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What is Theory?

Patrick Thaddeus Jackson

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Introduction

In a 1955 article, Kenneth W. Thompson reports on a remarkable gathering of “scholars, analysts, and diplomatists” to discuss theoretical approaches to the study of international politics. The guest-list was a veritable “who’s who” of mid-twentieth century American international relations (IR), including such luminaries as Hans Morgenthau, Arnold Wolfers, William T.R. Fox, Paul Nitze, and Reinhold Niebuhr; George Kennan was not present, but submitted a paper for discussion. Thompson reports that the members of the gathering wrestled with a number of problems involved in the formulation of a theory of international politics, including the relationship between history and theory, the role of theory for practitioners as opposed to its role for scholars, and the question of whether theories necessarily involved normative evaluation. While the answers that Thompson reports are intriguing, even more striking is the justification for holding the gathering in the first place:

Theory in the study of international politics perhaps deserves a special priority because of the urgency of the problem and the stridency of the debate generated by competing approaches each claiming to have preempted the field.

(1955:733)

Thompson acknowledges the call for theory emanating from the realm of practical politics, as practitioners seek answers to pressing political problems, but he also notes that the scholarly field of his time is beset by numerous claims to have finally

figured out *the* key to the understanding of the international realm. Both the existence of many such claims, and the universalizing character of the claims, bring to mind not just 1955, but the clash of traditionalism versus behaviorism during the 1960s (Knorr and Rosenau 1969), the rise of work on interdependence to challenge realist presumptions during the 1970s (Keohane and Nye 1977) and the realist counter-thrust at the end of the decade (Waltz 1979), and the “inter-paradigm debate” that dominated IR scholarship during the 1980s and on into the 1990s (Banks 1985; Wæver 1996). If anything, our contemporary scholarly landscape is even more fractured and fragmented than the landscape that Thompson confronted in the mid-1950s, since our disagreements continue and extend many of the points of controversy aired at that conference, but we've had half a century to proliferate a wider variety of answers.

Concerns with the “scientific” status of the enterprise of IR scholarship, and the relationship between IR theory and political practice, animated the gathering on which Thompson reported in 1955, which was a conference sponsored by the Rockefeller Foundation (Guilhot 2008:294–5). Indeed, these concerns go back to the origins of the field, and even before. And they have not yet been solved: there is still no field-wide consensus on what it *means* to have a “theory of international relations,” let alone a consensus on what such a theory ought to contain or what goals it ought to advance. Instead, there are numerous voices advancing arguments that perhaps fall loosely into traditions or schools. These identifiable scholarly communities do not actually display sufficient internal coherence to merit a label like (Kuhnian) “paradigm” or (Lakatosian) “research programme,” inasmuch as great diversity remains *within* each community on fundamental matters of ontology and epistemology, and inasmuch as the substantive claims that these communities advance (e.g., that democratic regimes do not go to war with one another) are for the most part empirical claims that are, in principle, straightforwardly testable (Jackson and Nexon 2009). Hence, the lack of field-wide consensus cannot be explained as a function of incompatible or incommensurable philosophical assumptions held by different groups of researchers (Chernoff 2005:181–5). Explanations must be sought elsewhere, whether in the nature of the subject-matter or in the vagaries of academic politics. Diversity, both in theories of world politics and in notions of “theory” circulating in IR, is an observed social fact of life in the field, and almost a century of debate seems to have brought us no closer to any definitive resolution.

How should we deal with this conceptual diversity? The problem with the traditional ways of organizing debates about these foundational issues in IR – especially the widely used notion of “three great debates” (or, depending on how one counts debates, perhaps four great debates, as in Wæver 1996) – is that they freely intermingle substantive and philosophical concerns. This is especially true of the supposed realist/idealist debate (which may, in fact, not have happened at all in the way that we remember it; see Schmidt 1998, and his essay “A History of International Studies” in this volume), and of the “interparadigm” debate of the 1980s; most of the issues at stake in those controversies were substantive considerations about world politics, and only secondarily, if at all, involved considerations about the philosophical issues involved in the production of knowledge. But the question “what is theory?” is *not* in the first instance a question about substantive issues, since presumably what makes something a theory is not the specific objects or phenomena on which it focuses. So in order to meaningfully order the ongoing controversies about the character and status of theory, the first necessary step involves bracketing specific claims about world politics in order to focus on their philosophical underpinnings.

