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Why "isms" Are Evil: Theory, Epistemology, and Academic Sects as Impediments to Understanding and Progress[1]

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This essay probes tensions between our professional practices and the quality of our professional output in the field of international studies. We organize ourselves into academic "sects" that engage in self-affirming research and then wage theological debates between academic religions. This occurs at both the level of theory and epistemology. Unfortunately, this academic sectarianism produces less understanding rather than more. Theoretically, we should focus on developing contingent, mid-level theories of specific phenomena. Epistemologically, we should recognize there are multiple valid and perhaps even complementary paths to understanding.

Our duty as scholars is to enhance understanding of the world and, if possible, to identify levers that when manipulated can facilitate progress toward more humane and normatively desirable ends. Society privileges us as academics with a relatively prosperous and certainly desirable lifestyle so that we can add to the stock of human knowledge and, hopefully, provide some insight into how to improve social life. Most important, society—or at least, Western society— entrusts us with the power to organize our professional lives and our academic inquiries in any way that we, as scholars, think appropriate. We are a genuinely self-regulating profession both in what constitutes knowledge and in how we define and create incentives for professional success. In this essay, I probe some tensions between our professional practices in the field of international studies and the quality of our professional output. We are not giving society what it deserves, not only in terms of policy-relevant research where the "the gap" is of long-standing (George 1993). We are not giving society what it deserves even in terms of basic theoretical and empirical knowledge about world politics, a domain that we as scholars claim as our own.

My critique of our profession is a common one, but one worth repeating. Most generally, we organize ourselves into academic "sects" that engage in self-affirming research and then wage theological debates between academic religions. This occurs at both the level of theory and epistemology. In turn, we reward those who stake out extreme positions within each sect. Unfortunately, this academic sectarianism, a product of our own internal political struggles, produces less

understanding rather than more. Some reasonably fear intellectual "monocultures," as McNamara (2009) has called the possible hegemony of rationalism. But the current cacophony is not a sign of productive intellectual ferment in the pursuit of meaningful knowledge.[2] Rather, we have produced a clash of competing theologies each claiming its own explanatory "miracles" and asserting its universal truth and virtue.

Instead, a large measure of intellectual humility is in order. Theoretically, we are far from the holy grail of a universal theory of international politics—if indeed such a grail even exists. We should focus instead on developing contingent, mid-level theories of specific phenomena. This analytical eclecticism is likely to be more productive (Sil and Katzenstein 2010). But we also need a lexicon for translating otherwise incommensurable theories and making them mutually intelligible. In the following section, I outline the problems with theoretical sects and affirm the case for analytic eclecticism. I then end with one possible "Rosetta stone" that aims to facilitate conversation across research traditions by suggesting that all theories of international studies can be disaggregated into the basic and common concepts of interests, interactions, and institutions.

Epistemologically, there is perhaps an even deeper divide that is, unfortunately, not so easily bridged. The nomothetic vs narrative divide cuts through all of the social sciences and possibly beyond. This divide endures because scholars—either innately or through socialization—find one form of explanation more intellectually satisfying than the other. Yet, in international studies, we have reified this divide and, as with our theories, have formed mutually exclusive churches. Rather than claiming one or the other epistemology is always and everywhere superior, we should recognize that both are valid and perhaps even complementary paths to understanding. The question is not which approach is inherently superior, but which yields greater insights under what circumstances. The second major section below takes up epistemology and its consequences for professional practice and knowledge.

Theory: Academic Sects

International studies has long been divided into several so-called paradigms or what Sil and Katzenstein (2010), following Laudan (1996), more appropriately call "research traditions."[3] These research traditions include realism, liberalism, Marxism, neorealism, neoliberal institutionalism, constructivism, postmodernism, and feminism (see Reus-Smit and Snidal 2008). Recently, this list has grown to include a variety of non-ism "isms" including the English school, International Political Sociology, Open Economy Politics, and others.

Each research tradition has a unique set of core assumptions about the nature of world politics. These assumptions, in turn, are often incommensurate across traditions. They specify different units of analysis (individuals, groups, and states), varying interests of actors (wealth, power, and status), even different decision-making processes (logic of consequences vs logic of appropriateness). Perhaps

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[2] See Phillips and Weaver (2011). My own contribution to this debate (Lake 2009a), I fear, has been frequently misinterpreted. My description of Open Economy Politics and its progressive nature was not meant to imply its superiority to other approaches to IPE, but rather to suggest how one coherent research tradition could make significant theoretical and empirical contributions. For a more developed and critical view of OEP, see Lake (2009b).

