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Progress in International Relations

Beyond Paradigms in the Study of Institutions

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Progress is hard to measure, especially in one's own era. There is certainly no consensus on the general meaning of the term, even less so when applied to scholarly inquiry. Moreover, the implications of new ideas or concepts can take generations to become apparent. It is doubtful that many of the early experimenters with electricity envisioned the role harnessing that force would eventually play in modern life. Nor did Adam Smith, writing against the mercantilist orthodoxy of his day, expect economic theory to develop its axiomatic form or become the new orthodoxy two centuries later. As a subject, progress is as elusive as it is in reality.

Nonetheless, it is worthwhile periodically to take stock of where scholarly inquiry stands, where it appears to have succeeded, where it has failed, and which directions appear most promising at a particular juncture in space and time. Well aware of the pitfalls that await, I attempt such a stock taking in this essay. I organize my reflections into three general questions. What is the nature of our enterprise and what should our standards be? Where has there been significant progress in the field in meeting these standards, and why? Why hasn't there been more progress? I address these questions, first, for international relations as a whole and, second, for the study of international institutions.

Standards of Inquiry

Our purpose as scholars is to increase understanding of world affairs, to help ourselves and others understand better how the world "works." Policies for improving the world may or may not follow directly from an improved understanding. Knowledge may suggest changes that enhance human welfare, but it might just as well reveal innate characteristics immune from the interventions of even the most enlightened social engineer. Our goal, then, is not necessarily to improve the world—although, I repeat, this can be an important and valuable by-product of scholarly understanding—but to improve our knowledge of the world we inhabit and, in part, construct. Progress, in turn, can thus be defined as improved understanding at one of two levels: general explanations that fit a variety of phenomena and cases more accurately and particular explanations that apply to a small number of phenomena or cases more fully.

There are many ways of understanding, including religious and ideological beliefs, empiricism, and the relationalism of postmodern theories. My preferred approach is that of scientific realism, in general, and the deductive method, in particular. This approach rests upon the explication of basic assumptions, the logical derivation of propositions from those assumptions, the deduction of observable implications, and the testing of implications through carefully designed quasi-experiments.

The scientific method is not perfect, to be sure. Scientists have no monopoly on understanding. But the clarity of its procedures and the opportunity to prove arguments wrong make the scientific method superior, in my view, to the major alternatives. Given costly information and the limits of cognitive capacity, no one scholar can produce a complete body of theory and evidence. Active debate and contestation are a necessary part of what is inherently a collective enterprise. Through competition and repeated inquiry, the scientific community probes, refines, and elaborates insights that lie beyond the skills and imagination of any individual. Having clear procedures and standards of evaluation is essential, in turn, to progress. Given sufficient time and resources, scholars employing the scientific method can determine with some degree of objectivity whether assumptions 1, 2, . . . n produce proposition P, whether P implies hypothesis $X \rightarrow Y$, and whether the empirical record supports this inference. Over time, inquiry may even approximate the best explanation, as theory gets closer and closer to the real world (but of course never mirroring the real world, as this would then be description). No other method possesses the same potential for "objective" evaluation or, at least, consensus among the scholarly community.[1]

Areas of Progress

There have been significant areas of progress in international relations. At the risk of slighting others, I will focus on two. We have greatly improved our understanding of trade policy. Employing what is sometimes called open economy politics (OEP),[2] we now see clearly how domestic groups define their material interests with regard to their positions within the international economy and how those interests are aggregated by political institutions into national policies or, through interstate bargaining, into international outcomes. Peter Gourevitch's now classic study of the first great depression was dismissed as "Marxist claptrap" when it was first submitted for publication, most likely because it assumed that groups pursue their material interests.[3] Yet it laid the modern foundation for OEP. These insights were integrated with the pure theory of international trade by Ronald Rogowski, building on the Heckscher-Ohlin model and Stolper-Samuelson theorem (collectively, the HOS model), and Jeff Frieden, drawing on the Ricardo-Viner or specific factors model.[4] Although the HOS and specific factors models were treated as alternatives that could be reconciled only over different time scales, Michael Hiscox has recently made the level of factor mobility a variable rather than an assumption, and thus is able to identify when one or the other model ought to apply; in doing so, he significantly improves the fit between theory and evidence.[5] We now have a clear, deductive, synthetic, and empirically supported theory that produces nontrivial insights into how economic opportunities structure society, who lobbies for protection, and when they are likely to receive it.[6]

Cynics can, of course, dismiss this progress by snorting that Schattschneider said it all sixty-five years ago, but the father of pluralism at best described a general and relatively constant process that produced an extreme result at a particular moment in time—the Smoot-Hawley Tariff passed in the opening days of the Great Depression.[7] He lacked an explanation for variations in trade policy outcomes.