Whatever it might precisely mean, the notion of ‘theory’ participates in a semantic and conceptual network populated by other typical commonplaces of the European Enlightenment, such as “science” and “reason”. To “have a theory,” or to “theorize” a phenomenon, is to claim something of a privileged epistemic status, reflected in the conventional scholarly hierarchy between theorists and those who merely labor among the empirical weeds; theory both orders empirical observations and somehow supervenes on them. In so doing, of course, a theory can provide “a base or fixed point upon which analysis can be founded” (Thompson 1955:738) – analysis, yes, but also action. Implicitly, this is the attraction of theory for practitioners, since a theory could in a sense *warrant* or *ground* their actions; the gathering Thompson describes had as one of its main aims the preparation of the intellectual groundwork for just such a grounding of practice. Indeed, Thompson describes a significant portion of the discussion as revolving around the relations between and among three different senses of the word “theory”: normative theory, a “general theory of politics,” and the set of assumptions on the basis of which a given actor is acting (ibid.:740). Bringing these into alignment would then be the Holy Grail of IR scholarship, and a significant step toward the completion of the project of the European Enlightenment as well.

But this is far from the only, or inevitable, way to think about theory in IR. Thompson's three types of theory are paralleled, in some ways, by Marysia Zalewski's triptych of theory as “tool,” theory as “critique,” and theory as “everyday practice” (1996:341) – a general theory of world politics is a tool for explaining and understanding it, while a normative theory lends itself to a critique of existing institutions and organizations, and the theories-in-use of various actors certainly inform and are implicated in their everyday practices. Unlike Thompson, Zalewski is very skeptical of the project of trying to align these three kinds of theory, and

seeks instead to exacerbate the differences between them so as to present the reader, and the field, with a set of stark contrasts and perhaps choices. Far from bemoaning the lack of field-wide consensus, Zalewski affirms and even celebrates the diversity of the field when it comes to these fundamental matters.

Thompson's and Zalewski's positions on theory are each internally consistent, although each signals a different *philosophical ontology*: a different way of conceptualizing the “hook-up” between theory, theorists, and the world (Shotter 1993:77–9; Patomäki and Wight 2000:215). Thompson's position is *dualist*, presuming the existence of a mind-independent world “out there” to which knowledge (particularly theoretical knowledge) refers and approximates; as such, his desire for integration among the three aspects of theory is comprehensible. Zalewski, on the other hand, is more of a *monist*, rejecting the mind/world (and theory/world) dichotomy in favor of a more complex interrelationship between observers and their objects of study. Dualism and monism are important, although underappreciated, commitments that inform debates about the production and status of knowledge in the social sciences in general (Jackson 2008); dualists and monists, one might speculate, would have different senses of “theory” and hence different approaches to the formulation of theoretical knowledge of world politics. It is at this level, and not at the level of specific claims about sovereignty or anarchy or interdependence, that meaningful differences about the nature and character of IR theory emerge.

But for all of their philosophical differences, there is something strikingly similar between Thompson and Zalewski: both are engaged in the traditional scholarly activity of writing an essay primarily targeted at an audience of other scholars. I make this observation not to impugn or caricature either scholar; in point of fact, each of their broader corpuses of work clearly indicates engagements with audiences outside of the academy. Rather, I am simply making the point that people in their everyday lives, practitioners, and in general people outside of the specific *habitus* and language-game of the academy do not spend much time explicitly and rigorously articulating their sense of theory. The very act of doing so places both authors, or at least this particular activity engaged in by both authors, on the same side of a line separating the attempt to produce knowledge about world politics from the attempt to affect some particular change(s) in world politics. Much like dualism/monism, this vocational distinction can hardly help but produce distinctly different senses of “theory.” Not just *what* we mean by “theory,” but indeed *how* we mean “theory” and precisely what else it is connected to semantically and conceptually, is shaped by these broader commitments.