[3] Kuhn (1970) used the concept of paradigm to denote a single, overarching set of assumptions that define a near universally shared "normal science." Scholars of international relations have used the term to refer to multiple approaches that each contains different assumptions. We have expropriated Kuhn's concept for its antithesis, the existence of many theories in what he would have regarded as a pre- or unscientific phase of inquiry. Laudan's research traditions share Kuhn's focus on shared foundational assumptions but can coexist and compete for extended periods of time. I use the term research tradition throughout but it would not be inappropriate, following common practice in international studies, to use paradigm as a synonym.

most important, they embody different visions of world politics as inherently conflictual, more cooperative, or the "open" product of the actors' own actions. As in all research traditions, these assumptions define the boundaries of inquiry, what is "known" and unknown, and even what questions and puzzles are worth asking.[4] Although I think the association is more tenuous or at least lacks a one-to-one correspondence (see Epistemology: The Enduring Divide), sets of assumptions may also imply certain epistemologies and methods.

This diverse range of research traditions reflects the complex state of world politics and the tentative state of our knowledge. International studies deals with the largest and most complicated social system possible (barring extraterrestrials, see Wendt and Duvall 2008; or perhaps zombies, see Drezner 2011). We do not have—and are not likely to have for the foreseeable future—a general, universal, and empirically powerful theory of international studies. We are far from a grand unified field theory of international studies, if such a thing is even possible. Rather, we will almost certainly continue to have many different partial theories that, at best, provide insight into limited pieces of the overall puzzle of world politics. As scholars, we should accept these limits with humility and grace and, indeed, embrace partiality.

Five Pathologies

The problem is not that we have too many research traditions, but how we respond to this plethora. Nor is the problem our professional structures and incentives, as many other disciplines operate within the same environment and have responded in more productive ways.[5] Rather, we have adopted over time a set of professional practices that produce five linked pathologies.[6] These pathologies, in turn, transform research traditions into insular "sects" that eschew explanation in favor of theology. Fortunately, if these pathologies follow from professional practices, they can be remedied by changing those norms. Recognizing these practices and highlighting their effects is a necessary first step toward change.

First, we *reify research traditions.* We are all familiar with the mandatory literature review in articles and books wherein complex literatures are grouped into one or more schools for the primary purpose of demonstrating what is new and unique about the author's own contribution. These are necessary for positioning our work in the field. We likewise organize courses—and especially introductory courses and graduate field seminars—in terms of the "great debates" or "great books" where we assign exemplary works that help students identify the core traits of each tradition. We also organize our handbooks of international studies by research tradition (see Reus-Smit and Snidal 2008). This is how we introduce novices into the shared understanding of the field and its practices. In classifying research into such categories, we collectively construct the research traditions in

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[4] As one example, the assumption that the international system is anarchic has had profound effects on shaping the discipline of international studies, leading to a neglect of various forms of international authority. This neglect is now being usefully challenged by the growing literatures on global governance (see Held and McGrew 2002; Kahler and Lake 2003; Avant, Finnemore, and Sell 2010; Lake 2010); on state-to-state authority relations (Lake 1999, 2009c; Weber 2000; Cooley and Spruyt 2009) on private authority in the international system (Cutler, Haufler, and Porter 1999; Hall and Biersteker 2002), and on supranational authority (Hurd 1999; Cronin and Hurd 2008).

[5] These pathologies are not necessarily unique to international studies. Other social science disciplines appear to suffer from similar problems. But international studies is afflicted with a particularly acute form of the disease.

[6] Although bemoaning our tendency to stereotype intellectual opponents below, I recognize that I am painting with a broad brush here. These are common tendencies in the field, not traits of any individual scholar. For this reason, I intentionally do not cite any individual as "exemplary" of any pathology. I also admit to having suffered from all of these pathologies myself at moments in my career. I do not exempt myself from the criticisms I level against the profession, and indeed may be more guilty than most.

which we live. But in classifying research in these ways, we also necessarily lose subtlety, emphasize lowest common denominators, and simplify scholarly inquiry into easily recognizable schools. In short, through often well-intentioned practice, we force research into artificial traditions that we mistakenly believe have real standing and meaning.

Sometimes, categories are also self-imposed as scholars search for a shared identity with like-minded others. We are active participants in creating our own professional fragmentation. More insidiously, however, even while perhaps proclaiming our own broadmindedness and denying that "our work" fits cleanly into any particular research tradition, we nonetheless classify others into these neat, simplified categories.[7] We may believe ourselves to defy academic stereotypes, but we are usually all too ready to label our intellectual opponents as a this or a that. Just like ethnic identities in times of conflict, it matters less how we may think of ourselves and more how others think of us. We may congratulate ourselves on our intellectual cosmopolitanism and flexibility, but we are in the end branded by others as adherents to one or another tradition and credited—or more likely damned—with all of the failings of the approach. One's research tradition is not entirely of one's own making.

Second, having reified research traditions, we then reward extremism. We all cite the same canonical sources in our own and other research traditions.[8] We assign these same works in our courses. These canonical works are typically read to embody the assumptions of a research tradition in pure form. Their purpose is to communicate meaning and information to other scholars—citing "Waltz 1979," "Keohane 1984," or "Wendt 1999," for instance, carries a world of meaning to sophisticates who have learned the research traditions. These canonical works serve a useful purpose by orienting debates within the field.