More important, current progress is tempered by significant theoretical anomalies. If we accept that political rhetoric is not entirely instrumental, then a remarkably large number of political leaders and their followers continue to believe that national welfare is enhanced by pursuing principles of competitive advantage rather than comparative advantage. Nor does the wave of liberalization that began in the 1980s seem to be driven by the near simultaneous victory of free traders around the globe.[8] National policy still appears to be more than the sum of particularistic interests. Despite these limitations, however, substantial progress is evident.

A second area of progress is the so-called democratic peace. This topic first gained prominence as a mix of philosophical inquiry and empirical evidence,[9] but it has since generated a cottage industry of theorists, who have refined and extended Kant's propositions into normative and institutional variants, and empiricists, who have developed measures and challenged findings. The state of the art continues to evolve here, but my reading of the growing literature is that (a) the finding that democracies do not fight one another is well-established; but (b) may be overdetermined, as most democracies during the cold war were on the same side of the bipolar divide;[10] (c) there is some support for a normative theory of the democratic peace;[11] but (d) the institutionalists have a better-specified and more robust theory; that (e) generates a surprisingly large number of additional predictions that are also empirically supported.[12] Although each of these steps is still debated, we now know substantially more about how differences in regime type affect the chances for war than we did even a decade ago.

These two areas of progress have much in common, and a bit of intuition and casual empiricism combine to suggest that those commonalities may have a lot to do with the relative success of these research programs. *First, both are disciplined by a strong empirical foundation.* They begin with a real-world phenomenon—trade protection and war, respectively—over which there is broad agreement on the historical contours. There are numerous disputes over how best to measure the phenomenon at issue—nominal tariffs versus residuals from a gravity model of trade, war versus crisis—but analysts generally concur on the broad outlines of the targets at which they are shooting.

Second, there is an emphasis on deductive rigor. Scholarly debate has forced analysts to clarify the assumptions they make, revealing strengths and limitations, and to state propositions in clear and falsifiable ways, thereby making hypotheses amenable to empirical test. By working within the standards of scientific inquiry, scholarly interactions and cumulative knowledge are facilitated. Both can be seen as exemplars of the power of the scientific method in world politics.

Finally, both literatures have drawn upon and integrated their analyses into broader bodies of theory. In the case of OEP, theorists have grounded their analyses in the pure theory of international trade and theories of collective action. In the democratic peace, effective use has been made of general theories of war, especially those stressing problems of asymmetric information,[13] and theories of domestic institutions. Analysts do not reinvent the wheel each time they sit down at their computers. Rather, they extend and tailor theories developed for other purposes to the specifics of the question they seek to answer. In short, they are engaged in what is often disparagingly called normal science.

Impediments to Progress

In contrast, many areas of international studies have made relatively little progress. All too often, research has not led to increased understanding but only to more debate and greater confusion. Although not an exhaustive list, I see three general and recurring problems in current research practices.

First, we debate assumptions, not implications. In international relations, arguments commonly focus not on assessing the empirical power of theories but on the truth status of the assumptions upon which they are built. The so-called relative gains debate epitomizes this tendency. Joseph Grieco originally charged that by assuming states maximize absolute rather than relative gains, neoliberal institutionalists overpredict the level of cooperation within the international system.[14] Without any metric for assessing the level of cooperation or clear deductions of exactly how much cooperation to expect under what circumstances, this was an unanswerable accusation. The debate quickly sank into charges that neoliberals thought cooperation was "too easy" and realists thought it was "too hard," and the argument became more a test of faith about the world than about the world itself.[15]

In two important papers, Robert Powell and Duncan Snidal subsequently demonstrated that each approach could be generated as a special case of the other.[16] Starting from neoliberal assumptions, Powell showed that if the cost of using force were sufficiently low, states would behave "as if" they were concerned with relative gains.