Vocational orientations, like philosophical ontologies, are somewhat removed from the everyday concerns of IR scholarship. But they also provide a good way of coming to grips with the wide variety of ways that IR scholars use and understand theory. In the remainder of this essay, I will develop philosophical ontologies and vocational orientations into ideal-typical typologies that can be used to organize approaches to theory and thus map the course of the perennial debates that surround these issues. Like Max Weber, my concern throughout is with the idealized logic of each vocational orientation or philosophical ontology, and should not be mistaken for descriptions of concrete scholars or policy makers. Rather than “a ‘presuppositionless’ copy of ‘objective’ facts,” ideal-types are

formed through a one-sided *accentuation* of *one* or *more* points of view and through bringing together a great many diffuse and discrete, more or less present and occasionally absent *concrete individual* events, which are arranged according to these emphatically one-sided points of view in order to construct a unified *analytical construct* [*Gedanken*]. In its conceptual purity, this analytical construct [*Gedankenbild*] is found nowhere in empirical reality; it is a utopia.

(Weber, M. 1999:191)

The analytical utility of an ideal-type is that it permits us to clarify our intuitions about general social forms and processes such that we can observe the concrete ways that a specific case conforms to and deviates from those intuitions. Ideal-types do not allow us to evaluate concrete situations in normative terms, but they do allow us to draw together conceptually those features of a concrete situation that might otherwise appear to be quite accidentally linked.

To put this another way: by constructing ideal-type distinctions between different philosophical ontologies and vocational orientations, I aim to draw out differences in a too-pure way, but that abstract purity is precisely what makes the analysis useful – with an ideal-typical model in hand and in mind, we may catch a glimpse of the abstract features and dynamics contained in the model when we look to actual cases, and absent the model we might not have been able to see those features and dynamics in the first place. This is a feature of all models, formal or informal (Clarke and Primo 2007), and in this sense all models – whether they are models of political processes or models of orientations toward politics – are ideal-typical, and are therefore

neither to be directly tested against actual data nor to be used as a basis for a normative judgment on the actual world. Rather, the ideal–typical model helps us see things in a different light, and to comprehend them. The value of an ideal–type lies in its intellectual utility, not in its descriptive accuracy, so in what follows I will not be seeking to capture all of the nuances of particular people's sensibilities about theory; instead, I am deliberately focusing on a couple of underlying issues, so as to present a typology that is spare enough to be useful but rich enough to allow its extension to the myriad of authors I simply do not have space to cite or discuss in this essay.

I have chosen to work with *two* ideal–typical schemes in this essay, largely because neither one seems wholly adequate to the debates I am seeking to organize. Whether the two schemes are completely autonomous or in some sense complementary is a question I will defer discussion of until after I have elaborated each scheme individually.

Vocational Orientation

The name of Alexander George is prominently associated with efforts to get the scholarly and policy worlds speaking to one another. George's career moved back and forth between these two worlds: civil affairs officer in postwar Germany until 1948, PhD from the University of Chicago in 1958, influential member of the RAND Corporation's social science department from 1948 to 1968, professor of political science at Stanford from 1968 to 1990, and a fellow at the United States Institute of Peace after his retirement from Stanford (Palmer 2006). RAND and the United States Institute of Peace (USIP) are both civilian organizations with close ties to the US government; RAND is a private defense contractor originally established as an Air Force project, while USIP is chartered and funded by the US Congress. But both have similar missions: to provide useful scholarly insight into world politics, and in that way to improve the quality of policy. George was both a product of and an advocate for that system, devoting much of his career to – in his own words – “bridging the gap between theory and practice in foreign policy” (1993:xiii). Benefiting from experience on both sides of this divide, George was very clear that most of the accommodations required to bring scholarship and policy making closer together had to be made by scholars:

Scholars should recognize that for their research to be more relevant and useful in policymaking they should not define concepts and variables at too high a level of abstraction. The more abstract a concept, the more remote it is from its referent in the real world, and the greater and more difficult the intellectual demand on the practitioner to make that linkage and to benefit from it.

(ibid.:139–40)

In George's view, it is incumbent on scholars who want to affect the policy process that they downplay, or even provisionally abandon, any ambition to produce general accounts of phenomena in world politics, instead focusing on the provision of specific accounts of a more limited reach. From the perspective of practitioners, scholarly research is often too remote from actual events to inform real–time deliberations, and too concerned with overarching syntheses rather with case–specific particularities. The adjective that practitioners use to denigrate scholarship that they don't find particularly useful? “Theoretical.” Thus defined, from the perspective of its critics, “theory” seems to mean abstraction, generalization, and remoteness from concrete situations, and it is precisely these attributes that George argues have to be attenuated or abandoned in order to produce scholarship that is of genuine relevance for policy makers.

