I CAN HAS IR THEORY?

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When scholars discuss the state of international-relations theory they often refer to rather different, if sometimes overlapping, things. Some have in mind the "paradigms" or "isms" that supposedly dominated the field in the 1980s and 1990s, such as realism, liberalism, and Marxism. Some mean the absence of the so-called "great debates" that supposedly occupied the field at key moments in its last century of development. Others focus on the strength of middle-range theorizing; they are concerned with the persuasiveness, innovativeness, or sophistication of the explanatory models linking putative causes to outcomes. One's assessment of the state of international-relations theory depends a great deal on which interpretation one adopts. Each implies different markers of vibrancy, such as debates between well-organized schools of thought with disparate core assumptions, field-wide controversies about the character of international-relations scholarship, and the presence of a robust set of explanatory models.[1]

In this essay, we argue that *none* of these three interpretations provide an adequate framework for evaluating the state of international-relations theory. We argue, instead, that international-relations they and international theory (we use these terms interchangeably) ought to be understood as claims about the scientific ontology of world politics, including its actors, proper units of analysis, and how such elements fit together. International-relations theorization may involve comprehensive schema for conceptualizing world politics—such as that offered by structural realism—or debates about specific elements of that schema—such as that between proponents of different ways of cashing out agent-structure co-constitution with reference to the relationship between states and international structures (see, e.g., Goddard and Nexon, 2005).

This understanding of international theory helps us to elucidate two crucial points. First, discussion of the "end of IR theory" tends to conflate *substantive* and *methodological* concerns. Both matter a great deal to the conditions of international-relations theorizing, but they cannot be reduced to one another. Different accounts of the state of international theory often rely on different methodological standards for what "counts" as theory. Failing to distinguish between substantive theories and methodological concerns risks obscuring the fact that we purchase certain kinds of theoretical innovation at the cost of methodological monoculture. At the same time, it sometimes leads us to miscategorize complaints about methodological monoculture as arguments about the decline of international-relations theory (see McNamara, 2009).

Second, it brings into proper focus the degree to which this debate reflects the intersection of intellectual considerations with sociological dimensions of international-relations scholarship. The state of international theory, as well as perceptions of its condition, cannot be separated from the field's hierarchy of prestige, regional and national differences, the job environment faced by young scholars, demographic changes, and a host of other factors. For example, much of the "end of IR theory" claim only makes sense if we believe that the content of a few leading Anglophone journals reflect the state of the field writ large. If we fail to consider the processes that shape the selection and display of those contents, we overlook the degree to which the "end of IR debate" may tell us more about the vocational space of professional international-relations scholars and scholarship than it does about the content of our ideas about world politics.

We proceed by first examining the three major ways of framing the "end of IR theory" discussion. We unpack their tacit assumptions about the meaning of "international theory" and provide some thoughts about the implications of each framing for assessing the health of international-relations theorizing. In the next section, we develop our argument about the nature of international theory as concerning the scientific ontology of world politics. We then discuss the relative health of international theory (properly understood). We offer some conjectures about what factors might lead to the conclusion that international-relations theory is in decline. Based on these conjectures, we offer some recommendations for how to "improve" the condition of international theory. We conclude with a call to get over ourselves and, among other things, get on the project of theorizing world politics.

[1] Here we note that our discussion concerns Anglophone international-relations theory, which might itself be a symptom of a larger problem.

Paradigms, Great Debates, and Middle-Range Theory

Many of the existing disagreements about the state of international-relations theory hinge on different understandings of what constitutes "theory" and what indicates its relative health. Our sense, which finds reflection in many of the pieces in this issue, is that there are three major frameworks for discussing the health of international theory. We term these *paradigm wars*, *great debates*, and *middle-range theorization*.

Paradigm Wars

A great deal of recent handwringing over the status of international-relations theory concerns the role of the "isms" in the field. Some, such as Katzenstein (2010), call for "analytical eclecticism" as a way of circumventing the presumed incommensurability that separates different schools of thought about international politics (see also Jackson and Nexon, 2009). Others, including Lake (2011), suggest that we dispense with theoretical aggregates altogether: "we have produced a clash of competing theologies each claiming its own explanatory 'miracles' and asserting its universal truth and virtue."[2] Still others lament, (although rarely in writing) the decline of articles that seek to adjudicate disagreements among realism, liberalism, constructivism, and other "isms". They argue, for example, that the "isms" capture important, enduring, and serious disagreements about the character of world politics; that they help maintain communities of scholarly discourse; and that they protect and nurture "infant" theoretical propositions that would otherwise fail to find outlets in prominent publishing venues (cf. Nau, 2011). All sides agree, at least implicitly, that the field was once characterized by large clashes between coherent theoretical aggregates. Scholars and scholarship could take their bearings from how their work mapped onto these "paradigms." Now, however, those clashes are, for better or worse, fading away.

Indeed, if by the end of international-relations theory we mean the passing of the "paradigm wars" as the primary motor of debates in the field, then a terminal diagnosis may be appropriate. The most recent "Teaching, Research, and Policy Values of International Relations Faculty" (TRIP) survey suggests that 22% of international-relations scholars do not use paradigmatic analysis in their work. This is the same number of scholars who self-identify as constructivists and more than the number of scholars who self-identify as realists (16%) and liberals (15%); only 5% of scholars surveyed suggested that their research was motivated by paradigmatic considerations. Far more named "issue area" (39%) and "current events/policy relevance" (33%) as the primary driver of their academic work. Clearly, location with respect to an international-relations theoretical paradigm has lost its salience as *the* central point of scholarly identity.[3]

The reduced importance of the "paradigm wars" is not necessarily a bad thing. The dominant "isms" were never paradigms in any meaningful philosophical sense—they never contained incommensurable content that differentiated them from one another and required reference to external criteria of theory adjudication. The distinguishing characteristic of a paradigm in the Kuhnian sense is that it contain core elements—ontological, linguistic, and epistemological—that preclude direct testing of its theories against those found in another paradigm. The very fact that the "inter-paradigm debate" of the 1980s and 1990s revolved around a series of empirical tests between paradigms raises some doubt about the mutual incommensurability those theoretical aggregates. Indeed, as we have argued in detail elsewhere, approaches such as "realism," "liberalism," and "constructivism" do not, in of themselves, qualify as Kuhnian paradigms or Lakatosian research programs (Jackson and Nexon, 2009; Wight, 1996).[4]