Conversely, building on neorealist assumptions, Snidal showed that as the number of players increased, concerns for relative gains would rapidly dissipate and that in large-N situations states could do no better in maximizing their relative gains than to maximize their absolute gains. Both Powell and Snidal needed to make extra assumptions or impose additional "structure" on the theories to produce these results—and this is controversial—but it reflects more the incompleteness of neorealism and neoliberalism than any particular "agenda" these authors had in this clash. As I read it, the debate was then resolved with Grieco, for the neorealists, and Robert Keohane, for the neoliberals, essentially agreeing that it is an empirical question as to which of the two approaches might apply in any particular situation.[17]

Revealingly, scholars then immediately dropped this line of inquiry. No research of which I am aware has tried to test the empirical predictions that come out of the Powell and Snidal models. No one has taken up the challenge of identifying *empirically* the conditions under which one or the other perspective is more appropriate. Once the argument was reframed as an empirical question rather than a debate over first principles, scholars simply lost interest.

An important lesson emerges from here. Paradigmatic debates are helpful in highlighting and clarifying assumptions. Without the provocation of Grieco's neorealist critique, an important foundation of neoliberal theory might have remained implicit. Criticism forces all of us to be more clear and precise in our theoretical formulations. But all too often debates dissolve into tests of the truth status of our assumptions rather than tests of their empirical implications. This adds little to our collective understanding.[18]

Second, we work on separate islands of theory. This is a well-known problem, pointed out by Benjamin Most and Harvey Starr more than fifteen years ago.[19] Despite the attention devoted to more synthetic topics like "grand strategy" in recent years, the criticism remains accurate. Economic sanctions and war are treated in separate literatures, for example, even though both are tools used to coerce other states and thereby influence their behavior.

The failure to build bridges between these separate islands and to conceptualize adequately the relevant policy alternatives has hobbled inquiry. Choice theory presumes that actors assess any single policy against the next best options,[20] but we typically develop and test our theories only in absolute terms. Thus, we study whether or not countries form alliances, for instance, as if this were a simple dichotomous choice rather than the selection of one option from a range of alternatives. At various times, unilateralism, imperialism, and unification have all been understood as appropriate responses to external threats, as have appeasement and undermining the enemy's own state or empire.[21] The real question we ought to be asking, then, is why states ally rather than appease, arm, build empires, form voluntary confederations, or undermine opponents.

The failure to specify relevant alternatives, in turn, produces significant selection bias. If we truncate the set of policy alternatives that we are trying to explain—say, studying only alliances rather than a range of alternatives—we systematically (and unwittingly) underestimate the effects of our independent variables. Elements like power disparities that we expect to be associated with the creation of alliances, and which simple extension suggests might have an even greater impact on strategies like appeasement, will appear to be less important and less significant than they really are.

Conversely, if we truncate the set of policy alternatives when trying to estimate their effect on something else, we produce highly uncertain results. For instance, if we were to array a variable of, say, deterrence success along the vertical axis and actions of increasing hostility along the horizontal axis, a study of military mobilizations or crises alone would produce a stack of points clustered far from the origin, as "lower"-level or less threatening interactions were intentionally excluded and vertically dispersed, reflecting "natural error." Nearly any line drawn through this stack of "mobilization" points would be as good as any other. As this is the design commonly employed in studies that seek to discern whether deterrence "works," we can easily see why reasonable scholars continue to disagree over how to interpret the evidence![22]

Selection bias either distorts our results or renders them more uncertain. What we look at—on which island we choose to conduct research—significantly influences what we see. Failing to conceptualize alternatives adequately, even those that we rarely observe in practice, significantly impedes progress.

Third, we fail to operationalize our variables, especially our dependent variables. The problems in measuring "cooperation" were alluded to above but provide a particularly clear example of this impediment to progress. Robert Keohane's quite reasonable definition of cooperation as mutual adjustment in policy is now widely accepted.[23] Nonetheless, the concept of "adjustment" remains ambiguous, and there is no metric against which change can be measured.

We expect there to be "more cooperation" in dilemmas of collaboration rather than coordination,[24] but this is a deduction rather than a confirmed empirical finding. How much cooperation occurs? How has the level evolved over time? How does it vary across issue areas? Without answering such basic questions, most theories of cooperation simply cannot be tested.

We need to devote more time and resources to developing operational definitions and collecting systematic data. The Correlates of War Project was criticized in its early years for failing to produce important research findings. The payoff to this enterprise, however, is now abundantly clear in the study of the democratic peace. None of the work cited earlier would have been possible without the empirical foundation laid by J. David Singer and his colleagues. Much the same could be said for the Polity data set developed by Ted Robert Gurr and his associates, now generally accepted as an appropriate (if still not perfect) set of measures of "regime type."