The gap between scholarship and policy making is a profound one. But as George acknowledges, the reason for its profundity cannot be blamed on simple volition, as though scholars and policy makers could communicate more meaningfully simply by trying a bit harder. Nor can it be blamed on a simple division between “theoretical” scholarship and “non–theoretical” policy making, inasmuch as the very attributes that policy makers criticize in academic scholarship can be found in any operative world–view whatsoever – and in that sense, everyone has and operates with a “theory” about world politics. So there is something else going on when a policy maker tosses the accusation of “theory” at a scholar, or a scholar claims to have discerned a “theory” in how policy makers act on information and make decisions. One word, two patterns of use – two *meanings*, if we take seriously Wittgenstein's dictum that meaning is often defined as use (Wittgenstein 1953: sec. 43). And to go further with Wittgenstein for a moment, the use of words is indicative of a broader “form of life” within which the speakers and the listeners are embedded; this form of life is what gives sense to the linguistic patterns that we can observe.

The worlds of scholarship and policy making are, in this sense, constituted by different expectations about the character and purpose of knowledge about world politics (Walt 2006:37). In particular, scholarship on world politics and policy making on the

international or world stage are different *vocational orientations*, a term I choose with some care to catch up a number of aspects of the difference between the two worlds even while underscoring at least two important similarities between them. “Orientation” suggests a direction, a stance, a way of comporting oneself toward something; more subtle than a perspective or a worldview, an orientation refers to those habits of thought and action out of which daily practices arise and within which they make some sense. “Vocation” carries a time-honored ambiguity between “job” and “calling,” between professional career and life’s work – an ambiguity I want to maintain, because even though scholarship and policy making are a life’s work for some people, they are jobs for many more, and in either case there are specific expectations associated with success in each. This is the first similarity I want to point out between scholarship and policy making: they both take place in professional environments, domains within which people work, and as such it is possible to sketch some generic features of what it is like to work in each domain.

The second similarity is not just that scholarship and policy making are both orientations, but that in a curious way they are different orientations toward the same thing: world politics. Now, granted, what interests policy makers about world politics is sometimes very different than what interests scholars about world politics; this is true at any level of analysis or in any issue-area. But there is often an odd family resemblance between what scholars say about a topic and what policy makers say about it, especially when they use similar phrases, like “the balance of power” or “economic development.” It is this family resemblance that makes possible forums in which scholars and policy makers swap ideas about a topic, appearing to almost – but not quite – speak the same language. That feeling of an almost-conversation, an almost-exchange, lingers after many such forums that I have attended over the years, and I believe that if we are honest with ourselves we can identify the same vague disquiet attending our own reflections on many of those forums. It is perhaps odd for scholars to see their work stripped of qualifications and nuance in order to be fashioned into an instrument for more unequivocal conclusions than they would themselves be comfortable making; it is perhaps equally odd for policy makers to have themselves and their actions be pigeonholed as a case of some broader phenomenon, or to be analyzed in terms of a set of analytical propositions to which they would not fully assent.

A particularly striking example of this divergence, of an almost-conversation in which the parties deploy the same words and phrases but to very different effect, surrounds the strange career of Samuel Huntington’s “clash of civilizations” argument (1993; 1996). Originally aired in *Foreign Affairs*, a journal combining scholarly commentary on contemporary events with articles by policy makers covering similar territory, the argument then found its fuller form in a bestselling book published, notably, not by an academic press, but by the commercial press Simon and Schuster. Several printings later, the phrase Huntington coined, if not the specifics of the argument, is firmly ensconced in public awareness, and is used as a shorthand for the notion that the world is characterized by fundamental and irreconcilable differences of values, and that these differences will lead to conflict.

The public perception of Huntington’s argument belies the complexity of the responses of both the scholarly and the policy-making community. Scholars came out swinging against the notion that civilizations were internally homogenous (Melleuish 2000; Schäfer 2001) and conducted quantitative tests of the notion that the most important determinant of conflict was civilizational membership (Russett et al. 2000; Fox 2002). The overwhelming scholarly consensus was that Huntington’s thesis was misleading at best and downright wrong at worst, and these days the “clash of civilizations” only shows up in most academic journals or books as an occasion for scorn or refutation. For almost all scholars of world politics, Huntington’s claims are no longer worthy of serious academic attention.