Although they are typically bold, clear statements of a new approach, like the research traditions they come to embody these iconic works are themselves reified, with meanings attributed to them that the author may not have intended. In interpretation, they lose subtlety, sophistication, and—most important— qualification. Wendt, for instance, has written that, properly understood, many of the issues between rationalism and constructivism dissolve (Fearon and Wendt 2002, 67). Keohane (1984, esp. 9 and 245), in his own writings, has always positioned himself between traditions as beginning from realist foundations and then developing the role of institutions on that footing. Nonetheless, these canonical works are commonly interpreted by the international studies community as staking out intellectual positions that are notable precisely for their "purity" or, more accurately, their extremism.

These canonical works—in their stylized and reified forms—shape the discipline. Sitting at the top of the profession, in turn, their authors receive disproportionate professional rewards, not least because our universities and profession value visibility, whether defined in terms of citations or valuable space on graduate syllabi. These rewards are appropriate in recognizing the unusual contributions of scholars whose research sets the agenda in the field. But these rewards also create incentives for younger scholars to compete for recognition by adhering to a sectarian position and, more important, taking even more extreme positions. Indeed, although everyone writing on international studies no doubt hopes to make an impact on the discipline and our understanding of world affairs, authors of canonical works—aware that they are breaking new ground —are often quite sensitive to how they fit into and are dependent on existing streams of political thought. In the quest for recognition, however, their

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[7] Constructivists refer to these moves as "ego-casting" and "other-casting."

[8] Yet, we do not assign a valance to citations; positive citations to foundational works on which others build are given equal weight as negative citations that begin "X is wrong because...."

followers often become more shrill and single-minded, further reifying the approaches with which they identify.

For reasons of self-identification and increased professional visibility, we also create incentives for adherents of nascent approaches to create new "isms," often embodied in new organized sections that they can control within professional associations. Creating a new section provides a measure of legitimacy, standing, and status for adherents and certifies the approach as one that should be taken seriously by others. It also provides opportunities for entrepreneurs to earn visibility in general and become intellectual leaders of a new research tradition. If there is a tendency in academia towards intellectual fragmentation, the disproportionate rewards for intellectual extremism create a further centripetal force. Ironically, professional associations, often originally formed as a common ground for scholars, become a force for further fractionalization in the discipline. At both the individual and collective levels, extremism breeds further extremism.

Together, these first two pathologies lead to a proliferation of research traditions within international studies. The existence of these multiple traditions is not in itself a bad thing. To the extent that they organize research and produce new ideas they can be progressive. But when combined with three further pathologies, they begin to inhibit rather than aid scholarly inquiry.

Third, we mistake research traditions for actual theories. As noted, traditions are defined by shared sets of core assumptions. In principle, these core assumptions may be sufficient to generate deductively valid hypotheses or other forms of explanation. In practice, however, these shared assumptions are more often incomplete and must be supplemented by additional assumptions to yield specific hypotheses and explanations. That is, the core assumptions orient scholars working within a research tradition, but they are seldom complete enough to explain specific outcomes of interest. Waltz (1979:118, 121), for instance, was emphatic that two and only two assumptions—anarchy and the desire to survive—were sufficient to predict that states will tend to balance against stronger powers. But these assumptions are, in fact, consistent with a much larger range of behaviors, including cooperation and collective security organizations, on the one hand, and bandwagoning, on the other. In other words, these same assumptions are consistent with both balancing and not balancing, and thus, the research tradition is theoretically indeterminate (Lake and Powell 1999:23–24). To deduce the proposition that states tend to balance, additional assumptions must be added to the core assumptions of neorealism. Similarly, Powell (1991) and Snidal (1991a,b) showed that both neorealism and neoliberalism did not generate unique predictions about relative and absolute gains maximization, as commonly thought (Grieco 1993), but were actually indeterminate as well; with additional assumptions, each tradition could generate predictions of relative and absolute gains behavior as a special case. That most research traditions are insufficient to generate hypotheses about actual interstate behavior is further suggested by the proliferation of theories within each that all claim allegiance to the same core assumptions. Thus, we have offensive realism, defensive realism, neoclassical realism, and more, all sharing a common set of assumptions understood as realist but differing in the auxiliary assumptions they employ.[9] This explains how numerous theories can co-exist and be unified within a single research tradition.[10]

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[9] For exemplars, see Mearsheimer (2001) on offensive realism, Waltz (1979), and Walt (1987) on defensive realism, and Lobell, Ripsman, and Taliaferro (2009) on neoclassical realism. The distinctions were first drawn by Snyder (1991:11–12).

[10] These theories are also often indeterminate as well. More assumptions do not necessarily guarantee a theory is complete and actually generates its purported propositions in a logically deductive manner.

In turn, since their assumptions are typically incomplete, research traditions cannot be assessed directly. As they are often underspecified and do not generate deductively valid hypotheses themselves, they cannot be "tested" on their own terms. Sometimes, since with different auxiliary assumptions they may predict both a behavior and its opposite, they cannot be tested at all. One can probe the explanatory power of a theory, but usually not of a tradition. Nonetheless, scholars often pit traditions against one another in head-to-head competitions.[11] In these battles, since each tradition is incomplete and yields few logically deductive predictions, no empirical evidence can shed any meaningful light on the explanatory power of the approach or, inversely, broad ranges of behavior may be equally consistent with its assumptions. Without determinate predictions, scholars play a game of "heads I win, tails you lose" in which their preferred approach is almost always supported.