Ideally, measures should follow theory, not the other way around. There is something arbitrary, it is easily admitted, about one thousand battle deaths as the definition of war. But we have to start someplace. Just as wars were eventually scaled into the larger class of militarized interstate disputes, so other seemingly arbitrary operationalizations may generate follow-on data sets. Building more systematic measures is a necessary but greatly undervalued activity.

Progress in the Study of International Institutions

The study of international institutions, in my view, has enjoyed mixed success. There has been enormous progress, especially over the last half century.[25] Compared with the descriptive summaries of the activities of international organizations in the 1950s and the behavioral analyses of the 1960s, we now have strong theories that explain the formation and maintenance of international institutions, and we have detailed studies of how and where they "matter."[26] We are now seeing a new generation of innovative midlevel theories designed to account for variations in legalization and institutional design.[27] Reflecting this progress, institutionalism is now generally regarded as one of the major, mainstream approaches to the study of international politics.

The sources of this success are two. First introduced by John Ruggie, extended by Robert Keohane and Joseph Nye, and the subject of a special issue of *International Organization* edited by Stephen Krasner, the concept of regime extended the range of what is regarded as an international institution, creating both a larger phenomenon to study and greater variation in the dependent variable used by scholars.[28] This progressive "problem shift" in the study of international institutions mitigated—but, as I shall explain below, did not wholly eliminate—the second impediment discussed previously. In addition, international relationists productively drew upon emerging work elsewhere in the social sciences on the sources and roles of institutions, including that by economists Douglass North and Oliver Williamson and sociologist John Meyer.[29] By doing so, international relationists were able to identify pitfalls, benefit from clear lines of debate in cognate fields, and move quickly up the learning curve.

At the same time, institutionalism has failed to achieve the successes of OEP or the democratic peace, and it suffers in greater or lesser degree from nearly all of the impediments to progress noted previously. After a flowering in the early 1980s, institutionalism has not developed at the same pace, and it has failed to meet the high expectations of many of its proponents. Institutionalism is not, I want to emphasize, a failed research program. Far from it. But there remain grounds for considerable improvement.

First, institutionalism lacks a ready operationalization of its key variable, whether regime or institution. Despite a seemingly clear consensus definition,[30] one that most of us can recite in our sleep, there is no consistent agreement on what practically and empirically constitutes a regime. Most agree that formal organizations or agreements constitute regimes, but there is no agreement on how to "count" or "code" organizations. Is the World Trade Organization (WTO) one overarching regime on goods and services or is it a series of smaller and still incomplete regimes on tariffs, property rights, investment, and so on? Even though the concept was explicitly formulated to include informal agreements and persistent practices, the problem of operationalization is magnified for tacit regimes. Is the "nuclear taboo" a regime or not?[31] Is there an "atrocities" regime?[32] How would we know? The concept of institution—sometimes understood to be broader, the same, or narrower than a regime—suffers from the same ambiguities. More than any other factor, this lack of a clear, operational definition stymies further progress.

Second, following from the problem of operationalization, institutionalism lacks an accepted empirical pattern or puzzle that motivates inquiry. By this, I do not mean that institutionalism lacks empirical content. There are many fine studies of how institutions arise, operate, and evolve. Rather, what I mean is that, absent an identified pattern of institutions across issue areas, time, regions, or some other relevant dimension, analysts do not know what it is they are seeking to explain. To date, institutionalists have concentrated on demonstrating that contrary to realism, institutions exist and play an influential role even within an anarchic and perhaps inhospitable international system. Given the intellectual hegemony of realism, this was an important and possibly necessary first step. But to demonstrate that institutions matter does not cumulate naturally into institutional variation that then needs to be explained. Scholars must still impose some analytic dimension on the set of cases and identify some consistent pattern, perhaps by issue area (are institutions more prevalent in trade than monetary affairs, economic than security relations?) or by type of institution (are some institutions "stronger," more hierarchical, more legalized than others?). In contrast, OEP largely agrees on its dimension of variation, openness to goods and factor movements across national borders, and on certain observable facts, especially variations in openness over time (more countries were more open in 1910 than in 1935, in 2000 than in 1960). The democratic peace is motivated by a single (if still contested) fact, namely that democracies do not fight each other. No similar empirical puzzle exists for institutionalism. We are just now beginning to see work that does pose dimensions of variation,[33] but operationalization remains poor and patterns ambiguous. For further progress, it is essential that scholars agree on the relevant dimensions of institutional variation, develop operational indicators that capture this variation, and establish a pattern that they can then seek either to explain or to use to explain other phenomena.