Things were and are very different in the policy world, where the major responses to Huntington’s argument involved either preparing for or striving to prevent a clash of civilizations from occurring. Mohammad Khatami, then the President of the Islamic Republic of Iran, called for a “dialogue of civilizations” in September 2000; subsequently, the United Nations proclaimed 2001 the Year of Dialogue Among Civilizations, featuring a variety of international conferences and other public efforts to explore the potential for a peaceful celebration of cultural differences. On the other hand, policy makers outside of the US took Huntington’s argument as confirmation of what they’d always suspected about the West’s designs and schemes, and read the claims about a clash through the lens of their own traditional debates about the expansion of putatively “Western” values and political practices (Tsygankov 2003; O’Hagan 2004). Policy makers found in Huntington’s broad-brush depiction of global areas of conflict a helpful way to think about the world and to bring some order to their intuitions about global cultural diversity. Huntington’s portrait of world politics narrowed the myriad of important factors to two (material power and cultural difference), called attention to a number of trends – among them demographic shifts and changes in worker productivity – that were working to the advantage of countries outside of the US and its European allies, and had a simple overall recommendation for the US: don’t try to intervene in every conflict all over the world. Policy makers, then, took Huntington’s claims a lot more seriously than scholars did.

A not insignificant part of this difference in response is probably explained by the fact that Huntington, whether deliberately or as an unintentional result of his own career path that moved back and forth between academia and the policy world, produced

something very close to what George would call “policy-relevant knowledge,” combining general accounts of strategies, limited generalizations about when a strategy might or might not work, and “actor-specific knowledge of the behavioral patterns of the adversary” (George 1993:103). Huntington provided a road-map to regions where US involvement might be unlikely to succeed, and advocated deferring to regional powers in such cases. The argument was easy to understand, written with a minimum of academic jargon, and provided just enough historical evidence to lend it plausibility. Whether the argument had passed the kind of vetting to which scholars ordinarily subject arguments is, plainly speaking, nowhere near as relevant to policy makers as it is to scholars. Huntington's argument was “theoretical” enough for scholars to try to operationalize and evaluate it, but not so “theoretical” that policy makers would dismiss it out of hand. The scholarly debates that put the argument to the test, most of which quickly turned into discussions about precisely what a “civilization” *is* (Hall and Jackson 2007), were precisely the sort of thing that would look overly abstract to policy makers; likewise, the adoption of arguments like Huntington's by policy makers would look to scholars like ideology rather than an effort to ground policy on sound scholarship.

These different appraisals of the value of Huntington's argument reflect, or exemplify, the two vocational orientations of scholarship and policy making. The distinction I am drawing here has its roots in Max Weber's classic effort to distinguish between the two vocations of “science” (*wissenschaft*) and “politics” (*politik*); because of the broader sense of these two words in German than in English, “scholarship” and “policy making” are perhaps equally plausible translations of Weber's categories. Weber's analysis moves freely between the “external” conditions of each domain and the “internal” psychological dispositions of the people working in those domains, seeking to ideal-typically express the different habits and expectations associated with each vocation. To say that scholarship and policy making are separate endeavors is thus to make a *logical* claim about the use of knowledge and language appropriate to different realms of activity. Using the lecture-hall as exemplary of a scholarly setting, Weber argues that “when we speak of democracy in the course of a lecture, our task is to examine its various forms, to analyze them in order to see how they work, and to establish the consequences of this or that version for people's lives.” In doing so, the scholar must remain non-partisan, and “take good care not to use his position at the lectern to promote any particular point of view” (2004:20). But it does not follow from this characterization that scholarly analysis is somehow devoid of values. Indeed, Weber suggests that

There is simply no “objective” scientific analysis of cultural life – or, put perhaps somewhat more narrowly but certainly not essentially differently for our purposes – of a “social phenomenon” *independent* of special and “one-sided” points of view, according to which – explicitly or tacitly, consciously or unconsciously – they are selected, analyzed, and representationally organized as an object of research.