In general, the "isms" provide a poor way of mapping different kinds of theories in the field. The "isms" tend to lump together specifications of actors, attributes of those actors and their environments, and behavioral consequences of interactions between those actors as though they formed a seamless whole—and as though that seamless whole *belonged* to one or another "ism." For example, the inter-paradigm debate transformed the "balance of power" into a "realist" proposition—much to the annoyance of followers of the English School—and granted constructivists an improper monopoly over explanations resting on the

[2] On the use of the term "aggregates" to describe "isms," see Elman and Elman (2002).
[3] See Maliniak, Peterson and Tierney (2012). One complication: many of the scholars surveyed in TRIP are comparativists,. This may skew the results downward for adherents to international-relations paradigms.
[4] On forms of incommensurability, see Kitcher (1982); Sankey (1991); Sankey (1993).

importance of "norms." Too often, the field treats "isms" as little more than an unfalsifiable assertion about the predominant influence of a preferred causal factor or process, a description of the kinds of things that a group of scholars like to focus on, or wagers about important aspects of international politics (e.g., "institutions fail to tame power politics"). These are silly ways of delimiting theoretical aggregates. Liberals, and their progeny, do not enjoy ownership over economic variables (Sterling-Folker, 2002). Nor are all exercises of coercive power evidence for, or the domain of, realist theory (Dillon and Reid, 2009). And the inevitability of power politics is a wager that depends on no specific body of theoretical and analytical propositions (Jackson and Nexon, 2009).

Despite their intellectual incoherence, the "paradigm wars" *did* facilitate theory development by forcing scholars to foreground assumptions concerning the stuff of international politics. They rewarded efforts to link ontological premises and conclusions in a logically tight manner. Participants may have overplayed—whether explicitly or implicitly—claims about incommensurability, but their debates made clear that different theoretical and analytical commitments can generate different conclusions about world politics.

Indeed, some opponents of the "isms" also tend to obscure the degree to which their own commitments are far from "neutral" when it comes to studying world politics. Statistical and quasi-statistical modes of inquiry, as we discuss below, are themselves part of a "neopositivist" approach to knowledge creation that reflects one of a number of different ways of doing social science (Jackson, 2011). Lake's (2011: 473) *interests*, *interactions*, and *institutions* approach may, as he argues, be compatible with some varieties of "rationalist," "constructivist," "realist," and "Marxist" thought, but it involves substantial commitments to choice-theoretic methodological individualism.[5] At the very least, the "isms" debate kept theory in the forefront of international-relations scholarship; one of the questions about the current era, in which the "isms" debate seems to be fading from the scene, is whether and how theoretical assumptions will be discussed in the field. We need to be careful about calls to "just get on with it," as the "it" at stake often amounts to a set of basic but contestable wagers about world politics—or, sometimes, an approach that *actually does qualify* as a paradigm.[6]

Great Debates

The "paradigm wars" might be seen as one of a number of discipline-wide, high-stakes debates about theorizing international politics. According to the numbering of these "great debates" that is conventional in US circles, there have been three: an epic clash between "realists" and "idealists" in the 1930s and 1940s; a tussle over whether IR could or should use formal and statistical techniques (as opposed to the methods of diplomatic history) in the late 1950s and 1960s; and what Lapid (1989) christened the "third debate" in the late 1980s and 1990s, which featured "positivists" committed to notions of scientific progress opposing "post-positivists" questioning this Enlightenment narrative of cumulative knowledge. The "inter-paradigm debate" among the "isms" doesn't appear in the conventional US narrative, although Wæver (1998) makes a compelling case that it *ought* to count as a "great debate" alongside the other three. All of these "great debates" are supposed to both represent and also fuel theoretical innovation. They bring, the argument goes, fundamental disagreements about the study of world politics into sharp relief. These controversies, in turn, advance the field.

[5] Rathbun (2011) advances a more pointed critique. He argues that Lake's scholarly practices suggest that his approach only genuflects toward alternatives—that it is, at heart, rationalism masquerading as a non-paradigmatic approach. And, he argues, rationalism is, in fact, the dominant paradigm of the field: "Today we have hegemony, and worse, a hegemony that claims not to be coherent or even to exist. I think the complaint that many have is not that they can't get into some of the bigger IR journals because they are constructivists or liberals or whatever, it is because they are not rationalists."

[6] Choice-theoretic methodological individualism comes close to the kind of integrated set of assumptions that Kuhn characterized as a paradigm, especially since it is virtually incommensurable with non-individualist alternatives such as those elaborated in, e.g., Nexon (2009) and Tilly (1999). That the advocates of such an approach often fail to acknowledge that they are making any particular assumptions about social life at all demonstrates one of the costs of the end of the "paradigm wars."

Those who focus on "great debates" correctly note that no contemporary controversy has these characteristics. But the greatness of these past debates is far from clear. The realist/idealist clash is largely aspirational victor's history written by self-proclaimed realists in an effort to permanently bury their opponents (e.g., Lynch, 1999; Schmidt 1998).[7] The "second debate" controversy about the use of formal and statistical techniques was limited to a few high-profile special issues of journals, featured a restricted cast of interlocutors—more or less Hedley Bull pitted against a plethora of US-based scholars—and didn't actually engage the relevant epistemological issues in a thorough-going way (Kratochwil 2006). As for the "third debate" between so-called "positivists" and "post-positivists,"[8] it is unclear that this is a single debate at all, or that it had a significant impact on the field. Even those journals that carried major statements of epistemological position did so alongside articles that conducted empirical analysis without any reference to these overarching methodological questions. Although drive-by citations of Lakatos became common for a while, they usually appeared in pieces that, at best, barely genuflected in the direction of Lakatosian criteria for adjudicating research programmes.