Third, although institutionalists have drawn productively on other literatures, as already noted, they have not expanded their horizons far enough, and in so doing have fallen into what Keohane and Martin call the "endogeneity trap."[34] Missing from the literature are the institutionalist models so prevalent in American politics and, to a lesser extent, in comparative politics.[35]

Institutionalism has revolutionized the study of American politics, shifting the emphasis from public opinion and the "imperial presidency," on the one hand, to Congress and bureaucratic oversight, on the other.[36] In highly simplified form, this approach assumes that politics is prone to cycling (whether due to collective preference intransitivities or multiple dimensions) and tends to produce multiple equilibria. Institutions, in turn, structure choice and "induce" one equilibrium from the many possible—in essence, determining which outcome we actually observe.[37] In this way, institutions do most of the heavy lifting in explaining policy outcomes. William Riker famously criticized institutionalists for not recognizing that any instability in preferences would immediately translate into cycling over institutions.[38] As institutions are endogenous, he pointed out, they cannot "solve" the problem of cycling. Institutionalists responded, quite correctly in my view, that institutions are embedded into larger social structures and reinforced by "vested interests" created by past polity choices. Thus, for many issues and policies, institutions can reasonably be treated as exogenous and theorists can proceed to examine how and why they influence politics without falling prey to the question that so bedevils international relationists—"do institutions matter?"[39]

International relationists have avoided this line of reasoning, I believe, because we too readily assume that institutions are weak, transient, and therefore part of the "game." If institutions are always both a product and producer of political choices, it will be difficult—perhaps impossible—to show that they exert an independent effect, which was exactly Riker's point. International relationists fail to take the next step, however, and examine the degree to which institutions are embedded in larger social structures and therefore are more "fixed." After fifty years, General Agreement on Tariffs and Trade/World Trade Organization rules and norms have had a tremendous effect on the pattern of private investment in economies around the world. As tariffs have fallen and markets opened, uncompetitive industries have been purged, export sectors have grown, and multinational corporations dependent upon the free movement of goods, services, and capital have flourished. Thousands of private investment decisions are made every day premised on the belief that trade will remain open. These decisions, in turn, create and reinforce "vested interests" in the preservation of this international institution. The situation here is not substantially different than that in other, highly constitutionalized areas of politics. The way out of the endogeneity trap is not by searching for the independent effects of institutions but by demonstrating how deeply embedded they are in the social structure of world politics.

Finally, international relationists have adopted an overnarrow conception of institutions. Focusing only on "anarchic" institutions composed of sovereign states and based on the principle of sovereign equality, like NATO or the United Nations, they have ignored the broader range of institutions that have existed historically and, as I have tried to show elsewhere,[40] continue to exist today. In anticipation of the Versailles peace conference and the difficult problems of constructing new states in Eastern Europe and dividing Germany's overseas empire among the victors, President Woodrow Wilson impaneled a study group known as The Inquiry to prepare background papers on many topics. One volume, a comprehensive study of *Types of Restricted Sovereignty and of Colonial Autonomy,* identified ten categories of polities ranging from semisovereign states to administered provinces.[41] Covering several hundred years of international history, this volume describes a range of increasingly hierarchical international institutions—a set now virtually forgotten in studies of international relations. Similar forms persist today, although norms of formal sovereignty make it politically incorrect to call such hierarchies by their traditional names—as witnessed by the unwillingness of the Western powers to acknowledge the de facto mandate they exercise over Kosovo. But from the United States' de jure protectorate over Micronesia to its de facto protectorate over Saudi Arabia and Kuwait and Russia's protectorates and informal colonies in the former Soviet Union, hierarchies abound in international politics.[42]

This variance in institutional form could and should be profitably exploited. The failure to allow for and examine more hierarchical institutions not only ignores important dimensions of real world politics but—as highlighted previously—creates selection bias in our studies of institutions. As with alliances, truncating the variation in institutions as a dependent variable underestimates the effect of independent or causal variables, implying that those factors that affect international institutions are more significant than past studies have revealed. Transaction costs, informational asymmetries, the need for credibility, and other variables that institutionalists have used to explain institutions may be more important than they themselves recognize.[43]