(1999:170)

This inevitability of value-commitments does not mean that “*research* can only have *results* which are ‘subjective’ in the sense that they are *valid* for one person and not for others” (ibid.:183–4). Indeed, the distinctiveness of scholarship is not that it embodies no value-commitments, but that it does something distinctive with those commitments. The diversity of value-commitments places a specific duty on the practicing scholar:

A systematically correct scientific demonstration in the social sciences, if it wants to achieve its goal, must be recognized as correct even by a Chinese (or, more accurately, it must constantly *strive* to attain this goal, although it may not be completely reachable due to a dearth of documentation). Further, if the *logical* analysis of the content of an ideal and of its ultimate axioms, and the demonstration of the consequences that arise from pursuing it logically and practically, wants to be valid and successful, it must be valid for someone who lacks the “sense” of our ethical imperative and who would (and often will) refuse our ideal and the concrete *valuations* that flow from it. None of these refusals come anywhere near the scientific value of the *analysis*.

(ibid.:155–6).

Weber's basic point here is that even someone who rejects our values should be able to acknowledge the validity of our empirical results within the context of our value-commitments. The decisive issue is *internal validity*: whether, given our assumptions, our conclusions follow rigorously from the evidence and logic we provide. There is no scientific or scholarly way to criticize value-commitments themselves; nor is scholarship about advancing or advocating any particular set of value-commitments. Rather, the scholar is “*wholly devoted to his* [sic] *subject*” (2004:10) and strives only for explanation and understanding – and, perhaps,

education.

This respect for systematic procedures of explaining and understanding contrasts markedly with the cacophony of the political realm, in which partisan opponents struggle to control resources and to enact their preferred programs without much caring about the logical or factual defensibility of their positions. Unlike the scientist, the politician bases her or his actions on a calculation of the likely political outcome, and on the role played by power in determining that outcome:

When we say that a question is “political,” ... we always mean the same thing. This is that the interests involved in the distribution or preservation of power, or a shift in power, play a decisive role in resolving that question, or in influencing that decision or defining the sphere of activity of the official concerned. Whoever is active in politics strives for power, either power as a means in the service of other goals, whether idealistic or selfish, or power “for its own sake,” in other words, so as to enjoy the feeling of prestige that it confers.

(ibid.:34–5)

Making policy is thus about determining a concrete course of action, and requires jumping into a struggle with express intent to affect the outcome. “If you speak about democracy at a public meeting there is no need to make a secret of your personal point of view,” Weber notes; “The words you use are not tools of academic analysis, but a way of winning others over to your political point of view” and “swords to be used against your opponents: weapons, in short.” This is quite different from the scholarly use of the same or similar words as “plowshares to loosen the solid soil of contemplative thought” (2004:20), since contemplative thought aims not at the concrete enacting of any specific action, but at the comprehension of issues and phenomena divorced from a specific partisan agenda.

Remembering that these are ideal-types is important, because the lines between scholarly and policy knowledge are considerably less clear in particular empirical cases than they are in Weber's discussion. Indeed, it is quite possible to see the *wissenschaft-politik* distinction repeating itself, fractal-like (Abbott 2001), within institutionalized academic and policy-making settings. Just as there are academics who are more interested in making concrete recommendations about courses of action (e.g., Jentleson 2002; Walt 2006) and academics who are more interested in clarifying and contemplating basic assumptions and perspectives (e.g., Wendt 1999), there are policy makers who are more interested in carrying out concrete proposals and policy makers who are more interested in reflecting on practice with an aim to improve it. This suggests that Weber's account needs to be complicated a bit in order to capture the diversity of vocational orientations actually available. If the basic distinction is between a contemplation of (world) politics and the enacting of specific programs within (world) politics, we should think at a minimum about this distinction recurring once within each camp. A simple diagram of the resulting positions might look like Figure 1.