However, references (even if not entirely accurate) to these "great debates" sometimes serves a positive function with respect to international-relations theorizing. As Wæver (1998) argues, engaging with such controversies foreground big questions concerning the study of world politics. The absence of such debates allows people to go about their business in something approximating what Kuhn called "normal science." Snidal argues—and many would agree—in favor of this state of affairs. Whether "paradigm wars," epistemological controversies, or whatever, can make life difficult for scholars. We imagine that most international-relations researchers would, for obvious reasons, prefer to go about their business without having to worry about, for example, being attacked for getting their theory of language wrong.

Thus, how one feels about "great debates" depends on a number of factors: temperament, intellectual toolkit, and so on. Great debates help to hold the discipline together by giving everyone something (intellectual) to discuss, even if the relevant discussions sometimes look more like shouting matches. Attitudes about such debates are also likely contingent on other beliefs about international-relations theory. Those who believe that theorizing *necessarily* involves broader ontological and epistemological questions, and that those questions must be explicitly hashed out, probably thrive on "great debates." Indeed, we suspect that those who lament the absence of a contemporary "great debate" are likely equating "theory" with these broader ontological and epistemological questions; for such commentators, the increasing tide of "normal science" work in international-relations scholarship does indeed spell the "end of IR theory."

Middle-Range Theorizing

On the other hand, many of those who stress the vibrancy of contemporary international theorizing focus on middle-range theory. Theory, in this sense, is a set of stories that make sense of correlations. Some of these stories are told in the style of game-theoretic models, as in the "analytic narratives" advocated by some rational-choice scholars (e.g., Bates et al., 1998), while others might focus on the social logics of individual action (e.g., March and Olsen, 1998), or on a set of processes such as "path dependence" and "sequencing" (e.g., Steinmo, 2008). The current rage in the US is to choose from a set of widely accepted "causal mechanisms" (such as "credible commitments," "audience costs," "veto players," "shaming," "framing effects," and "threat") and construct a standard story around them: 'credible commitments + veto players → observed outcome.' But in any case, these stories are almost always linked to empirical correlations. These correlations supposedly validate the explanatory story even as the story explains the correlations. A more valid (more "true" story) is one that has been evaluated through repeated correlational

[7] See also Thies (2002); Quirk and Vigneswaran (2005).

[8] Scare-quotes here because these terms are very misleading. international-relations "positivists" are largely Popperian falsificationists committed to hypothesis-testing as a means of evaluating claims, even though this position is known in the philosophy of science as "post-positivism." And international-relations "post-positivists" are a motley crew. They run the gamut from critical realists interested in dispositional causal properties to feminists and post-colonial scholars seeking to disclose the partial and partisan character of the positions from which dominant forms of knowledge are generated..

tests. A sufficient number of such stories, in those mode of analysis, will produce a more or less comprehensive theory of world politics—at least eventually.

Middle-range theorizing, at least in this idiom, is alive and kicking. It constitutes the vast majority of the "empirical" scholarship that gets published in top-ranked American journals such *International Organization* (*IO*), the *American Political Science Review* (*APSR*), and *International Studies Quarterly* (*ISQ*). Such work follows a fairly predictable script:

- Identification of a research question that involves the possibility of a systematic, law-like connection between input X and output Y;
- Discussion of mechanisms that might plausibly link X and Y; test of a hypothesis about the extent to which X and Y covary across cases; and
- Conclusion about the relative strength of "middle-range theories" (the standard stories involving linked mechanisms) in light of empirical evidence, including both at the level of initial correlation and with reference to "process" evidence such as quotations from elites, policy documents, and so forth.

Reviewers at such journals generally vet articles with respect to how well they enact this script.[9] In fact: the holy grail remains a "well-theorized explanation." Neither a correlational finding devoid of this kind of middle-range theory nor a set of speculative theoretical propositions about world politics is likely to make it past reviewers in the United States.

The flowering of middle-range theory comes with costs. First, this kind of explanatory enterprise depends not on a set of shared assumptions about world politics, but on a set of shared (and rarely voiced) set of assumptions about the character of knowledge and how one should go about generating knowledge. The privileging of correlational evidence when it comes to validating a conjecture is, as we alluded to earlier, far from an innocent methodological position. It makes a number of commitments concerning the law-like character of good knowledge, the representational nature of empirical claims, and the "Humean" account of causality. In other words, such middle-range theorizing generally depends on a neopositivist worldview, and on a wager that neopositivism—as distinct from other, equally "scientific" methodological perspectives—provides a definitively superior grasp of the world. Absent this methodological consensus, the categorical demand that an explanatory story be accompanied by correlational evidence (whether large-n or small-n evidence makes little methodological difference) carries no weight.

Second, the demand for middle-range theorization can be self-defeating. It is true that statistical relationships are only "interesting" insofar as they involve theoretical expectations. But nothing in a neopositivist approach actually *requires* that every such finding be accompanied by a middle-range theoretical account of the mechanisms and processes that explain observed relationships. By forcing empirical articles to conform to the aforementioned script, we encourage scholars to deploy "off the shelf" mechanisms—even if these mechanisms have little evidentiary support in the cases at stake, require heroic assumptions, or otherwise face significant problems. As Goemans argues, data analysis that "establish/suggest new patterns of behavior…. Really does not get published." The presumption is that we "*must* have [middle-range] theory, even if its *bogus* theory [emphasis original]."[10] And, indeed, many of the "off the shelf" mechanisms we regularly find in international-relations scholarship often owe their presence to a combination of their disciplinary acceptance—whether warranted or not—and a fetish for middle-range theorizing.

So the overall picture on middle-range theorizing is mixed. There's a lot going on, and some of it—whether derived from insightful formal models, psychological experiments, interesting findings and theories in

[9] Although there are a variety of genres and subgenres of articles, some of which trigger different reviewer expectations and modes of appraisal.

[10] Hein Goemans, comment on Nexon (2012). Available at: http://duckofminerva.blogspot.com/2012/03/professionalization-and-poverty-of-ir.html#comment-478958169. Accessed 2 July 2012.

cognate disciplines, or a host of other sources—qualifies as quite healthy. But there are also worrying signs on the horizon. The expectation that middle-range theory and statistical (or quasi-statistical) analysis be conjoined places undue restrictions on both activities.