Fourth, we narrow the permitted subject matter of our studies to those topics, periods, and observations that tend to confirm the particular strengths of our tradition. Realists, for instance, tend to focus mainly on security policies of great powers where their assumptions appear to fit slightly better, and then find evidence for the power of realism. Liberals tend to focus on economic policy where their assumptions appear to fit slightly better, and then find evidence for the power of liberalism. Neoliberal institutionalists study institutions, which not coincidentally tends to affirm the important role of institutions. Constructivists study changes in norms, and find their approach persuasive. English school scholars often focus on the socialization of polities within the international system, and find evidence of the power of their approach and so on. This narrowing of empirical focus need not be a conscious strategy but can be a natural by-product of the search for confirming evidence. Having spent years developing a theory, it is not unreasonable to apply it where it is most likely to fit—at least as a first, initial test of plausibility. Journals and book publishers are also notoriously loath to publish null findings, a belief that need not be true to have a major effect on the choice of research topics by scholars of different traditions.

By narrowing its empirical focus, however, each tradition affirms itself by studying that which it does best and ignoring subjects that do not conform to expectations. This produces self-affirming sects that come to believe in the power of their tradition based on a selective reading of the possible empirical evidence. It is here that research traditions move from the realm of objective social science to theology. Having adopted a tradition, we then look only for evidence that affirms our prior belief in the rightness of that tradition. Practice becomes not an attempt to falsify theories through ever more demanding tests, but to support theories that were adopted prior to their confrontation with evidence. In essence, we eschew social science theories that can, in principle, be falsified for beliefs that are largely impervious to evidence.

Fifth, scholars within each research tradition aspire for their approach to be the *scientific paradigm.* Rather than accept that our favored tradition is inevitably partial and limited in scope and domain, we seek intellectual hegemony. We claim that our particular tradition with its unique set of assumptions is a general approach that can and should be treated as a universal or near universal paradigm. I have always found the phrases "I am realist" or "As a neoliberal institutionalist, I think..." to be peculiar statements. The only logical construction is that the speaker is asserting that his or her particular tradition is superior to all other known traditions, a claim that all questions can be answered by theories based on the assumptions of that tradition. Thus, by the third and fourth pathologies, we validate our often incomplete theories through favorable and selective

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[11] For classic examples, see Doyle (1997), Grieco (1990), and Odell (1982). For a contemporary example, see Brooks and Wohlforth (2008).

evidence but, by the fifth, we then assert they are universal and superior to—or at least worthy of respect by adherents of—other equally self-validated traditions.

There are, perhaps, strong reasons of self-identification to seek intellectual hegemony. As scholars, much of our self-worth is entwined with our ideas. To vanquish the alternatives, if even in our own minds, validates our contributions and ourselves. There are also strong professional incentives to win the contest for intellectual hegemony. To establish one's tradition as *the* tradition promises to put the original adherents at the top of the field. Even if individual scholars are not so narrowly instrumental, intellectual combat is like an arms race. Each tradition perceives the failure to compete for hegemony as ceding ground to opponents, and thus, each tradition believes it must compete in expectation that others will compete for dominance. To admit the partial nature of one's theory is to risk being subsumed as a special case within someone else's tradition—a lower status. Thus, everyone aspires to hegemony if only to prevent others from conquering the field. But like arms races, this intellectual competition leaves everyone worse off than if they could simply cooperate, which in this case means admitting the partial nature and limited empirical evidence for every theory now known in the field.

These five pathologies combine to divert professional debate from the substance of world politics to first principles. Having created academic sects based on incommensurate assumptions and supported by selective evidence, we do not seek to assess which approach helps us understand world politics best (or helps us understand which range of phenomena best). We focus instead on the inherent superiority of this or that set of assumptions. Rather than seeking to understand the world—our highest obligation as scholars—we debate assumptions seemingly without end. What are the fundamental units of world politics? Are individuals, groups or social collectivities, or organizations "rational"? Do actors seek power, welfare, justice, or something else? Which matters more, system or unit, structure or agency? Without comparable propositions derived from these competing research traditions and assessed against the same patterns of behavior, there is no possible answer to such existential questions. This makes for a continuing and lively debate of course, but it adds little to our understanding of world politics and nothing at all to practical policymakers. Rather than seeking to understand the complex and often frightening world around us, we spend far too much of our intellectual time and energy debating assumptions as if they mattered in absolute terms. It is here that research traditions tip over from being useful organizing devices to theologies. Assumptions stop being treated as more or less useful simplifications of a complex reality and become beliefs that are accepted or not as truths. We have left the realm of scholarly inquiry and entered the world of academic religions. By whatever definition, we have stopped doing "science."