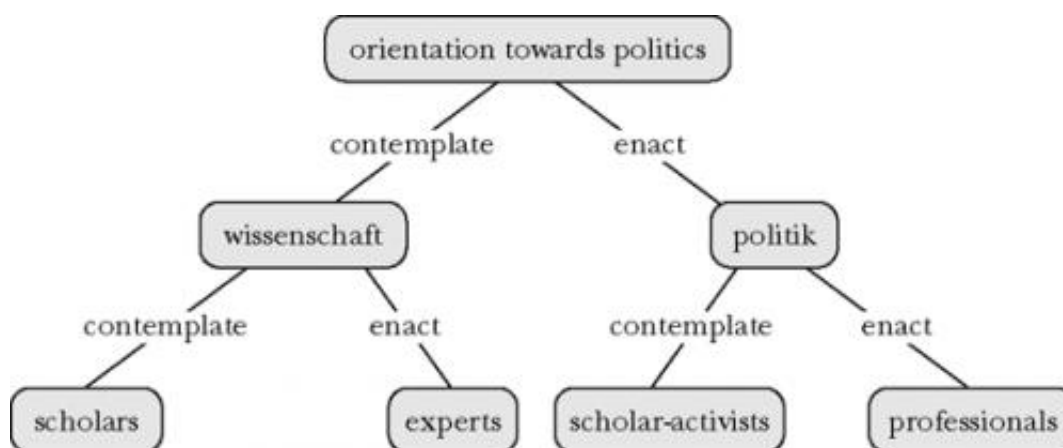


Figure 1

We thus have *four* vocational orientations: “scholars,” pure contemplators; “experts,” whose explanation and understanding of

political phenomena give rise to concrete recommendations for action as they move over toward the world of policy making; “scholar-activists,” or perhaps “reflective practitioners,” whose first commitment is to the world of policy but who step back from that world in an effort to make some general sense of it; and “professionals,” or perhaps “practitioners,” the full-time policy makers whose focus is on getting things done in real time. These orientations are ideal-typical, but together they form a more differentiated ideal-type than Weber's initial binary, one that allows us to bring controversies about the role and status of knowledge more firmly into view. In particular, the four-part distinction makes possible a revealing explication of the meaning of the word “theory,” since what that term means and what its value is understood to be changes depending on where the speaker is located with respect to questions of contemplating and enacting.

The first thing to note is that we are not looking at a simple continuum running from “theory” on the left-hand side to “practice” on the right-hand side. Instead, we have *relative* positions: professionals are more inclined toward enacting policy than scholar-activists, and experts are similarly more concerned with practical results than scholars are. This makes scholars and scholar-activists the “local contemplators” of their part of the diagram, just as it makes experts and professionals “local enactors.” This in turn suggests a certain positional similarity between each of these pairs of positions: scholars and scholar-activists agree on the need to step back from the immediate in order to achieve a broader view, while experts and professionals agree on the need to dive into a situation or an issue-area in order to do much about it. Another way of saying this is that experts and professionals are concerned to *use* theory in order to generate concrete recommendations, while scholars and scholar-activists are concerned to *build* theory by examining concrete situations in a more detached way.

However, and this is the second thing to note, the positions in each half of the diagram also share certain things in common with one another that they don't share with positions in the other half. IR scholars and experts begin in the same place, vocationally speaking: with a commitment to contemplating world politics with an eye to explaining and understanding it. The division between them comes second, and this means that whatever debates they might have about the importance of “pure theory” versus “applied theory” come from a place – both conceptually and, often, organizationally (Büger and Gadinger 2007:98-9) – that is already more contemplative, and more removed from the immediate practical demands of politics, than either scholar-activists or professionals. This means that, from the perspective of either scholars or experts, the day-to-day activities of scholar-activists and professionals are likely to appear “atheoretical,” far too wrapped up with practical details and specific policies. The reverse is also instructive: academics, whether scholars or experts, look too far removed from actual politics when viewed through the eyes of scholar-activists and professionals; academic concerns and academic work are often too “theoretical” to be of much use, at least not without important modifications.

It is in the light of these distinctions that Alexander George's advice to IR scholars becomes especially salient. George starts by noting that “the eyes of policy specialists [professionals, in my terminology] quickly glaze over at the first mention of the word ‘theory’ or the phrase ‘scientific study of international relations’,” largely because of the abstract terms and technical jargon that often seem to accompany such phrases (1993:6). George therefore calls for IR scholars to develop “policy-relevant knowledge,” specific kinds of conceptual and behavioral models and “generic knowledge” of the conditions under which particular policies are likely to work; this kind of knowledge stands a better chance of being useful to policy makers (George 1993:137-9; see also Jentleson 2002:191-2). But although George conceptualizes this as a simple question of bridging a gap between theory and practice, I would argue that what he is calling for is more complicated than this. George is not calling on IR academics to become policy makers, or even to become adjuncts to policy-making processes; he is calling for a clearer form of communication between these two worlds. And his recommendation for academics, understood in the light of the four vocational orientations, is that they should learn to function more like experts. The concern here is to produce theory that is useful, even while acknowledging that what academics produce is different than what policy makers produce, so that the gap will never be eliminated – only bridged. The form of that bridging follows the logic one of the two kinds of positional similarities I sketched above: by taking on the role of “expert,” academics may find it easier to convey information to policy makers, especially to the professionals among them.

