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Qualitative & Multi-Method Research

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Letter from the Section President

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This issue of the Qualitative and Multi-Method Research newsletter includes an important announcement from Ron Rogowski, lead editor of the *American Political Science Review*. Because this is likely to be of interest to more than just members of QMMR, we are taking the unusual step of also sending this issue to our colleagues in the Politics and History and International History and Politics sections. Although of course both sections include scholars from a variety of research styles, we believe that Ron's memo, which addresses the underrepresentation of qualitative research in *APSR*, will be relevant to many of their members.

Ron's memorandum acknowledges that *APSR* has not published enough qualitative research, and calls for a fresh start. To break the bad cycle of non-submission-non-publication, Ron asks scholars who use qualitative methods in their research to send *APSR* their best work. In a brief response, I endorse Ron's call, and ask readers to lend their support to Ron's efforts. To help get this initiative off to a good start, the response includes an announcement of an award, to be offered in 2011 through 2014, for the best qualitative submission to (not necessarily publication in) *APSR*.

We hope that, even if Ron's call is not of interest, other aspects of this issue of the newsletter will still attract your attention. This issue includes two symposia that are likely to have a broad appeal. The first is a discussion of Rudy Sil and Peter Katzenstein's analytical eclecticism, with essays by Peter Haas, Andy Bennett, Alice Ba, and T.V. Paul, and with a response by Rudy and Peter. As telegraphed in the title to their book *Beyond Paradigms*, analytical eclecticism provides an alternative to the "schools" approach which has dominated much of North American political science, especially the international relations sub-field. The second symposium consists of essays on causal mechanisms and process tracing. The contribution from Gary Goertz and Jim Mahoney is drawn from their *A Tale of Two Cultures* project, forthcoming from Princeton University Press. David Waldner's essay, "What are Mechanisms and What are They Good For?" draws from his chapter in the *Oxford Handbook of the Philosophy of Science* to offer an account that mechanisms play in causal explanations.

The newsletter also contains the QMMR section's fall announcements, including citations for the awards that were presented at the September 2010 business meeting. I would particularly like to draw readers' attention to the inauguration of the David Collier Mid-Career Achievement Award. This new award was established in David's name to honor his extraordinary contributions—through his research, graduate teaching, and institution-building—as a founder of the qualitative and multi-method research movement in contemporary political science. As noted in more detail inside the newsletter, the first recipients of the award were Lisa Wedeen of the University of Chicago and James Mahoney of Northwestern University.

Let me close this note with a quick plug to our colleagues in International History and Politics, and Politics and History: if you found this issue of the newsletter useful, previous numbers are available at the section's website at http://www.maxwell.syr.edu/moynihan_cqrm.aspx.

Qualitative Methods and the *American Political Science Review*

Getting Qualitative Research Back into the APSR

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Call it a bad equilibrium or a self-reinforcing cycle—*APSR* has clearly been stuck in one for many years when it comes to qualitative research in political science (with "qualitative" understood to include all branches of methodology associated with the qualitative tradition, broadly defined). We, as the current team of Editors, want to break the cycle. We want to see, and have hoped from the beginning to see, a lot more excellent qualitative work in the *Review*. We regret that that has not happened, and we hope that this brief missive can be the beginning of a new and more successful effort.

The kind of self-reinforcing cycle we face is by now too familiar to require much discussion, and (in fairness) is not unique to the *Review*: Scholars in one part of the discipline see less and less that interests them in a given journal, no longer read it regularly, assume their work will not be welcome there, and hence stop submitting. Other journals, and *APSR* with respect to particular fields, have experienced a similar narrowing. But, as regards qualitative work, the *APSR* has reached a far lower nadir: Among the roughly 700 new submissions that we received last year, precisely eight—not eight percent, eight total papers—were self-designated as "qualitative," and in our group recollection not many more involved empirical (as opposed to mostly normative) interpretive research. As we look back, we have received precisely 43 self-designated qualitative submissions, out of a total of 2,249, during our entire term (up to late August of this year) as Editors.

The causes lie in the distant past and are not very relevant to the question of a remedy. And the simple response of our group, and of prior editors, that "we cannot publish what we do not receive," while true, is no longer adequate. We want, and have wanted since the inception of our term as Editors, a conversation that involves *all* of political science, not just particular (if quite large) subsets. We have done our best to achieve that, including teaching each year by one co-editor (usually the Lead Editor) in the annual Institute for Qualitative and Multimethod Research (IQMR). Our presentations there have always included wheedling (not to say begging) for new submissions to *APSR* from that group.

But (if the expression be forgiven in this venue), the numbers do not lie, and clearly none of our efforts so far have been anywhere near adequate, and indeed as disappointing to us as to our qualitative colleagues. So how do we break the cycle that so evidently prevails?

Colin Elman, who has worked with us closely and very helpfully on this issue, has suggested it might do good to (a) send a specific invitation to a broader audience (as we do here); (b) say a bit more about what kinds of qualitative submissions would be of particularly interest to the broad *APSR* audience; and (c) give some general hints about maximizing chances of acceptance.

To get one issue out of the way first: We have no shortage of space in the *Review*, and hence no worry that accepting more qualitative pieces would "crowd out" the kinds of work that are already well represented in our pages. Our page constraint from Cambridge University Press is generous, and we have never come near to running up against it. No matter how warm the welcome extended, it might seem forced or insincere from a family of eight living in a two-bedroom apartment; but what we have is closer to the Biltmore (OK, we exaggerate). There is, in short, plenty of space.

So what kinds qualitative work would be particularly welcome? Above all, original research (exactly as our "Instructions to Contributors" stress), and particularly research that addresses or extends an ongoing conversation in political science. By "original research" we mean exactly that: works that only exhort ("this is how political science *should* be done"—without actually doing it) or that only advocate ("this is the policy the U.S. *should* adopt toward China") or that only survey the existing literature (even if they divide that literature into intriguing new categories) inevitably face a higher barrier, regardless of approach. (As *cognoscenti* will have noted, we have also not been publishing much by way of the "coolest new estimator" in quantitative political science, unless that estimator is put to some use in real and important research.)

By "an ongoing conversation in political science," we mean some subject that political scientists are already actively addressing, in our pages or those of other general-appeal disciplinary journals (or books). To name only a few random examples, about which qualitative scholars manifestly have a lot to say: processes of state-building and legitimacy (or lack thereof), violence (including ethnic and sexual violence), the out-break of wars (or their avoidance or settlement), authoritarianism or post-authoritarianism. It is always possible, of course, to start a new conversation, but an otherwise excellent piece that lacks any evident connection with current discussions in political science, or that addresses chiefly an audience in some other field of social science, will inevitably face a higher hurdle.

So, assuming the topic is interesting and the work is excellent, what else can an author do to maximize chances of acceptance? Three hints, which are quite generally applicable and have been addressed to virtually all our audiences, should suffice: (a) Write lucidly, waste no words, and avoid jargon (or, at least, explain arcane terms, remembering that ours is a general audience of political scientists). (b) Develop a clear argument and exposition, including usually a few paragraphs of "road map" at the beginning. (Again, people in your particular field may see immediately where you are going and why it matters, but ideally *APSR* will open your work to a broader audience.) (c) Never hesitate to recommend qualified referees (so long as these have no conflict of interest) or to warn us against ones that might be biased, either for or against you or your work. These are general, and practical, suggestions, but they bear repeating here.

We are confident of our ability to find appropriate, unbiased referees for almost every piece that comes to us. Our team of co-editors includes, and has included from the beginning, scholars with diverse scholarly orientations—ones who have done interpretive scholarship, historical research, extensive fieldwork in other cultures and languages, or all of the above—and all are catholic in their tastes. Expertise that we lack is almost always available in our larger Editorial Board. As we continue to rotate and widen the group of co-editors, we endeavor to maintain and expand that breadth of capacity and receptivity. We have also become much better at turnaround times, so normally if we are going to reject a piece we do it quickly, and usually with advice to the author(s) about where the paper might more appropriately be placed. Online submission makes the process quick and easy. So, except for the inevitable wounds to the ego (which all of us have experienced), the price to be paid for submitting to the *Review* is small and the reward—both to you and to us—potentially quite large.

To repeat: The Editors very much want the *Review* to reflect the best work being done in all areas of political science. We want it to be the outlet to which all political scientists send their best work. We strongly believe that our review process has no bias against qualitative work, and would hasten to remedy any such bias if one were demonstrated. But nothing can change until we get more qualitative submissions. That is what has to happen, and what I am pushing here (with the help of others) to make happen. As all of us have said to numerous audiences of political scientists, national and regional: Please send us your best work!

Give Pieces a Chance: Submitting Manuscripts to the APSR

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The section is very grateful to Ron Rogowski for encouraging qualitative submissions to *APSR*.[1] We enthusiastically endorse his efforts, and hope that many more manuscripts will be sent to the journal. For far too long, *APSR* has been trapped in a vicious cycle, with scholars loath to submit until their perceptions of the journal changed, but with more qualitative material first needing to appear before that conversion could occur. We believe that the best chance to make progress is to give the journal a chance to climb out of the deep hole it has dug itself into. What can the section, individual scholars, and the *APSR* editors do to help bring this about?

For its part, the section is committed to taking three actions. First, we offer our wholehearted support to Ron's initiative. We encourage our members to send their material to *APSR*, and to share Ron's call with other scholars doing qualitative research.

Second, to help get this initiative off to a good start, the section is inaugurating an annual award for the best manuscript submitted to the journal which uses qualitative methods. To broaden its impact, we will seek to manage the award in cooperation with other APSA sections. The award will be offered in 2011 through 2014, and the winner in each year will receive $2,000. To be eligible, the manuscript need only be submitted to (not necessarily published in) the journal. More details on the mechanics will follow.

Third, we commit to return to the question of qualitative submissions to the journal on a regular basis, and to publicize the results of Ron's initiative. We will circulate data on submissions and acceptance rates, as well as asking scholars who submitted to *APSR* to share their experiences.

As for individual scholars, we know that you are writing terrific qualitative articles and sending them to other outstanding outlets like *Comparative Politics*, *World Politics*, *Comparative Political Studies*, *International Security*, and *International Organization*. We would ask everyone who is about to send a manuscript to one of the top journals to send it to *APSR* first.

We realize many readers will have misgivings. Some will believe that the current situation at *APSR* is the product of a conscious choice to exclude qualitative work. According to this view, the journal is happy with the status quo, and believes that it already receives the best work in the discipline. Complaints about categories of work not published are thus simply the price to be paid for holding the line against "non-science." Skeptics who hold this position view periodic pronouncements that "*APSR* is your journal, too" as self-serving and insincere, and made only to ensure that the Association continues subsidizing the publication of a narrow slice of research.

Other readers may not believe *APSR* deliberately excludes qualitative work, but feel that the editors have epistemological commitments which bring about the same result by narrowing their decisions. Some of the material missing from the journal comes from research traditions where (for example) formal theories are not automatically held to be superior to ordinary language arguments; where scholars do not believe that particular knowledge is only ever right by accident; where cases are chosen by decision rules other than random selection; and where scholars who study a few cases can nevertheless be political scientists and not historians. The suspicion is that the *APSR* editors are like engineers on a train, proudly announcing that they will stop at any worthwhile station, but failing to recognize that the pre-laid tracks will make sure that some stations will never be visited.

APSR editors will very likely resent both these descriptions: The first suggests they are pusillanimous, the second oblivious. But rather than have an argument about whether these are misperceptions, we want to ask both individual authors and the *APSR* editors to proceed as if the other side's argument had some merit.

Authors, let's assume that misgivings are misplaced, and that *APSR* treats qualitative work no differently than any other top journal to which such material is regularly submitted. Send to *APSR* with the assumption that it is like any other journal which has similar submission mechanisms, double-blind review processes, standing in the discipline, and so on. The modal response will be rejection, but that is true for every top journal. The key question is how *APSR* responds to a body of qualitative submissions, and we will only know that if they get them.

As for the *APSR* editors, Ron's memo is a terrific step in repairing the relationship between the journal and qualitative research communities. That being said, however, we hope that the editors will follow up with some of the measures alluded to in his memo. In particular, we think it would be very helpful if the current editorial team could include more scholars who are unambiguously rooted in qualitative epistemologies, and who are knowledgeable about potential reviewers who are similarly qualified to make well-informed assessments. This would send a strong signal that *APSR* values and welcomes diverse research styles, and that it intends submissions to be appropriately evaluated. Apart from the signal such appointments would send, *APSR* would be strengthened by adding editors who can minimize the likelihood of having reviewers raise issues such as selection on the dependent variable, the impossibility of gaining inferential leverage from process tracing, and so on.

A fresh start needs both sides to engage in some reflection. It asks the authors to put away their pitchforks, and to instead give the *APSR* a chance to demonstrate their good faith. It asks the editors to acknowledge that they have a responsibility to help sustain the momentum towards a broader and more inclusive journal.

As things stand now, at least four outcomes are possible: (1) the number of qualitative submissions will not increase; (2) the number of submissions will increase, but little of the work will be published; (3) a proportion of the submissions will be published, but absent continuing attention from third parties, the journal will then slip back into the current status quo; and lastly (4) a new, virtuous, and lasting cycle will be established, with the appearance of qualitative research generating further submissions.

There is one thing we can all agree on. Of these possible outcomes, only the fourth is supportable in the long run. If *APSR* cannot get into balance with the Association, it is likely that critics will insist on the journal finding its own market by separating subscription from membership. With an opt-in mechanism, *APSR* would then only be bought by the research communities which currently submit to and read the journal. This would be unfortunate, because the Association would lose a major asset. It is to Ron Rogowski's credit that he is making such great efforts to reach out to scholars who have fallen out of the habit of submitting to the *APSR*. We hope he succeeds.

Note

[1] "Qualitative" is used here to include the full range of approaches in the qualitative tradition, broadly defined.

Symposium: Beyond Paradigms and Research Programs?

Introduction

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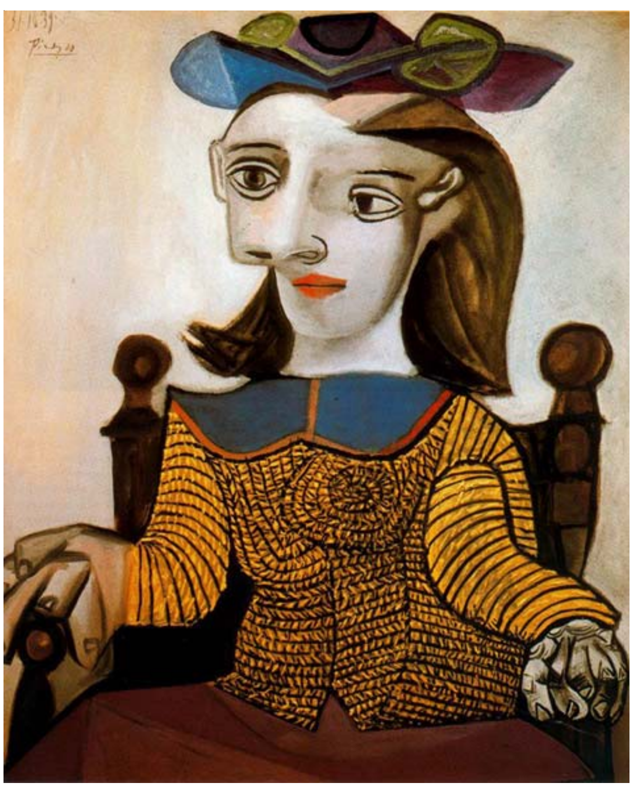
This symposium on Rudy Sil's and Peter Katzenstein's (Sil/K) analytic eclecticism (Sil and Katzenstein 2010a, 2010b) in the *Qualitative & Multi-Method Research Newsletter* comes from the roundtable on analytic eclecticism at the 2011 Annual APSA Conference.

I was at the Brohan Museum in Berlin, admiring Pablo Picasso's 1939 Der Gulbe Pullover (Lady in a Yellow Sweater) when I received Rudy's email request to chair this session, and to participate. My appreciation of the overall project is thus framed by this context.

Analytic eclecticism is a work of art in the sense that it helps us see the world in new ways and to appreciate multiple perspectives, as is the goal of all good art. Most contemporary ways of understanding world politics provide only monochromatic or narrow visions of a rich and complex reality, and thus oversimplify a complex subject of study. A dramatic change in perspective, such as analytic eclecticism, provides a fruitful entrée for understanding contemporary world politics under conditions of globalization and complexity. Art history provides a convenient metaphor for thinking about the reception and diffusion of such path-breaking works of art. Consider Picasso's Der Gulbe Pullover.



Picasso's modernist eye changes our own appreciation of perspective and representation, as does the broader notion of analytic eclecticism. Thus it leads us as viewers to see the world in a different light. But radical new artistic movements require institutional change if they are to prosper. In order for the Impressionists, and later the German modernists, to gain a niche in art circles they not only had to produce their work, they had to create a market niche through publicizing and promoting it, finding dealers and galleries willing to represent them, creating juried exhibits that would present them—since existing juried exhibits were conservative and would reject new styles—and soliciting patrons.

Make no mistake. Sil/K's manifesto on analytic eclecticism is a work of art. Our essays in this symposium treat two aspects of the artistic rendering of Sil/K. The first two, by Andrew Bennett and Peter M. Haas, discuss the broader themes and context of Sil/K's analytic eclecticism. Bennett focuses on causal inference and social mechanisms and various schools of thought in world politics whose efforts can be advanced through the application of analytic eclecticism. Haas discusses analytic eclecticism in a metatheoretical framework, looking at its philosophy and sociology of science foundations, and its utility for advancing understanding about international environmental politics. The essays by Ba and Paul discuss analytic eclecticism from the perspective of writers who were singled out by Sil/K for their exemplary work, and tell revealing individual stories of two authors who may not have regarded themselves as necessarily working within the tradition of analytic eclecticism, and of the challenges and rewards of analytic eclecticism for their own research projects.

References

Sil, Rudra and Peter J. Katzenstein. 2010a. "Analytic Eclecticism in the Study of World Politics." *Perspectives on Politics* 8:2 (June), 411–431.

Sil, Rudra and Peter J. Kazenstein. 2010b. *Beyond Paradigms: Analytic Eclecticism in the Study of World Politics*. London: Palgrave Macmillan.

From Analytic Eclecticism to Structured Pluralism

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Beyond Paradigms provides a clear vision of how political science can advance by de-emphasizing paradigmatic debates and using diverse theories to develop policy-relevant research. The book makes two vital contributions. First, it draws on the philosophy of social science to diagnose the problems and costs associated with organizing political science around self-styled Kuhnian paradigms or Lakatosian research programs, such as the "isms" in the international relations subfield: (neo)realism, (neo)liberalism, constructivism. Second, it demonstrates that an alternative way of doing business already exists, presenting numerous examples of analytically eclectic research that builds upon but goes beyond paradigmatic frameworks to produce theoretically interesting and policy-relevant results. Although the book is focused on the international relations subfield, the authors rightly note that these contributions can promote closer connections to other subfields in political science and to the social sciences more generally, including psychology, sociology, history, and economics (2010: 36).

These are major achievements that deserve the attention of all political scientists. In this brief review I first put these achievements in the context of developments in political science and the philosophy of social science. I then suggest ways in which *Beyond Paradigms* could be even clearer on the issues of theoretical cumulation, standards of progress, and ways to locate eclectic research within the field. In particular, I argue that creating a taxonomy of the theories about social mechanisms that constitute the building blocks of analytically eclectic research can provide the discursive and pedagogical benefits that paradigms have conveyed without incurring the costs associated with reifying these paradigms.