Indeed, the pernicious thing here is not neopositivism *per se* as much as the marginalization of alternatives and a lack of sufficient discussion of the relationship among different modes of inquiry. The tremendous growth of scholarship that seeks to elucidate plausible stories about phenomena in world politics linked to cross-case correlations is purchased, in some ways, by the relative decline of serious methodological reflection on the *status* of knowledge about world politics. If it were the case that good knowledge were inextricably linked to neopositivist epistemic practices, this would be unobjectionable, but the linkage is quite tenuous; many other methodological stances exist and have their own forms of rigor (see Jackson and Nexon, 2009). Hence, the channeling of theoretical innovation in IR into a neopositivist framework may be producing not explanatory richness, but increasingly brittle conjectures. What looks like growth from one angle may, when viewed from another angle, look like stagnation.

International Theory as Scientific Ontologies of World Politics

The preceding discussion illustrates a key aspect of international-relations theory that is widely, albeit not unanimously, held in discussions of the state of the field: *doing international-relations theory means relating conceptual tools to empirical observations*. We believe that this task is best achieved when a rich variety of conceptual tools are available. Defenders of the "isms" debate or the "great debates" narrative emphasize the ways that those epic clashes generated novel ways of thinking about world politics—even though they often postponed questions of explanatory purchase. Critics of those clashes, together with proponents of middle-range theorizing, emphasize the connection between ways of thinking and the task of explaining concrete happenings in the world. In principle, they care less about the sources for those ways of thinking—even if, in practice, they have strong commitments to the assumptions that go into middle-range theorizing. But, despite their differences, these positions agree that theory has an explanatory purpose: international-relations theory is supposed to help us make sense of world politics. The disputes center on whether present-day international-relations theory (and theorization) adequately serves that function.[11]

In highlighting this point of agreement we do not mean to suggest that international-relations scholars agree about what constitutes "explanation." Rather, we intend to sidestep those disagreements. Philosophical discussions concerning methodology—about the epistemic status of claims and their proper use in the process of generating knowledge—is related to, but distinct from, the content of those claims themselves. For example, a claim about states balancing under conditions of anarchy does not, in and of itself, tell us how we should evaluate it, let alone use it to explain anything in particular. It might be a testable hypothesis. It might be a consequence of an ideal-typical model. It might be a derivation based on deep causal powers of the structure of world politics. Regardless, *as a theoretical claim* it stands conditionally independent from the methodology that we might use to set it into motion and build an explanation using it.

To be more precise, we think that international-relations theory is centrally involved with *scientific ontology*, which is to say, a catalog—or map—of the basic substances and processes that constitute world politics. International-relations theory as "scientific ontology" concerns:

- The actors that populate world politics, such as states, international organizations, individuals, and multinational corporations;
- Their relative significance to understanding and explaining international outcomes;

[11] Certainly there are IR scholars who argue that theory has additional functions, such as normative critique and the dispelling of false consciousness. But the minimal consensus definition of "international-relations theory," we feel, would be the one that emphasizes the explanatory function of theory, regardless of what *else* theory might be thought to do.

- How they fit together, such as parts of systems, autonomous entities, occupying locations in one or more social fields, nodes in a network, and so forth;
- What processes constitute the primary locus of scholarly analysis, e.g., decisions, actions, behaviors, relations, and practices; and
- The inter-relationship among elements of those processes, such as preferences, interests, identities, social ties, and so on.

International theory, understood in this way, is not simply a descriptive catalog; the point of a scientific ontology is to enable *explanations*. Scientific ontologies are therefore distinct from *philosophical ontologies.* The latter concern themselves with the "hook-up" between the mind and the world. In other words, philosophical ontology pertains to methodology; it speaks to questions of what is meant by "explanation." Scientific ontology deals with substantive claims about the world and objects in it; it pertains to theory (Patomäki and Wight, 2000).

Any particular explanation of something involves both theory and methodology—a set of substantive claims and a set of procedures for making use of them—but the two registers are, or ought to be treated as, logically and philosophically distinct (Jackson, 2011). The failure to clearly differentiate between philosophical and scientific ontology makes sometimes muddles discussions about the state of international-relations theory.

As we argued earlier, the problem with the "paradigm wars" stemmed not from the fact that different international-relations scholars had different understandings of world politics. Many did, in fact, operate with different scientific ontologies that, in turn, produced different theories. The error came in assuming that these differences rose to the level of incommensurable content, and hence that (for example) realism, liberalism, and constructivism should be treated as Kuhnian paradigms or Lakatosian research programmes. This, in turn, implied that each "paradigm" ultimately rose or fell *in toto*: that the fate of, for instance, individual "realist" or "constructivist" theories said something about "realism" and "constructivism" writ large. It also implied the necessity of adjudicating across each "paradigm" with reference to second-order criteria, such as that offered by Lakatos (1978) in his account of "progressive" and "degenerative" research programs (e.g., Elman and Elman, 2002; Vasquez, 1997; Vasquez, 1998).

The "great debates," especially the first debate, touched on issues of scientific ontology and substantive theory. However, the bulk of debates after the so-called "realist-idealist" dispute tended to be about methodology rather than about the substances and processes of world politics. And while the standard stories found in middle-range theories are always at least implicitly undergirded by broader scientific ontologies, middle-range theory in contemporary Anglophone international-relations scholarship needlessly presumes a methodological consensus around the procedures of hypothesis-testing. Although scientific ontologies of international-relations serve explanatory functions, they need not imply a specific mode of explanation.

