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Theory is dead, long live theory: The end of the Great Debates and the rise of eclecticism in International Relations

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Abstract
The history of the discipline of International Relations is commonly told in terms of 'Great Debates.' These intellectual clashes resolved little and, indeed, continue to this day. Underneath this narrative is an alternative history of significant progress through mid-level or eclectic theories of world politics. After reviewing the Great Debates, this counter-narrative is developed and then supported by reviews of open economy politics and democratic peace theory. A new line of cleavage is emerging in the field between positivists and post-positivists, however, that overlaps with the second and final Great Debates. Like all others before it, this potentially great debate — if it occurs — will be inconclusive. The field would be better off focusing on important real-world problems and achieving progress within each approach according to its own criteria for success.

Keywords
democratic peace, epistemology, International Relations theory, open economy politics, paradigms

Introduction

The field of International Relations has a long tradition of Great Debates, but both grand theory and clashes between competing grand theories now appear to be on the wane. Many international relationists bemoan the dominance of 'normal science,' a phrase

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almost always uttered in a derogatory manner. Yet, if grand theory was king, it was an evil tyrant. The Great Debates did help clarify the differing assumptions scholars make in their theories, though this is a low bar to clear.[1] Despite the attention they received, however, nothing was ever resolved. Indeed, as I shall explain, each of the three Great Debates continues to this day. More insidiously, the debates took the form of 'paradigm wars' in which the focus was less on how to explain world politics and more on which set of assumptions best captured the inherent nature of humans as political animals or states as political organizations.[2] The paradigm wars perverted the discipline and turned inquiry into contests of a quasi-religious belief in the power of one or another 'ism' (Lake, 2011). I, for one, do not mourn the tyrant's passing.

Flourishing in the interstices of the Great Debates, however, has always been a rich ecosystem of other theories, often competing, that never rose to the level of Great Debates but nonetheless produced significant progress over time in improving our understanding of international relations. This article focuses on one such strain of theory, mid-level theory, that today forms the basis for a more progressive and eclectic approach to International Relations (Sil and Katzenstein, 2010). Mid-level theory is not new but is rising to greater prominence now that the Great Debates have lost their stridency. It is less exciting than the Great Debates. Mid-level theory, precisely because it is eclectic and focuses on what 'works,' does not inflame the passions like allegiance to this or that paradigm. Yet, mid-level theory, I argue, can form the basis for a progressive discipline of International Relations that the paradigms have never provided. This contender for the crown deserves support.

This article proceeds in four steps. The first part reviews the Great Debates in International Relations and some of the recent critical historiography on these periods of supposed scholarly ferment. The second part develops an alternative narrative of the field that highlights mid-level theory. The third part surveys two streams of mid-level theory to demonstrate the potential for progress within the approach. The fourth part outlines what may become another Great Debate between positivists and post-positivists that, in turn, overlaps with the second and final Great Debates. This is a real line of cleavage in the discipline today which threatens to become ever wider in the years ahead. Like all the others, this nascent debate, if it occurs, will be inconclusive. We would all be better off, I argue, focusing on achieving progress within each approach that addresses important real-world problems.

The Great Debates debated

The history of the field of International Relations is typically told as a series of Great Debates, epic battles between titans that shaped the direction of inquiry and knowledge for decades afterwards. What was at stake in each of these debates has been — and continues to be — reinterpreted in light of developments in the real world and current disciplinary struggles. Indeed, there is now a significant body of revisionist history that challenges what the debates were supposedly about — and even if they occurred at all (Dunne et al., 1998; Schmidt, 2013). History, even disciplinary history, is always contested. Even more so, the 'Whiggish' story of theoretical and empirical progress embedded in the history of the Great Debates is almost certainly wrong (Schmidt, 2013). Yet, the conventional account appears to retain some hold on the discipline; as Wæver (1998: 715) notes, 'there is no other established means of telling the history of the discipline.'[3]

The first Great Debate, begun on the eve of World War II, supposedly unfolded between the idealists, dominant from the founding of the discipline in the early 20th century, and the realists, who triumphed in the decade after 1945. Coming out of a tradition of formal-legal theory and progressive politics, idealists focused on the potential role of institutions in improving the human condition and mitigating conflict between states. Their quest was, in part, driven by the destructiveness of World War I, but foundered on the inability of international institutions to prevent World War II — though of course that was a high hurdle for any progressive reform.[4] Claiming to take human nature and 'the world' as they are, but really taking them by assumption, realists sought more clearly to explain actual patterns of world politics and to identify pragmatic steps leaders might take to improve diplomacy and world order (see Morgenthau, 1978). This 'clear-eyed' or, as some might say, more cynical interpretation of human nature and focus on the conflict inherent in the struggle for power both informed Western policy during the Cold War and, in turn, was reinforced by that decades-long struggle (Kahler, 1997).