Paradigms, Research Programs, and Eclecticism

Beyond Paradigms persuasively argues that over-emphasizing paradigms as the central organizing principal of research in international relations has inhibited progressive and policy-relevant research and diverted attention and energy into unproductive scholastic debates. Many political scientists have long shared the sense that framing our arguments and organizing our syllabi in terms of grand schools of thought, including Kuhnian paradigms, Lakatosian "research programs," or other such large-scale "isms," does not actually represent the way most of us think about politics, political science, or our research. Indeed, Albert Hirschman warned in 1970 of the danger that an excessive focus on building paradigms could become a "hindrance to understanding" (Hirschman 1970). Hirschman's warning went largely unheeded in the subfield of international relations, where the reification of the "isms" into grand schools of thought has been especially stark. This approach has become even more exaggerated as scholars have introduced a host of name-brand versions of realism and liberalism—classical, neoclassical, defensive, and offensive realism, commercial and institutional liberalism, etc.—as if it were possible to achieve simple but widely applicable "covering law" style generalizations if only we could get the right theoretical assumptions within the general framework of one paradigm or another. Many scholars sense that this approach has become unproductive, but lacking any clear alternative for framing the field in ways that transcend the confines of the "isms" without giving up the convenient terms of reference that the isms have provided, political scientists have plodded along with the "isms" in our research and teaching.

This state of affairs is partly due to the fact that most political scientists are woefully out of date in their reading of the philosophy of social science. Most contemporary political scientists have not read much philosophy beyond the 1960s and 1970s, when first Thomas Kuhn and later Imre Lakatos strongly influenced political scientists' views of their field. Kuhn focused his attention on the history of the physical sciences and used the term "paradigms" to argue that "science" consists of large-scale views of the world with many underlying and interlocking assumptions, and that new paradigms occasionally replace old ones through "scientific revolutions" that focus on resolving the anomalies of existing views. Kuhn argued that the outcome of these revolutions is decided as much by the sociological process of achieving support among scientists as by any independent standards of proof or progress (Kuhn 1962).

Lakatos later critiqued Kuhn for having an unacceptably subjective approach to scientific progress, and he attempted to provide methodological rather than sociological standards for assessing scientific progress. Lakatos argued that theories should not be falsified at the first sign of anomalies, but should be thought of in the context of larger "research programs" with unfalsifiable "hard core" assumptions and falsifiable "outer belt" assumptions. In this view, research programs might eventually be able to resolve anomalies in progressive ways that uncovered "novel facts," or they might ultimately prove "degenerative" through their inability to do anything more than provide ad hoc explanations of individual anomalies (Lakatos 1970). Still, Lakatos retained Kuhn's assumption that the focal activity of science was constructing, defending, testing, refining, and sometimes shifting sharply between the large-scale webs of interconnected assumptions that he termed "research programs" to distinguish his understanding of science from Kuhn's emphasis on paradigms.

International relations scholars have taken Lakatos quite literally and set about trying to specify the "hard core" and "outer belt" assumptions of each of the leading paradigms (see, for example, Hopf 1998). This reification of the isms contributed to a "cookie cutter" style of journal articles that was particularly prominent in the 1990s. Each scholar would take a substantive problem or a puzzle, begin their article with "Neorealism says this about my problem, neoliberalism says this, constructivism says this," and then corral evidence to dethrone two of the isms and enshrine the third as the one true faith.

The limitations of this approach are now quite clear. As Sil and Katzenstein convincingly argue, the isms are not in fact mutually exclusive or incompatible with one another, and no single paradigm can provide the kind of comprehensive and policy-relevant research that can be achieved by combining theoretical variables from different research traditions. Indeed, even the scholars most prominently associated with the leading international relations schools of thought— the "neorealist" Kenneth Waltz, the "neoliberal" Robert Keohane, and the "constructivist" Alexander Wendt—have readily drawn on alternative traditions in their work. Keohane has discussed sociological as well as transactions costs approaches to institutions (Keohane 1988), Wendt has pointed to the importance of material power and the centrality of states in the international system as currently (but not timelessly) constituted (Wendt 1995), and as Sil and Katzenstein note (2010: 38), Waltz's earlier work emphasizes the importance of combining different images or levels of analysis to understand world politics. Although these scholars have not wedded themselves as exclusively to one ism or another as tightly as is commonly thought, their identification as leading and successful innovators in their respective schools of thought led many of their followers to focus their research more exclusively within the borders of a single paradigm.

Against this backdrop, Sil and Katzenstein make the case that an alternative approach has continued a quiet but productive existence obscured by the attention devoted to the isms. This approach, which they term analytic eclecticism, does not reject the insights generated within paradigmatic schools of thought. Instead, drawing on more recent developments in the philosophy of science such as scientific realism, it shifts the explanatory focus from large-scale paradigms to the narrower theories about causal mechanisms that have been developed within the context of paradigms. Analytic eclecticism emphasizes using combinations of such theorized mechanisms from different paradigms to understand policy-relevant phenomena. Sil and Katzenstein devote three chapters to analyses of over a dozen excellent examples of analytically eclectic work in the areas of global security, political economy, and regional as well as global governance.

The Challenges of Cumulation and Progress in Research

Beyond Paradigms makes an enormous contribution by properly diagnosing a central problem in the field, identifying the solution, and demonstrating that the solution has already proven viable in extant research. At the same time, there are important costs associated with moving beyond paradigms. For all its faults as an organizing principle in the field, the reliance on paradigms provided a common vocabulary and a sense that progress was being made (though claims of progress have often been contested, as Sil and Katzenstein point out (2010: 217)). Paradigms have gathered groups of theories about causal mechanisms together in ways that have proved memorable and easy to convey. The challenge for an alternative approach is to provide a similarly useful framework for discourse without creating new constraints on theorizing and research.

Sil and Katzenstein are very cognizant of this problem. They note the contributions of paradigms in providing a basis for easy discourse (2010: 35), which is one reason that they do not advocate entirely breaking from the isms. They also state explicitly that analytic eclecticism is not a license to act as if "anything goes" in research (2010: 16). Still, *Beyond Paradigms* could be even clearer on how analytically eclectic research can achieve cumulative progress, how it can be located within the field, and how it can be communicated to colleagues and students. For example, by explicitly eschewing a "synthetic guide to eclectic research" (2010: 3) out of concern that this would be yet another hindrance to understanding, they miss an opportunity to gain more converts to their cause and to provide clearer advice for both research and pedagogy. The term "eclecticism" compounds this problem, as it can be misread as suggesting a non-cumulative approach with procedures and standards that are difficult to codify or communicate. Merriam-Webster's online dictionary defines "eclectic" as "selecting what appears to be best in various doctrines, methods, or styles," which is clearly what Sil and Katzenstein intend, but it also includes as synonyms the words "indiscriminate," "kitchen-sink," "motley," "ragtag," "patchwork," and "promiscuous." Perhaps the term "structured pluralism," which Sil and Katzenstein reference from Dow (2004), better captures the kind of research the authors want to encourage.

Sil and Katzenstein add to the problem by stating that analytically eclectic research "may not constitute progress in the sense of... continuous refinements in a given theory" (2010: 47). Even though the qualifying word "may" suggests that cumulation is also possible, it would be better to make the justifiable positive case that approaches combining theories on diverse mechanisms can indeed lead to cumulative theoretical gains. Such progress has already been achieved through eclectic research on the inter-democratic peace, democratization, alliance burden-sharing, and other problem-driven topics.

One line of argument that would help here would be a clearer explication of what cumulation of eclectic research would look like. The authors reference the "combinatorial logic" (2010: 18) of bringing theories about diverse mechanisms into the same explanatory framework, and they also invoke Robert Merton's notion of "middle range theories" (2010: 22, 37, 209), but it remains unclear how such combinatorial logic works or what would constitute cumulation in middle range theories. Two useful approaches that deserve mention in this context are Charles Ragin's discussion of fuzzy set theories (Ragin 2000), and my own analysis with Alexander George of typological theorizing (George and Bennett 2005: 233–262). Both allow for combinatorial theorizing that combines many concepts and mechanisms, treats cases as configurations of variables or mechanisms, and allows for cumulation as new concepts, variables, theories, or cases are added to previous research findings.

The ambiguity on the prospects for and paths toward cumulation in *Beyond Paradigms* is intensified by the authors' focus on Larry Laudan's writings on the philosophy of science. As Sil and Katzenstein argue, Laudan's concept of "research traditions," which allows for researchers drawing on different theoretical frameworks that coexist for long periods of time, better fits actual practices in political science than Kuhnian paradigms or Lakatosian research programs. Sil and Katzenstein rightly acknowledge, however, that "unlike Kuhn and Lakatos, Laudan offers no uniform model of how to track the progress or decline of successive or competing approaches" (2010: 7). The problem is even deeper, as Laudan has only very vague advice on how to judge not only among large-scale research traditions, but among competing explanations of historical cases or competing theories on individual causal mechanisms. Sil and Katzenstein also offer only very general advice on how to judge research on its "quality of evidence, falsifiability, and generalizability" (2010: 216).

In this context, I find that Lakatos's concept of "novel facts" remains useful even though his distinction between the hard core and outer belt of research programs does not hold up (Bennett 2003). As later elaborated by other scholars, "novel facts" can be judged by the standards of "use novelty" and "background theory novelty" (Elman and Elman 2003). Evidence has use novelty and can be used to provide an independent test of a theory if this evidence was not used to generate the theory. This provides a check on the well-known psychological tendency toward confirmation bias. A hypothesis has background theory novelty if it proposes to explain a phenomenon that is unexplained by or anomalous for other theories. Following the logic of Bayesian updating, background theory novelty increases our confidence in the likely truth of a theory. Scholars need not agree on paradigms, research traditions, or delineations of hard cores and outer belts to be able to agree that specified evidence would have use novelty or background theory novelty relative to specific theories or explanations.

A Taxonomy of Theories about Causal Mechanisms

The question remains as to whether it is possible to devise a synthetic guide to research that provides a framework for communication and draws on diverse theories without becoming so structured that it confines the pluralism Sil and Katzenstein seek. From my own work on causal mechanisms, and partly inspired by Katzenstein's and Sil's earlier work on analytic eclecticism, I have been working to develop such a framework. My current working draft of this effort, presented below in Table 1, can be construed as a more detailed version of Katzenstein's and Sil's diagram (2010: 21) illustrating how eclectic scholarship combines structural, agent-centered, material, and ideational theories.

The top three rows of Table 1 include the three leading paradigms of international relations theory, but by foregrounding the kinds of mechanisms upon which they draw and backgrounding paradigmatic labels, the table seeks to avoid the problem of reifying paradigms into mutually exclusive schools of thought. Power, legitimacy, and functional efficiency form the basis of sociological as well as political science approaches to institutions (Mahoney 2000). The column labels across the top of Table 1 provide the different possible combinations of moves between and among agents and structures. The intersections of these four columns and the top three rows provide examples of theories about the relevant underlying mechanisms. These examples are only illustrative, as there are many different theories about social mechanisms that could be located within the relevant categories, including collective action theory, principle-agent theory, theories about cognitive

Table 1: A Taxonomy of Theories on Social Mechanisms

	Agent to Agent	Structure to Agent	Agent to Structure	Structure to Structure
Legitimacy: Constructivism, Logic of Appropriateness	Emulation Socialization	Culture as Enabler and Constraint	Norms, Entrepreneurs, Framing	Unintended Evolution of Social System
Power: (Neo)Realism Logic of Consequences	Hegemonic Socialization	Resources as Enabler and Constraint	Institutional Reform, Revolution	Power Transitions
Functional Efficiency: Neoliberalism Logic of Consequences	Emulation	Evolutionary Selection	Functional Competition, Innovation	Macroeconomic Sequences, Path Dependency, Adverse Selection, Moral Hazard
Levels of Analysis:	Individual	Systems, States, Orgn's, Small Groups Enable and Constrain Individuals	Individual Affects Higher Levels of Analysis	System-to-System Dynamics
Fields of Study:	Psychology	Economics, Sociology, Social Psychology	Economics, Sociology, Social Psychology	Economics, Evolutionary Biology

heuristics and biases, and so on. The fourth row of the table indicates the levels of analysis associated with each combination of agents and structures. Finally, the bottom row of the table serves the important function of providing links to other social sciences that constitute useful sources for cross-fertilization of theoretical ideas.

Table 1 is a taxonomy of theories rather than a theory of everything. It is meant to serve as a useful platform for locating research within the field and a basic checklist to assess whether a researcher has omitted important categories of theorized causal mechanisms. It is also intended to provide a sense of order and finiteness among the diverse theories upon which eclectic research can draw without unduly limiting these theories. Like recent efforts noted by Sil and Katzenstein (2010: 220) that bring together historical, rational choice/economic, sociological, and other institutionalisms (Mahoney 2000), it provides a basis for general questions that are almost always relevant to political research: *For which actors does an institution or practice have or lack legitimacy? Which powerful actors had an interest in creating this institution, and which powerful actors see it as in their interests to sustain, change, or abolish it? Through what combination of legitimacy, power, and functional efficiency did this institution win out over alternatives when it was first formed, and what combination of factors sustains it? To what challenges might it be vulnerable, by what agents or structures with what combinations of legitimacy, power, and efficiency?* Although very general, such questions, and the taxonomy that undergirds them, can provide reassurance to those reluctant to move beyond paradigms that research drawing on diverse mechanisms can be orderly, cumulative, and easily communicated to colleagues and students.

A Revitalized Prose for the Study of Politics

Most of the explanatory power that political science has achieved resides in the theories about causal mechanisms that scholars have developed in the context of paradigms, not in paradigms themselves. Like Moliere's bourgeois gentleman, political scientists have been writing in the prose of theories on causal mechanisms without realizing it. *Beyond Paradigms* exposes the limitations of thinking exclusively in terms of paradigms, but it does not discard the useful theories that paradigmatic research has generated. By grounding political science more firmly in the contemporary philosophy of social science, the authors direct attention and energy away from the inward-looking and excessive focus on inter- and intra-paradigmatic debates that has marked the field for the past several decades and toward the use of diverse theories to understand political phenomena relevant to policymakers and citizens. A new grammar for the study of politics has finally come into view.

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Practicing Analytic Eclecticism

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"The central problem of our age is how to act decisively in the absence of certainty." —Bertrand Russell

"We can't all be intellectuals or there wouldn't be anything to eat." —Cuban farmer quoted on NPR 9/21/10

As Bertrand Russell indicates, the challenge of our time is to deal with problems characterized by uncertainty. The Cuban farmer's quip provides a pragmatic corollary to Russell's observation, or injunction. Grasping this challenge entails reorienting how we study world politics, as well as broader attention to the ways in which collective understanding is organized. Moreover, as Sil and Katzenstein (Sil/K) hope, such knowledge may actually be transmitted to decision makers to improve our plight. In short, they seem to be suggesting that we can better understand the dynamics of politics by talking about them in a more focused way.

Following the cease-fire in the most recent war of the great debates declared in the 1998 50th anniversary issue of *IO*, we have been waiting for the next direction for the field. Sil/K's effort seeks to provide the guidelines for developing policy-relevant and analytically warranted understandings about world politics. Their analytic eclecticism (Sil and Katzenstein 2010a; 2010b) provides a framework for addressing what they regard as the limits of current world politics: a growing sense of irrelevance from the perspective of policy consumers, a tiresome battle over epistemological and ontological ineffables, and a growing gap between the abstract findings of much research and the perceived needs of both policy makers and academics about how to act decisively in the absence of certainty.

These laments are not new, but are surely worthy of being repeated. The gurus to whom the book is dedicated recognized them. Hoffman has addressed these concerns more directly than did Deutsch or Haas. Sil/K fully recognize the antecedents for their position, quoting approvingly from such academic luminaries as Albert Hirschman and Charles Lindlom (especially Hirschman 1970).

Analytic eclecticism is a bridge-building effort. Sil/K aim to find common ground amongst competing research approaches—which they call research traditions after Larry Laudan— and to better offer those results to decision makers. If not solving the paradox identified by Kratochwil and Ruggie (1986) about how mainstream approaches to IR have incompatible ontologies and epistemologies, at least analytic eclecticism points us in a direction to deal with the paradox. Sil/K ask for a relaxed understanding of causal warrants in order to move ahead on developing useful knowledge based on clear findings about the domains under which specified causal mechanisms operate. For those of us who move in hermeneutic circles, this offers a refreshing opportunity for developing meaningful research.

In the following sections I reflect more broadly on the enterprise: I discuss the implicit social epistemology of the project, their philosophy of science underpinnings, and the sociology of science associated with analytic eclecticism, and then conclude with an application of analytic eclecticism to international environmental politics (IEP), and some suggestions about advancing the analytic eclecticism project in the future.

Analytic Eclecticism and the Philosophy of Science

First, their philosophy of *social* science. Analytic eclecticism quite clearly rests on a philosophy of social science, rather than philosophy of science more broadly. The study of world politics is a *social* activity in a triple sense: social activities are being studied, the study is a social activity, and the effects of the study are, at least Sil/K hope, likely to have social consequences. As such, they fit with other constructivist approaches to world politics, as is shown in the table below (for similar efforts see Jackson 2010: 37; Wendt and Friedheim 1995: 693; and for a similar taxonomy applied to fact/value distinctions see Haas 1990: 223).

While surrounded by stalwart companions, Sil/K's own philosophical orientation is not fully revealed. By looking at their previous publications and the school of thought (used advisedly) associated with the gurus to whom the book is dedicated and from whom they borrow implicit support, one would presume at least a minimal reliance on reproducible results, a consensus theory of truth (in which confirmed claims are warranted through social discussion and reproduction), and a hope for cumulative knowledge.

Sil/K celebrate overdetermination, arguing that most interesting social phenomena worthy of study have multiple interacting mechanisms. Such causal complexity cannot be understood in reductionist terms, as observed outcomes are generally emergent properties resulting from the interplay of various causal forces. Thus, all of you who have rejected studies because of a high Durbin Watson should go back and look more closely at those variables with heteroskedasticity.

Table 1: Foundations for Social Inquiry

Relationship between Researcher and Researched World (Subject/Object)	**Social Approach to Formulating Confident Claims about the World (Epistemology)**		
		Correspondence Theory of Truth	Consensus Theory of Truth
	Independent	King, Keohane, and Verba (1994)	Popper (1972); Ragin (1987); Goertz and Starr (2003)
	Socially Constructed (Analytically Separable)		Archer (1988); Adler (1997); Ruggie (1998a, 1998b); Haas (2003); Brady and Collier (2004); George and Bennett (2005); Haas and Haas (2009); Sil and Katzenstein (2010a, 2010b)
	Socially Constrained (Codetermined)	Marxism, World Systems Theory, Bhaskar (1986) Wendt (1999)	Foucault (1972); Feyeraband (1978); Shapiro (1984); Dryzek and Niemeyer (2008)

While much of world politics may well be contingent, it should be possible to develop broader understandings about the conditions (temporal, spatial, functional, and in terms of the distinctive nature of individual units) under which causal mechanisms (or social forces) are likely to obtain.

Sil/K also try to carve a third way between the relativism of Kuhn (and Feyeraband) and the stringency of Lakatos, by reintroducing Larry Laudan. Laudan's benefits are that he focuses on research traditions, which we can also think of as schools of thought or communities of practice. Knowledge cumulation is a social activity. In *Science and Relativism,* Laudan (1990) presents a Socratic dialogue between a pragmatist, a relativist, a realist, and a positivist. While a splendid *tour de force* for presenting contending views, no one wins. All we have are competing perspectives, and we should just accept it. Recognizing the limits of each approach is valuable, but he stops at letting them speak for themselves, without trying to move to the next step. If we think of such dialogic approaches to knowledge cumulation as a strong technique—as we all do in oral exams associated with comprehensive exams and in job hires, where at the very least we hope for three recursive steps in considering possible arguments—Laudan only provides one step.

Borrowing from Laudan is a mixed blessing. While it is always nice to try to find someone who lies between a set of dichotomous positions in order to advance an argument, I'm not sure Laudan is the best candidate. The shortcomings are that it is difficult to rigorously define a research tradition, and that Laudan explicitly rejects the prospect of cumulative knowledge: "I propose a definition of scientific progress which does not demand cumulative development"(Laudan 1977: 6).