Limiting variation in institutions as an independent variable, in turn, makes our estimate of their effects more uncertain. Implicit in most studies is a comparison of anarchic institutions, which may be common internationally, and hierarchic institutions, which are often presumed to characterize domestic political systems.[44] The real question for international relationists is not whether international institutions matter in an absolute sense but how much and in what ways they matter relative to the supposedly more established and effective institutions of domestic politics. As with the case of deterrence failures, if one were to plot points for institutions of increasing hierarchy on the x-axis, thereby capturing this comparison, and "effectiveness" on the y-axis, the research design most commonly employed in international relations would produce a stack of points near the "anarchic" origin, with some vertical dispersion reflecting natural variation in effectiveness; with no further observations at other values of the x-axis, almost any regression line drawn through this stack of points would be just as valid as any other, thus preventing us from determining whether international institutions matter a little, a lot, or not at all. Given this research design, it is not surprising that reasonable scholars have been able to read the record of institutional effectiveness very differently.[45]

In short, by truncating variation in institutions, international relationists forfeit much-needed leverage over their subject. By extending the continuum of institutions to include hierarchies we gain a better understanding of institutions both as products of political choices and as producers of political outcomes.

The Way Forward

The study of international institutions needs to become the study of governance. Governance is a generic problem of all polities and can be defined as the design, construction, and maintenance of mechanisms to reach collective decisions and to enforce agreed-upon bargains.[46] The WTO, International Monetary Fund, and Organization for Economic Cooperation and Development are all "anarchic" institutions that govern relations between states, just as protectorates, confederations (like the European Union), and even the state itself are hierarchic institutions that govern relations between polities.

The conceptual shift from institution—a human product that we often seek to explain—to governance, the function that institutions are designed to fulfill, naturally enlarges the purview of international relations and shifts attention to the question of why certain institutions and not others are chosen under what circumstances. Focusing on governance is no guarantee against artificially truncating the range of variations in institutions, but it makes unintentional selection bias less likely.

Perhaps more important, a focus on governance endogenizes the state—that primordial institution of modern politics. The nature of the state, its forms and functions, its size and shape, all become properly part of the study of international institutions. Moreover, the question of why some governance functions are assigned to or appropriated by states while others are either settled cooperatively by mutual agreements between states or allocated to private actors—like nongovernmental organizations or multinational corporations—or supranational organizations becomes a central concern. By making the shift to governance, the study of institutions will be considerably enriched.

Conclusion

International relations has a long history of paradigmatic contests. Indeed, much of the history of our field can be defined in terms of its "great debates."[47] As I suggested, these contests have performed a useful role. But they also produce or at least exacerbate the problems just identified. Paradigm warriors are particularly prone to argue about the truth status of assumptions, to defend islands at the core of their approach rather than build bridges to those occupied by others, and to neglect research design in hopes of scoring debating points. In the continuing debate with neorealists on whether institutions "matter," institutionalists have continued in this not-so-grand tradition.

We are stuck in a convention trap, where however much we may individually abhor some of the standards and practices in our field we are nonetheless compelled to perpetuate them for our own success and that of our graduate students.[48] Who hasn't sniffed at the work of a colleague as being merely "normal science"? Who hasn't reviewed an article and concluded that it is quite solid but not sufficiently innovative to warrant publication in a top journal? Who hasn't counseled a promising graduate student to be bold and provocative rather than solid and empirical? And yes, despite the complaints developed in this paper, I teach the introductory graduate course in international relations as a series of paradigmatic clashes, spending a week or more on each of the various "isms" that populate our field.

A necessary step in changing conventions is to expose the insidious side effects of current practices. Evidence of a possible realignment of priorities is already under way in the nature of the "training" we now expect advanced graduate students to receive. But more needs to be done. We must insist that scholars ask interesting empirical questions. We must ensure that normal science has a prominent place in our journals. We must encourage scholars who seek only to debunk received wisdom or to add incrementally to our understanding of world affairs. We must reward scholars who contribute the years necessary to define concepts systematically, code cases, and build data sets. And we should continuously ask paradigm warriors the important question "What have you helped us understand lately?"