Critics now dispute the notion of a coherent 'idealist' school in the interwar years and argue that it was challenged, even in its supposed heyday, by pluralists who questioned the state-centrism of their colleagues and focused on the modern condition of interdependence and transnational relations, though the concepts were not yet framed in those terms (Kahler, 1997; Long and Wilson, 1995; Schmidt, 2013: 16–17). Similarly, it is not clear that realism was a single school. There were strong continuities between prominent 'realists' and earlier thinkers, especially those who focused on 'power politics' and the balance of power — ideas that were hardly new to the postwar era.[5] To make the story even more complex, there were substantial overlaps in the thinking of the so-called idealists and early realists (Osiander, 1998). Presaging the behavioral revolution of the second debate, the dividing line might be drawn more clearly between 'idealists' who focused on international organizations and 'realists' who broadened the field to study international 'politics' (Schmidt, 2013: 17). The claim that realism somehow 'won' this first debate is also erroneous, in my view, as that victory was never truly consolidated and was soon challenged by liberalism (Cooper, 1968; Keohane and Nye, 1972, 1977; Rosecrance, 1987), neoliberal institutionalism (Keohane, 1984), and ultimately constructivism (Ruggie, 1998; Wendt, 1999). This first Great Debate as the clash of alternative paradigms did not end, but rather is a continuing thread in the history of the discipline and even overlaps with the final Great Debate below.

The second Great Debate, reaching its height during the late 1960s, is usually cast as a battle between traditionalists and behaviorists or 'scientists.'[6] Traditionalists emphasized the complexity of world politics, the role of contingency and leadership in diplomacy, and the unique nature of each historical juncture. Claiming that no scientific theory could ever capture the interplay of so many factors nor explain choice by human beings who could learn by experience, traditionalists focused on moments of inflection in history in which world politics could have gone one way or the other (Bull, 1969). Behaviorism came late to International Relations, at least compared to other areas of political science, but it arrived with the force of deep arguments already rehearsed elsewhere in the social sciences. Behaviorists eschewed the uniqueness of particular episodes and sought to identify classes of events, looking for and highlighting commonalities across, for instance, all interstate wars (Singer, 1969). Having identified classes of

events, behaviorists could then build up generalizations by induction or empirically confirmed deductions. By the 1970s, the behaviorists declared victory.

This standard account of the second debate is also increasingly challenged. The attempt to define International Relations as a science began not with the behavioral revolution in the 1950s, but in the Chicago School of the 1920s and 1930s. In turn, the behaviorists were promoting not science in general but a particular kind of science focused on systems theory, a still deeply contested domain (Schmidt, 2013: 19). Like the first Great Debate, this one also remains a running battle with traditionalists still vibrant throughout the field, especially within the English School (Linklater and Suganami, 2006), and possibly even dominant in Europe where constructivism and various forms of reflexivity have taken deep hold on the discipline. Despite the behaviorists' assertions of supremacy, this second Great Debate remains alive and is still a major cleavage in the field.

The last Great Debate is in many ways the hardest to identify, as the conventional narrative is not one but two or possibly three different stories (Schmidt, 2013: 15). Some see a third debate arising in the 1970s between realism, liberalism, and radicalism and a fourth debate occurring in the 1980s between neorealism, neoliberalism, and constructivism (Wæver, 1996). This is not an unreasonable characterization, given the attention devoted to the clash of paradigms in this era. As implied above, this debate or set of debates is better understood in my view as a continuation of the first debate. Others see the third debate (sometimes the fourth or fifth debate) as pitting the 'positivists,' as the successors to the behaviorists are sometimes called, against 'reflectivists,' giving it a more ontological cast (Keohane, 1988; Lapid, 1989). It is this what I will call 'final' debate that I want to focus on here. The term positivist was always something of a misnomer, as few were naive methodological falsificationists in the sense outlined by Popper (1963: 33–39). An alternative label, 'rationalists,' fit equally poorly. Rather, positivists were a grab bag of approaches grouped by a general commitment to social science as a method and the assumption that individuals and other political actors are intentionalist and calculating in their actions.[7] By the 1980s, these so-called positivists constituted the 'mainstream' of the discipline of International Relations. Reflectivists, in turn, were hardly a single school either. Broadly united by a belief in the potential openness of various 'taken for granted' aspects of world politics, several strands of theorizing competed, including at least constructivism (Adler, 2013; Wendt, 1987, 1999), post-modernism (Ashley, 1986; Devetak, 1996), critical theory (Brown, 1994; Linklater, 1992), and feminism (Enloe, 1990; Tickner, 1992). Several of these approaches share the normative positions of idealism; others are more social versions of realism. As explained in more detail in the fourth part of the article, some reflectivists by epistemology are critical realists, more are anti-positivists who believe social change is the purpose of inquiry, not explanation (Jackson, 2011). The diversity of approaches under the heading of reflectivism likely made this final debate less salient, as the assault on the positivists was less unified than in the past cases. Perhaps for this reason, and perhaps because it overlapped so much with the paradigm wars of the first Great Debate and the epistemological differences in the second, some challenge the idea that there ever was a final Great Debate, seeing it more as a series of small battles than an epic struggle. More accurately, the final debate might be best understood as the fracturing of the field into multiple, overlapping

identity groups, each seeking to bolster and affirm its own theoretical 'turf' against not only the mainstream of the discipline but against each other as well.