There are alternatives to Laudan that accept that social construction of research traditions, and yet cling to the prospect of cumulative knowledge through engaged debate within scientific traditions, or communities, even if they have less to say about communication between communities. Alternatives to Laudan include the late Popper (1972), Toulmin (1972: appendix), Donald Campbell (1988), Alker (1996), Rorty (1998), and the pragmatists (Bauer and Brighi 2009; Friedrichs and Kratochwil 2009; Haas and Haas 2009)

Analytic eclecticism has many attractive applications to studying concrete problems and to understanding causal mechanisms and their interplay (McAdam et al. 2008). In essence Sil/K ask us to choose mechanisms and then study them and see what difference they make, and when. A number of causal mechanisms have been widely appreciated, and efforts are just beginning to study their interplay and the domains under which each is likely to occur. Rather than focusing on variables, this moves our attention to social forces. Then we can get back to fundamental questions, if we feel like it.

We are being asked to suspend our disbelief for a while, pending the return to see the domains in which mechanisms operate, and the generalizability of findings across domains. I think that this is a justifiable move. But I have two misgivings. The first is how to identify the useful questions for midlevel study? The second is what happens after this period of pursuing analytic eclecticism? Do we return to broader inductive efforts to build broader insights—cumulative knowledge—about the various nuances that have been revealed through analytic eclecticism research about the contingencies of analytic eclecticism studies?

Still, we are left with various unresolved issues. How to identify a good puzzle? How do we know if the puzzle answers cumulate? To what extent are findings generalizable? Is it worth sacrificing the pursuit of ambitious grand truths to yield small insights?

Only, I think, if we can build back onto them in the future. Such a move to building midlevel useful theoretical findings rests on the eventual move to cumulate individual works into a broader picture. What do we do in 5–15 years when we return to this crop of research to assess the findings?

My concern is that the current focus on methodologically resolvable questions runs the risk of losing the opportunity to have people who will be able to rebuild the edifice of social study after we have followed the path of analytic eclecticism to develop an assemblage—call it a data set—of mid-level findings that can be reorganized, since the findings won't speak for themselves. Do we run the risk of encouraging a generation to focus on micro-level research at the expense of more macro themes (Tilly 1984), which at the very least provide the parameters for a more nuanced effort to repackage the collective findings?

Analytic Eclecticism and the Sociology of Knowledge

This brings us to the question of sociology of knowledge: how is analytic eclecticism likely to be conducted in practice, given the field of world politics as we know it? The question is how to encourage young scholars to take up the challenge of analytic eclecticism, in a job environment that discourages excessive risk taking?

Sil/K rely on exemplars from single-author works, and yet this seems to be at odds with the growing frequency of jointly authored works. There is a growing tendency in major journals to publish jointly authored works. Major university presses feature edited volumes that combine individual chapters from different perspectives. Well-received edited volumes and special issues of *IO* have followed a typical pattern with an adversarial piece written by the loyal opposition or devil's advocate (Krasner 1983; Katzenstein 1996; Haas 1997; Koremenos, Lipson et al. 2001).

While it isn't clear that any of these models are superior in terms of their ability to persuade—they appear somewhat like Laudan's stylized debate between incommensurate viewpoints —my point is that the challenge is to imagine analytic eclecticism work that doesn't rely entirely on individually authored works.

Making the world safe for analytic eclecticism practitioners, especially those at the beginning of their careers for whom such research choices entail taking more time and also possibly stepping outside the mainstream, seems to require at least three additional professional efforts:

(1) Tolerance and support in major departments;

(2) New journals, or tolerance by journals to alternative methods and multivariate findings;
(3) Institutional funding.

Tolerance in the major Ph.D. granting departments for analytic eclecticism is necessary. Students need to be encouraged to consider multiple causal mechanisms in their graduate work in seminars and in their doctoral research. Serious attention to complete explanations of alternative mechanisms—for instance how social construction can lead to learning and transformation, rather than just new focal points—requires efforts to get out of the classical reliance on the paradigm with which professors are comfortable, and to encourage a fair treatment *and application* of alternative approaches.

Journals should also be more open to analytic eclecticism work, and to smaller-n, comparative and qualitative methods. This entails resocializing the editorial boards of existing journals as well as considering the creation of new journals—just as *Journal of Theoretical Politics* was created to publish material on rational choice and game theory, *International Theory* for work spanning normative and positive IR theory, *Review of International Organizations* for political economy studies of IO, and *Global Environmental Politics* for a wide array of studies of international environmental politics.

Finally, no new research program (or tradition) is complete without institutionalized funding resources. NSF, are you listening?

IEP and Analytic Eclecticism

International environmental politics (IEP) is an empirical domain neglected by Sil/K that also reflects improved insights and useful knowledge derived from the application of an analytic eclecticism type approach. Most of the work that has contributed to advances in IEP, though, are not single-author pieces at all, but are edited volumes and jointly authored monographs.

One area of IEP that seems to have progressed following analytic eclecticism type dictates has to do with the study of international law and regimes. The study of IEP has progressed from dichotomously opposed paradigmatically informed positions to more refined ideas about the causal mechanisms associated with various patterns of environmental regimes. The transformational school of international environmental law argued that all environmental regimes were ripe with transformational potential (Chayes and Chayes 1995). Conversely, the managerial or enforcement school argued that regimes were only as good as their inducement: most significantly, monitoring, verification, and sanctioning (Downs 2000).

Building on nearly two decades of empirical analysis of international environmental regimes (Haas and Sundgren 1993; Andresen et al. 2000; Haas 2001; Miles 2002; Haas 2003; Breitmeier et al. 2006; Haas 2007), provisional consensus converged around processes of cooperation and coordination: learning and inducements. Compromise and coordination subject to inducements leads to disjointed treaties and regimes that contain uniform national regulatory obligations chosen for their political and emotional appeal. Learning subject to organized knowledge networks leads to treaties and regimes that are more comprehensive in form with differentiated national obligations, and substantive commitments based on expert consensus about causes and effects of environmental decline. Inducements occur in the absence of any other notable factors, leading to patterns of collective action that adopt politically attractive compromises with which there is sufficient compliance to improve environmental conditions. Learning occurs through the involvement of epistemic communities, and leads to outcomes that have more demanding commitments that also provide better environmental outcomes that are politically more robust, when complemented with entrepreneurial and resource-endowed IOs.

George Downs, clearly associated with the incentives school, concedes that transformation sometimes occurs with the necessary involvement of epistemic communities, although he still clings to the testable hypotheses that induced regimes are more likely to be effective in the short term than are learned regimes (Downs et al. 2000) although it is still possible of course that learning regimes are more sustainable, robust, and effective in the longer term (Young 2010).

Further work is needed to identify the way in which these international-level mechanisms operate on different types of national actor; that is, the variation in national responses to inducements and learning (Haas 2003: 264). Also, we do not understand the reasons why states would choose to assign issues to venues prone to the different causal mechanisms.

Conclusion

Analytic eclecticism addresses many of those core questions that keep us up at night regarding how we can come up with justifiable and interesting insights into world politics, fraught with torment about irresolvable philosophical incommensurabilities. The next task is to clarify the steps necessary for creating meaningful, useful knowledge through analytic eclecticism.

How do we deliver wisdom from analytic eclecticism to policymakers? Even once good information is made available, the mechanisms by which it is best provided to decision makers is not clear, and how to make them wish to hear it is also unclear (George 1993; Wallace 1996; Lepgold and Nincic 2001; Jackson and Kaufman 2007; Krasner 2009). Surely there must be something better than standing in line with all the other aspiring advisors to the prince from think tanks and hoping to get one's 15 minutes.

How do decision makers and academics actually learn? While analytic eclecticism may be a good route to developing useful knowledge, we still don't know how to persuade people of its utility. Phil Tetlock (2005) famously distinguishes between foxes and hedgehogs: foxes are better than hedgehogs for dealing with the domain of issues for which Sil/K are addressing. But Tetlock cannot tell us what attracts individuals to become foxes or hedgehogs, or how to persuade hedgehogs to become foxes. I would like to propose a survey of APSA members to see if anyone has ever changed their mind about intellectual orientations, and what led to that. Such an empirical study would help guide analytic eclecticism in terms of the demand for its work.

In conclusion, analytic eclecticism gives me hope about our ability to understand and influence world politics. By just lightening up and looking at explanations for a significant puzzle, the study of world politics may move back on track to help develop understanding about complex challenges to sustainability, security, justice, and prosperity. By thinking more seriously about the implications of the research questions, we may generate more useful knowledge. Analytic eclecticism just makes me sleep better at night.

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Reflections on Analytic Eclecticism and the Field

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> "I never set out to be eclectic—it was the ultimate taboo in graduate school, after all—but I could not find a solution to my puzzle using just one paradigm."
> —Timothy Sinclair, *Beyond Paradigms* (2010: 124)

Rudra Sil's and Peter Katzenstein's *Beyond Paradigms* is about many things. It is about the philosophy of science; it is an argument for "metatheoretical flexibility" and "theoretical multilingualism"; and perhaps most of all, it is about what they see to be a need for a more pragmatic ethos in the service of better explanations for the complex phenomena that make up our world politics. In addition to these substantive points, it is also, however, very much a response to a perceived and experienced structure and culture of American social science that biases parsimony and strongly pushes us towards mono-causal explanations. In this sense, analytic eclecticism is offered as a corrective to approaches that are seen to prioritize parsimony at the expense of complexity and even "getting the answer right."

In this short essay, I offer some thoughts and reflections about the challenges of doing analytically eclectic work and the trends of our field.[1] I do so by drawing on my own work, the various contributions Sil and Katzenstein have solicited from authors identified as representative of the approach (of which, I am one), and with reference to the concerns and preoccupations highlighted during our August 2010 roundtable. The views and experiences expressed by their contributors offer concrete illustrations of some of the challenges highlighted in *Beyond Paradigms*.

The Process of Becoming Analytically Eclectic

In *Beyond Paradigms*, Sil and Katzenstein offer a methodological and philosophical defense of analytic eclecticism. To illustrate both the value and challenges of analytic eclecticism, they draw on recent studies; they also do something quite novel, which is to solicit author reactions to analytic eclecticism (and its label) and how we each came to pursue a more pluralist approach. In so doing, they have, in a sense, opened up a window on the research process—not just in terms of substantive questions regarding our specific empirical problems but also in relation to concerns about our field and subfields and how those concerns also informed the research process. We were each asked to respond to three questions: (1) Were you self-conscious in adopting an eclectic approach? (2) What is the most important thing you learned from the reactions to your work from others in your field? And (3) what is the most important advice you can offer to other authors? Each of these questions, but especially the first two, speak to questions about the perceived structure and culture of political science.

Indeed, one of the striking and not so striking themes to emerge from the set of responses received is how so many of those surveyed *came to*, as opposed to began with, an eclectic approach. If anything, there was even resistance to adopting such an approach outright. The fear and very real (methodological and professional) concern was to be charged with ad hocery—to display lack of rigor, to be found epistemologically and ontologically inconsistent, to be criticized for a "lame" "mishmash" of a "garbage can approach" (Jabko 2010; Paul 2010; Barnett 2010).

Thus, few set out to be analytically eclectic. Rather, analytic eclecticism was the product of an organic (though still directed) research process. Most seemed to agree with Sinclair above that their projects *had to* become analytically eclectic to be true—that is, their empirical problems "made them do it" even against their social science trained inclinations and desire for parsimony. Scholars followed the crumbs, "fretted" over their contradictions, and then reconciled themselves to the fact that the world might not in fact fit into our established theoretical boxes (Finnemore 2010).

The concerns expressed pointed to the ways our training directed us to certain kinds of questions, and by extension, certain kinds of conclusions over others. One theme that clearly emerges from both the different contributions in the book and the discussion at our roundtable is the fact that we are products of our own disciplinary socialization. Just as in Barnett and Finnemore's discussion of how "the very means and ends that [bureaucratic actors] value are shaped by the…culture in which they operate" (Barnett and Finnemore 2004: 19), so too are the means and ends we pursue and value as political scientists. The problem is that while that culture can discipline us in ways that tend towards more focused explanations that moreover are also based on a common language, they can also blind us from seeing other things that are taking place, how they interact, and the ways that different forces might condition each other in ways that result in unexpected outcomes.

Myself, I did not begin with a conscious intent to be eclectic, though I probably was more open to eclecticism at the start compared with some others. Mostly, I think my relative initial openness to a more pluralist approach had to do with the nature of my empirical problem—namely, the Association of Southeast Asian Nations (ASEAN). I began my project with a very clear interest in ideas as focal points and frames for the cooperative enterprise that is ASEAN. My focus on ideas was my response to what I saw to be some limited realist explanations based on some indeterminate conceptualizations of state interests, and most of all, their inability to capture the full complexity associated with the form, content, and trajectories of ASEAN and its regionalisms. But while I was very clear about the insufficiency of extant materialist explanations, neither could I completely dismiss them either. ASEAN's world is one of small to middle powers and this puts them in a different category compared with, for example, the transnational networks that have been the focus of so many constructivist discussions. On the one hand, one could argue that transnational networks are "weaker" because they lack the same legal standing that ASEAN's states have; but by the same token, they are also more free and autonomous as they are not under the same political, legal, or normative constraints and obligations that states are.

Basic world power concerns and the realist question of divergent interests were all things I thus had to account for—not just because of ASEAN states' structure of relations with world actors that material accounts emphasize but also because these were running themes and preoccupations in how ASEAN elites talked about regionalism. The theoretical and empirical "whens" and "hows" as regarded different material and ideational expressions, as well as the dynamic ways they came together, consequently became a driving focus of my project.

In my pursuit of such empirical and theoretical whens and hows, "process" became critical to the project in more ways than one. Partly, it was the method: The process of tracing ASEAN and particular ideas over time was what revealed the importance of the consensus process in ASEAN and importantly, its cumulative effects on both specific regional initiatives and the overall regional enterprise over time. It was through process tracing that a conceptualization of cooperation as an extended, cumulative process involving ideational and social negotiations, as much material came to be as important as my original interest in specific ideas. Process tracing was the method that revealed the patterns of ideational-material interactions, the different ways they mattered, and how material considerations can come to take on the status of intersubjective fact. In this way, I was compelled towards a more complex and dynamic explanation of ASEAN and its regionalisms than that with which I began.

In the process of writing my book, however, I nevertheless found myself confronted with some of the same concerns that were expressed by others—namely, a concern about it being perceived as less than "pure." A consideration of existing approaches and of how different explanations had been polarized in both international relations and in the ASEAN literature also made me acutely aware that the ways in which debates are framed have a way of bounding what we are inclined to see and how we see it. This is a struggle for the researcher interested in getting the explanation right, but it is also a struggle in terms of how one's work is received and interpreted. My particular concern was that I would end up satisfying no one on the material-ideational divide. In *Beyond Paradigms*, my concerns are echoed by Nicholas Jabko, T.V. Paul, and Frank Schimmelfennig, among others, who detail their experiences of being labeled one "camp" or another, as well as "closet" rationalists, in their efforts to provide a more complex causal story.

There are certainly many methodological and institutional reasons for such polarized debates and what T.V. Paul describes as the need to "pigeonhole" each other in "paradigmatic categor[ies]" (Paul 2010); but at least one reason may also be how we ourselves frame the debate and our questions. As Peter Haas at our roundtable put it, there's a "formula" we all have been socialized to follow, where every piece begins with a rough caricature and an extreme to be debunked. And so the "discipline" expects (to quote Michael Barnett) "a wrestling match" that will produce a clear winner in A *or* B. Perhaps it is no surprise then, that we come off as falling short when the wrestling match produces no clear winner and we conclude it is a combination of A *and* B.

On the other hand, this kind of dialectical process may also be what is necessary before more systematically complex explanations can emerge. It is not just that the incompleteness of each pole compels the inclusion of additional factors (Sil and Katzenstein 2010a: 412); it is also that the very exchange may also encourage explanations that are more complex and creative, as opposed to merely additive. By one argument, the metatheoretical pluralism being advocated cannot exist without well-formed paradigms.

"Lighten up!" (But Be Pragmatic About It)

The importance of a disciplined eclecticism is why many of those covered in *Beyond Paradigms* also argue for caution. While there is more or less broad agreement amongst those featured in the book that we should be more open to pluralism and more "complex, causal stories" (Sil and Katzenstein 2010a)—that we should "lighten up" in terms of our epistemological and even ontological commitments— there is also just as broad agreement that analytic eclecticism should not be undertaken casually.

For one, as noted above, we worry about ad hocery not just because of our concerns about how our work will be assessed and received in our respective fields, but also because the standards of good research require that we guard against an "everything goes" approach. Eclecticism should not mean randomness but instead a disciplined and problem-driven pluralism. Indeed, for this reason, even though all those covered in *Beyond Paradigms* ultimately felt compelled towards more pluralist explanations, there is also the view that one *should not* begin with the intent to be analytically eclectic. Schimmelfennig was perhaps most explicit in making this point: "It does not make sense to pursue an eclectic approach as a research goal. Rather eclecticism is the unintended result of research that seeks to explain specific events as well as possible" (Schimmelfennig 2010). It is, as Sil and Katzenstein argue, the problem that drives the framework, not the other way around.

In this sense, our paradigmatic and theoretical debates, such as those highlighted above, may also serve analytic eclecticism well—even if it remains a double-edged sword. On the one hand, as suggested, they can focus our attention in overly narrow ways and sometimes on the wrong things. But on the other, such debates may also impose a kind of discipline. For example, it can help focus our attention so that we don't fall victim to the garbage can explanation. It can also compel greater due diligence. As clearly illustrated by the examples featured in *Beyond Paradigms*, an acute awareness of established paradigms and anticipated objections to more pluralist approaches may have made each of the scholars highlighted initially more resistant to multi-causal explanations, but it also appears to have made them each more self-consciously critical of their own conclusions—a mark of good scholarship—and much more conscientious in terms of how they evidenced their arguments about how things came together. In their argument for a pragmatic ethos, Sil and Katzenstein argue not just for a pragmatic engagement of our empirical problems at hand, but also a pragmatic engagement of existing paradigms as a way to produce more focused, as opposed to scattered, analysis.

The other caveat comes from a different kind of pragmatic concern all together—and that is that we all need jobs and careers, which depend on our being able to frame our questions in ways that can be understood and accepted as legitimate by others. This is Cornelia Woll's concern, especially for younger scholars. The willingness to challenge the canon is, of course, the stuff of groundbreaking and innovative scholarship but for the average student coming up in the ranks, it can require quite a lot of confidence and even chutzpah. For those challenging the very values and identity of the discipline, it can even mean the perceived need to hide one's identity—as we saw in the Perestroika movement's challenge to the "perceived hegemony of formal and quantitative approaches in favor of methodological pluralism, qualitative inquiry, and again an orientation to pressing public problems" earlier this century (Dryzek 2006: 487). At minimum, as Jabko concludes, "it's challenging to not be a well-behaved follower of one of the main political science churches" (Jabko 2010). The fact that successful revolutions in political science (in terms of resetting the discipline's agenda and commitments) have arguably been rare only underscores the challenge (Dryzek 2006). Notably, Sil and Katzenstein have been very careful themselves not to characterize analytic eclecticism in terms of revolutionary change.

There are also other practical considerations. For example, even those who describe themselves as having been less constrained in their training and thinking found themselves confronting the material challenges of publishing given how both academia and publishers compartmentalize subfields and disciplines. The problem of such compartmentalization could be structural in terms of the organization of a publishing house, but it could also be more substantive as pluralist work can confront competing expectations from the different subfields and approaches they are trying to bridge. Either way, the result is the same: It can be more difficult to publish work that doesn't fit all in one box. Consequently, those like Woll conclude that academic work may be necessarily and "always a compromise between the substance of our research and the constraints of our profession" (Woll 2010). Such concerns further highlight how material incentives can work against those seeking to cross paradigms, subfields, and disciplines (Tarrow 2008).