For example, an international-relations theory that focuses on strategic bargaining among leaders may be embedded in methodologies that prize large-N hypothesis testing. But scholars may deploy the exact same scientific ontology in the service of formal models whose explanatory utility has nothing to do with hypothesis-testing procedures. Similarly, relational theories that build their account of world politics via social ties, networks, and social location may cash out on social-network analysis (SNA) measures deployed as variables in a multi-variate regression; they may also cash out in ideal-typical accounts of the dynamics of particular network structures (e.g., Hafner-Burton and Montgomery, 2006; Nexon, 2009)

The State of International-Relations Theory

If we are right, then the "end of international-relations theory" means an absence of robust and vigorous debate about scientific ontology. The field has not reached such a state of affairs, as evidenced by the explosion of interest in the "practice turn," ongoing debates about the Open Economy Politics (OEP) approach to International Political Economy (IPE), and other data points.[12] But there is a palpable sense, including among many of the contributors to this forum, that something is amiss. Certainly, the overwhelming number of "major works" they invoke were published in the twentieth century.[13] Perhaps the selection of this topic for *EJIR*'s first special issue indicates something in of itself? Instead of major discussions about the scientific ontology of world politics—discussions that draw in a wide variety of perspectives—it seems as if scholars associated with different theoretical frameworks are most likely to come together as scavengers; they loot the corpses of international-relations theory under the banners of "post-paradigmism," "analytical eclecticism," and "puzzle-driven research" (Nexon, 2011).

Of course, the "end of IR theory" might be little more than an illusion cobbled together from a few anecdotes. If it is, one reason might reside in demographic factors. Only one participant in this forum is under the age of forty (and barely at that). Every contributor, moreover, has enjoyed some degree of success in her or his sphere of theoretical expertise. It should not be surprising, then, if we are not exactly voracious when it comes to seeking out and developing competencies in new arenas of international-relations theorizing. If younger professors, or graduate students, had been part of this forum, they might have painted a very different picture of the state of international-relations theorizing.

But let us assume for a moment that something is rotten in the state of international-relations theory. What might account for the decline of international theory—or, at least, the pervading sense that it is in decline? We offer, with some sense of irony, a series of speculative hypotheses. Some of the processes they involve are mutually reinforcing, while others point in distinct directions.

Class Bias

Successful and established scholars—and those they choose to bring along for the ride—get to pronounce on "big theory" issues more often then most others in the field. They are more likely to receive invitations to submit pieces to forums; it is not unheard of for them to get different treatment at journals and publishers; they wind up on "event panels" at conferences dealing with discipline-wide questions; and they otherwise enjoy privileged voices at focal points for discussion in the field. With success comes privilege, of course, but we often act (without good justification) as if success in one arena—say empirical research—translates into competency to pronounce on epistemology, scientific ontology, or other specialized areas of theory.

This state of affairs arguably creates major barriers to the circulation and diffusion of new international theory. It keeps fresh and innovative voices out of the conversation. It centers discussion on the hobbyhorses of the same small number of major personalities. And it doesn't necessarily reward the most sophisticated international-relations theorizing.

The Dominance of Neopositivism

This line of argument suggests that neopositivist hegemony, particularly in prestige US journals, undermines international-relations theorization via a number of distinct mechanisms:

- It reduces the likelihood that international-relations theory pieces will be published in "leading" journals because neopositivism devalues debate over scientific ontology in favor of moving immediately to middle-range theoretic implications;
- It reduces the quality of international-relations theorization by requiring it to be conjoined to middle-range theorizing and empirical adjudication; and

[12] On the former, see Adler and Pouliot (2011); Neumann and Pouliot (2011); Hopf (2010). On the latter see Lake (2009); Oatley (2011) and the 2009 special issue of the *Review of International Political Economy* on the "American School of IPE" (16, 1: 1-143).

[13] Indeed, one could argue that the achievement of a critical mass of interest in Pierre Bourdieu and providing theoretical heft to the "practice" part of the existing constructivist catechism hardly indicates healthy international-relations theorizing. It isn't as if we are talking about the importation of an obscure intellectual figure; the "practice turn" does not, in many of its manifestations, involve a radical reshuffling of the international-relations theory deck.

- It forces derivative middle-range theories to be evaluated through neopositivist standards.

According to the TRIP survey, *IO*, *ISQ*, *International Security* (*IS*), the *APSR*, *World Politics*, the *European Journal of International Relations* (*EJIR*), and the *Journal of Conflict Resolution* are the six most important academic journals to the field. Outside of the US these numbers change somewhat, but most of the "top" slots remain occupied by *IO*, *ISQ*, and *IS*.[14]

As we discussed earlier, these American journals favor theoretical claims…but only of the middle-range variety. They occasionally publish articles dealing with philosophical ontology, methodology, epistemology, and the political theory of international relations. However, pieces dealing explicitly with the scientific ontology of international relations rarely find their way into them. For example, it is not at all unheard of for reviewers at *IO* to reject "pure theory" manuscripts on the grounds that such pieces have no place in the journal. Indeed, those seeking to publish international-relations theory pieces often have to also squeeze in middle-range theories, as well as some form of hypothesis testing of those derivative propositions. Given the limited (and often shrinking) space available for articles, the result is less room for the elaboration of international theory.[15]

The neopositivist bias also reduces richness in the implementation of international-relations theory. Research articles, whether quantitative or qualitative, in prestige US journals are almost always neopositivist in methodology. Indeed, even middle-range theoretic articles that draw on post-structuralist and critical concepts tend to evaluate their claims in a neopositivist idiom. Such tendencies also exist in major non-US journals, such as *EJIR*, where articles that assess middle-range theories not infrequently default to neopositivist modes of evaluation. Such a set of default expectations has upstream implications, insofar as they shape which international-relations theories prosper and which disappear into the ether.

For example, structural realism receives a not insignificant number of references in the form of following: "despite structural realism's predictions, X country failed to balance." Of course, structural-realist theory makes, on its own, few specific predictions about foreign policy—let alone the foreign policy of middle-tier and minor powers. So part of structural realism's success results from its use as a straw-man middle-range theory in the neo-positivist idiom, despite the fact neither shoe fits.[16] On the other hand, some international theories are much more difficult to translate into these terms, while others simply do not circulate among those inclined to do so. As we have argued, scientific ontologies *can* be translated into multiple methodologies. But that does not imply that they *should* be cashed out through a single evaluative framework (see Jackson, 2011).