No approach won this debate, although the positivists remained ensconced at the center of the field. Positivists either subsumed the critiques offered by the reflectivists, as when Richard Ashley's (1986) perceptive critique that neorealism was statist before it was realist led to a decade of innovative positivist theorizing of the concept of sovereignty (for instance, Krasner, 1999), or just simply ignored and marginalized them. At the same time, the end of the Cold War — despite the absence of any change in the objective nature of the international system — supported more reflectivist views on the socially constructed nature of previously 'objective' structures, propelling the debate along (Evangelista, 1999; Katzenstein, 1996). Like the earlier episodes, this final Great Debate — whatever it was — was never resolved. In a rather anti-climactic way, the various schools of thought, positivist and reflectivist alike, have simply retreated to their own corners of a multi-sided boxing ring, occasionally tossing a punch in one or the other direction but more often talking amongst themselves and complaining of not being taken seriously by others.

Like all narratives, this account of our field — even in its revisionist forms — leaves out parts of the story, and for this reason, I am reluctant to recount it to new students or reify it by frequent retelling. It is, of course, a North American or possibly US-European story, with few voices from the developing world being heard (Hoffmann, 1977; Tickner and Waever, 2009; Wæver, 1998). Even within this frame, however, in focusing on the Great Debates the conventional story wittingly or not ignores many other possible narratives — some of which are explored elsewhere in this special issue. Without minimizing the importance of other counter-narratives, I focus on one alternative — mid-level theory — that is often overlooked precisely because it is so close to the dominant one. Telling its story puts the debate about the 'end of theory' in International Relations in new light.

In the interstices

Percolating alongside the paradigms of the first Great Debate, the epistemologies of the second Great Debate and the ontologies of the final Great Debate have always been a variety of eclectic, mid-level theoretical approaches sharing a positivist and intentionalist approach to politics. Rooted in critical approaches of the interwar period (for instance, Beard, 1934), these alternatives flowered with the Vietnam War and the shattering of the myth of a national interest that could be discerned by an objective observer. To account for the diversity of opinion within states, scholars drew anew on psychology (Jervis, 1976), bureaucratic politics (Allison, 1971), interest groups (Gourevitch, 1977), public opinion (Holsti and Rosenau, 1984), political institutions (Waltz, 1967), and state structures (Katzenstein, 1978). The object of this theorizing was to explain political differences over foreign policy and why different countries might react to common stimuli in different ways at different times. Thus, Gourevitch (1977) studied how the first Great Depression was refracted through different constellations of interest groups in several European countries and resulted in differing trade policies, Katzenstein (1978) examined how interdependence was conditioned by varying state structures in the advanced industrialized countries, and Jervis (1976) highlighted the role of cognitive bias in interpreting and responding to crises. Parallel with this 'domestic' turn in International Relations,

others drew on game theory to explain how states might produce sub optimal outcomes despite their good intentions (Axelrod, 1984; Oye, 1985; Schelling, 1960; Snyder and Diesing, 1977). Much of this work came together in the 1980s in the newly revitalized field of security studies (Posen, 1984; Snyder, 1984; Walt, 1987) and the nascent field of international political economy (Frieden, 1988b; Goldstein, 1988; Gowa, 1983; Haggard, 1990; Milner, 1988; Odell, 1982).

Although sometimes forced into one or another paradigmatic 'box' to conform to the dominant narrative of the field, these approaches were never clearly realist, liberal, or later constructivist. All were, moreover, characterized by methodological pluralism, drawing equally on history and case studies, on the one hand, and large-*n* data sets, on the other. Often emphasizing historical contingency, they nonetheless also looked to broader patterns of world politics. Finally, all were — and still are — mid-level theories that focus on parts of the political process, rather than the whole, and study the effects of one or more variables on policy choice and outcomes. Bridging and in many cases simply violating boundaries between the levels of analysis, this strain of theorizing about international politics was self-consciously eclectic.

Even as the traditional narrative and the discipline have focused on the Great Debates — watching the titans battle towards inconclusive ends — these mid-level theorists have labored to produce and assess theories of foreign policy and international affairs with varying assumptions that apply to specific issues and even limited historical periods. It is hard to measure how much research in International Relations is directed towards and fits neatly within the Great Debates, and how much falls into this more eclectic tradition. In my admittedly biased and subjective estimate, it seems to me that the majority of work in our field since its founding has likely fallen into the latter category.[8] Yet, mid-level theory has been left out of the traditional narrative of the field, or relegated to a subordinate theme or the footnotes.[9]

This alternative or counter-narrative yields a different conclusion on the state of theory — or the end of theory — in International Relations. Since the final Great Debate sputtered to a close, the field has suffered in the view of some from the absence of any great discipline-wide struggle. This is liberating since the 'isms' of whatever form, as argued elsewhere, have exerted a pernicious effect on the discipline for decades (Lake, 2011). Thus, if by the end of theory we mean the absence of a new Great Debate, I can only stand and cheer. In the silence created by the end of the Great Debates, or at least their temporary hiatus, eclectic theory has gained new prominence. Eclectic theory is not new. As the counter-narrative indicates, it has been here all along, but relegated to the wings, bit players rather than the heroes of the story. But it is decisively theoretical, if not of the grand form favored by those who wish for the return of the Great Debates. Eclectic theory is the source of some of the most progressive research in our discipline. In my view, it represents the future of International Relations — not a new future, since it already has a long history, but the future nonetheless.