An Alternative

Rather than forming sects and debating theology, imagine the contributions that we as scholars could make if we devoted our professional and intellectual energies to studying things that matter. Imagine reorganizing our research and professional associations around problems, not approaches. Imagine as well a graduate field seminar not organized around research traditions but topics like Global Climate Change, Growth and Development, Economic and Political Inequality, and Genocide and Political Violence. The seminar discussion could then focus on "what do we know" rather than "what are the central tenets of this particular sect"?

Likewise, the International Studies Association is divided into relatively autonomous sections, many (but not all) of which are defined by research tradition.

Imagine instead sections on these same sorts of substantive topics—not the Scientific Study of International Processes but a section on War, not Feminist Theory and Gender Studies but Female Political Empowerment. We might then attend panels that focus, say, on what we know about economic inequality and, more important, what we learn from new research.

Professional practices are surprisingly robust things. Turning a discipline may be like turning a supertanker. As suggested, there are also individuals and organized interests vested in current professional practices, not least of which are the organizational gatekeepers who set themselves up as enforcers of purity and agenda-setters for the discipline. Institutionalized practices combined with entrenched and powerful elites militate against change.

But much can be done, I hope, through the actions of individual scholars. I no longer teach the "isms" at the introductory level or in field seminars for graduate students. With Jeffry Frieden and Kenneth Schultz, I have written an undergraduate textbook that avoids sectarian debates and focuses on substantive topics (Frieden, Lake, and Schultz 2010). A volume I edited with Robert Powell, directed more at first-year graduate students, explores this approach at a higher level (Lake and Powell 1999). Both are, I am gratified, well received, especially by younger scholars just starting their teaching careers. There is, I believe, a growing frustration with the dead hand of the isms and a quest for alternative ways of organizing intellectual inquiry. We can break free of the old order. A first step in changing professional practice is to stop replicating that practice in our scholarly lives.

In addition, we should embrace partiality. That is, we should acknowledge that all current theories are partial and state explicitly their boundary and scope conditions. Modesty in acknowledging such limitations is actually in the self-interest of scholars. A common but too easy criticism to make of another's work is "yes, you may explain that" but "you can't explain this," where "this" happens to be the critic's area of specialized knowledge that, in turn, supports her favored research tradition. With properly stated boundary and scope conditions, we would know whether the theory was intended to apply to "this" and whether the critic makes a valid observation. Through changes in editorial policy, authors should be required to include a short paragraph on the boundary conditions of their analysis and to state explicitly what their theory cannot explain. Even if editors do not require it, we as individual reviewers can insist on it. More important, the end—deeper knowledge—will hopefully justify the embrace of partiality. We are all touching different parts of the proverbial elephant, even while claiming to be holding it in its entirety. By pooling our knowledge of the different parts, we might then be able to describe the whole animal more effectively. We might also then have something constructive to say to policymakers who want to control the elephant.

This is not, I want to make clear, a plea for atheoretic or necessarily policy-relevant research, although the latter certainly has its place. We need theories to explain real-world patterns, not merely to describe them. And we need basic theory to reveal causes even when they are not amenable to manipulation by policymakers. But we should, as Sil and Katzenstein (2010) argue, embrace analytic eclecticism. A single scholar ought to be able to work on questions of war with theories of rational unitary states, questions of global environmental change with theories of individual norms, and questions of trade policy with theories of sectors pursuing their material interests without fear of being criticized for inconsistency.

An International Studies Lexicon

While supporting analytic eclecticism, the only real alternative to the status quo, I nonetheless fear an intellectual tower of Babel. To enhance understanding, we

need to be able to communicate across theoretical traditions, compare assumptions, and interpret findings. Ideally, we want modular theories—separate, self-contained, and partial theories—that connect more or less well to other theories to carry out larger explanatory tasks. Even lacking such theories, we need a lexicon that allows translation across theories. Such a lexicon requires a further subtle but important shift in emphasis from what makes us *distinct* as scholars to what makes us *common*.

One possible lexicon is to think of politics as composed of some actor pursuing *interests* while engaged in *interactions* with other actors within *institutions*. Indeed, any complete theory of politics must specify all of these components: the units of analysis (for example, individuals, groups, organizations, and states), their interests (for example, power, wealth, and identity), the nature of their interactions (for example, zero-sum bargaining, multisum cooperation, reproduction of inherent inequalities, socialization), and how thickly institutionalized is the world (for example, ranging from institutions are epiphenomenal to the units' ability to interact at all requires a dense system of shared cognitive and language structures). These component parts may be taken by assumption (for example, individuals pursue identity in zero-sum bargaining—a possible model of ethnic conflict) or allowed to vary to explain some outcome of interest (for example, used as an independent variable, perhaps allowing political institutions to vary between federal and centralized systems, democracy and autocracy, etc.). These components can also be combined to explain each other, addressing questions such as why groups of type A but not type B form or why individuals develop a particular conception of their interests. Indeed, as a step toward a modular theory, a component taken as given in one theory can be taken as an object to be explained in another. We might, for example, take regime type as an independent variable in one analysis—explaining the frequency of war across dyads, as in the democratic peace—but have a separate theory that explains why some countries are democratic and some autocratic.[12] We might also begin with institutions and then theorize how they shape and define the interests of the resulting, socially constituted groups.