The State of the Field

Perhaps not surprisingly, given some of the main concerns above about reception, both our APSA roundtable and a number of those surveyed in *Beyond Paradigms* concluded that what is necessary is a change in the culture of our departments and graduate programs; we also need better questions—questions that allow us to be more open about different forces and influences we discover along the way; and perhaps most of all, we need greater humility about what we know (Blyth 2006).

To look at our discipline in 2010 is to see mixed trends. On the one hand, there is evidence of greater pluralism in a number of areas. Methodologically, the trend is towards mixed methods. A generation brought up on King, Keohane, and Verba's *Designing Social Inquiry* has been expanding its sights. We are looking at not just causal effects but also causal mechanisms and causal processes, which opens the door to both less determined conclusions and more complex causal chains that are also more sensitive to context and contingencies, as well as intersecting and sequencing influences. In APSA, a qualitative methods section is now titled "qualitative and multi-methods." In comparative politics, there appears now to be consensus that there does in fact exist an "eclectic center" (Kohli 1995). In international relations, many realists have been pressed to consider questions of ideology and perception, while many constructivists have moved away from purely ideational explanations. Indeed, the range of scholarship covered in *Beyond Paradigms* is a case in point, with some (Schimmelfennig, Solingen, and Schiff) concluding that the positive reception their work received was a sign that some of the old paradigmatic debates had grown "stale" (Schimmelfennig 2010), that "the discipline has become more receptive to questions about and alternatives to paradigmatic conventions" (Solingen 2010).

On the other hand, the structural and disciplinary obstacles that provide starting assumptions for *Beyond Paradigms* also remain relevant. There has indeed been the growth of perspectives and even trends towards greater pluralism in some of our approaches, but it also remains a question how much we really talk to one another. Both authors and publishers should have real concerns about sending out manuscripts and articles to those who do not share one's approach or metatheoretical commitments because to do so risks a rejection based on differences that have less to do with the specific case in question than with the "dogmatic allegiances" that *Beyond Paradigms*' authors worry about (Paul 2010). Our other solution, then, is to create new corners for ourselves, new APSA sections (there are now 42), and new journals, a solution that not only may not solve the problem but may in fact entrench one's marginality in relation to an established and entrenched core.

In the end, *Beyond Paradigms* is not calling for a revolution in how we "do" political science or what political science is. Rather, it is more like the mid-range theorists that are the subject of its attention. Speaking to "the discipline's DNA disputes about epistemology and method" (Ubertaccio and Cook 2006: 573), it maintains a common language that makes it more intelligible and more legitimate to the rest while at the same time opening the door for new intersections between the different strains of our discipline.

Note

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A Plea for Puzzle-Driven International Relations Research

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This presentation is a bit of a personal research story as others in this panel are better qualified to talk about the methodological and epistemological foundations of eclecticism. The key reason I support and pursue eclectic approaches is the puzzle-driven research agenda that I have consciously adopted from the beginning of my scholarly career as opposed to a paradigm-driven approach. I believe that many puzzles in international relations cannot be fruitfully explained using a single paradigm as it forces the scholar to pigeonhole the explanation into one or the other perspective, even if it may not be accurate. I favor analytical richness, even if it means some loss of rigor and parsimony, and often a single-paradigm approach cannot offer that. I also believe that international phenomena are much more complex than we understand and often the historical contexts and situations of states, societies, and individual leaders need to be explored in order to obtain a convincing understanding of the issues that we try to grapple with. While I see value in paradigms, most often they can degenerate into a sort of dogmatic allegiance and ideological preference, something that in my view social scientists should avoid. By being open one should be willing to disprove one's paradigm after conducting the empirical research if the puzzle cannot be explained by a given paradigm that the scholar has adhered to. By not doing so, many IR theorists tend to lose the philosophy of science requirement while conducting their research and writing. However, paradigms can be useful starting points for an analyst to pursue an eclectic approach.

There are different ways to pursue eclectic research. One is to combine insights from different IR theoretical paradigms like realism, liberalism, and constructivism. This can be done fruitfully, if we seek middle-range theories, and if it is done carefully. There are IR phenomena that cannot be addressed without adopting such a perspective. Take the case of nuclear proliferation. There is a tendency (as some prominent recent works show) to argue that liberal or normative reasons are largely behind the non-proliferation policies of different countries globally. This is very problematic as it is beyond doubt that the non-proliferation regime is a creation of the hegemonic powers although others have joined in for realpolitik and normative reasons. It might well be true that Argentina and Brazil gave up nuclear weapons largely for economic reasons—but to say Taiwan, South Korea, and Japan all do not have nuclear weapons today primarily because of reasons of economic openness is problematic. Without a continuous American security umbrella they would probably have gone nuclear. In fact the first two tried, only to stop under American pressure. In my book *Power versus Prudence: Why Nations Forgo Nuclear Weapons* (McGill-Queen's, 2000), I used an eclectic approach, although privileging classical realist ideas such as prudence and security interdependence while incorporating liberal views on economic calculations of states in their nuclear choices. I think a regional approach to studying nuclear proliferation carries much promise. Some regions are dominated by enduring rivalries and protracted conflicts and they offer the fertile grounds for a classical, structural, or neoclassical realist explanation. Other regions where more cooperative institutions and economic interdependence exist could be places where liberal and constructivist variables, including the norms of cooperation, are present the most. Until the former end their rivalries, these liberal and constructivist mechanisms may not be all that valid for understanding their behavior.

I carried this regional approach with Norrin Ripsman in *Globalization and the National Security State* (Oxford, 2010). The attempt here is to divide up the world— i.e., how different categories of states (great powers, states in regions of conflict or cooperation, and weak states) are affected by globalization in the area of national security. A breakdown of the different actors and regions includes the major powers (U.S., Russia, China), states in cooperative regional sub-systems (EU, ASEAN, Mercosur), states in competitive regional sub-systems (South Asia and the Middle East), and those states that can be considered weak or failing (most of Africa). Second, we considered on a selected country-by-country and region-by-region basis whether and to what extent globalization has affected the national security state. This allowed us to consider whether the effects of globalization, if they manifest themselves in the national security realm at all, do so evenly or unevenly across states and regions. Most globalization arguments are presented as if transnational political and economic forces are transforming national security states uniformly throughout the world. In contrast, we consider whether the relative power and position of a state in the international system determine the degree to which these changes affect it. Our key finding is that to the extent that globalization has affected the pursuit of national security, it has done so unevenly. States in stable regions have transformed their national security establishments the most to meet the challenges of globalization, while those in conflict-ridden regions have done so the least, although the latter are tremendously affected by many negative forces associated with globalization. The great powers have adapted to globalization only when it was consistent with their own strategic imperatives. Finally, the very weak or failed states of sub-Saharan Africa have had their fragile national security establishments buffeted by the pressures of globalization, which have added further impetus to state collapse.

Today Globalization offers a fertile arena for scholars to pursue an eclectic approach. Take the case of rising China and India. It seems these states are economically globalizing and are joining international institutions—thus pursuing two of the Kantian pillars. Yet they are also pursuing realpolitik goals. Their combined national approaches to status and power are not easy to capture with a pure liberal or realpolitik approach.

While working on *Tradition of Non-use of Nuclear Weapons* (Stanford, 2009), initially I tried to see if a deterrence-only approach can explain the phenomenon of nuclear non-use and I immediately realized that such an approach will not make sense, as the non-nuclear states did not possess the retaliatory capability to deter militarily. As I pursued research further I realized an eclectic approach is needed—one that links normative and instrumental calculations of nuclear weapon states in order to explain nuclear non-use most effectively. The explanation lies between material and normative perspectives, respectively giving importance to the logic of consequences and logic of appropriateness (although I weigh in favor of the former over the latter). It rejects arguments of both a powerful taboo-like prohibition and simple realpolitik skepticism. I give much more weight to the tradition than realpolitik skeptics do, but not as much as constructivists do.

In this book, I developed an explanation for the emergence and persistence of the tradition of non-use of nuclear weapons, based on material factors and reputation. The relevant material concerns, especially the intrinsic physical effects of nuclear weapons, have moral, legal, and normative as well as rational connotations. Further, the reputation of a state is partly based on other states' views of morality and law and whether the former is adhering to these in its security and foreign policy behavior.

In general, rarely have I seen a trenchant criticism of my work based on my preference for eclecticism. What is most noticeable is the tendency of scholars to place me in a paradigm category even when I am clearly stating in my works that I am avoiding a single-paradigm approach. For instance, much of the citations of my *Power versus Prudence* treat it as a pure realist perspective when I have provided an eclectic and situational analysis. This is largely because many scholars, especially younger ones, often are looking for foils and pigeonholing to prove their points of view in oppositional terms. In the book and article manuscripts that I review on nuclear proliferation that treat my work in this way, I try to correct this anonymously, but often without much success.

Open-mindedness and a willingness to work through the causal mechanisms and pathways rather than simply narrating variables are essential to pursue an eclectic approach fruitfully. We need to know how these different variables (often drawn from different paradigms) are connected and how they affect or cause the outcome, singularly or in unison, that we are trying to explain. Explanation should be the most important objective of the scholarly enterprise, not simple description of a phenomenon. Eclectic approaches, if not used carefully, can end up in thick descriptions of a mish-mash variety, a danger that scholars should try to avoid. Also, sometimes a single paradigm can explain a given phenomenon better and if so, one should be willing to accept that possibility without branding himself as a card-carrying member of a paradigm. Sil and Katzenstein offer us a coherent set of philosophical foundations and a flexible analytic framework for research that does not fit into existing paradigms. Now, it is up to other scholars to do more of the problem-focused empirical work needed to make this a core approach in the discipline.

Analytic Eclecticism: Not Perfect, but Indispensable

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Following the pragmatist bent of our conceptualization of analytic eclecticism, we view the four excellent commentaries offered above as a welcome opportunity to engage in open-minded dialogue and to clarify certain defining attributes of eclectic scholarship. Two commentaries are offered by Andrew Bennett and Peter Haas, who are sympathetic critics of our work: They accept our premise that research confined to paradigms has built-in limits, but then offer some frank assessments of the challenges and limitations of analytic eclecticism. They raise important issues about the implications of analytic eclecticism for the cumulation and assessment of theories, and for some of the risks that existing institutional practices pose to eclectic scholarship. We regard their remarks less as a critique of our arguments and more as impressive forays into the sorts of discussions that we hope will displace stale inter-paradigm debates if academic scholarship is to become more connected to the world of policy and practice (Calhoun 2009; Nye 2009; Shapiro 2005). The other two commentaries, by Alice Ba and T.V. Paul, come from "insiders" in the sense that both are authors of works we discuss in our book (2010b) as exemplars of eclectic scholarship. Admittedly, they have little incentive to critique a book that casts their work in a favorable light. Their thoughtful and candid reflections, however, go a long way toward helping us clarify some important points about the different pathways to analytic eclecticism and the complexities involved in coding what counts as eclectic scholarship.

Let us begin by making clear what these scholars are commenting on: Bennett and Haas are addressing an article published in *Perspectives on Politics* (2010a) as well as our book, *Beyond Paradigms* (2010b); Ba and Paul are primarily reacting to the latter. The article is a general programmatic statement, outlining the intellectual rationale and philosophical foundations for eclectic work in both comparative politics and international relations. It develops a consistent set of markers for distinguishing between scholarship embedded in research traditions and analytically eclectic research. In addition, it underscores eclecticism's reliance on an expansive and open-ended definition of causal mechanisms, at least for analyzing problems that feature "more extensive endogeneity and the ubiquity of complex interaction effects" (Hall 2003). The book offers the same general argument, but focuses solely on international relations and employs the term "paradigm," which is more commonly used to characterize contending schools of thought in that field. The book is not an anthology but offers instead a sustained, coherent argument. We did not ask for permission to label as "analytically eclectic" any of the fifteen works discussed—five in each of three substantive chapters discussing eclectic research in the analysis of security, political economy and global governance. Nor did we extract any passages or selections from any of these studies. Instead, we deployed our own criteria for deciding which studies constitute reasonable approximations of analytic eclecticism in international relations (2010b: 19–23): open-ended problem formulation, a complex causal story featuring mechanisms from multiple paradigms, and pragmatic engagement with issues of policy and practice. We then discuss the content of several exemplary studies in a manner that highlights their eclectic character, their distinctiveness vis-à-vis paradigm-bound research on the relevant topic, and their value-added within the context of scholarly efforts to understand particular aspects of international life. Finally, we incorporate brief 400-word statements (presented in fifteen boxes in the book) from each of the scholars, each responding to specific questions we posed about their experiences in producing eclectic scholarship.

Bennett's essay is running a bit ahead of us. It pushes us after the publication of our book to confront more fully issues that we see as arising if there were to follow a wider acceptance of the assumptions and practices associated with analytic eclecticism. Because of the prevalence of a paradigm-centered view of international relations scholarship, we were primarily focused on clarifying the trade-offs between work embedded in paradigms or research traditions on the one hand and eclectic research on the other. We also sought to highlight the advantages of eclectic scholarship for addressing specific problems in world politics. Since, to date, there does not yet exist a critical mass of eclectic research on any given problem, we touched only briefly on the questions Bennett wishes us to now pursue. We welcome the implied optimism about the potential impact of our formulation of analytic eclecticism on scholarly practices. And we see his articulate commentary as an invitation to reflect further upon the implications of adopting an eclectic approach for the character, utility, and evolution of social scientific knowledge.

First, it is worth noting that our ambitions are more limited than Bennett's. We are gratified that Bennett notes our tracking of current trends in the philosophy of science. It is, however, worth emphasizing that we proceed from foundations that are more closely associated with neopragmatism rather than with versions of philosophical realism or neopositivism. Certain strands of realism and pragmatism do have much in common, but pragmatism is more explicit about several points we consider crucial for defending analytic eclecticism: it calls for greater scope for deliberation among a more inclusive community of inquirers; it discounts the separation between abstract knowledge and practical insights relevant to a specific situation; and it views all knowledge as a set of tentative insights that can never be confirmed as "*the* truth" but can be updated and recombined in different ways in concrete situations (Sil and Katzenstein 2010b: 43–48). Furthermore, analytic eclecticism does not aspire to general theories that cover different types of problems. Rather it is restricted to the level of "middle range" theory as identified by Robert Merton's understanding of the term. This means that theoretical constructs—concepts and propositions—are designed to gain traction on a set of similar problems clearly demarcated and easily identified in concrete situations that recur in world politics. An eclectic approach that is grounded in pragmatism and is limited in aspiration to middle-range theorizing does not aim at ambitious, holistic understandings of "theory cumulation" across entire disciplines or subfields. And it does not facilitate a direct assessment and comparison of theories that deal with different problems even if these are loosely categorized under similar labels such as "security" or "political economy."

At the same time, as Bennett recognizes, the combinatorial forms of knowledge we associate with analytic eclecticism can be organized in such a way that they allow us to identify discrete configurations of causal mechanisms that are portable across those contexts within which a given problem is identified. Within these limited contexts theory cumulation is possible insofar as there is a progressively deeper understanding of how general mechanisms interact in different environments to generate different or recurrent processes. It is also possible to develop and compare different kinds of analytically eclectic arguments, each proposing distinctive configurations of mechanisms in relation to similar kinds of problems. We have not done this in the book, as we considered instead an array of problems on which eclectic scholarship has provided fresh insights in comparison with arguments developed within paradigms or research traditions. However, once a critical mass of eclectic scholarship has emerged in relation to any given problem, the next step would indeed be to compare eclectic middle-range theories in terms of how plausible the interconnections between general mechanisms are, and how consistently the combined effects of a particular configuration of mechanisms are evident in a given context or environment. An adequate response to what Bennett is asking for would require a companion volume once it becomes easier to locate multiple eclectic approaches to a given problem. For the time being, we are fully cognizant of both the limits and possibilities for theory-cumulation and theory-comparison at the level of mid-range theorizing. We view both as valuable endeavors, but only within a more concretely delimited intellectual terrain, bounded not by disciplines or fields of study but by the attributes of similar concrete problems with similar scope conditions.

Bennett also asks us to consider whether we can assess the long-term value of eclectic scholarship through the lens of a Lakatosian conception of scientific progress (Bennett 2003; Elman and Elman 2003). Such a conception would be based on the identification of "novel facts"—the value of which is established through "use novelty" and "background theory novelty"—and is not incompatible with eclectic modes of scholarly inquiry. Bennett is absolutely right in noting that there is no need to establish paradigms or designate "hard cores" and "outer belts" in order for scholars to arrive at a consensus that specified evidence would have use novelty or background theory novelty relative to specific theories or explanations. However, one aspect of our argument is critical to bear in mind here: Scholars do have a great deal of leeway in how they frame their core problems. Problems can be posed in such a manner that they not only encourage the use of certain methods (Sil 2004), but also draw greater attention to particular mechanisms in particular domains or levels of social reality. Extending Ian Shapiro's (2005: 184) observation about the formulation of research questions in the discipline, we argue:

> The issue, in our view, is not whether the social sciences ought to be problem-driven or method-driven but rather how problems are identified and formulated. Projects embedded in different research traditions frequently address similar or related substantive issues but parse these issues in order to focus on specific aspects in keeping with their theoretical priors. Such simplification of social reality is certainly understandable, even necessary. However, the *extent* to which and the *manner* in which large parts of that reality are simplified in the formulation of problems matter for the purpose of generating insights that bear on the choices and actions of actors coping with complex substantive problems. A pragmatist conception of analytic eclecticism invites us to consider how the problems as defined within research traditions might (or might not) relate to each other and to concrete dilemmas related to policy and practice. (Sil and Katzenstein 2010a: 418–19)

This suggests that the designation of "novel facts" is not a straightforward process when paradigm-bound scholarship is juxtaposed with eclectic arguments about similar problems. For example, "use novelty" would require drawing upon new observations that can serve as additional evidence to support a claim initially based on some other evidence. However, eclectic scholarship often generates claims that are significantly more complex for the simple reason that the scope of the problem is expanded so as to partially reverse the simplifications made by paradigm-bound researchers in posing their questions. Similarly, background theory novelty is a useful standard when a proposition purports to explain anomalies or unexplained phenomena; but what constitutes an anomaly or an unexplained phenomenon is often shaped by the substantive focus and scope conditions inherent in a research question as posed by individual scholars. Since eclectic scholarship is often aimed at problems that incorporate or subsume the more narrowly framed questions taken on by adherents of paradigms, the designation of what constitutes a "novel fact" becomes more problematic than would be the case with theories that take on similar questions formulated within a paradigm.

We do not wish to intimate that Bennett's suggestion is not useful or that theory incommensurability is an intractable problem. In fact, we go to some lengths to establish that the problem of incommensurability is often over-emphasized and that it can be mitigated by translating concepts and analytic principles upon consideration of the empirical referents used to operationalize these (Sil and Katzenstein 2010a: 414–15; 2010b: 13–16). This effectively suggests that applying the Lakatosian standard of novel facts involves a two-step process. The first involves the difficult task of translation and comparison of related problems that have been cast at different levels of abstraction, are formulated in different theoretical vocabularies, and have different scope conditions. Only after this first step has been taken can we be assured that we are discussing a set of theories (whether paradigm-bound or analytically eclectic) that are actually addressing the same problem. And only then can we have a meaningful discussion of whether a given eclectic approach has "use novelty" or "background condition novelty." But before we can tackle intricate problems in the philosophy of the social sciences, the most immediate need is a critical mass of eclectic scholarship that is organized around the complex, messy problems of real-world politics rather than around intellectual puzzles designed to test or apply the concepts and analytic principles associated with existing paradigms.