Progress via mid-level theory

Progress is a contested term that takes different meanings in different ontologies (see below). Positive and intentionalist eclectic theories are best judged by Lakatos's (1978)

definition of progress as the accumulation of knowledge and discovery of new facts. The paradigms in International Relations — like all paradigms (Kuhn, 1970) — are defined by sets of assumptions that form the basis for more specific theories. International Relations paradigms, however, are typically incomplete, meaning that the assumptions are, as a set, insufficient to predict specific behaviors or outcomes. As a result, the 'hard core' assumptions of each paradigm must be augmented by auxiliary assumptions (Elman and Elman, 2003; Lakatos, 1978), implying that it is possible to have multiple specific theories of, say, war that have equal claim to being 'realist.'[10] These multiple possible theories within and across paradigms could be judged by their contributions to knowledge — in short, by progress. Instead, advocates of different theories within the same paradigm have chosen to compete for the soul of the approach, each claiming to be the true exemplar.[11] In turn, advocates of each paradigm have likewise chosen to defend their core assumptions against criticism or charges of insufficiency. This is, as philosophers of science recognize, normal (Lakatos, 1978). But in International Relations, this competition has had the effect of turning theories into faiths that must be defended at all costs. Unfortunately, this all too often transforms the research enterprise from one of explaining world politics into one of 'proving' realism, liberalism, or whatever is the one true faith to which all should adhere (Lake, 2011). The paradigm wars have not led to progress under almost any definition and certainly not in the one posed here.

Eclectic, mid-level theory begins from a very different point (Sil and Katzenstein, 2010). Rather than defending any single set of assumptions, it builds theories to address specific problems of world politics. Theories of war, for instance, will likely contain different assumptions and focus on different processes from theories of human rights or the environment. Mid-level theories may also violate the levels of analysis in International Relations, sometimes treating branches of government, for instance, as the units of observation in one country and the state as the unit in another (Milner, 1997). In this approach, there are no primordial units of analysis, only methodological 'bets' about which unit of aggregation is most likely to produce tractable and empirically powerful explanations (Lake and Powell, 1999). In similar ways, rather than slavish adherence to power, security, or wealth maximization as a universal goal, the units posited in different theories are also understood to possess different interests — including socially constructed interests — tailored to the specifics of the issue under investigation. It is precisely this 'mixing and matching' of assumptions, issue areas, units, and interests that makes this sort of theorizing 'eclectic.'

Importantly, in the last decade or so there has been an increasing and, in my view, appropriate demand that mid-level theories have explicit 'micro-foundations' or, as George and Bennett (2005) call it, a 'causal mechanism.' In other words, theories are preferred which can link the incentives and actions of real individuals or possibly groups of relatively homogeneous individuals to policy outcomes in a consistent way. It is not enough to simply posit that internationally oriented business is likely to get investment protections, for instance. Ideally, theory will specify how owners and shareholders as a group define their interests, mobilize for political action, develop strategies to counter the actions of opposing political groups, and bring pressure to bear on politicians embedded in political institutions that, in turn, constrain their actions; theory should then explain how states with different societally generated

policy preferences and different mechanisms for aggregating those preferences then interact with one another to produce, say, tighter or looser bilateral investment treaties. 'Good' theory thus links the entire chain of action in a way that is consistent in its assumptions. The full chain is often left implicit, as implied in the call for greater attention to micro-foundations or causal mechanisms. Theorists often focus on one or another link, emphasizing interest formation over, say, how political institutions aggregate those interests or how states bargain. But theories that start with, say, 'business' as the unit of analysis are increasingly called upon to have at least a story if not yet a fully defined theory about why the individuals and firms that comprise that entity — the micro-level actors — will act in ways consistent with the theory's more explicit components and predictions. This demand for micro-foundations has disciplined mid-level theory, taming its otherwise open-ended eclecticism. It has also been an enormously productive move that has led to better specified theories as well as a greater range of testable hypotheses within each theory.

The progressive nature of mid-level theory can be illustrated by two well-known literatures, both of which I have written about before and will, therefore, only briefly summarize (Frieden and Lake, 2005; Lake, 2009). A first research program is open economy politics (OEP) that emerged as a mid-level theory to explain trade policy (Gourevitch, 1977; Hiscox, 2002; Rogowski, 1989) but which was later extended to monetary and financial relations (Bernhard et al., 2003; Frieden, 1988a, 1991), foreign direct investment (Jensen, 2006; Pinto, 2013), immigration (Leblang et al., 2007), foreign aid (Milner, 2006), regulation (Buthe and Mattli, 2011; Richards, 1999; Woods and Mattli, 2009), corporate governance (Gourevitch and Shinn, 2005), and global governance (Kahler and Lake, 2003). It now forms a comprehensive approach to explaining a large range of foreign economic policies.