This lexicon provides a means of translating theories into comparable language but does not, in my view, privilege one tradition over another. One theory might assume that groups pursue material interests created by their position within the international division of labor. Another might see groups as formed by a shared understanding of their political enemies, a social construct. But the lexicon allows us to recognize that both are specifying who are the actors salient for some analysis and what are their interests. One theory may be deductive, abstract, and rational-materialist, the other historically specific, contingent, and sociological, but recognizing that both are theorizing interests allows us to begin to assess which provides a better explanation for what range of phenomena and circumstances.

There is nothing inherently rationalist or constructivist, realist or Marxist, English or North American about this set of categories. For instance, the question of how do individuals conceive of their interests can take a variety of answers. Interests may be internal to the actor, or the product of its social environment. Individuals may be understood to be highly intentional in the pursuit of their interests, or to follow a logic of appropriateness. Interests may be material or more normative in origin. Likewise, analysts can posit an essentially zero-sum bargaining problem between two states, while another might disaggregate states into a variety of competing groups with some engaged in international cooperation. Any specific theory will be colored by our priors, as they are now, but they will also differ dramatically by issue area or the problem we are seeking to explain.

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[12] Lake and Powell (1999) describes this as a "boxes-within-boxes" approach.

I would not expect a single theory of interests or interactions or institutions to apply equally well to all puzzles of world politics. But by applying this lexicon, we can begin, at least, to standardize theories and see more sharply where they are the same and where they are different. Moreover, since each theory tends to emphasize variance in some categories, and often by default fails to specify the other dimensions, attention to interests, interactions, and institutions may lead to better specified theories even on their own terms.

In summary, we should study interesting questions not theology, and organize ourselves professionally and intellectually around problems not academic sects. We should focus on what we have in common, not what makes us distinct. This will be difficult, no doubt, because of the professional pathologies I have outlined earlier, but it is the only way forward and the only basis on which we can really conceive of ourselves as a single discipline. Above all, we should recognize our duty as scholars is to promote not ourselves but understanding and social progress.

Epistemology: The Enduring Divide

There is, however, a deeper and more difficult to bridge epistemological divide in international studies between nomological and narrative forms of explanation. These two different ways of knowing are not exhaustive, but they are central to our field.[13] They underlay much of the Second Debate in international relations between the behavioralists and traditionalists (Knorr and Rosenau 1969). Although it overlapped with a number of other issues, this same epistemological divide drove the so-called Third Debate as well (Lapid 1989). This same division creates what Cohen (2007) has recently called—somewhat inaccurately in my view—the transatlantic divide in international political economy. With roots back to Aristotle, who appears to have unselfconsciously combined both forms, the cleavage is obviously of long standing. We cannot expect the divide to go away or for one side to fully triumph over the other—nor should we wish for this result. Advocates of each epistemology should recognize the legitimacy and strengths of the other.

The nomological approach builds generalizable explanations from covering laws, either as statements of enduring relationships or more frequently by assumption. Employing the hypothetical-deductive method, which yields potentially falsifiable predictions of an "if X-then-Y" form, tests are conducted through methods that ideally approximate the classic scientific or controlled experiment in which one parameter of interest is varied while all others are held constant. True experiments, still somewhat rare in international relations (see Tomz 2007; Hyde 2011), remain the gold standard with the highest internal validity, followed by quasi-experiments (for example, regression discontinuity and matching designs) (see Voeten 2009), and then correlational designs (for example, regression models).

Within the nomological approach, cause takes one of two possible meanings. First, on the basis of a well-defined research design, the analyst can show beyond reasonable doubt that $X \rightarrow Y$, and by correspondence that $\neg X \rightarrow \neg Y$. Second, on the basis of a well-defined theory, the analyst can show that X co-occurs with Y or that ΔX co-occurs with ΔY. In the first case, the conclusion follows from the experiment or quasi-experiment itself, and may stand even if the theory is not

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[13] On different epistemologies in international studies, see Jackson (2011). The nomological and narrative protocols, as developed here, do not correspond precisely to his categories. Neopositivism is largely the same as the nomological approach. Critical realism and analyticism share much with the narrative protocol, as both depend on abduction, but are not exact. Reflexivity is quite distinct but sometimes draws upon narratives to highlight alternative currents or voices in the historical record that open practices to critical scrutiny.

fully developed. In the second, cause rests on the quality of the theoretical deduction and the robustness of the correlation between X and Y. Most nomological studies in international relations are of the weaker correlational form. Based on each set of tests, the theory is revised, extended, and enriched—or sometimes, but rarely, rejected. In our post-positivist era, science is an iterative cycle of deduction, testing, and revision.