Like Bennett, Haas also wants to push us forward, to engage questions that we would consider to be significant only if and when analytic eclecticism becomes more widely practiced in the discipline. In the section focused on the philosophy of science, Haas appreciates the utility of eclectic perspectives that train their sights on the interplay of diverse mechanisms. But he insists (rightly) on asking what comes next. In particular, he wonders if we are holding back too much, perhaps constrained by a general commitment to Larry Laudan's (1977, 1996) philosophy of science. Haas is right to note some of the limitations of Laudan's work. Since our interest in Laudan is mediated by a more fundamental commitment to pragmatist philosophy, we are not wedded to every aspect of Laudan's philosophy of science. Our reading of pragmatism (2010a: 416–418; 2010b: 43–48) provides the bridge that Haas is looking for—connecting our commitment to a rigorous social science with our conviction that there is much to be gained from an "assemblage of mid-level findings" organized around concrete problems that have both scholarly and practical import.

We take from Laudan the concept of a research tradition. It offers a more realistic framing of the shifting controversies in the field than the more rigid conceptions of scientific progress offered by Kuhn or Lakatos. Operationalizing a research tradition rigorously is difficult, as Haas points out. But this is even more true of paradigms and research programmes which, as we note in the book, are concepts that do not map on to the messy intellectual history of social science disciplines and subfields. In social scientific research, boundaries are unavoidably fuzzy and encompass more diversity in ontologies and epistemic principles than is commonly recognized. This is why we opt to understand controversies in the social sciences through the lens of Laudan's "research traditions," which eschews a stylized rendering of scientific progress and allows for scholars working in multiple traditions. In the book, we use the term "paradigm" for the sake of simplicity, but we define it as interchangeable with "research tradition."

We also borrow from Laudan the notion that the usefulness of a theory depends more on its ability to solve problems than on its contribution to the cumulation of knowledge. Indeed, a pragmatist perspective sees the very idea of knowledge cumulation as more of an abstract ideal than an ongoing process to which all research inevitably contributes. There is no presumption that eclectic scholarship will gradually pave the way toward all-encompassing "grand truths" that will become firm guides to action and policymaking. Nor would we characterize the results of eclectic, problem-centered mid-level analysis as "small insights" relative to the "broader insights" supposedly generated by more ambitious theoretical claims. In fact, it is precisely grandiose ambitions for unified theories and knowledge cumulation that, in combination with the availability of material and organizational resources, have given rise to the inter-paradigm debates that remain prevalent today.

Moreover, the most self-conscious and self-confident adherents of a paradigm treat knowledge cumulation in an even more restrictive manner. They see cumulation as predicated on acceptance of the metatheoretical assumptions and theoretical principles associated with their preferred paradigm. Yet, the only people who might concur with such a view are those working in the same paradigmatic tradition! Thus, what constitutes "progress" or "cumulation" for some does not constitute progress or cumulation for the field as a whole. Given this predicament, the "small insights" of eclectic scholarship are actually quite "big" in terms of their potential impact, both on the relevant scholarly fields and on policy-relevant debates and discussions. Mid-range eclectic analyses may be modest compared with general laws or grand theories, but we see this modesty as purposeful for the valiant attempts at mediating between different paradigms and connecting academic theories to public discourse and policy debates. For these reasons we do not regard eclectic mid-range theorizing as a way station to a grand theory around which a discipline or subfield will eventually reorganize. Rather, eclectic work, alongside paradigm-bound research, continually accommodates and encourages efforts that aim at translation, comparison, and dialogue within and beyond academic circles.

Haas' remarks about the sociology of knowledge present a more immediate and direct challenge to analytic eclecticism, or at least to scholars considering eclectic approaches. Haas is of course correct that the structures of the discipline militate against the eclectic scholarship we advocate; that was the reason why we wrote the book in the first place. Beyond that, Haas raises the important issue of collaborative work. Haas notes that we rely on single-author works to make the case for eclectic scholarship that is useful, compelling, and rigorous. This overlooks the fact that one of the studies we discuss is actually coauthored (Barnett and Finnemore 2004) and that our book is itself an exercise in collaboration. We thus see no inherent value in single-author eclectic work. Intellectual and practical reasons may push two or more eclectically oriented scholars to collaborate, as we ourselves have. Faced with the intellectual requirements and professional risks associated with eclectic work, having a coauthor or contributing to an anthology may have intellectual, psychological, and professional advantages.

In the contexts of hiring, grant competitions, and promotion, collaborative work of all stripes faces special hurdles in many (but not all) fields and in many (but not all) major research universities. Whatever their intellectual orientation, younger scholars in particular must demonstrate their ability to work independently in designing and executing research projects. Although scholarly research does not need be tied to single individuals, in real life, jobs, tenure reviews, and promotions focus largely on the track record of individuals. This is true in general and does not constitute a specific barrier against the recognition of eclectically oriented work. Whether the social sciences stand to benefit by encouraging and rewarding collaborative research is an important question, but it is one that needs to be addressed on its own terms.

On the difficulties of publishing eclectic scholarship in academic journals, we do not have any fixed prescriptions. New journals, as Haas points out, may be helpful in opening up space for eclectic research that existing journals may shy away from. But the low visibility of new journals means that articles published in them generally are overlooked and are not given the credit they may deserve when the time comes to assess an individual's scholarly productivity and professional standing. The editorial boards and reviewers of well-established journals do learn of course from intellectual currents in the field, but we know little about such learning processes in different parts of the discipline. In the end, if some journals and their editorial boards are clearly partial to specific approaches or methodologies, scholars will have to accept that fact and move on with their lives. At the same time, good social science journals do have to compete with each other for readers and so can ill afford to ignore shifting currents in the field. This means that they have an incentive to occasionally stretch their boundaries to accommodate articles that do not fit their "normal" profile. In these instances, eclectic scholarship may well prove to be a stronger candidate for publication in a journal that favors one paradigm, at least compared with scholarship embedded in a rival paradigm.[1]

Finally, it is important to underline the encouraging fact that we were able to identify eclectic analyses that *have* been published by first-tier journals and top university presses. Many scholars, both senior and junior, are moving away from paradigm-bound research in their own work even as they continue to view the field of international relations as still dominated by contending paradigms. As we observe in the book (2010b: 25), in the 2008 TRIP survey (Jordan et al. 2009: 9, 33), 36 percent of the American respondents (and about the same percentage of the sum total of all respondents from nine other countries combined) indicated that their own work did not fall within one of the major international relations paradigms. This figure constitutes a significant minority; and the percentage is noticeably larger than in previous iterations of the same survey (e.g., Maliniak et al., 2007). Leaving aside the studies we discuss in our book, in several fields scholars are increasingly gravitating towards more eclectic styles of work, although not always self-consciously so. For example, Haas discusses how empirical research in the field of international environmental law has moved away from debates between the "transformational school" and the "enforcement school," and has converged around a provisional consensus on the importance of complex processes that incorporate both inducement mechanisms and learning mechanisms.

The commentaries from Alice Ba and T.V. Paul offer us an opportunity to clarify several important points about the production and identification of analytic eclecticism. First, analytic eclecticism has no set recipe and is not an end in itself. There are many different ways through which different scholars ended up generating work that we code as eclectic. In Ba's case there was no explicit commitment to an eclectic mode of inquiry. Instead her emphasis on *process* pushed her increasingly to look at complex configurations of factors that did not neatly fall within the boundaries of a single paradigm. In Paul's case the movement towards eclecticism cut across several research projects. It began with a limited effort to add nuance to core realist arguments by adding elements from neoliberalism in his earlier book (Paul 2000). In subsequent work Paul (2009) made a more explicit effort to merge ideational aspects normally associated with constructivism with materialist factors emphasized in neorealism and neoliberalism. These observations, as well as the reflections of the other thirteen authors whose work we engage in the book, provided an unexpected bonus in helping us see more clearly and state more forcefully that analytic eclecticism "is not meant to constitute a discrete new 'ism' to replace or subsume all other 'isms' in the field of international relations. It is, however, a useful heuristic for capturing the common requirements of metatheoretical flexibility and theoretical multilingualism necessary for substantive analyses that are not embedded in any one paradigm" (2010b: 25). Ultimately, what makes an eclectic research strategy worth pursuing is the desire to better understand complex, socially important real-world problems that existing paradigm-bound theories either fail to address or address only in part.

In different ways, Ba and Paul also point to the difficulties of promoting an eclectic approach in a field accustomed to viewing scholarship through the lens of inter-paradigm battles. For both, the most unexpected response to their books was not unanticipated criticisms of substantive claims but rather efforts to label the studies using familiar categories. Paul refers to the "pigeonholing" of his work, while Ba refers to her fears that the complex processes she lays out would not satisfy a discipline wedded to the idea of contending paradigms. Old (paradigm-focused) habits die hard. We are not surprised by this reaction. It was a major reason why we wrote our book. We hope that analytic eclecticism and the pluralist spirit it embraces will become a more established part of our disciplinary lexicon, so that authors can focus on the merits of their own work without being concerned about being pigeonholed or about having to manage the expectations of scholars adhering to competing research traditions.

This is a good place to note a particularly vexing problem we encountered when identifying eclectic scholarship: Some authors characterized their books as refined versions of constructivism. This is true of Ba (2009) as well as Martha Finnemore (2003) and Nicolas Jabko (2006). Jabko, for example, characterizes the approach in his book as "strategic constructivism." This pattern is also evident in more general metatheoretical perspectives on international affairs such as the "pragmatic constructivism" articulated by Peter Haas and Ernst Haas (2009). We address this problem directly in our book (2010b: 41–43), emphasizing the need to distinguish between programmatic commitments to constructivism as a distinct paradigm and "weak" identification with constructivism for tactical reasons. The latter, we view as an artifact of the timing and intellectual environment within which constructivism emerged: it was the third of the three major IR paradigms to arrive on the scene, at a time when some neorealists and neoliberals were gravitating towards a rationalist "neo-neo synthesis" (Waever 1996: 163; see also Keohane 1989: 165). Thus, for those wishing to incorporate ideational factors in their analyses, a reasonable path to take has been to distinguish themselves from purely rationalist analyses and to identify themselves with constructivism even if ideational constructs are not actually privileged in ontological or theoretical terms. For such scholars, we hope, analytic eclecticism may present a more appealing alternative to having to squeeze uncomfortably into one of the existing paradigms.

The real issue is the quality and value-added of the individual eclectic approaches. Ba and Paul both underline another caveat that we repeatedly make in both our article (2010a) and book (2010b). Analytic eclecticism does not offer a carte blanche to produce either idiosyncratic stories for each and every case or a never-ending laundry list of factors that potentially influence each and every outcome. Ba is wary of "adhocery" that might trade away standards of good research in exchange for an "everything goes" approach. Paul similarly warns that eclectic analysis should be wary of falling into the trap of providing "thick descriptions of a mish-mash variety." Instead analytic eclecticism gives researchers a license to cut across or operate in between the boundaries separating research traditions. The research contributed by adherents of paradigms identifies important causal mechanisms and makes significant contributions that deserve our serious attention. A commitment to analytic eclecticism does not provide a warrant for ignoring these contributions but rather, as Bennett suggests above, invites us to figure out how different types of mechanisms normally explored in isolation from one another might interact as part of more complex configurations in a given context. As Ba notes in her contribution, our conceptualization of analytic eclecticism is a call "not just for a pragmatic engagement of our empirical problems at hand, but also a pragmatic engagement of existing paradigms as a way to produce more focused, as opposed to scattered, analysis."

We conclude by addressing Bennett's qualms about our choice of the term "analytic eclecticism" in lieu of an alternative that would carry fewer negative connotations. We are surprised that for Bennett the term has negative connotations; the initial spontaneous and unprompted responses to our book and article, from both close colleagues and scholars we do not know, suggest otherwise. In any case, there were many reasons why we chose this term. Some are accidental. Sil had always been struck by the use of the term "eclectic" in a well-known symposium on theory in comparative politics (*World Politics* 1995) in which Katzenstein, Peter Evans, Atul Kohli, James Scott and others highlighted the limits of simplifications in the name of parsimony and acknowledged the "eclectic messy center" at the heart of comparative politics. At the time, however, there had been no serious effort to define "eclectic" scholarship or to explicate the rationale for it. In subsequent work, both Sil and Katzenstein independently developed distinct but complementary understandings of what eclecticism entailed and what it could contribute to, respectively, comparative politics and Asian regional security (Sil 2000, Katzenstein and Okawara 2001/02). When we decided to join forces at a conference in 2003, it made sense to stick to a term that both of us were already comfortable with and that we both were beginning to get identified with in different circles.

Over time we became more self-conscious in embracing the term "eclecticism." Gunther Hellmann (2003: 149) has pointed out that the field of international relations has a particularly strong penchant for "stigmatizing as eclectic whatever approach to current problems in international politics does not fit along the established axes of scholarly enlightenment." This view is also echoed in the statement from Timothy Sinclair that Alice Ba cites in the epithet for her contribution to this symposium: eclecticism has been presumed to be "the ultimate taboo," especially in graduate training. We think that this phase in the evolution of international relations scholarship is passing and that the time has come to turn a fresh page. In our work we thus emphasize the value-added, in both intellectual and practical terms, of eclectic styles of inquiry. It is not our intention to dismiss the scholarship that has emerged from paradigms or traditions. We are, however, convinced that any field that continues for too long to define, pursue, and evaluate research solely through the lens of paradigmatic assumptions and inter-paradigm contests risks missing out on crucial insights about the complex processes and intersecting mechanisms that account for interesting outcomes in world politics. Analytic eclecticism offers a promising way forward.

Note

[1] For example, while it is true, as Haas notes, that *The Journal of Theoretical Politics* was created to publish work on rational choice and game theory, this is the same journal that published Sil's (2000) article on "The Foundations of Eclecticism."

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Symposium: Causal Mechanisms, Process Tracing, and Causal Inference

A Tale of Two Cultures: Causal Mechanisms and Process Tracing

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Our intuitive understandings of causality include a generative process in which a cause yields an effect by triggering the operation of certain mechanisms. When individuals are presented with data suggesting an association between two variables, they routinely want additional information related to mechanisms before declaring the association to be causal in nature (Ahn et al. 1995). Social scientists are no different: they believe that causal effects are transmitted through linking processes of one kind or another.

The large social science and philosophy of science literature that has developed around the idea of a "causal mechanism" encompasses a heterogeneous set of arguments and definitions (see the suggested readings for this essay). For our purposes, we do not need to delve into the complexities of this literature. Instead, for the purposes of this essay, we understand causal mechanism to mean the intervening processes through which causes exert their effects. We propose that any relatively well-developed theory or model will provide a discussion of causal mechanisms. This is equally true for theories tested in the quantitative and qualitative research traditions: They propose ideas about the causal mechanisms that link independent variables to dependent variables.

The key issue we explore in this essay is how the qualitative and quantitative traditions *empirically assess* theories about mechanisms when making causal inferences. In the qualitative culture, researchers carry out this assessment by attempting to observe mechanisms through process tracing and the analysis of causal process observations (Collier, Brady, and Seawright 2010a, 2010b; George and Bennett 2005). Almost by definition, the effort to observe mechanisms requires *within-case* analysis. The case-oriented nature of qualitative research is well suited for looking inside cases in the effort to locate mechanisms.

By contrast, statistical methods are not designed to observe mechanisms within particular cases. Inference using statistics—whether with observational or experimental data—depends on the *cross-case* analysis of many observations. In this tradition, researchers may presume that a given mechanism is at work if a variable exerts its predicted effect in a statistical test. However, they do not normally try to empirically study mechanisms themselves. The reasons why variables exert causal effects are part of the theory, but not usually included in the statistical test.

Mechanisms and Causal Inference

One learns early on that "correlation is not causation" through examples of spurious correlations, such as the association between fertility and the number of storks living in a region. Students are taught in their first methods classes to try to think of third antecedent variables that might cause both variables and thus explain the correlation (e.g., a variable measuring urban versus rural location might explain both number of storks and rate of fertility). When students are first presented with these examples, however, the reason that they suspect the correlation may be spurious usually comes from the absence of intuitive causal mechanisms, not because they believe there is an antecedent variable that explains away the correlation. One is skeptical of the stork–fertility correlation as a causal relationship because there is no plausible mechanism (Porpora 2008).

Within the social sciences, most statistical methodologists assume that causal inference with observational data is extremely difficult. Observational studies lack the random assignment of a controlled experiment, requiring control variables to meet the assumption of conditional independence. We have heard from (typically young) quantitative methodologists that regression is simply data description.[1] It is regarded as—at best—a blunt tool for causal inference (see also Collier, Brady, and Seawright 2010b).

Recognition of the challenges of making causal inferences with observational data has fostered growing interest in experiments. Social scientists are now engaged in experiments of all sorts, including survey experiments, laboratory experiments, and field experiments (experiments have always been used in psychology, of course). In political science, it is now common to see articles in the top journals employing experiments. Even when discussing regression designs, methodologists now often adopt the terminology and logic of experiments, such as treatment and control. For some quantitative methodologists, in fact, the new slogan might be:

No strong causal inference without an experiment.

With a good experiment, one can assess the average effect of a given treatment without observing causal mechanisms. As Green and colleagues (2010: 206–7) put it, "One can learn a great deal of theoretical and practical value simply by manipulating variables and gauging their effects on outcomes, regardless of the causal pathways by which these effects are transmitted."[2] However, in the social sciences—as in all sciences—scholars still want to fill in the black box of experiments if at all possible. When researchers present experimental findings, they routinely have to answer questions concerning the mechanism linking treatment and effect. They work hard to answer, because a well-developed theory identifies the mechanism behind an observed effect.[3]

In the qualitative culture, by contrast, researchers regard the identification of mechanisms as crucial to causal inference. They see mechanisms as a nonexperimental way of distinguishing causal relations from spurious correlations:

> Mechanisms help in causal inference in two ways. The knowledge that there is a mechanism through which *X* influences *Y* supports the inference that *X* is a cause of *Y*. In addition, the absence of a plausible mechanism linking *X* to *Y* gives us a good reason to be suspicious of the relation being a causal one.... Although it may be too strong to say that the specification of mechanisms is always necessary for causal inference, a fully satisfactory social scientific explanation requires that the causal mechanisms be specified. (Hedström and Ylikoski 2010: 54; see also George and Bennett 2005)

One might even say that a norm has developed in the qualitative culture that making a strong causal inference requires processing tracing within individual cases to see if proposed causal mechanisms are present. Thus, for qualitative scholars the slogan might be:

No strong causal inference without process tracing.

The quantitative and qualitative research traditions therefore have different ideas about strong causal inference. Unsurprisingly, they may view each other's standards with some skepticism. For instance, the idea that process tracing provides a strong basis for causal inference is not widely embraced in the quantitative culture. King, Keohane, and Verba (1994) suggest that process tracing is "unlikely to yield strong causal inference" and can only "promote descriptive generalizations and prepare the way for causal inference" (227–28). Other scholars stress that causal mechanisms are not "miracle makers" that resolve fundamental difficulties in causal analysis (e.g., Gerring 2010; Norkus 2005). From a statistical point of view, inferences about causal mechanisms must meet the requirements of good causal inference that apply to any potential treatment or variable. Causal mechanisms do not require a new understanding of causality (King, Keohane, and Verba 1994), though the econometric issues involved in estimating the average effects of causal mechanisms are distinctive (e.g., MacKinnon 2008).

Process tracing can intersect with large-N analyses in various ways (Collier, Brady, and Seawright 2010c). Sometimes the causal mechanism is worked out first in case studies and then large-N analyses are used to confirm the finding. For example, consider Snow's famous study showing that water—not air—

is the mechanism of transmission for cholera (Snow 1965 [1855]; see also Freedman 1991; Dunning 2008; Collier, Brady, and Seawright 2010c).[4] Snow started his research as would a typical qualitative analyst: with the intensive examination of $Y = 1$ cases, i.e., people with cholera. He noted that the causal agent seemed to be something that attacked first the alimentary canal. This would make tainted water or food the likely mechanism of transmission. He made other key observations: Sailors only developed the disease when they landed or took on supplies, the disease followed lines of commerce, and individuals living in buildings with a private water supply were often free from the disease. He carried out a method of difference design using two adjacent apartments, one of which had contaminated water. He did the same with selected individuals. He then convincingly tested the hypothesis with a quasi-experiment that drew on data from a large number of households that received water from different sources. The large-N natural experiment confirmed the causal mechanism that he had developed through qualitative research.