The fundamental building block of OEP is interest, or how an individual or group is affected by a particular policy. Actors who benefit from a policy are expected to expend resources in the political arena to obtain that policy — as a shorthand, to lobby. Conversely, actors that lose from a policy are expected to lobby against it. In short, politics is fundamentally about winners and losers from alternative policies. Importantly, OEP uses economic theory to deduce what types of individuals can be reasonably assumed to share identical interests. A key divide within the approach is between the Ricardo–Viner or specific factors theory of international trade, which assumes that capital and labor are fixed in particular occupations and, thus, will tend to have similar interests over economic policy, and the Heckscher–Ohlin–Samuelson (HOS) theory of international trade, which assumes that all factors are mobile across occupations within countries and, therefore, capital and labor will possess opposing interests (Frieden and Rogowski, 1996). Having defined the relevant unit of analysis, OEP goes further to derive interests from the distributional implications of alternative economic policies and, in turn, how a group is located relative to others in the international economy. Firms vary by whether they are in the tradable or non-tradable sectors, produce import-competing or export-competing goods, use imported components, and so on. Factors of production, in turn, vary by their scarcity relative to the world economy. Using Mundell's (1957) equivalence condition — that flows of goods and factors across international borders are equivalent in their effects on relative rates of return — OEP then derives expectations

about how factors will be affected by a large range of economic policies and, thus, identify their interests over those same policies.

Institutions aggregate conflicting societal interests, with varying degrees of bias, and condition the bargaining between opposing groups. OEP is consistent with and draws heavily on the literature on comparative political institutions, much of which is not connected immediately to economic policy (see Cox, 1997; Tsebelis, 2002). Institutions also condition the bargaining between groups, largely by setting the reversion point for policy in the absence of some compromise and defining possible side-payments, cross-issue deals, and logrolls. OEP recognizes, in a way that the earlier domestic interests approach did not, that interests are central but not enough. However well specified, interests are refracted through political institutions that often have an independent effect on policy choices.

With domestic interests aggregated through institutions into a national policy — or, more accurately, a national ideal point — states then bargain when necessary to influence one another's behavior and to determine the joint outcome of their actions. International institutions, in turn, condition how this bargaining takes place and what outcomes are reached. This is the third and final step in the OEP approach.

Together, interests, institutions, and international bargaining explain the choice of policies by countries and the outcomes experienced by the world economy. Analysis within OEP proceeds from the most micro to the most macro level in a linear and orderly fashion, reflecting an implicit unidirectional conception of politics as flowing up from individuals to interstate bargaining. Although any one analysis may focus more or less on a single step in the causal chain, any complete explanation begins with interests, proceeds to examine the role of domestic institutions, and concludes with bargaining in international institutions and, then, ultimately the policy or outcome to be explained.

OEP has proven to be enormously productive and progressive, starting as a theory of trade policy and quickly expanding to include a range of other economic phenomena. This is, I believe, its greatest strength. Nonetheless, OEP, especially as practiced in the United States, has been the subject of critical assessment (see Weaver and Phillips, 2010). Constructivist scholars, in particular, have challenged the exogenously determined conception of interests that underlies the theory (for example, see Abdelal, 2001, 2007; Chwieroth, 2010). Even within the approach, the most telling criticism of OEP is that individuals do not appear to have stable preferences defined by their position within the international division of labor. Individuals do not identify themselves politically as either labor or capital, or as members of the steel, textile, or non-tradable sector, as posited by the theory. Rather, they have more complicated sets of interests defined by religion (Scheve and Stasavage, 2006), gender (Goldstein et al., 2007), and more (Rho and Tomz, 2012). Individuals also have more complicated identities as producers, as assumed by the theory, and consumers, and can be primed to think about their interests on policy in different ways (Naoi and Kume, 2011). This critique shakes the foundations of OEP, threatening to topple the entire superstructure. Nonetheless, OEP has driven nearly three decades of productive but eclectic research in international political economy. How well it responds to this critique, and especially whether it can develop a more nuanced but still deductive conception of interests, will be the challenge in the years ahead.

A second area of mid-level theory that has proven truly progressive is the so-called democratic peace theory. This theory is decidedly non-realist, but it fits poorly into any of the other paradigms. In 1795, Immanuel Kant posited in *Perpetual Peace* that republican states, lodged within international organizations and enjoying the fruits of international commerce, would be relatively more pacific in their relations with one another than with other countries.[12] This prediction, made when there were but a handful of democracies in the world, remained a topic of philosophical discussion for nearly 200 years. Small and Singer (1976) published the first empirical paper on the democratic peace, finding that democracies were, indeed, more peaceful than other states. Doyle (1986) ignited a new research program in his article on the 'liberal peace' published a decade later. Drawing explicitly on Kant, Doyle deduced and showed empirically that liberal democracies would not be more pacific overall but they would be more peaceful in their relations with one another. Empirical political scientists quickly confirmed that democracies were not more peaceful in general (i.e. there was no 'monadic peace') but were more peaceful within democratic dyads (i.e. a 'dyadic peace' or the 'democratic peace'). Within three years of Doyle's article, Levy (1989: 270) was able to write that the 'absence of war between democratic states comes as close as anything we have to an empirical law in international relations.'