Notes

1. A colleague once asked a postmodern theorist of international relations how he would evaluate two works on the same topic. If he had to decide which was better, how would he choose? After a brief pause, the theorist replied that he would prefer the one that revealed the most hidden power. My colleague was too polite to ask the obvious follow-up: how would he know this result?
2. The label is from Robert H. Bates, *Open-Economy Politics: The Political Economy of the World Coffee Trade* (Princeton, N.J.: Princeton University Press, 1997).
3. Peter A. Gourevitch, "International Trade, Domestic Coalitions, and Liberty: Comparative Responses to the Crisis of 1876–1896," *Journal of Interdisciplinary History* 8, no. 2 (1977): 281–313. Personal communication, summarizing the reviews received when the paper was submitted to a leading journal of international political economy.
4. Ronald Rogowski, *Commerce and Coalitions: How Trade Affects Domestic Political Alignments* (Princeton, N.J.: Princeton University Press, 1989); Jeffry A. Frieden, *Debt, Development, and Democracy: Modern Political Economy and Latin America, 1965–1985* (Princeton, N.J.: Princeton University Press, 1991). Work parallel to that of Gourevitch, Rogowski, and Frieden was occurring in economics under the heading of "endogenous tariff theory." See Stephen P. Magee, William A. Brock, and Leslie Young, *Black Hole Tariffs and Endogenous Policy Theory: Political Economy in General Equilibrium* (New York: Cambridge University Press, 1989).
5. Michael Hiscox, "Class versus Industry Cleavages: Inter-Industry Factor Mobility and the Politics of Trade," *International Organization* 55, no. 1 (2001): 1–46.
6. OEP has also proven superior to the next best alternative. Although I think the theory of hegemonic stability was dismissed prematurely (see David A. Lake, "Leadership, Hegemony, and the International Economy: Naked Emperor or Tattered Monarch with Potential?" *International Studies Quarterly* 37, no. 4 [1993]: 459–89), I admit that OEP clearly provides a better explanation for the cross-sectional pattern of protection we observe than this systemic alternative. Even on comparative grounds, OEP constitutes a better theory.

7. E. E. Schattschneider, *Politics, Pressures, and the Tariff* (New York: Prentice Hall, 1935).
8. Beth Simmons and Zachery Elkins, "Globalization and Policy Diffusion: Explaining Three Decades of Liberalization" (paper presented to the annual meeting of the American Political Science Association, Washington, D.C., September 2000).
9. Michael W. Doyle, "Liberalism and World Politics," *American Political Science Review* 80, no. 4 (1986): 1161–69.
10. Henry S. Farber and Joanne Gowa, "Politics and Peace," *International Security* 20, no. 2 (1995): 147–84.
11. Zeev Maoz and Bruce Russett, "Normative and Structural Causes of Peace, 1946–1986," *American Political Science Review* 87, no. 3 (1993): 624–38.
12. See Bruce Bueno de Mesquita et al., "An Institutional Explanation of the Democratic Peace," *American Political Science Review* 94, no. 4 (1999): 791; and Kenneth A. Schultz, "Do Democratic Institutions Constrain or Inform? Contrasting Two Institutional Perspectives on Democracy and War," *International Organization* 53, no. 2 (1999): 233–66.
13. James D. Fearon, "Rational Approaches for War," *International Organization* 49, no. 3 (1995): 379–414.
14. Joseph M. Grieco, "Anarchy and the Limits of Cooperation: A Realist Critique of the Newest Liberal Institutionalism," *International Organization* 42, no. 3 (1988): 485–507.
15. John Mearsheimer, "The False Promise of International Institutions," *International Security* 19, no. 3 (1994–95): esp. 19–20.
16. Robert Powell, "Absolute and Relative Gains in International Relations Theory," *American Political Science Review* 85, no. 4 (1991): 1303–20; Duncan Snidal, "Relative Gains and the Pattern of International Cooperation," *American Political Science Review* 85, no. 3 (1991): 701–26.
17. Joseph M. Grieco, "Understanding the Problem of International Cooperation: The Limits of Neoliberal Institutionalism and the Future of Realist Theory," in *Neorealism and Neoliberalism: The Contemporary Debate*, ed. David A. Baldwin (New York: Columbia University Press, 1993); Robert O. Keohane, "Institutional Theory and the Realist Challenge after the Cold War," in ibid.
18. There is, of course, a postmodern critique that our discourse itself shapes the world in which we live, but this raises a deeper set of issues than I want to dwell on in this essay.
19. Benjamin A. Most and Harvey Starr, "International Relations Theory, Foreign Policy Substitutability, and 'Nice' Laws," *World Politics* 36, no. 3 (1984): 383–406.
20. David A. Baldwin, *Economic Statecraft* (Princeton, N.J.: Princeton University Press, 1985).
21. See David A. Lake, *Entangling Relations: American Foreign Policy in its Century* (Princeton, N.J.: Princeton University Press, 1999); Robert P. Hager Jr. and David A. Lake, "Balancing Empires: Competitive Decolonization in International Politics," *Security Studies* 9, no. 3 (2000): 108–48.