Sometimes one has a large-N statistical finding, usually with observational data, but the causal mechanism is not clear. In this setting, too, the process tracing plays a critical role in confirming the causal mechanism proposed in the large-N theory. For example, a long line of cross-national quantitative studies have found a positive relationship between economic development (usually measured with GDP per capita) and democracy (see Robinson 2006 for a literature review). This relationship is in fact considered one of most robust statistical findings in political science or sociology. Yet, for nearly everyone, the finding seems incomplete because it leaves behind a black box and does not allow scholars to adjudicate among rival theories of mechanisms. For qualitative researchers, this black box must be filled with a close analysis of the actual sequences that lead to democracy in particular cases. One must move from the statistical association to qualitative research aimed at identifying mechanisms before causation can be established.[5]

Rueschemeyer, Stephens, and Stephens (1992) is a good example of the effort to observe causal mechanisms within individual cases using historical research. They propose that development fosters changes in the balance of power among different classes (especially landlords and workers), and that this changed balance of power is a critical mechanism for democracy. More specifically, they hypothesize that development fosters two necessary conditions for full democracy: (1) the absence of powerful landlords, and (2) the presence of strong, prodemocratic working classes.[6] Although these factors are nearly universal mechanisms, they are not sufficient conditions; democratization depended on other factors related to the state, political parties, and international system.

In the above examples process tracing is used to verify a posited causal mechanism or to identify the causal mechanism behind large-N results. From the qualitative perspective, the critical thing is that process tracing occurred at some point in the research process. The investigator looked inside the black box by actually observing intermediary processes within specific cases.

However, when there is no process tracing, qualitative scholars are often skeptical. They need convincing from the process tracing of individual cases to believe that large-N findings are really causal. It is not hard to find examples where the intensive examination of individual cases leads to doubts about hypothesized causal mechanisms in statistical or formal analyses:

> 1. Cusack, Iverson, and Soskice (2007) argue that the economic preferences of business and labor are the key mechanisms linking coordinated labor markets to proportional representation electoral systems in West Europe. These authors find a significant statistical relationship between labor market coordination and proportional representation systems. However, they do not examine the institutional preferences of business and labor that are hypothesized to drive this relationship. Kruezer (2010) scrutinizes this argument by examining whether historical research provides any evidence that these actors cared about the form of electoral systems. After looking at each of Cusack, Iverson, and Soskice's 18 cases, he concludes that their proposed mechanism is not operating: "I was unable to find any evidence linking the institutional preferences of business organizations, unions, parties, or their respective leaders to labor markets. As a matter of fact, I was unable to find any evidence that business or unions explicitly preferred one electoral system over another. There is plenty of discussion of parties' institutional preferences, but none of it points to economic factors" (376).[7]
>
> 2. Using cross-national statistical analysis, Collier and Hoeffler (2001) and Fearon and Laitin (2003) find that there is a strong negative relationship between GDP per capita and civil war. The two sets of authors disagree, however, about the causal mechanism: Collier and Hoeffler understand the mechanism in terms of the effects of poverty on economic opportunities, whereas Fearon and Laitin view the mechanism in terms of the capacity of the state to prevent civil war. Based on case-study evidence, Sambanis (2004) finds only marginal support for either mechanism. He proposes that GDP per capita likely exerts its effect in interaction with other variables: "[T]he reason that countries have different proclivities to civil war might have more to do with the way other independent variables, such as ethnicity and democracy, behave at various levels of income" (266). He suggests that the lack of empirical support for the mechanisms proposed in the theories by Collier and Hoeffler and by Fearon and Laitin calls into question their practical utility: "If large-N studies make incorrect assumptions about causal paths, they will lack explanatory power.... We know that by increasing GDP per capita, we will *somehow* reduce the risk of civil war, but a more targeted policy intervention might be both more effective and easier to implement" (273).
>
> 3. Acemoglu and Robinson (2006) show that military coups are more likely in countries with higher levels of economic inequality. Using game theory, they identify a mechanism to explain this relationship: the amount of redistribution

> under democracy will be higher in unequal societies, and thus elites have greater incentive to enlist the military to overthrow the democracy. Slater and Smith (2010), however, criticize this explanation by using case study evidence that shows "militaries are virtually never the agents and very rarely the allies of economic elites." Militaries normally carry out coups for reasons that have nothing to do with the specific economic interests of elite classes. Hence, they argue that the causal mechanism associated with Acemoglu and Robinson's game theoretic model is not present in the vast majority of military coups.

In short, qualitative researchers often view skeptically experimental and nonexperimental analyses that fail to show the causal mechanism at work in individual cases. In the qualitative culture, one cannot have a strong explanation if all mechanisms are left as unopened black boxes.

Process Tracing in Qualitative vs. Multimethod Research

Especially with the rise of "multimethod" work, process tracing is no longer the exclusive domain of qualitative research. Among quantitative researchers in some fields, it has become *de rigueur* to include individual case studies in the overall analysis. This trend is related to the downgrading of regression analysis that we discussed earlier. The intensive process tracing of selected cases is seen as a complement to large-N research in contexts where experiments are impossible. Regression results provide some evidence that a postulated causal mechanism is at work in a large population of cases. Process tracing in selected individual cases is then used to explore whether the causal mechanism works as advertised.[8] This multimethod strategy is very common in many prominent recent works. Scholars first present large-N statistical results and then follow them up with analyses of individual case studies (e.g., Fortna 2007; Lange 2009; Lieberman 2003; Pevehouse 2005).[9]

Despite the convergence on process tracing as a useful tool of causal inference, the specific way in which the method is conducted varies across the two cultures. These differences are closely related to the dominant causal models of the two traditions. In the qualitative tradition, scholars adopt a logic/set theory approach to causation when conducting process tracing within cases. This comes naturally because, with set-theoretic causes, particular cases within a population follow the same causal pattern that applies to the population as a whole. When carrying out process tracing, one treats the cause in the individual case as having the same effect as in the model for the whole population.

This stability from the population to the cases is most easily seen with necessary conditions. If A is a necessary condition for Y within a population, then it must be a necessary condition for any individual case (or subset of cases) from that population. For a substantive example, consider the hypothesis that an authoritarian regime is necessary for genocide. If valid, this hypothesis will remain true for any case of genocide. One can carry out process tracing under the assumption that the hypothesis should consistently work across all genocides.

Stability from the population to the cases also applies to sufficiency in a causal model, such as the following Boolean equation: $Y = ABc + DE$. If a case has either combination (i.e., either $A * B * c$ or $D * E$), then it will have the outcome of interest. Process tracing to investigate mechanisms would choose a case where either ABc or DE is present, but not both, to avoid ambiguity of causal mechanism. The analyst would then explore how, say, A, B, and c *interact* to produce the outcome. The mechanism would be understood in terms of how these variables work together to produce the phenomenon of interest. These variables are not controls but rather parts of an interactive package that generates the outcome.

When an analyst conducts process tracing on a causal combination in a particular case (assuming that the case exhibits only one combination that generates the outcome), he or she can treat each of the individual variables in that combination as necessary for the case to experience the outcome. That is, if the case of $Y = 1$ exhibits only one causal combination, then each of the individual causal factors of the combination is essential for the outcome to occur in that one case. For instance, if a case with the combination $D * E$ lacked either D or E, then it should not experience the outcome if the model is correct. Hence, one can normally "process trace" the individual variables of causal combinations in particular cases in the same way as necessary conditions.

One upshot is that process tracing in qualitative research often involves the analysis of necessary conditions and the assessment of counterfactuals for individual causal factors.[10] If one believes that A is a necessary condition or an INUS condition for Y, one normally asks if Y would have still happened in the absence of A in case studies. Likewise, when identifying the mechanisms linking the necessary condition A to Y, one looks for other conditions—i.e., the interacting variables of a sufficiency combination—that worked with A to generate the outcome. When analyzing causes that are jointly sufficient for an outcome, one stresses the interaction between variables: The mechanism is closely related to how these variables work together as a causal package.

Turning now to multimethod research, process tracing in individual cases is complicated by the fact that, with statistical analysis, causal processes are not necessarily consistent as one moves from the population to the individual case. For instance, imagine that in a statistical study the impact of X_1 is strongly positive in the population. Does this mean that X_1 cannot have a strongly negative impact for a particular subset of cases? The answer, of course, is "no." The impact of X_1 as one moves from a superset to subsets to particular cases is always contingent in statistical models; there is no mathematical reason why X_1 could not be negatively related to the outcome in particular subsets, i.e., the stability of parameter estimates is a contingent phenomenon. Thus, when carrying out process tracing, one cannot be certain that causal mechanisms will operate as expected in randomly selected particular cases.[11]

In multimethod research, the analyst usually starts with a statistical analysis that reaches significant results for a variable of interest. In order to carry out process tracing, the analyst then tries to select one or more cases where this variable should play the role that the theory assigns to it. Perhaps because of the instability of findings as one moves from the population to subset of cases, however, multimethod researchers virtually never make a direct link between the data set value for the individual case, the parameter estimate in the statistical model, and the observations from the individual case study. When multimethod researchers carry out process tracing in particular cases, the specific regression parameter estimates tend to drop out of the picture and one retains only the sign and whether the variable is statistically significant or not.

Multimethod researchers informally extend the statistical causal model associated with quantitative research to the individual case analysis when conducting process tracing.[12] In the additive model used for the whole population, there are multiple causes, and the main variable of interest is just one of many. The effect of this variable is understood roughly as a causal weight or contributing factor for the dependent variable. Consequently, when conducting process tracing on that variable in a single case, the multimethod researcher explores how it "contributed to" or "added weight" in favor of the outcome. However, the multimethod analyst does not ordinarily view the individual variable as necessary for the outcome. Consequently, process tracing in multimethod research is not built around counterfactuals in the same way that is true of process tracing in qualitative research.

Another distinctive feature of process tracing in multimethod research concerns the role of variables other than the main one of interest (i.e., the control variables in the causal model). Since these control variables are not of special interest, they are not ordinarily emphasized in the process tracing analysis. The effects of the control variables must be acknowledged, but the attention is directed at the main variable of concern. By contrast, in qualitative research, each variable in an INUS model is considered important, and thus the overall package of variables must be analyzed carefully when doing process tracing. The only time process tracing might take a similar form in multimethod research would be if main effect of interest was an interaction term. However, we do not know of examples of multimethod research in which the analyst uses process tracing to validiate the posited mechanisms behind an interaction term.

In sum, each culture tends to remain true to its causal model when conducting process tracing in individual cases. Qualitative researchers apply a set-theoretic model based on necessary conditions and packages of interacting conditions that are jointly sufficient to the individual cases. As a general rule, one can identify a qualitative approach to process tracing by asking whether the analyst treats causes as necessary conditions when conducting process tracing for specific cases, and asks about the mechanism linking the interacting conditions when discussing sufficiency. By contrast, multimethod researchers normally adopt an additive approach to causality when conducting process tracing within particular cases. They explore through process tracing whether the individual factor of interest contributed to or added weight in favor of a specific outcome in a particular case. Because so many other causes are assumed to matter, they do not make the assumption that the factor of interest was necessary for the outcome.

Conclusion

In the qualitative culture, it is standard and natural to study causal mechanisms and to use process tracing for case studies. One draws the inference that *X* is a cause of *Y* in part by tracing the process that leads from *X* to *Y* within one or more specific cases. This process tracing is facilitated by the set-theoretic understanding of causality that characterizes this tradition. Because causes are treated as necessary and/or jointly sufficient for outcomes, researchers can employ counterfactual analysis and use method of difference comparisons of adjacent time periods when evaluating them with case studies.

In the quantitative culture, growing concerns about the ability of regression analysis to generate strong causal inference has pushed research in two directions. On the one hand, experiments of various kinds are increasingly common. With an experiment, one can do a good job of estimating the average effect of a treatment without testing for mechanisms. Yet, since nearly all social science theories propose ideas about mechanisms, the black box left behind by experimental research can be viewed as problematic.

On the other hand, multimethod research in which quantitative analysts combine regression with case study analysis is also increasingly common. A variable that exerts a significant effect in a regression analysis is further examined with case studies to determine whether it works in ways posited by the theory being tested. Unlike in the qualitative tradition, however, the use of process tracing in multimethod research usually sees causes in light of an additive model. There is less interest in specific causal configurations and interactions between variables; and the researcher does not usually ask if *X* was necessary for *Y*. The main goal is to show that *X* made a contribution to *Y*.

Notes

[1] "Without an experiment, a natural experiment, a discontinuity, or some other strong design, no amount of econometric or statistical modeling can make the move from correlation to causation persuasive. This conclusion has implications for the kind of causal questions we are able to answer with some rigor. Clear, manipulable treatments and rigorous designs are essential. And the only designs I know of that can be mass produced with relative success rely on random assignment. Rigorous observational studies are important and needed. But I do not know how to mass produce them." (Sekhon 2009: 503)

[2] The enterprise of studying mediators using experiments faces many difficulties (see Bullock and Ha, forthcoming). Likewise, statistical techniques to assess mediators with observational data require very strong assumptions and are hard to carry out in practice (e.g., Imai et al. 2010).

[3] In addition, while experiments solve some problems of observational research, they have their own problems. Good discussions of the promises and pitfalls of experiments are found in Druckman et al. (forthcoming) and Morton and Williams (2010).

[4] This example is at the center of a debate between Brady, Collier, and Seawright (2006), Collier, Brady, and Seawright (2010c) and Beck (2006, 2010).

[5] As Rueschemeyer, Stephens, and Stephens (1992: 4) put it: "The repeated statistical finding [of a relationship between development

and democracy] has a peculiar 'black box' character that can be overcome only by theoretically well grounded empirical analysis.... Comparative historical studies, we argue, carry the best promise of shedding light into the black box.... Historical research gives *insight into sequences* and their relations to surrounding structural conditions, and that is indispensable for developing valid causal accounts. Causal analysis is inherently sequence analysis."

[6] "Democracy could only be established if (1) landlords were an insignificant force, or (2) they were not dependent on a large supply of cheap labor, or (3) they did not control the state" (270). "The organized working class appeared as a key actor in the development of full democracy almost everywhere, the only exception being the few cases of agrarian democracy in some of the small-holding countries" (270).

[7] In their rebuttal, Cusack, Iverson, and Soskice (2010) argue that party leaders were identified with economic interests, and thus one would not expect labor and business leaders to actively push for a particular electoral system. Instead, they suggest that party preferences should be the focus of tests concerning the causal mechanism.

[8] As Collier, Brady, and Seawright (2010a, 197–98) put it, "important gaps in causal inference based on quantitative analysis of data-set observations can be filled by evidence derived from qualitative, causal-process observations."

[9] Some scholars working in the tradition of game theory have also turned to process tracing as a means of testing the observable implications of their formal models, e.g., Bates et al. (1998).

[10] For a more skeptical view on the role of counterfactuals in process tracing, see George and Bennett (2005: 230–31).

[11] One response by multimethod researchers has been to think carefully about how to best select cases for process tracing in light of preliminary regression results.

[12] Of course, there will always be counterexamples. A particularly interesting one is Brady's (2010) analysis of the 2000 presidential election in Florida. He uses a set-theoretic argument to refute a large-N quantitative analysis. We leave it as an exercise for the reader to show the set-theoretic nature of his argument.

Suggested Readings and References

The literature on causal mechanisms embodies many different understandings and definitions of causal mechanism. For various views, see Bunge (1997); Elster (1989); Falleti and Lynch (2009); Gerring (2008); Hedström and Swedberg (1998); Mahoney (2001); Mayntz (2004); McAdam, Tarrow, and Tilly (2001); Norkus (2005); and Stinchcombe (1991). On the method of process tracing, see George and Bennett (2005); Hall (2003); and Roberts (1996). The concept of a causal-process observation is developed by Collier, Brady, and Seawright (2010a). On multimethod research, see Gerring (2011) and Lieberman (2005). On the use of experiments and statistical methods to study mechanisms, see Bullock and Ha (forthcoming); Glynn and Quinn (2009); Green, Ha, and Bullock (2010); Holland (1988); Imai et al. (2010); MacKinnon (2008).

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What are Mechanisms and What are They Good For?

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Almost a century ago, Bertrand Russell called the idea of causality "a relic of a bygone era, surviving, like the monarchy, only because it is erroneously supposed to do no harm."[1] Into the 1980s, widely read statements of the philosophy and methodology of political science omitted any reference to causality. But in the past quarter-century, causality has made a comeback. Gary Goertz and James Mahoney begin their essay, "Causal Mechanisms and Process Tracing," by stating that we all share an intuitive mechanism-based understanding of causation. The ubiquity of concepts like causal inference, causal effects, causal processes and pathways, and causal mechanisms in contemporary discourse supports their claim; recent research in cognitive psychology, summarized by Sloman (2005), affirms it as well. But perhaps this apparent consensus masks some fundamental disagreements. For example, Jasjeet Sekhon emphatically claims that we have over-inflated the importance of mechanisms, as "We do not need to have much or any knowledge about mechanisms in order to know that a causal relationship exists" (Sekhon 2008: 292). We may have strong grounds, Sekhon insists, for believing that an intervention has great therapeutic value without any understanding, intuitively or theoretically, about the underlying mechanisms. I believe this claim is correct; while mechanisms may be useful for causal inference (George and Bennett 2005; Waldner 2007; Hedström 2008), we can certainly make causal inferences without reference to causal mechanisms and without using process tracing. I thus disagree with the slogan Goertz and Mahoney coin for qualitative researchers, "No strong causal inference without process tracing."

This essay expresses dissent of a different kind. It shifts the emphasis from the role mechanisms can play in causal inference to the function mechanisms perform in providing causal explanations. Inferences are not adequate explanations; they are invitations to provide the explanatory adequacy that mechanisms provide. But while mechanisms explain, the definition of mechanism held by many social scientists impedes this explanatory function. Too often, causal mechanisms are equated to causal pathways or causal processes; I argue below that this equivalence is incorrect (see also Steel 2008; Russo 2009). Equating mechanisms to pathways and processes substantially underdetermines the meaning of mechanism, such that many claims about mechanisms are not really about mechanisms at all and thus provide only limited explanatory value-added. In contrast to this very expansive usage of the term mechanism, I would like to defend a relatively narrow definition of mechanism.

Scholars turned their attention to causal mechanisms to understand the process generating observed associations. Goertz and Mahoney share what I believe to be the conventional wisdom in defining mechanisms as pathways in a causal diagram. They define causal mechanisms as "the intervening processes through which causes exert their effects." John Gerring (2008: 163) also adopts the language of a causal model or graph, giving the core meaning of mechanism as "the causal pathway, or process or intermediate variable by which a causal factor of theoretical interest is thought to affect an outcome. Thus: $X_1 \rightarrow X_2 \rightarrow Y$, where X_1 is the exogenous cause, X_2 the pathway(s), and Y the outcome."

Recent developments in causal graphical models clarify the problem with equating causal mechanisms to intervening variables or pathways. Figure 1 depicts a directed acyclic graph, or DAG.

