth Simmons), capital mobility (Michael Webb), capital mobility and economic ideology (Kathleen McNamara, Richard Cooper, and Paul Krugman), domestic bureaucratic politics and informational manipulation by transgovernmental financial elites (David Cameron and Amy Verdun), the hegemonic role of the United States (Robert Gilpin, Dorothée Heisenberg, and Joseph Grieco), hegemony and path-dependent international institutions (Robert Keohane), prior macroeconomic convergence and use of exchange-rate pegs as commitment devices (Thomas Oatley and Geoffrey Garrett), general public opinion support (Matthew Gabel), institutionalized capital-labor bargaining systems

(Peter Hall and Robert Franseze), the role of interstate side payments (Peter Lange), the shift to a service-sector economy (Torben Iversen), market constraints on state intervention (Michael Loriaux), policy competition in the financial services industry (Andrew Sobel), structural conflict between strong and weak currency countries (Matthias Kaelberer and Benjamin Cohen), specific national modes of institutionalizing monetary policy (James Walsh and John Goodman), security externalities of monetary power (Jonathan Kirschner), and international monetary instability (Randall Henning). To claim that the US debate is theoretically narrow, let alone demonstrates the limits imposed by a positivist philosophy of science, cannot be substantiated.

Third, even if his criticisms were correct, Smith's position suffers from a third weakness. It proposes no workable alternative except freezing the academic status quo. He offers no nonpositivist criterion for adjudicating competing claims within his own preferred (that is, nonrationalist, nonpositivist) paradigm and epistemology, let alone across paradigms and epistemologies. Instead, he treats diversity as always superior to non-diversity—a sort of theoretical “affirmative action” in which anything goes.

Such a position will not do. We need look no further than Kratochwil's brilliant and devastating critique of “pluralism for its own sake” in this forum to see why. Enforcing theoretical pluralism by fiat is no less arbitrary than enforcing theoretical homogeneity by fiat. The contributors to this forum are quite explicit about the result they seek, namely to protect certain theories from any sustained, let alone fatal, empirical or theoretical challenge. This state of affairs evades the central issue of social scientific methodology: How do we know when, and what do we do if, a theoretical conjecture proves weak or wrong? The discussion does not acknowledge in any systematic way the possibility that a nonrationalist theory might be incorrect, let alone offer intersubjectively neutral guidance about how to address such a situation. Smith advances his proposal in the name of “pluralism,” but its consequence is to privilege the perpetuation of the status quo among (or, indeed, to expand the reach of) both establishment and critical theorists.

This response to Smith may seem abstract. Why not, the reader may ask, just let thousand flowers bloom? What's wrong with pluralism? The reason is that letting flowers bloom can be misleading, because it gives the impression that there is equal empirical support for all conjectures and encourages us to believe that any plausible one is as valid and accurate as any other. In this regard, we need to remember that recent constructivist writings are, in fact, replete with claims that could be subjected to straightforward empirical analysis with no violence to the underlying theories. Few constructivists or postpositivists actually dispense with evidence; they tend, however, to use it more loosely. Indeed, it is sometimes rather easy to disconfirm the resulting claims (see, for example, Diez 1999; Moravcsik 1999). Yet, as long as the claims are insulated from empirical challenge and synthesis, the status quo rules. In the end, then, it is not the mythical American establishment, but critical theorists, who have chosen to play the academic conservatives suspicious of genuine dialogue and the revision of orthodox authorities.

The broader implication is clear. Scholars of international relations should dwell less on the metatheoretical, ontological, and philosophical status of social science, thereby postponing the day when the specific problem of theory synthesis itself is addressed concretely. We should think more about the ways in which theoretical syntheses might help us understand concrete events in world politics. The opportunities and incentives for doing so are increasingly visible among midrange theories of concrete phenomena in the study of world affairs. Let's get on with the empirical research!