22. Christopher H. Achen and Duncan Snidal, "Rational Deterrence Theory and Comparative Case Studies," *World Politics* 41, no 2 (1989): 143–69.
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30. Krasner, *International Regimes,* 2.
31. Nina Tannenwald, "The Nuclear Taboo: The United States and the Normative Basis of Nuclear Non-Use," *International Organization* 53, no. 3 (1999): 433–68.
32. Christopher Rudolph, "Constructing an Atrocities Regime," *International Organization* 55, no. 3 (2001): 655–91.
33. Goldstein et al., *Legalization and World Politics;* Koremenos, Lipson, and Snidal, *The Rational Design of International Institutions.*
34. Robert O. Keohane and Lisa L. Martin, "Institutional Theory as a Research Program," in *Progress in International Relations Theory: A Critical Assessment and Application of Imre Lakatos's Methodology of Scientific Research Programs,* ed. Colin and Miriam Elman (forthcoming).
35. An important exception is Lisa L. Martin, *Democratic Commitments: Legislatures and International Cooperation* (Princeton, N.J.: Princeton University Press, 2000).
36. See Gary W. Cox and Mathew D. McCubbins, *Legislative Leviathan: Party Government in the House* (Berkeley: University of California Press,

1993) and D. Roderick Kiewiet and Mathew D. McCubbins, *The Logic of Delegation: Congressional Parties and the Appropriations Process* (Chicago: University of Chicago Press, 1991) for examples.

37. Kenneth Shepsle, "Institutional Arrangements and Equilibrium in Multidimensional Voting Models," *American Journal of Political Science* 23, no. 1 (1979): 27–59.
38. William H. Riker, "Implications from the Disequilibrium of Majority Rule for the Study of Institutions," *American Political Science Review* 74, no. 2 (1980): 432–46.
39. On endogenizing institutions in international relations, see Peter A. Gourevitch, "The Governance Problem in International Relations," in *Strategic Choice and International Relations*, ed. David A. Lake and Robert Powell (Princeton, N.J.: Princeton University Press, 1999).
40. Lake, *Entangling Relations*.
41. W. W. Willoughby and C. G. Fenwick, *The Inquiry Handbooks*, vol. 16 (originally published as *Types of Restricted Sovereignty and of Colonial Autonomy*, Washington, D.C.: GPO, 1919; reprint, Wilmington, Del.: Scholarly Resources, 1974).
42. For the former see Lake, *Entangling Relations*, 147–48 and ch. 6. For the latter see Kathleen Hancock, "Specific Assets and White Knights: Explaining Ukraine's Success in Escaping Russia's Grasp" (paper presented at the annual meeting of the American Association of Slavic Studies, Denver, Colo., November 2000).
43. Most importantly, this selection bias has led analysts to largely ignore what I have elsewhere called "governance costs." See Lake, *Entangling Relations*.
44. Kenneth Waltz, *Theory of International Politics* (Reading, Mass.: Addison-Wesley, 1979), 81–93; alternatively Helen Milner, "The Assumption of Anarchy in International Relations Theory: A Critique," *Review of International Studies* 17, no. 1 (1991): 67–85.
45. Although the "problem" of institutional effectiveness has not previously been formulated in precisely this manner, recognizing it helps explain the focus of many institutionalists on the European Union. As the most hierarchical of the contemporary and voluntarily negotiated institutions, it provides crucial analytic leverage on the question of institutional effectiveness. Yet it is only one additional data point, and it may well be an outlier on which we would be well advised not to place much credence.
46. Miles Kahler and David A. Lake, "Globalization and Governance" (paper presented to the annual meeting of the American Political Science Association, Washington, D.C., September 2000).
47. Peter A. Katzenstein, Robert O. Keohane, and Stephen D. Krasner, "International Organization and the Study of World Politics," *International Organization* 52, no. 4 (1998): 645–85.
48. For examples of such convention traps, see Gerry Mackie's chilling discussion of footbinding and infibulation. Gerry Mackie, "Ending Footbinding and Infibulation: A Convention Account," *American Sociological Review* 61, no. 6 (1996): 999–1017.