Figure 1: A Directed Acyclic Graph

$$X_1 \rightarrow X_2 \rightarrow X_3$$

A graphical model is fully described by a set of vertices, V, and a set of edges, E. Vertices represent random variables, edges represent relations of probabilistic dependence between adjacent nodes on a pathway. From these basic ingredients, a causal graph is constructed by representing direct causal relationships with an arrowhead, such that edges have a direction. A causal graph is acyclic if it contains no directed path that begins and ends at the same node. That the DAG depicted in Figure 1 omits the edge $\{X_1, X_3\}$ tells us that X_3 is independent of X_1 conditional on X_2, denoted:

$$X_3 \perp\!\!\!\perp X_1 \mid X_2 \tag{1}$$

Were conditional independence not true, we would have a different DAG, as in Figure 2.

Figure 2: An Alternative Directed Acyclic Graph

$$\begin{tikzcd} X_1 \arrow[r] \arrow[rr, bend left] & X_2 \arrow[r] & X_3 \end{tikzcd}$$

But note that the exact same information about conditional independence is conveyed by Figure 3, called simply a graph, or an independence graph, and hence omitting arrowheads denoting direction. Figure 3 also tells us that X_2 screens off X_1 from X_3, making them conditionally independent. But in the independence graph, causation is not inferred from relations of probabilistic dependence.

Figure 3: An Independence Graph

$$X_1 — X_2 — X_3$$

The translation from association to causation occurs by two assumptions, the causal Markov condition and the faithfulness condition.[2] These assumptions basically imply that the DAG is a properly specified model and there are no hidden confounders. Otherwise, the two graphs convey exactly the same information. Edwards (1995: 192) thus states that "In some cases it is a matter of taste whether a given model should be thought of as directed or undirected." Pearl (2000: 21–22) prefers a pragmatic defense of the causal interpretation. If we adopt the causal interpretation, then we can use DAGs to derive valid conditioning strategies (Morgan and Winship 2007).

The key point, for our purposes, is that DAGs convey information about probability distributions generated by mechanisms, but they give no direct information about mechanisms themselves. The causal pathway represented by X_2 does not explain the correlation between X_1 and X_3. It screens off that correlation, so that the marginal association becomes conditional independence. We are still in the world of statistical dependencies, without any clear insight into the nuts and bolts of the causal relations; any substantive causal interpretation comes from outside of the model, not from knowledge of the causal mechanisms.

It is certainly true that knowledge of pathways is important, perhaps crucial; a model that contains X_2 is preferable to a model that excludes X_2. But it seems odd to assert that there are mechanisms generating a probabilistic relationship between the nodes of a graph and simultaneously to refer to an intervening node as a mechanism. Note as well that every time we insert an additional node into a causal pathway—adding, for example, X_4 between X_1 and X_2—we raise a new question about the underlying causal mechanism. This is not the old complaint about infinite regress; causal models contain a finite number of vertices and hence a final number of mechanisms. Rather, the point is to distinguish mechanisms from variables. Crucially, mechanisms embody *invariance*.

Take the concrete example depicted in Figure 4. Wondering why depressing the gas pedal is correlated with the car moving faster, we flesh out the causal pathway: The drivetrain rotates at a faster rate. The middle node screens off the root and terminal nodes: Once we know that the drivetrain is rotating faster, we know that the car must be accelerating, independent of any knowledge about the state of the gas pedal.

Figure 4: Adding Variables to a Causal Model

Depress Gas Pedal → Car Accelerates

Depress Gas Pedal → Drivetrain Rotates → Car Accelerates

Have we explained the relationship between the gas pedal and the rate of acceleration? Yes, in the restricted sense that we have additional information about how a car works. But no, because by adding a new variable, we have added another mechanism, represented by the second arrow. So now we have two questions: Why does depressing the gas pedal make the drivetrain rotate faster, and why does that make the car accelerate? So there appears to be a difference between adding nodes to a causal model, on the one hand, and *explaining the relationship between existing nodes*, on the other hand. Adding more nodes only increases the number of questions we must answer, and so has limited, but not negligible, explanatory value.

If we do not fully explain by adding more variables, how do we explain? Mechanisms explain because they embody an *invariant property*. The first mechanism, linking the gas pedal to the rotating drivetrain, is combustion; the second mechanism, linking the rotating drivetrain to acceleration, is the relationship of torque to force. Combustion is a high-energy initiated, exothermic (heat-generating) chemical reaction between a compound such as a hydrocarbon and an oxidant, such as oxygen. The heat generated by combustion increases pressure in a sealed cylinder and impels a piston. A similarly brief description could be given of the relationship between torque, force, and acceleration. The key point is this: Combustion is not a variable. In the proper circumstances—the combination of specific compounds and oxidizing agents, with a high-energy initiation to overcome the stability of dioxygen molecules—combustion occurs with law-like regularity. That regularity can in turn be explained by more fine-grained physical processes at the sub-atomic level.[3]

By saying that a mechanism like combustion is invariant, not a variable, I am stating that it cannot be directly manipulated; one cannot intervene to turn combustion off.[4] It will be very tempting to reduce combustion to the status of a variable. To get combustion, one needs fuel, oxygen, and an ignition system, so we can add each of these elements to the causal model and argue that they are individually necessary and jointly sufficient for combustion. Combustion is just a dependent variable and we can manipulate its presence by removing one of the three necessary conditions. In the Boolean notation favored by Goertz and Mahoney, with capital letters denoting presence and lower-case letters denoting absence, we have the causal combination of fuel, oxygen, and ignition causing combustion,

$$\mathrm{FOI} = \mathrm{C} \quad \text{and} \quad \mathrm{fOI} = \mathrm{c} \qquad (2)$$

But that misses the point entirely, for two reasons. First, the original causal model assumed the car had fuel and a functioning ignition system; that's why depressing the gas pedal caused acceleration. We do not explain the original causal model by removing one of its presuppositions; rather, removing an antecedent cause modifies the causal model. Second, we need to distinguish between blocking a mechanism from working and intervening to set the value of a mechanism; the former is possible, the latter is impossible. For example, if we drain all the fuel from an engine, there will be no combustion. But an exogenous intervention on a variable consists of directly setting the value of the variable, eliminating all other infuences on that variable while leaving the rest of the causal model intact. A valuable property of DAGs is that they represent these ideal, exogneous interventions. Pearl (2000) has introduced the *do* operator to model interventions. Whereas passive observation calculates the conditional probability, $P(Y = y \mid X = x)$, the *do* operator calculates the effect of exogenous interventions, $P(Y = y \mid \text{set } X = x)$. An intervention is modeled with a surgery that creates a new, mutilated graph, eliminating all arrows leading into X. To treat a mechanism like combustion as a variable on which we could directly intervene implies Figure 5.

Figure 5: An Implausible Intervention on a Mechanism

Fuel and Oxygen = Present

Gas Pedal = Depressed   Set Combustion = Off

Ignition = Present

Figure 5 removes all the arrows leading into combustion, representing an exogenous intervention to set combustion = off. But Figure 5 is weird because one of these things is not like the others. The three nodes on the left represent variables; an ignition system can function or malfunction; fuel can be present or absent, etc. But combustion, as a mechanism embodying an invariant principle, cannot be set on or off. If a fuel-air mixture is combined with a functioning ignition system, combustion occurs, and so combustion should not be represented in the causal graph. You can alter the *rate* of combustion; that is what depressing the gas pedal does. But you cannot set the mechanism to a different value. Mechanisms embody invariant causal principles and, in particular instantiations, they generate observed correlations. Mechanisms are thus distinct from causal pathways; they explain the location of the directed edges (arrowheads). Returning to the original causal model in Figure 4, it is the invariant causal principle combustion that explains why (given a few more mechanical details) depressing the gas pedal makes the drivetrain rotate. Mechanisms explain the relationship between variables because they are not variables.

Discussing social mechanisms adds complexity, because there is much debate about the inventory of mechanisms, such as debates between rationalists and constructivists, and because there is debate whether mechanisms require micro-level reductionism. But social mechanisms also embody invariance. For example, intentionality is an invariant principle, taken to be a constant feature of human consciousness. Intentionality means, informally, that consciousness is directed upon an object, that humans have desires about objects, and that agency pursues these desires. We can easily intervene to shape specific preferences; subsidizing an object increases demand for that object. But that intervention, like making fuel present or absent, is on a variable; the mechanism, here intentionality, is an invariant property. Alternatively, think of education as a variable, but certainly not a mechanism, in the terms I am using. Education itself causes nothing; rather, the cause is the event "getting an education," which alters some property of an intentional agent, for example by altering her resource endowment or changing her identity and preferences and hence shaping the intentional actions that follow. Similarly, interpretivists would argue that the search for meaning is an invariant property, one which is differently instantiated in different contexts.

Let us then define a causal mechanism as an agent or entity that has the capacity to alter its environment because it possesses an invariant property that, in specific contexts, transmits force, information, or meaning. I am insisting on a distinction between causal mechanisms and a group of terms we use for intervening variables, such as causal factor, causal pathway, or causal inference and causal explanation. Causal inference is the activity by which we determine membership in the set of vertices (causal variables included in the model) and the set of edges (relations of direct causation between two variables). Causal inference is how we construct or confirm causal graphical models, determining which nodes must be included, which nodes can be excluded, and along which potential paths to place arrows. Information about causal pathways can help us decide that one of several hypothetical causal models is the correct one or that a hypothesized causal relationship is in fact spurious. Causal explanation, on the other hand, explains *why* the arrows exist on some paths but not others, and *how* probabilistic dependencies and conditional independencies are generated. It gives us causal knowledge of how things work.

Consistent with this claim that causal inference is distinct and often distant from causal explanation, experimental work in political science generates valid inferences without identifying causal mechanisms. Experiments are ideal interventions, represented by mutilated graphs. But they give little insight into underlying mechanisms and hence neglect causal explanation. For example, Hyde (2007) reports the findings of a natural experiment that randomly assigns international observers to precincts in the 2003 Armenian presidential elections. International monitors, Hyde concludes, decrease electoral fraud. But this estimation of a causal effect gives no insight into mechanisms. Hyde makes plausible speculations, but acknowledges that the precise mechanisms are unknown. Similarly, Wantchekon (2003) reports the findings of a natural experiment studying the effect of clientelist versus public policy mobilization strategies. But Wantchekon too is forced to use qualitative reasoning external to the experiment to speculate about plausible mechanisms. Experiments generate reliable causal models, but they invite explanation, they do not provide explanations.

The distinction between causal pathways and causal mechanisms is probably easier to maintain than the distinction between explanation and inference. The term explanation is connotatively rich; there is surely more than one way to explain. Causal inferences that support counterfactual interventions have explanatory relevance. Put different, it is not explanatorily irrelevant to know that depressing the gas pedal causes the car to accelerate. "We have at least the *beginnings of an explanation*," James Woodward (2003: 10) claims, "when we have identified factors or conditions such that manipulations or changes in those factors or conditions will produce changes in the outcome being explained." Woodward makes a very reasonable point, but there is a reason as well why I emphasize the phrase "beginnings of an explanation." If intervention-supporting knowledge begins an explanation, mechanism-identifying knowledge completes the explanation. Thus, for proponents of process tracing, the distinction between a causal pathway and a causal mechanism imposes an obligation to not claim too much. Identifying causal pathways is a critical ingredient of science. But the identification of mechanisms has been celebrated as going one step further, as adding deep explanatory knowledge (Salmon 1998). Those who identify processes but not mechanisms should take great pains to demarcate properly their explanatory accomplishments. For skeptics, there are obligations as well. It would be inappropriate for a skeptic to reject the mechanism project by defining mechanisms as no more than intervening variables.

Having tried to articulate the meaning and value of mechanisms, let's return to Sekhon's critique of mechanisms cited above; if our goal is exclusively to model the results of an intervention, knowledge of mechanisms is emphatically *not* necessary. There are very specific contexts in which mechanisms can aid inference, confirming some hypotheses and disconfirming others; but, as long as we understand causal inference to mean something distinct from causal explanation, there are abundant non-mechanistic tools for causal inference. But two caveats remain necessary. First, there is more to social science than policy evaluation. Sometimes we genuinely want to know what makes things work; we want mechanism-based explanations, even if they have limited relevance for contemporary interventions. Second, we can in fact learn how to intervene more effectively through knowledge about mechanisms. Historical examples about the discovery of aspirin and penicillin are not the full story about modern pharmaceuticals, after all. Gene therapy, for example, depends on knowledge of mutant and functional alleles.

There are methodological implications as well, although I can only point in their direction here. Most importantly, I would not endorse Goertz and Mahoney's conception of process tracing because their account, I believe, obscures the critical distinction between variables and mechanisms. In their view, causation is treated set-theoretically, with the potential for multiple causal combinations to be sufficient for an outcome, as in $Y = ABc + DE$. Process tracing then involves exploring how these variables interact to produce the outcome. Mechanisms, as I have defined them, receive little attention, as when they write:

> [W]hen identifying the mechanisms linking the necessary condition *A* to *Y*, one looks for other conditions—i.e., the interaction variables of a sufficiency combination—that

> worked with *A* to generate the outcome. When analyzing causes that are jointly sufficient for an outcome, one stresses the interaction between variables: the mechanism is closely related to how these variables work together as a causal package.

It is certainly possible to blend their conception of causal combinations or factors with my conception of causal mechanisms as embodying invariant causal principles. Doing so would require dropping the idea that mechanisms are intervening processes; and it would require new methodological strategies which I describe elsewhere as concatenation (Waldner 2011).

Notes

[1] Portions of this essay are derived from Waldner (2011). I thank Gary Goertz and James Mahoney for graciously inviting me to comment on their essay, and I thank Andrew Bennett, John Gerring, Gary Goertz, and James Mahoney for very helpful comments on this essay.

[2] The Markov condition states, informally, that a random variable is independent of all variables other than its descendants, conditional on its parents.

[3] The last sentence does not imply infinite regress; a finite number of steps lead from the observed macrostructural world to the subatomic world.

[4] Invariance is not equivalent to non-manipulability by an experimenter; a variable might take on multiple values but still be non-manipulable. This point is emphasized by many contributors to the literature on potential outcomes (See, for example, Holland 1986).

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Book Notes

Book descriptions are excerpted from publishers' websites. If you would like to recommend a book to be included in this section, email Joshua C. Yesnowitz, the production editor of QMMR, *at jcyesnow@bu.edu.*

Brady, Henry E. and David Collier. 2010. *Rethinking Social Inquiry: Diverse Tools, Shared Standards*, 2nd ed. Lanham, MD: Rowman & Littlefield Publishers.

With innovative new chapters on process tracing, regression analysis, and natural experiments, the second edition of *Rethinking Social Inquiry* further extends the reach of this path-breaking book. The original debate with King, Keohane, and Verba—now updated—remains central to the volume, and the new material illuminates evolving discussions of essential methodological tools. Thus, process tracing is often invoked as fundamental to qualitative analysis, but is rarely applied with precision. Pitfalls of regression analysis are sometimes noted, but often are inadequately examined. And the complex assumptions and trade-offs of natural experiments are poorly understood. The second edition extends the methodological horizon through exploring these critical tools. A distinctive feature of this edition is the online placement of four chapters from the prior edition, all focused on the dialogue with King, Keohane, and Verba. Also posted online are exercises for teaching process tracing. This updated edition breaks new ground with an introduction that scrutinizes the latest trends in methodology, and with innovative chapters on process tracing, regression analysis, and natural experiments.

Carlson, Allen, Mary E. Gallagher, Kenneth Lieberthal, and Melanie Manion, eds. 2010. *Contemporary Chinese Politics: New Sources, Methods, and Field Strategies*. Cambridge: Cambridge University Press.

Contemporary Chinese Politics: Sources, Methods, and Field Strategies considers how new and diverse sources and methods are changing the study of Chinese politics. Contributors spanning three generations in China studies place their distinct qualitative and quantitative methodological approaches in the framework of the discipline and point to challenges or opportunities (or both) of adapting new sources and methods to the study of contemporary China. How can we more effectively use new sources and methods of data collection? How can we better integrate the study of Chinese politics into the discipline of political science, to the betterment of both? How can we more appropriately manage the logistical and ethical problems of doing political research in the challenging Chinese environment? In addressing these questions, this comprehensive methodological survey will be of immense interest to graduate students heading into the field for the first time and experienced scholars looking to keep abreast of the state of the art in the study of Chinese politics.

Lebow, Richard Ned. 2010. *Forbidden Fruit: Counterfactuals and International Relations.* Princeton: Princeton University Press.

Could World War I have been averted if Franz Ferdinand and his wife hadn't been murdered by Serbian nationalists in 1914? What if Ronald Reagan had been killed by Hinckley's bullet? Would the Cold War have ended as it did? In *Forbidden Fruit*, Richard Ned Lebow develops protocols for conducting robust counterfactual thought experiments and uses them to probe the causes and contingency of transformative international developments like World War I and the end of the Cold War. He uses experiments, surveys, and a short story to explore why policymakers, historians, and international relations scholars are so resistant to the contingency and indeterminism inherent in open-ended, nonlinear systems. Most controversially, Lebow argues that the difference between counterfactual and so-called factual arguments is misleading, as both can be evidence-rich and logically persuasive. A must-read for social scientists, *Forbidden Fruit* also examines the binary between fact and fiction and the use of counterfactuals in fictional works like Philip Roth's *The Plot Against America* to understand complex causation and its implications for who we are and what we think makes the social world work.

Rueschemyer, Dietrich. 2009. *Usable Theory: Analytic Tools for Social and Political Research.* Princeton: Princeton University Press.

The project of twentieth-century sociology and political science—to create predictive scientific theory—resulted in few full-scale theories that can be taken off the shelf and successfully applied to empirical puzzles. Yet focused "theory frames" that formulate problems and point to relevant causal factors and conditions have produced vibrant, insightful, and analytically oriented empirical research. While theory frames alone cannot offer explanation or prediction, they guide empirical theory formation and give direction to inferences from empirical evidence. They are also responsible for much of the progress in the social sciences. In *Usable Theory*, distinguished sociologist Dietrich Rueschemeyer shows graduate students and researchers how to construct theory frames and use them to develop valid empirical hypotheses in the course of empirical social and political research. Combining new ideas as well as analytic tools derived from classic and recent theoretical traditions, the book enlarges the rationalist model of action by focusing on knowledge, norms, preferences, and emotions, and it discusses larger social formations that shape elementary forms of action. Throughout, *Usable Theory* seeks to mobilize the implicit theoretical social knowledge used in everyday life.

Article Notes

Beck, Nathaniel. 2010. "Causal Process 'Observation': Oxymoron or (Fine) Old Wine." *Political Analysis* 18:4, 499–505.

The issue of how qualitative and quantitative information can be used together is critical. Brady, Collier, and Seawright (BCS) have argued that "causal process observations" can be adjoined to "data set observations." This implies that qualitative methods can be used to add information to quantitative data sets. In a symposium in *Political Analysis*, I argued that such qualitative information cannot be adjoined in any meaningful way to quantitative data sets. In that symposium, the original authors offered several defenses, but, in the end, BCS can be seen as recommending good, but hopefully standard, research design practices that are normally thought of as central in the quantitative arena. It is good that BCS remind us that no amount of fancy statistics can save a bad research design.

Collier, David, Henry Brady, and Jason Seawright. 2010. "Outdated Views of Qualitative Methods: Time to Move On." *Political Analysis* 18:4, 506–513.

Both qualitative and quantitative research routinely fall short, producing misleading causal inferences. Because these weaknesses are in part different, we are convinced that multi-method strategies are productive. Each approach can provide additional leverage that helps address shortcomings of the other. This position is quite distinct from that of Beck, who believes that the two types of analysis cannot be adjoined. We review examples of adjoining that Beck dismisses, based on what we see as his outdated view of qualitative methods. By contrast, we show that these examples demonstrate how qualitative and quantitative analysis can work together.

Falleti, Tulia G. and Julia F. Lynch. 2009. "Context and Causal Mechanisms in Political Analysis." *Comparative Political Studies* 42:9 (September), 1143–1166.