The Proliferation of International-Relations Journals

According to Kristensen (2012) the number of international-relations journals and articles in the Web of Science database grew from 36 and 2,774 in 1980 to 82 and 4,535 in 2010. This (admittedly crude) indicator suggests a substantial increase in the number of articles and journals. Information overload and niche-specialization seem likely corollaries. Ironically, one implication might be the increased importance of a few "top journals": insofar as scholars face significant incentives for attention-conservation, particularly in the context of work outside of their immediate areas of research.[17] Thus, there may be a lot of interesting, novel, and important international-relations theorizing out there—but not in the "top journals" and therefore not destined to make a great deal of impact on the field.

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[14] *Foreign Affairs*, a non-academic journal, ranks at fourth for "greatest influence on the way IR scholars think about international relations. See Maliniak, Oakes, Peterson and Tierney (2007).
[15] The bias against "pure theory" articles at many journals provides part of the express justification for the founding of the journal *International Theory* by Duncan Snidal and Alexander Wendt.
[16] On the other hand, it is perfectly possible to derive such theories *from* structural realism, as neoclassical realists do on a regular basis. See Rathbun (2008).
[17] Research on blogging has suggested this kind of pattern: as the number of blogs increased the allocation of prestige became more power-law like in distribution. See Kottke (2003).

Increasing Standards for Scholarly Productivity

Scholars, particularly in the US and the UK, face ever-growing pressure to publish in order to advance their careers and the status of their home institutions. A number of consequences follow, some of which might disproportionately harm the apparent health of international-relations theorizing. An intensifying publish-or-perish environment:

- Reduces the quality of the typical peer-review process for manuscripts;
- Increases both supply-side and demand-side pressures for scholarly conservatism; and
- Decreases the time available for graduate students, researchers, and professors to *consume* scholarly work, particularly outside of their immediate area of research.

Why does this trend diminish the quality of peer review? Increases in the average number of submissions per scholar leads to a declining ratio of reviewers-to-manuscripts. Assuming that the pool of reviewers both qualified to review a given piece and also willing to do due diligence is even smaller, the fate of high-quality peer review looks grim indeed.[18] Thus, the irony that the greater the disciplinary importance of peer review, the less reliable the process will become.

We believe that the international-relations community remains in a kind of fantasyland about the mounting problems with peer review. In an era when acceptance rates at "top journals" are falling below ten percent—and editors face enormous pressure to triage submissions—it is difficult to escape the conclusion that we risk allocating prestige via an increasingly stochastic process.[19] The argument that this negatively impacts the state of international-relations theory, however, is a bit more difficult to make. But not impossible.

Why might novel international-relations theorizing be among the most vulnerable to these processes? On the one hand, they choke off supply. They place pressure on authors, particularly those at the early stages of their careers, to play it safe. Consider the low chances of getting a piece into a "prestige" journal, the psychological impact of repeated rejections, and the importance of publishing to success in a highly competitive job and promotion environment. The rational response is to make sure that one's work conforms as much as possible to the expectations of the lowest-common denominator reviewer.

Pressure to publish also reduces the time available for scholars to consume knowledge. It therefore reduces exposure to "new" ideas from outside the discipline and discourages scholar from reading widely within the discipline. This helps explain why virtually every international-relations theory article revolves around a small number of texts (such as Waltz, 1979; Wendt, 1999; Moravcsik, 1997).

On the other hand, the same processes diminish demand—understood as the ability to get a novel theoretical argument through peer-reviewers and editors. The challenges to success here are already steep. William Glen (1989) notes that:

> It is now well-known that an idea (and the paper containing it) will be rejected as a function of its novelty. As one applies that novelty rule along the spectrum from simple fact to technique to methodology to theory, one finds increasing potential resistance. Novel theories are most likely to challenge ruling paradigms, and thus contravene ideas that potential referees have already subscribed to in print. The theoretically insurgent paper is also more likely to be rejected simply because uncertainty mounts with the breadth of generalization, and it is commonly known that most new, large ideas fall victim to disproof more often than less inclusive ones. … [Even though] such ideas—

[18] Especially considering that reviewers are overstretched with deleterious consequences for their ability to turn out high-quality reviews. See Glen (1989).

[19] As one graduate student said in June of 2012, "mid-career associate professors came of age in an era when *IO* and *ISQ* received under two hundred submissions." According to its online report, *ISQ* received *over six hundred* in 2011.

> however volatile, more likely to cause an editorial tumult, and apt to be proven wrong—when correct, advance science out of proportion to their number.

As Stephen Cole (1995) writes of publishing in sociology journals:

> Among the [submissions of publishable quality]… there may be a negative correlation between the potential contribution to sociological knowledge and the chance of being accepted [for publication]. This is because those articles that have the highest chance of being accepted are those done by methodologically competent sociologists who deal with relatively narrow topics. Reviewers cannot find much wrong with these articles, which certainly deserve to be published, and recommend publication. But the broader the topic one deals with, the more innovative the theoretical ideas employed, the more challenging of existing beliefs, the more likely it is that reviewers will find something "wrong" with the article and recommend either major revisions or rejection. *Given the limitation of space, the editor usually has little choice but to publish the narrow articles and reject most of the broader articles* [emphasis added].

Such factors do, indeed, suggest that the accouterment of submission overload should particularly impact novel international-relations theory. Articles written on the subject will face particular difficulties in finding qualified reviewers, escaping substantively negative reviews, being rejected by reviewers who object to the entire endeavor, and so forth. And they will do so in an environment in which editors at the "prestige journals" are forced into relentless acts of triage.

What to do?

We really have no idea which, if any, of the preceding processes are undermining international-relations theorization in the field. We are still unsure if international-relations theorizing is in any sort of trouble. However, this uncertainty won't prevent us from making recommendations for the field. We provide four main ones.