Within the nomothetic approach, external validity or generalizability is not determined by the nature of the test but by the scope and boundary conditions of the theory. A general theory that has passed many difficult tests with high internal validity can be appropriately applied to other cases and time periods that are within its scope and boundaries. A theory that applies to all humans can be appropriately supported by experiments conducted on college students; it is not the subjects that define the limits of the theory, but the theory itself. Conversely, a theory that applies to contemporary Americans, perhaps because it assumes a set of Western values, cannot be supported or disconfirmed by experiments conducted on, say, Japanese college students. Similarly, a theory that takes states to be its units of analysis can neither be supported nor disconfirmed by experiments on college students, except by analogy. Generalizability is a theoretical not an empirical condition.

The second epistemology is the narrative protocol, composed of what Ruggie (1998:94) terms the descriptive and the configurative "orders" of information. The descriptive order links more or less "thickly" described events through time and seeks to identify the effect of each event on subsequent developments (Geertz 1973). The second, configurative order organizes these descriptive statements into an interpretive structure through "abduction," which entails the successive adjusting of a conjectured ordering scheme to the available facts (Peirce 1955:151–152; on the analogous principle of emplotment, see Polkinghorne 1988:19–20). The point of the exercise is to produce a narrative that is verisimilar and believable to others looking over the same events. Or, as Max Weber put it, the narrative protocol seeks to understand why relationships are "historically *so* and not *otherwise*" (quoted in Ruggie 1998:30).

Within the narrative approach, cause takes the form of "just-so" accounts. As implied by Ruggie (1998:94), the criteria for judging verisimilitude are largely subjective, unlike those for internal validity in the nomological approach.[14] Nonetheless, the narrative protocol typically provides a fuller or more holistic account of particular events. Often criticizing nomological studies for mono-causal explanations, narrative accounts typically emphasize the conjuncture of many variables that combine to produce the unique event under study.[15] As such, external validity remains a theoretical and not an empirical problem, and theories that pass hard tests of verisimilitude can also be generalized within appropriate scope and boundary conditions. Yet, the emphasis on uniqueness within many narrative accounts inhibits straightforward extensions of theories to new circumstances.

Despite much confusion on this point, these two epistemologies are not wedded to particular methods.[16] Epistemology is not the same as methodology. The

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[14] This lack of consensus on criteria for evaluating narrative accounts is, to my mind, one of the most severe limitations on progress in this approach. The qualitative research program (centered in the Consortium for Qualitative Research Methods), focused on building legitimacy for qualitative methods as a valid form of science, has not in my view fully grasped this problem and its implications.

[15] The criticism of mono-causal explanations is unjustified, in part. In the nomological approach, research designs endeavor to study one variable at a time while holding all else constant. Varying any of the constants, in turn, may also influence the main effect, especially in non-linear models. Part of the responsibility for this criticism, however, must be placed on advocates of the nomological approach. Scholars in this approach are often cognizant of the effects of control variables in principle but still fall to adjust their predictions for the ceteris paribus clause.

[16] Likewise, although many realists, neoliberals, and others are inclined toward the nomological approach, and many constructivists are inclined toward the narrative protocol, there is also no one-to-one correspondence between theory and epistemology.

nomological approach can be used with well-designed historical case studies (King, Keohane, and Verba 1994), even of the process-tracing variety (see George and Bennett 2005). The narrative approach can use large numbers of observations and statistical analyses within particular events.[17] Much attention has been drawn recently to "mixed methods" approaches, which I applaud. Our objective should always be to use the best possible method for any theory. Passing ever harder tests using different methods only adds confidence to our theories. But mixed methods do not by themselves address the epistemological differences. Methods and epistemology are separate issues and should be treated as such.

The question of epistemology in international studies suffers from the same pathologies for theories outlined earlier, and which I need not repeat here. We reify each approach, reward extremism, fail to specify research designs completely, apply epistemologies selectively where they are most likely to work, and then claim universality. Through these pathologies, we not only create academic religions of different theories but also become committed to academic sects with different epistemologies.

Like our theories, these epistemologies have become increasingly politicized and used as criteria and even weapons in power struggles within the discipline. Gatekeepers increasingly use one's adherence to this or that epistemological religion to determine who gets hired where, who gets access to resources, and who is accepted in various professional networks. We increasingly talk and interact only with others of our same epistemological persuasion.

Yet, although it may disappoint partisans, I can think of no objective reason to prefer one epistemology over another. Rather, the choice of epistemology by scholars appears to be largely subjective. We appear to be drawn to one or the other approach by intuition: one form of explanation simply feels right. Some are satisfied only when an event is placed in its full historical perspective with all the conjunctures and counterfactuals accounted for. Others are satisfied only when events accord with an appropriately derived hypothesis that has passed many demanding experimental tests. For myself, I read a lot in history—far more than I read in political science—and benefit from and enjoy these mostly narrative accounts immensely. But at the same time, I am usually not persuaded by causal claims that lack well-specified theories and experimental tests. In turn, while most of my own research has focused on the history of US foreign policy, the cases are treated within a nomological approach (see Lake 1988, 1999). One can move across the divide without finding the causal claims on the other side especially satisfying.