Political scientists largely agree that causal mechanisms are crucial to understanding causation. Recent advances in qualitative and quantitative methodology suggest that causal explanations must be contextually bounded. Yet the relationship between context and mechanisms and this relationship's importance for causation are not well understood. This study defines causal mechanisms as portable concepts that explain how and why a hypothesized cause, in a given context, contributes to a particular outcome. In turn, it defines context as the relevant aspects of a setting in which an array of initial conditions leads to an outcome of a defined scope and meaning via causal mechanisms. Drawing from these definitions is the argument that credible causal explanation can occur if and only if researchers are attentive to the interaction between causal mechanisms and context, regardless of whether the methods employed are small-sample, formal, statistical, or interpretive.

Friedrichs, Jörg and Friedrich Kratochwil. 2009. "On Acting and Knowing: How Pragmatism Can Advance International Relations Research and Methodology." *International Organization* 63:4 (October), 701–731.

This article moves from deconstruction to reconstruction in research methodology. It proposes pragmatism as a way to escape from epistemological deadlock. We first show that social scientists are mistaken in their hope to obtain warranted knowledge through traditional scientific methods. We then show that pragmatism has grown from tacit commonsense to an explicit item on the agenda of the international relations discipline. We suggest that a coherent pragmatic approach consists of two elements: the recognition of knowledge generation as a social and discursive activity, and the orientation of research toward the generation of useful knowledge. To offer a concrete example of what pragmatic methodology can look like, we propose the research strategy of abduction. We assess various forms of research design to further elucidate how pragmatic research works in practice.

Gerring, John. 2010. "Causal Mechanisms: Yes, But..." *Comparative Political Studies* 43:11 (November), 1499–1526.

In recent years, the importance of mechanism-centered explanation has become an article of faith within the social sciences, uniting researchers from a wide variety of methodological traditions—quantitative and qualitative, experimental and nonexperimental, nomothetic and idiographic, formal models and narrative prose. Despite its many virtues, there are reasons to be skeptical of social science's newfound infatuation with causal mechanisms. First, the concept of a mechanism-centered ("mechanismic") explanation is fundamentally ambiguous, meaning different things to different people. Second, the minimal objectives associated with the turn to mechanisms—to specify causal mechanisms and engage in detailed causal reasoning—are not at variance with traditional practices in the social sciences and thus hardly qualify as a distinct approach to causal assessment. Finally, the more demanding goal of rigorously testing causal mechanisms in causal arguments is admirable but often unrealistic. To clarify, this is not a polemic against mechanisms. It is a polemic against a dogmatic interpretation of the mechanismic mission. Causal mechanisms are rightly regarded as an important, but secondary, element of causal assessment—by no means a necessary condition.

Grofman, Bernard and Carsten Q. Schneider. 2009. "An Introduction to Crisp-Set QCA, with a Comparison to Binary Logistic Regression." *Political Research Quarterly* 62:4 (December), 662–672.

The authors focus on the dichotomous *crisp set* form of qualitative comparative analysis (QCA). The authors review basic set theoretic QCA methodology, including truth tables, solution formulas, and coverage and consistency measures and discuss how QCA (a) displays relations between variables, (b) highlights descriptive or complex causal accounts for specific (groups of) cases, and (c) expresses the degree of fit. To help readers determine when QCA's configurational approach might be appropriate, the authors compare and contrast QCA to mainstream statistical methodologies such as binary logistic regressions done on the same data set.

Hafner-Burton, Emilie and James Ron. 2009. "Seeing Double: Human Rights Impact Through Qualitative and Quantitative Eyes." *World Politics* 61:2 (August), 360–401.

Over the past two decades, human rights language has spread like wildfire across international policy arenas. The activists who sparked this fire are engaged in two different campaigns. The first is comparatively modest, involving the persuasion of tens of thousands of global elites such as journalists, UN officials, donors, and national political leaders. The second is broader and more complex: to have a real impact on the behavior of tens of millions of state agents worldwide. While most international relations scholars agree that the first campaign has made real gains, opinions are split on the success—past, present, and future—of the second. In part, these divisions fall along methodological lines. With some exceptions, qualitative scholars working in the empirical international relations tradition express more optimism than their quantitative counterparts, whose contributions to the subfield are relatively new. This article reviews several new books on human rights and shows how their insights engage with these ongoing methodological debates. The authors argue that both qualitative and quantitative approaches offer important strengths and that neither has a monopoly on truth. Still, the human rights discourse may be thriving, at least in part, for reasons unrelated to impact. The authors conclude with suggestions for a more systematic and multi-method research, along with a plea for scholarly attention to the potential downsides of international human rights promotion.

Lieberman, Evan S. 2010. "Bridging the Qualitative-Quantitative Divide: Best Practices in the Development of Historically Oriented Replication Datasets." *Annual Review of Political Science* 13, 37–59.

The proliferation of historically oriented replication data has provided great opportunities for political scientists to develop and to test theories relevant to a range of macrohistorical phenomena. But what is the quality of such data? Are the codings or quantitative mappings of historical events, processes, and unit characteristics based on sufficiently solid foundations equivalent to those found in detailed case studies? This article evaluates a set of the most transparently disseminated replication datasets across a variety of research domains from the perspective of best-practice qualitative-historical research. It identifies a wide range of practices, highlighting both fundamental and innovative standards that might be adopted in future research.

Tarrow, Sidney. 2010. "The Strategy of Paired Comparison: Toward a Theory of Practice." *Comparative Political Studies* 43:2 (February), 230–259.

Paired comparison is a strategy of political analysis that has been widely used but seldom theorized. This is because it is often assimilated to single-case studies or regarded as a degenerate form of multicase analysis. This article argues that paired comparison is a distinct strategy of comparative analysis with advantages that both single-case and multicase comparisons lack. After reviewing how paired comparison has been dealt with in comparative politics, the article details a number of its advantages and pitfalls, illustrates them through the work of four major pairing comparativists, and proposes what is distinct about the strategy. It closes with a number of suggestions for using paired comparison more effectively.

Wedeen, Lisa. 2010. "Reflections on Ethnographic Work in Political Science." *Annual Review of Political Science* 13, 255–272.

The objectivist truth claims traditionally pressed by most political scientists have made the use of ethnographic methods particularly fraught in the discipline. This article explores what ethnography as a method entails. It makes distinctions between positivist and interpretivist ethnographies and highlights some of the substantive contributions ethnography has made to the study of politics. Lamenting the discipline's abandonment of a conversation with anthropology after Geertz, this review also insists on moving beyond the anthropological controversies so powerfully expressed in the edited volume *Writing Culture* (1986) and other texts of the 1980s and 1990s. I contend that interpretive social science does not have to foreswear generalizations or causal explanations and that ethnographic methods can be used in the service of establishing them. Rather than fleeing from abstractions, ethnographies can and should help ground them.

Announcements

The David Collier Mid-Career Achievement Award

At the 2010 business meeting, the section inaugurated a new award, honoring David Collier's contributions—through his research, graduate teaching, and institution building—as a founder of the qualitative and multi-method research movement in contemporary political science.

The award recognizes distinction in methodological publications, innovative applications of qualitative and multi-method approaches in substantive research, and/or institutional contributions to this area of methodology.

To be eligible for the mid-career award, nominees must have defended their dissertation within fifteen years of the beginning of the year in which the award is presented. For the 2010 award, nominees had to have defended their dissertation in or later than 1995.

Not to minimize the very real contributions of the scholars who have won the paper, article, and book awards, but the Collier Award is the section's most important recognition. The award acknowledges sustained, multi-year contributions over three dimensions. As a mid-career award, it is intended both to reflect achievements already made, and the promise of more to come. As befits the relative stature of the award, it has been established with a larger monetary reward than attaches to the section's book, paper, and article awards.

When the section was establishing the award, once it was decided that it would recognize the three fields of publications developing qualitative and multi-methods, research using such techniques, and institutional development, it became clear very early on there was only one person this award could possibly be named for. David Collier has been, and remains, an extraordinary leader on all three fronts.

His most recent methodological projects (and by recent we mean in the last two or three years) have included the 2008 *Oxford Handbook of Political Methodology* (co-edited with Janet Box-Steffensmeier and Henry E. Brady); the 2009 volume *Concepts and Method in Social Science: The Tradition of Giovanni Sartori* (co-edited with John Gerring); and in 2010 two books, *Statistical Models and Causal Inference: A Dialogue with the Social Sciences* (co-edited with Jasjeet Sekhon and Philip Stark), and the almost completely rewritten second edition of *Rethinking Social Inquiry: Diverse Tools, Shared Standards* (co-edited with Henry Brady).

In disciplinary terms, perhaps just as important as his own methodological publications, Collier has been the guiding force behind more articles, books and book chapters than we are likely to be able to count. His colleagues, friends, and students have started projects they otherwise would not have without his guidance, and just as importantly, have produced better work because of his helpful advice.

David's substantive work is equally exceptional. His research monographs include *Shaping the Political Arena: Critical Junctures, the Labor Movement, and Regime Dynamics in Latin America* (co-authored with Ruth Berins Collier), *The New Authoritarianism in Latin America*, and *Squatters and Oligarchs: Authoritarian Rule and Policy Change in Peru*. He was also co-editor of "Regimes and Democracy in Latin America," a special issue of the journal *Studies in Comparative International Development* (2001).

As with his methodological work, David has trained crop after crop of exceptional graduate students, many of whom have gone on to become leaders in their respective sub-fields and methodological research communities.

David's institution building has been equally astonishing. He was a founder of the QMMR section, organizing the petition which created it. The rules at the time were that the petition needed 250 signatures. David announced we would stop when we had collected 1,000. It took him about three weeks, and that included collecting the signatures of every extant editor of the *American Political Science Review* and all but one of the surviving Presidents of the American Political Science Association.

If we listed every service David has done for the section since its inception we would need another issue of the newsletter, but we would be remiss not to mention one of particular importance. David has taught a short course on the Wednesday of the APSA meeting since before the section was the section (when it was the Committee on Concepts and Method), and every year since. This year he had approximately 100 students register for the class. We have never officially added up the number of graduate students who have attended his short course, but it must be well over 1,000 by now.

On any one leg of the tripod taken on its own—methodological writings, applied research, and institutional development—David has been remarkably successful. When considered in combination, his contributions are unique. The section is grateful to have the opportunity to mark its appreciation for his tremendous impact by establishing this award.

The David Collier Mid-Career Achievement Award

Recipients: Lisa Wedeen of Chicago University, and James Mahoney of Northwestern University.

Committee: Andrew Bennett, Georgetown University; Colin Elman, Maxwell School of Syracuse University; and Deborah Yashar, Princeton University.

The Collier Award committee considered the candidates carefully, and determined that two scholars had made extraordinary contributions across all three categories of substantive research, methodological writing, and institutional development. The award was not split, in the sense of being divided or shared. The committee concluded that among this year's candidates, two scholars richly deserved this recognition, and accordingly both separately received the award.

Lisa Wedeen's methodological writings have focused on interpretive and ethnographic approaches. She has published a series of prominent articles and chapters, most notably her "Conceptualizing Culture: Possibilities for Political Science," in the *American Political Science Review*. Most recently she published "Reflections on Ethnographic Work in Political Science" in the *Annual Review of Political Science*. Dr. Wedeen's applied research includes two University of Chicago Press books, *Peripheral Visions: Publics, Power, and Performance in Yemen* and *Ambiguities of Domination: Politics, Rhetoric, and Symbols in Contemporary Syria*. In addition to Wedeen's methodological and applied research, the committee was impressed with her terrific track record on institution building. She has been a leading figure in the qualitative research methods project, and more than anyone else has been responsible for the productive inclusion of interpretive and ethnographic approaches in that dialogue. Wedeen has sustained that engagement in several venues, including the annual Institute for Qualitative and Multi-Method Research, where she expanded interpretive offerings from one two-hour class to three full days of courses.

James Mahoney's methodological writings have focused on comparative-historical methods. He has written a series of articles on a variety of topics, including selection bias in qualitative research, causal inference, and path dependent explanation. His edited Cambridge University book (with Dietrich Rueschemeyer) on *Comparative Historical Analysis* considered a wide range of cognate techniques and discussions. More recently Mahoney has worked on probabilistic and continuous forms of set-theoretic analysis. He will (with Gary Goertz) shortly be publishing a Princeton University Press volume detailing the different foundations of quantitative and qualitative approaches to research. Mahoney's substantive research includes two single-author books, *Colonialism and Postcolonial Development: Spanish America in Comparative Perspective* with Cambridge University Press, and *The Legacies of Liberalism: Path Dependence and Political Regimes in Central America* with Johns Hopkins University Press, as well as an edited book (with Kathleen Thelen) on *Explaining Institutional Change: Ambiguity, Agency, and Power*, and a variety of articles and book chapters. Mahoney has also made significant contributions in institution building, serving as president of the QMMR section, regularly designing and leading modules at the annual Institute for Qualitative and Multi-Method Research, and building bridges between the disciplines of political science and sociology.

Giovanni Sartori Award for the Best Book Published in 2009 Developing or Appyling Qualitative Methods

Recipients: Edward Schatz, ed., *Political Ethnography: What Immersion Contributes to the Study of Power* (University of Chicago Press, 2009), and Evan S. Lieberman, *Boundaries of Contagion: How Ethnic Politics Have Shaped Government Responses to AIDS* (Princeton University Press, 2009).

Committee: Taylor Boas, Boston University; Jeffrey T. Checkel, Simon Fraser University; and Kathleen Thelen, Massachusetts Institute of Technology (chair).

The committee has decided to confer the Sartori award for 2010 on two books. These two books are different in many ways—among other things, one is an edited volume, the other a single-author monograph; one a more purely "methodological" contribution, the other a substantive work that is a model in the application of qualitative and mixed methods research. Each thus speaks in different ways to the Sartori award criteria and both are excellent works that we can recommend highly to all Section members.

Edward Schatz, ed., *Political Ethnography: What Immersion Contributes to the Study of Power.* (Chicago: University of Chicago Press, 2009).

In this important and unusually coherent edited volume, Edward Schatz and collaborators demonstrate that ethnographic methods can and should play a central role in political science. Directly addressing the concern that ethnography is too interpretive and atheoretical, the book demonstrates that the ethnographic tradition is in fact a rich and multi-faceted one. Some variants are indeed deeply interpretive while others can easily be employed in positivist-inspired research designs. The core of the book is a wonderful set of studies of ethnography in action, allowing readers both to see how the method works and how it contributes to a richer understanding of such central concepts as power. We commend the editor for bringing these diverse contributions together in such a coherent volume, including his excellent synthetic introductory and concluding chapters. Given the Section's concern to advance qualitative methodology across philosophical divides, the award committee felt the Schatz book to be a compelling and grounded demonstration of the methodological value-added of epistemological pluralism.

Evan Lieberman, *Boundaries of Contagion: How Ethnic Politics Have Shaped Government Responses to AIDS.* (Princeton, NJ: Princeton University Press, 2009).

This impressive contribution both explores an important substantive issue and provides an exemplar of qualitative and mixed methods research. Lieberman addresses the question of why different governments have attacked the HIV/AIDS pandemic in different ways and with different degrees of success. He finds that strong ethnic boundaries in countries such as South Africa create disincentives for political leaders to approach the problem as a shared national concern, even when the political auspices appear propitious. By contrast, weak ethnic boundaries (and associated weak "us-them" dynamics) promote more aggressive responses, again regardless of the particular political composition of government. Even more important from the perspective of our Section, the book stands out for its methodological contribution, as a model of high quality mixed methods. Based on a structured comparison of Brazil and South Africa, Lieberman builds a causal model that he then applies and tests on India, combining national-level qualitative analysis and statistical analysis of state-level variation. Following these case studies, a cross-national statistical analysis of a multi-country dataset probes the generality of the causal argument. The committee agreed that this book stands out as a model of the kind of work for which the Section stands.

Alexander George Award for the Best Article or Book Chapter Published in 2009 Developing or Appyling Qualitative Methods

Recipient: Dan Slater, "Revolutions, Crackdowns, and Quiescence: Communal Elites and Democratic Mobilization in Southeast Asia," *American Journal of Sociology* 115:1 (July 2009): 203–254.

Committee: Emilie M. Hafner-Burton, University of California, San Diego; Dara Z. Strolovitch, University of Minnesota; and Michael Tomz, Stanford University (chair).

This year's winner is Professor Dan Slater, from the University of Chicago. His award-winning article is "Revolutions, Crackdowns, and Quiescence: Communal Elites and Democratic Mobilization in Southeast Asia," *American Journal of Sociology*, July 2009.

Slater's article is an especially fine example of how researchers can use qualitative techniques to shed light on fundamental questions in political science. He asks: When do urban masses revolt, and when do their revolts lead to democracy? Previous researchers had attributed revolts to economic and political grievances. Slater maintains that revolts are also driven by emotional and cultural variables, which have not received enough attention in the literature. He calls attention to the role of communal elites, who possess nationalist and religious authority. When communal elites have autonomy, they can make emotional appeals that spark revolts and help them succeed. As Slater puts it, "democratic mobilization is more likely both to occur and to succeed in societies with politically autonomous communal elites."

Slater tests his argument by comparing the experiences of seven Southeast Asian countries. In the tradition of Alexander George, he pays close attention to research design. He carefully selects cases to ensure variation, not only in his own independent variable (the political autonomy of communal elites), but also in other plausible independent variables. He does a particularly admirable job of pitting his own argument against rival explanations, including economic development, economic shocks, electoral fraud, and international diffusion.

Like Alexander George, Slater also makes deft and effective use of process tracing. Having documented a correlation between his key independent variable and the final outcome, Slater delves deeply into the historical process that contributed to the outcome. Of course, in a journal-length article, Slater cannot go into detail about all seven cases. Again showing his emphasis on research design, he chooses the three cases—the Philippines, Vietnam, and Burma—that are most informative about his causal mechanism.

In summary, Slater's article truly honors the career of Alexander George, by using the best qualitative methods to advance knowledge of a core topic in political science. Congratulations, Dan, on your exemplary work.

SAGE Prize for the Best Paper Presented at the 2009 Annual Meeting of the American Political Science Association Developing or Appyling Qualitative Methods

Recipient: Marcus Kreuzer, Villanova University. "Historical Knowledge and Quantitative Analysis: The Case of the Origins of Proportional Representation."

Committee: Mirjam Kunkler, Princeton University; Ngoni Munemo, Williams College; and Benjamin Read, University of California-Santa Cruz (chair).

The winner of the 2010 Sage Award is Professor Marcus Kreuzer for his 2009 APSA paper, "Historical Knowledge and Quantitative Analysis: The Case of the Origins of Proportional Representation." The committee was impressed with multiple aspects of this paper. To begin with, it addresses the origins of proportional representation electoral systems, an important substantive question in comparative politics. In what was surely a labor-intensive process, the author and his research assistants checked, one by one, the sources, coding, and analysis of the two prominent works by Boix and Cusack/Iversen/Soskice with which the paper engages.

Merely assessing the coding and accuracy in the analysis of these two works would have been a significant contribution, but Kreuzer goes further by also replicating the quantitative analyses, showing the implications of different coding decisions, as well as examining the actual causal mechanisms the earlier authors identified. Building on the previous two works and his critical examination of their methodologies, Kreuzer makes an argument of his own about the roots of PR. Kreuzer offers a well framed, operationalized, tested and argued conception of the importance of historical knowledge in what we do as political scientists. Also, in highlighting the ways in which quantitative research, particularly at the coding level, depends on substantive historical knowledge, Kreuzer demonstrates the critical role that qualitative methods play. The paper makes a strong and compelling argument why our discipline needs to pay closer attention to how quantitative research codes its observations, and by doing so it encourages political scientists to be even more meticulous in their development of data from historical and contemporary sources.

The committee notes that this piece has been published in the May 2010 issue of the *American Political Science Review*, along with two responses. Without attempting to adjudicate this debate, we commend the author above all for underscoring the fact that the answers to such large questions matter, and that we must constantly and explicitly consider and reconsider how we reach those answers.

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