Early analytic approaches distinguished between normative and institutional theories in accounting for this regularity (Maoz and Russett, 1993). Normative theory predicted overall pacifism for democracies, but explained the tendency of democracies not to fight one another by the notion of a shared republican respect. This approach proved less progressive, failing to connect to other extant theories in the mainstream of the discipline or to generate additional propositions. Institutional theory was immediately more successful, but came in two variants. One version focused on the public constraints on governments that determined what goals they pursued within the international system (Lake, 1992; Maoz and Russett, 1993). The second focused on how those constraints provided greater information to adversaries (Bueno de Mesquita et al., 1999; Schultz, 2001). Support eventually leaned towards the latter as this was consistent with the more general bargaining theory of war that was also gaining prominence about this same time (Fearon, 1995; Powell, 1999).

With several variants of the theory in play, scholarly attention soon broadened. Importantly, although Kant framed the debate, the empirical law was well known by the time the theories were being developed. Scholars began from the observation that democracies tend not to fight one another, and then built and selected theories on their ability to explain this regularity. Recognizing that they could not adjudicate between contending theories using only a known fact that all were designed to explain, scholars quickly turned to deducing additional implications of their theories, soon predicting and finding that democracies were (1) not only less likely to fight one another, but more likely to (2) win the wars they do fight (Lake, 1992; Reiter and Stam, 1998), (3) use peaceful dispute resolution mechanisms (Breecher and Wilkenfeld, 1997; Dixon, 1994; Mousseau, 1998), (4) initiate wars against autocracies (Bennett and Stam, 1998), and (5) pay fewer costs and fight shorter wars (Bennett and Stam, 1996; Siverson, 1995).[13] Drawing on the same theories, democracies were also found to provide higher levels of public goods for their citizens (Lake and Baum, 2001), enjoy higher rates of economic growth (Baum and

Lake, 2003), and act in a variety of other distinctive ways (Bueno de Mesquita et al., 2003). In seeking to test between alternative theories of relative peace, scholars found a host of distinctive traits for democracies that had not previously been identified.

What is important for my purposes here is the stages through which the research program has progressed. A normative insight led eventually to an empirical observation and then to a flowering of mid-level theories all crafted to explain this central empirical finding. As theories multiplied, scholars sought to deduce additional implications from each and to assess these hypotheses against observed empirical regularities. Some agreement emerged on an institutional theory that both predicted a large number of new propositions that were then confirmed empirically and, importantly, fit with the larger approach of the bargaining theory of war. As Lakatos (1978) would expect, the more general theory that predicted the most new facts appears to have gained the widest support among scholars.

Democratic peace theory is not without its critics. Critical theorists challenge the transhistorical assertions of the theory (Barkawi and Laffey, 2001), and others point out that democracies have often engaged in normatively questionable behaviors in their own interests (Owen, 2002). A central challenge is from those who maintain that the core finding that democracies tend not to fight one another is spurious, or that democracy and the democratic peace are jointly determined by some third factor. Some argue that the democratic peace is really a capitalist peace, with democracy and peace following from the presence of liberal market economies (Gartzke, 2007). Others argue that the democratic peace is the product of bipolarity, and the historical accident that most democracies ended up as allies on one side of the Cold War for ideological and geopolitical reasons (Farber and Gowa, 1995). Proponents counter, however, that neither the capitalist peace nor bipolarity can account for the new facts generated by institutional variants of democratic peace theory. Despite continuing controversy, the lesson here is that mid-level theories are not judged by their fit with pre-existing paradigms, nor limited to particular methodologies. Rather, they are best measured by their empirical power and ability to generate new propositions that are themselves empirically confirmed.

OEP and democratic peace theory are exemplars of the progressive nature of mid-level theory. Neither fits well into the traditional paradigms that supposedly define the field. Both have developed and grown over decades, belying the standard narrative of Great Debates as the driving forces for research in the field. Neither are without problems and critics — such is the way of all theory. This may be 'normal science,' a turn opposed by those who yearn for a return to the Great Debates of yore. But as these examples indicate, rather than being stuck in first gear unable to get past first principles, mid-level or eclectic theory has moved the field of International Relations and our understanding of world politics decisively forward. These two examples provide, I believe, models for how we as a discipline ought to pursue and evaluate progress in our research.

Another Great Debate?

Even as mid-level theory offers a glimpse of a perhaps promising future, there is a real and emerging divide in the field of International Relations between positivists and

post-positivists. This divide may become yet another Great Debate, although such a debate — if it occurs — will likely be as inconclusive as the previous ones. In many ways, the positivist–post-positivist divide overlaps with the epistemological and ontological differences of the second and final debates, respectively. Indeed, it reflects two different paradigms — not in the usual, loose International Relations sense that I have been using so far, but in the Kuhnian (Kuhn, 1970) definition of a single, hegemonic theoretical approach — in which 'facts' in one paradigm are sometimes simply unintelligible in the other. Two communities are developing with different cultures, practices, and languages that render conversation and interchange difficult and fraught with misunderstanding.