I do not know whether these epistemological tastes are innate, genetically hard-wired, or learned, the product of early socialization. Just as beauty is found for some in pure math and others in poetry, I suspect that scholars come predisposed to prefer one form of explanation over the other. Although graduate school appears to shape one's views, suggesting socialization, there is also a lot of self-selection into graduate programs.[18] Similarly, academic departments are typically populated by scholars of diverse theoretical perspectives but tend to be more homogenous by epistemology, also suggesting socialization. Yet, again, scholars typically want to be surrounded by colleagues whose work they respect and admire, and what they find persuasive, in turn, is partly a function of epistemology. Thus, innate tastes can lead to intellectual clustering within departments and segregation between institutions.[19] That there is a transatlantic divide in international studies along epistemological lines is undeniable, and is probably the strongest evidence

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[17] For examples, see among others Schonhardt-Bailey (2006) and Chwieroth (2010).

[18] In my experience, the most unhappy graduate students are those who end up in programs that center on the "wrong" epistemology from their personal point of view.

[19] As Schelling (1969) showed in his models of racial segregation, the "prejudice" need not be strong for clustering to dominate. Drawing on personal experience again, it appears that hiring against theoretical type is relatively easy for most departments, but hiring against epistemological type remains much harder.

for socialization (Cohen 2007). Even here, however, professional gatekeepers within different intellectual communities—who themselves know intuitively that their preferred form of explanation is superior—may also shape the development of the discipline within their respective regions. The upshot is that we do not know much about the origins of epistemological preferences. Regardless of origins, however, epistemological differences exist and massively affect how we evaluate research in the field.

The nomothetic and narrative forms of explanation have coexisted for centuries, perhaps even millennia. One may appear to pull ahead at a particular moment, but the other always seems to stage a comeback. Neither is likely to win nor lose decisively. What then is to be done? Faced with alternative forms of explanation, what should scholars do? The usual route, as with theory, is to try to crush adherents of the other faith by seizing and occupying strategic positions within the disciplinary hierarchy. And we do see considerable disciplinary positioning occurring today, at least in international studies. Many think of this as a war in which truth and justice will prevail only if their side wins. I suggest an alternative course that embraces diversity at the level of the discipline, even if individual scholars continue to find one or the other approach more intuitively satisfying. As a community, we ought to do the following.

First, recognize the legitimacy of alternative forms of explanation and respect the approach used by colleagues. It is the supreme act of intellectual hubris to assume that whichever form of explanation intuitively appeals to you is the inherently better form. If the choice of epistemology is even partly subjective, then trying to win converts is a fool's errand, at best a temporary achievement.

Second, judge each approach on its own terms. We should focus on improving each approach as it understands its own success. Is the narrative sufficient? Are there alternative currents in the historical record that need to be told as well? Are there important events that are inconsistent with the narrative or that are distorted by the analyst to be made consistent with the narrative?[20] Alternatively, is the theory logically derived? Do the hypotheses follow from the premises? Are the variables operationalized appropriately? Is the research design sound?

Third, accept that each approach is likely to apply more or less well to different questions. At the moment, and most likely into the future, the narrative protocol can likely apply better to long-term changes in global norms, such as those against slavery and in favor of human rights. Changes in fundamental conceptions of right and wrong, legitimate and illegitimate behavior, are rare and typically do not lend themselves to nomological analysis. The processes unfold over a time scale in which it is difficult to hold features of the social environment constant. Disciplined narratives are likely to produce greater insights into these phenomena. Conversely, when social interactions are repeated frequently or within well-structured and stable institutions, nomological analysis may offer parsimonious and powerful explanations. We know a lot about the causes of wars and civil wars, given their frequency and observability. We also know a lot about economic policymaking within democratic institutions where the rules of the "political game" are well specified. Each approach has much to teach us. The question is not which epistemology is inherently superior, but when and where each approach offers greater insights and understanding.

Conclusion

As I began, our task as scholars is to understand better the world in which we live. Our privileged position as scholars in society rests upon this goal, or

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[20] This is where critical theory offers its most important contribution by giving voice to those silenced by the powerful and articulating alternative narratives.

at least its pursuit. We do not produce understanding by fighting theological wars between ourselves at either the theoretical or epistemological levels. Rather, we achieve understanding by asking questions about important phenomena that we do not now understand well, employing appropriate theories to answer these questions, and then being honest with ourselves and others about the strengths and weaknesses of the evidence we have been able to bring to bear.

Today, no single theoretical or epistemological approach deserves hegemony. Diversity of theory and method is necessary, at least at this stage of our intellectual development. Intellectual monocultures are rightfully feared. But the current cacophony is not what we should aspire to. Rather than useful debate we have turned inward to self-contained research traditions and epistemologies and, in turn, we focus on first principles. Intellectual progress does not come from proclaiming ever more loudly the superiority of one's approach to audiences who have stopped listening. Let's end the theological crusades and seek progress in understanding real problems of world politics. Perhaps then we will earn the privileges society has accorded us.

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