The Virtues of Engaged Agonism

Global consensus about the proper scientific ontology for world politics is the opposite of what we need for robust international theory. Indeed, that would kill international-relations theorizing by effectively ending it: we'd have nothing left but an orthodoxy—with its inevitable costs and benefits. Nor is the vibrancy of international theory served best by the sort of vaguely engaged pluralism likely to emerge from the why-can't-we-all-just-get-along genre of pleas for tolerance. Scientific ontologies are ultimately intended to help us *explain* stuff in the world. When scholars engage in international theorizing, then, they need to make clear what the *stakes* are: what conceptual, analytical, and middle-range theoretic problems their scientific ontologies help them to resolve—and that *cannot be solved effectively by alternative international theories*. This requires robust disagreement and debate.

For example, we have long been advocates of relational understandings of international politics, precisely because we think, among other things, that these approaches (1) *solve* problems created by rival scientific ontologies in accounting for continuity and change, (2) are *better* tailored to a variety of the phenomena we study—trade flows, diplomatic interchange, the activities of transnational actors—then rival scientific ontologies, and (3) subsume the -isms by showing how they specify dispositions from different configurations of social ties and categories.[20] If we are right, then people who advocate different scientific ontologies are *wrong* in at least some of their claims.

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[20] Yes, we did slip in one of our hobbyhorses. Also a citation dump: Jackson and Nexon (1999); Jackson and Nexon (2001); Jackson (2003); Jackson (2006); Krebs and Jackson (2007); Nexon (2009); Nexon (2010);

Note that such claims cannot be cashed out, let alone evaluated, solely at the level of ontological argument. But if, as we have noted above, combining international theorizing, middle-range propositions, and empirical evaluation of those propositions cannot be done in the shrinking space allotted to articles, then how is this possible? One answer, other than to write books that fewer and fewer people actually bother to read, is that international theory constitutes a highly collaborative venture. It requires scholars specializing in formulating scientific ontologies, those specializing in refining those scientific ontologies via empirical work, and brokers to connect the two endeavors. Consider what has made the "practice turn" so comparatively successful. It has the backing of influential scholars. It captures what people *were already doing* and gives it a common label.[21] It draws on a rich body of work in cognate disciplines and fields. In addition to all of these features, advocates of the "practice turn" make clear claims about what a practice approach is supposed to explain that other scientific ontologies cannot, such as the tacit understandings that make day-to-day international relations predictable, the dynamics of social facts and so on.[22]

All of this collective work provides precisely what one might reasonably expect of innovation in international theory: a novel—but not so novel that it is completely unrecognizable!—set of claims about what world politics is made of, formulated with an eye to explaining otherwise puzzling features of that world. Perhaps only a superhuman scholar could have done all of this on her or his own, since it would require *both* sophisticated empirical studies *and* productive conceptual refinements. Under present conditions of scholarly knowledge-production, the best alternative to preparing to be prepared for the coming of a new prophet is to think of the enterprise of international theory as oscillating between moments of conceptual formulation and concrete empirical demonstrations of explanatory productivity. International theorizing is no single one of these endeavors, but is instead the emergent product of both of them in dialogue.

Find Different Foils

International-relations scholars should stop citing the same few canonical theorists over and over and over and over again. Members of our discipline have put forward dozens— if not hundreds—of scientific ontologies that they claim are in some way superior to one or more of:

- Kenneth Waltz's structural realism—and John Mearsheimer's "offensive" variant;
- Robert Keohane's neoliberal institutionalism;
- Alexander Wendt's state-centric constructivism; and
- The English School.[23]

Yet somehow we are supposed to be surprised that international-relations theorizing might be stagnating? If we want vibrant international theorizing, we need to start engaging with the broader universe of

Goddard (2006); Goddard (2009b); Goddard (2009a); Carpenter (2007); Carpenter (2011). Note how cheesy this seems when actively signposted, rather than simply implemented without comment.

[21] As Randall Schweller once remarked to us, this is precisely why "neoclassical realism" succeeded; when Gideon Rose (1998) put the term into print, he described, defined, and connected together a body of disparate work. In doing so he transformed that work into an intellectual movement.

[22] Indeed, the "practice turn" found a gaping hole in existing constructivist work and drove a truck through it. In essence, many constructivist arguments about the scientific ontology of world politics concerned the structuring effects of "social facts": aspects of human life that appear to individuals as if they are "objective" even though they are, in truth, products of social interaction and therefore subject to alteration via human agency. But that scientific ontology is often disconnected from the middle-range theorizing of "norms constructivism," which has concerned normative claims that, rather than being naturalized social facts, are openly contested and debated. In this sense, the "practice turn" took a disjuncture among different levels of constructivist theory and proposes a way of bringing them into alignment.

[23] The big question right now is whether the "Practice Turn" will join this illustrious list. On the one hand, the "turn" is sufficiently amorphous to allow some of its ideas to be incorporated into most existing frameworks. On the other hand, it has a few leading representatives in Anglophone books and journals, so it is a rather good candidate.

international-relations theory. This means both that, on the one hand, authors need to reduce their fixation on Waltz, Wendt, and, on the other hand, reviewers need to stop demanding that authors mainly (or solely) engage with the "canon" in order to render their interventions of sufficient significance to merit publication.

As long as we continue this practice, the core-periphery structure of international-relations theorizing will persist: intellectual communities "outside of the mainstream" will turn their attention toward criticizing a few canonical texts but not engage with one another in a serious and sustained way. While some claim that "non-aggression pacts" should prevail within and among peripheral theoretical traditions, such rules of engagement both reduce the profile of the communities in academic writing and also harm the intellectual growth of those traditions. After all, criticisms produces not only better theories, but more (and more prominent) citations.

Write Less, Read More

We need to slow down the publication treadmill so that scholars have more time to read. Doing so should help break us from our addiction to a few canonical theories, insofar as it enables us to develop a broader knowledge of existing work in international-relations theory. It will also make everyone much happier, improve the quality of international theorizing, and provide controversial pieces with a better shot at being published by reducing the torrent of submissions that reinforce tendencies toward conservativism in the publication process.