The great irony of Kuhn's (1970) theory of scientific revolutions is that scholars choose between paradigms before their promise is completely demonstrated. In his case, he looked to the scientific community as a whole to understand the shift as a sociological phenomenon. Such community ties likely play a role in determining one's positivist or post-positivist inclinations today in International Relations: where one was educated, and with whom, appears to have a massively conditioning effect, even recognizing that an element of self-selection goes on in choosing a graduate program. Yet, given two competing paradigms in the field today, the choice also remains a highly subjective and personal assessment based on what appeals to each individual as a satisfying explanation of any given phenomenon. This is, perhaps, as it should be. My own sensibilities are likely already clear, but in the spirit of full disclosure they lean strongly in the positivist (and eclectic) direction. But I recognize that this is a subjective judgment. That my sensibilities lean in one direction does not mean that I cannot respect the subjective assessments of others with different intellectual beliefs who make alternative intellectual bets. Rather than another inconclusive Great Debate, there is room, I believe, for both approaches and, I hope, a little friendly competition.

As above, positivists are poorly described by this label. Nonetheless, the label has stuck despite the attempt to modify it with various prefixes (e.g. neopositivism). Positivist scholars generally believe that research and researchers can separate themselves from reality and objectively observe the world they inhabit, that science is and should be limited to observable implications and factors, and that the purpose of science is causal inference. Within these beliefs, of course, there is disagreement about the precise meanings of key terms, especially about what constitutes an adequate causal explanation.

In the last two decades, there has been significant progress in elaborating a social-scientific methodology of causal inference. Some positivists have turned new attention to old problems of the scientific method, employing laboratory and field experiments in new settings (Hyde, 2010; McDermott, 2011), elaborating new quasi-experimental designs, and working on new ways of solving the endogenity or 'identification' problem through selection and instrumental variables models or matching designs (Dunning, 2012). Other positivists have developed a distinct qualitative methodology. This effort was stimulated by King, Keohane, and Verba (1994), who posited a single logic of causal inference based on probability theory. This provoked others to argue for greater attention to 'within-case' variation, focusing on causal mechanisms, process-tracing, counterfactual analysis, and other innovative methods (Brady and Collier, 2010; George and Bennett, 2005; Tetlock and Belkin, 1996). Although seemingly two distinct cultures (Goertz and Mahony, 2012), in larger perspective, quantitative and qualitative research,

at least as understood in this literature, are two variants of the same positivist approach. There is now substantial agreement on the basic methodology of and standards for positivist social-scientific research.

Post-positivists remain a more diverse group, often united only by their rejection of positivism's adherence to an objective science limited to observable 'facts.' Jackson (2011) identifies three separate non-positivist ontologies: critical realism, which allows for objective observation but believes this can be extended to unobservable features of the world through abduction and other similar methods; analyticism, which believes that observation is always subjective but limited to observable features; and reflexivity, which holds that observation is inherently subjective and can access the unobservable. Across these dimensions, post-positivists also disagree on the purpose of inquiry, with some adhering to a form of social science that takes as its aim the understanding of how the world came to be as it is (or was, in different historical periods) and others taking a more political or 'critical' stance in which the goal of scholarship is to move the world in more ethical directions. As in the final Great Debate above, the fragmentation between post-positivists would likely make a single Great Debate difficult; if such a debate were to occur, it would likely be argued on multiple fronts by multiple voices.

Like positivists, however, post-positivists have made substantial progress recently in developing appropriate research methodologies and standards. Interpretivists pose an over arching research method based on the 'Chicago School' of anthropological and sociological fieldwork developed in the 1920s and 1930s (Schwartz-Shea and Yanow, 2012; Yanow, 2000).[14] Critical theorists have wrestled with the question of standards when research is inherently subjective, drawing insights from humanist scholars and post-modernists like Bourdieu, Foucault, and Latour (Salter and Mutlu, 2013). Recognizing that self-identified members of their school work on both sides of the positivist and post-positivist divide, even constructivists have posed a unifying methodology of inquiry (Klotz and Lynch, 2007). The attention to methods and standards by post-positivists in recent years lays the foundation for important research progress as it is defined by each approach, whether this be improved understanding, the revelation of forms of hidden social power, or ethical change.

Despite the progress in each paradigm, I admit to a deep ambivalence about the existence of this divide. One the one hand, I sympathize with Cohen (2007) and others who have argued eloquently on the need to bridge and, ideally, close the divide (see Weaver and Phillips, 2010). I too am loathe to see International Relations split into two separate disciplines sharing the same subject matter and intellectual history but divided by epistemology and ontology. Were this to happen, both sides would inevitable 'forget' some of what we think we know about world politics, and both would be intellectually poorer for it. On the other hand, any attempt to bridge the divide will either produce a pabulum of inconsistent approaches or profound frustration from dealing with incommensurable facts and 'explanations.' To borrow the title of one of Tickner's (1997) famous articles, the prevailing response from both sides might likely be 'You just don't understand' — and both would be correct.

More important, as Kuhn (1970) first argued, progress is only possible within paradigms. OEP and democratic peace theory, described above, made progress only through sets of shared assumptions and common epistemologies and ontologies that allowed theory to be extended to new topics, additional hypotheses to be deduced, and

propositions confronted with evidence according to agreed-upon standards. Were researchers in each area forced to defend their methodological, epistemological, and ontological assumptions at every turn, progress within the approach would have been severely hampered. As these research programs have developed, they have been increasingly challenged by accumulated anomalies, as expected, and will either be revised or superseded by some future theory. Although I have not reviewed research in the post-positivist approach in similar detail in this article, and others are undoubtedly better qualified to address this question, I see similar progress in the feminist security studies program from the early works of Enloe (1990) that opened the political space to include women, to Tickner's (1992) agenda-setting work, to more recent and substantive applications that reveal and highlight the ways in which gender deeply structures world politics (see Sjoberg, 2010; Sjoberg and Via, 2010; Towns, 2010). This too would have been even more difficult if researchers were forced to debate first principles at every turn. Within both positivist and post-positivist approaches, progress occurs within paradigms according to their own criteria for evaluating that progress. This suggests letting each paradigm develop on its own in its own fashion.

In the end, I prefer progress *within* paradigms rather than war *between* paradigms, especially as the latter would be inconclusive. The human condition is precarious. This is still the age of thermonuclear weapons. Globalization continues to disrupt lives as countries realign their economies on the basis of comparative advantage, production chains are disaggregated and wrapped around the globe, and financial crises in one country reverberate around the planet in minutes. Transnational terrorism threatens to turn otherwise local disputes into global conflicts, and leave everyone everywhere feeling unsafe. And all the while, anthropomorphic change transforms the global climate with potentially catastrophic consequences. Under these circumstances, we as a society need all the help we can get. There is no monopoly on knowledge. And there is no guarantee that any one kind of knowledge generated and understood within any one epistemology or ontology is always and everywhere more useful than another. To assert otherwise is an act of supreme intellectual hubris.

This is not a plea to let a hundred, a thousand, or ten thousand intellectual flowers bloom. Scholars working in cloistered isolation are not likely to produce great insights, especially when the social problems besetting us today are of such magnitude. All knowledge must be disciplined. That is, knowledge must be shared by and with others if it is to count as knowledge. Positivists and post-positivists are each working hard to improve and clarify the standards of knowledge within their respective paradigms. This is an important turn for both, as it will facilitate progress within each even as it raises barriers to exchange across approaches. So, if not a thousand flowers, it is perhaps better for teams of scholars to tend a small number of separate gardens, grow what they can best, and share when possible with the others and, especially, the broader societies of which they are part.

Conclusion

Do not mourn the end of theory, if by theory we mean the Great Debates in International Relations. Too often, the Great Debates and especially the paradigm wars became contests over the truth status of assumptions. Declarations that 'I am a realist' or

pronouncements that 'As a liberal, I predict …' were statements of a near quasi-religious faith, not conclusions that followed from a falsifiable theory with stronger empirical support. Likewise, assertions that positivism or post-positivism is a better approach to understanding world politics are similarly blinding. The Great Debates were too often academic in the worst sense of that term. Mid-level theory flourished in the interstices of these debates for decades and now, with the waning of the paradigm wars, is coming into its own within the field. I regard this as an entirely positive development. We may be witnessing the demise of a particular kind of grand theory, but theory — in the plural — lives. Long may they reign.

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Notes

1. As an example, see the so-called relative gains debate (Grieco, 1993). But once Snidal (1991) showed that one could derive 'absolute gains' behavior from a model that assumed states were pursuing relative gains and Powell (1991) showed that you could also do the reverse, making the debate not a theoretical but an empirical one, scholars quickly lost interest.
2. *Nota bene*: Until the fourth part of this article, I use the term 'paradigm' as commonly used in International Relations to refer to a family of theories sharing certain core assumptions and not in the Kuhnian sense of a single, hegemonic approach to an entire field (Kuhn, 1970).
3. For a current history of the discipline written from the perspective of competing paradigms, see Katzenstein et al. (1998). For the continuing influence of the paradigms, one need only consult the organization of the field's most distinguished handbooks; see Reus-Smit and Snidal (2008) and Carlsnaes et al. (2013).
4. For a discussion of the interwar peace movement both in and outside of the academy, see Lynch (1999).
5. For recent reassessments of early realists, see Cox (2000) on E.H. Carr and Williams (2007) on Hans Morgenthau.
6. The key essays in this debate were reprinted in Knorr and Rosenau (1969).
7. On the difference between rationalism and intentionalism, see Dennett (1989). Nearly all social science theories are intentionalist, but not all are rationalist.
8. For an attempt to survey the state of the discipline, see Maliniak et al. (2011).
9. Katzenstein et al. (1998: 23–29) discuss domestic politics, but even here it is largely defined by how it intersects with the major paradigms.
10. For instance, the rationalist theory of war (Fearon, 1995) and neorealist theory (Van Evera, 1999) are consistent with realism, as is power preponderance theory (Organski and Kugler, 1980).
11. See the debate between offensive (Mearsheimer, 2001) and defensive realism (Waltz, 1979), or that between classical realists (Morgenthau, 1978), neorealists (Waltz, 1979), and neoclassical realists (Lobell et al., 2009).
12. On the Kantian tripod, see Russett and Oneal (2001).
13. Drawn from Bueno de Mesquita et al. (1999: 791).
14. Note, this is different from the 'Chicago School' of International Relations that developed around the same time. This must have been a heady time in Hyde Park.

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