Look for Other Fish in the Sea

It would help correct a number of problems with contemporary international-relations theorizing if our field spent more time looking for international theory outside of a limited number of journals. On the one hand, we should be less snobbish about where articles appear. Many of the pathologies we discussed in the prior section are most intense at a small subset of Anglophone publications. There is, in fact, a great deal of interesting, innovative, and engaging international-relations theorizing in so-called "second tier" journals.[24] On the other hand, a lot of international-relations theorizing may not be happening in traditional outlets at all. As Charli Carpenter (2012) has argued, blogs, social media, and other digital developments are fundamentally transforming the field. Indeed, there's a wealth of interesting international-relations theory, middle-range theory, and other forms of theory happening on blogs and in other "non-traditional" settings.[25]

And While We're at it, We'd like a Pony

The preceding recommendations cut against prevailing structural pressures and incentives in the field. But that's central to our point: the field has gone through significant social, economic, and cultural changes in the last few decades. The "End of IR Theory" may simply be a euphemism for these changes, or it may constitute a warning about the state of international-relations theorizing. Whatever the state of international theory, its condition cannot be diagnosed solely with reference to a Platonic realm of ideas about "the international." Its challenges and opportunities, we believe, are rooted firmly in the shifting academic topography of not only our field, but our profession. In turn, those who are concerned about the direction

[24] And yes, there's a lot of forgettable stuff as well. But the same can be said of the "leading journals."
[25] For example, Rid (2012) compares the total number of downloads from Taylor and Francis security-studies journals to page views of his collective blog, Kings of War. He finds that Kings of War's number of page views exceeds the *combined* downloads of all articles from that publisher's top six security-studies journals. And even more startling set of figures comes from e-International Relations (http://e-ir.info), an e-journal run by primarily student volunteers that publishes papers written by undergraduates, graduate students, and established scholars. According to its editors (personal communication), it receives over 40,000 unique visitors and 110,000 article downloads per month—these numbers far outstrip those at the leading international-relations journals.

of international-theory should recognize that altering its course is not about writing a few articles or whatnot, but shifting the social and cultural structures of the discipline.

Concluding Thoughts

Asking whether international-relations theory has come to an end—whether a particularly fertile period of international theorizing that began in 1979 has now finished—makes several presumptions that may be false. Chief among these, perhaps, is the notion that the publication of Waltz's ubiquitous 1979 book *Theory of International Politics* (*TIP*) inaugurated a period of theoretical innovation that was somehow qualitatively distinct from what preceded it. The fact that Waltz's approach to theory and theorizing was grossly *misunderstood* in the ensuing decades suggests that whatever changed after 1979, it was not a shift to "Waltzian" systemic theorizing (see Goddard and Nexon, 2005; Rathbun, 2008; Wæver, 1996). Indeed, there might be more continuities between theorizing in the pre-*TIP* era and the decades following it than we might otherwise expect. The fact that the "science question" and the largely unshakeable dominance of neopositivist methodological commitments both precede *TIP* and post-date it certainly suggests an absence of radical ruptures.

Of course, whether one thinks that the period from 1979 to sometime in the mid-1990s was a robust period of international theoretical innovation also depends a great deal on how one thinks about "theory," as we have argued above. The "great debates" and the "paradigm wars" look innovative if one is primarily concerned with making substantive assumptions about world politics explicit; "middle-range theorizing" looks innovative if one is primarily concerned with generating stories that explain correlations. And lurking behind all of these judgments is the fact that the field during that decade and a half was considerably *smaller*—at least in terms of the major Anglophone journals and key scholarly players—quantitatively speaking. There's just a lot more *stuff* out there—both accumulated and being produced—that international-relations theorists should be aware of in order to do their jobs. The growing number of scholars, the growing number of outlets, globalization of the field's membership, and the impact of new communications technologies are all contributing to information overload—as well as the further fracturing of the field along methodological, theoretical, and substantive fault lines. Indeed, we have trouble thinking of a recent IR article or book that "everyone" talked about?[26] It's no accident that our international theory syllabi now talk about the post-1990s period as characterized by fragmentation and pluralism. Both of those terms provide ways of recognizing the same basic fact: there's just a lot more international-relations scholarship out there these days, and it doesn't lend itself to any obvious or neat summary.

All of this suggests to us that much of the "end of IR theory" discussion might actually be a form of *nostalgia* for a particular bygone era in which, supposedly, everyone was concerned about the same basic issues. Of course, nostalgia plays tricks on the mind. Not everyone was involved in those discussions, much international-relations scholarship was unconcerned with great debates or paradigms or middle-range theorizing, and the effect of a small scholarly world was produced by specific sociological and vocational factors. These factors have, in turn, largely evaporated in a brave new world featuring such exercises in evaluating scholarly productivity as the REF and Academic Analytics, as well as the proliferation of publication outlets both traditional (new journals and book series) and non-traditional (blogs, websites, twitter). As with other forms of nostalgia, perhaps recognizing that the past was marked by concerns about "separate tables" and "sects," US intellectual hegemony, the consolidation of prestige by a few scholars, a lack of gender and ethnic diversity, and the disappearance of articles and books into the recesses of libraries will help us get over the feeling that we've lost something precious.[27]

Hence: we should perhaps stop lamenting the "end of IR theory," and concentrate instead on the more manageable effort of evaluating how and whether any of the plethora of scientific ontologies of world

[26] This may come across as flippant, yet even those books and articles that seem like blockbusters in particular intellectual communities don't necessarily impinge upon the discourse of other ones.

[27] On complains about "separate tables," see Almond 1988. Indeed, some of the attraction of the "paradigm" notion in the 1970s was driven by a peculiar (but apt) interpretation: the fact that scholars of politics were factionalized by approach and assumptions did not, in fact, mean that they weren't scientists.

politics actually contribute to our understanding of it—with due consideration given to the variety of methodological ways that such scientific ontologies might be used in valid explanations. Pluralism is a great opportunity for insight, as long as we are not shackled to misleading visions of a scholarly field past in which everyone was on the same page. Multiple visions and multiple insights may, indeed, be the most appropriate thing for the complex and challenging world we now inhabit, and international theory can help to meet that challenge—if we put aside nostalgia, indifferent pluralism, anti-intellectual demands for quantity of publications, and our myopic focus on a relatively limited number of outlets.

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