That George would articulate this kind of advice is perhaps not surprising in light of his career path. As I have noted above, before joining the faculty at Stanford George spent two decades with the RAND Corporation. While RAND is a government contractor and policy think-tank, it is perhaps the most academic of such organizations, deliberately designed as “a campus without students” even to the point of having “departments” rather than sections or divisions (Abella 2008:33). RAND's mission was to place policy on a sounder knowledge base, and as such was always concerned with producing precisely what George called for: policy-relevant knowledge, useful theory, expertise. Vocationally speaking, RAND is an organization of experts, more removed from the partisan political process and the day-to-day tussles of policy making in the US than are, say, the Brookings Institution or the American Enterprise Institute. Even RAND's West Coast location underscores this distance from the policy-

making process. The difference between RAND and Brookings is the difference between experts and scholar-activists, and their relation to “theory” varies accordingly: experts use theory to produce policy recommendations, while scholar-activists use theory to frame their reflections on practical experience. George, though working outside of academia during his time in RAND, was never primarily a policy maker, and this is important for understanding the kind of theory he called for.

For an example of a second type of positional relationship, consider the closing words of Hedley Bull's classic book *The Anarchical Society*:

The search for conclusions that can be presented as “solutions” or “practical advice” is a corrupting element in the contemporary study of world politics, which properly understood is an intellectual activity and not a practical one. Such conclusions are advanced less because there is any solid basis for them than because there is a demand for them that it is profitable to satisfy. The fact is that while there is a great desire to know what the future of world politics will bring, and also to know how we should behave in it, we have to grope about in the dark with respect to the one as much as respect to the other. It is better to recognize that we are in darkness than to pretend that we can see the light.

(1977:319-20)

Bull's position is perhaps the most pure example of a contemplator's point of view that might be imagined: the study of world politics has basically nothing to do with the promulgation of policy-recommendations. This will obviously not appeal to experts like George, whose interest in the study in world politics is intimately intertwined with their desire to make concrete recommendations. Bull's audience here is primarily an academic one; he is intervening in a debate internal to academics, and not interested in speaking directly or indirectly to policy makers, except inasmuch as he is cautioning them not to expect something from the left-hand side of the diagram that it cannot, in his view, provide.

That Bull's audience here is primarily made up of other academics can be seen by considering the way that his position became one voice in the field's “second great debate,” pitting traditionalists against behaviorists and self-proclaimed “scientists” – *all of whom were vocationally oriented towards the production of knowledge*. Disputes about whether large-*n* quantitative or small-*n* qualitative techniques were a superior way to generate a reliable basis from which to speak about world politics are more or less entirely contemplative disputes, and depend – albeit implicitly – on a logically prior orientation to knowledge-production as the primary goal of thinking about world politics. It is striking that none of the contributors to the volume encapsulating large portions of the “second great debate” – Knorr and Rosenau's *Contenting Approaches to International Politics* (1969) – questioned this vocational orientation, and it is equally striking that Friedrich Kratochwil's recent attempt to reopen the debate (2006) remains almost entirely within the sphere of epistemic considerations.

This is in no way intended as a criticism of any of these scholars. It is simply an observation about a tacit consensus informing the debate: All of the participants in the debate presumed that their primary goal was to produce knowledge about world politics rather than to directly intervene in world politics. Indeed, J. David Singer elsewhere (but contemporaneously) contrasts unit-level and system-level scholarship in terms of, among other things, their relative policy-relevance. Singer notes that although “the systemic level produces a more comprehensive and total picture of international relations than does the national or sub-systemic level,” it does so by ignoring the “richer detail [and] greater depth” afforded by a unit-level focus. As a result, two different kinds of knowledge are generated from these two focuses: “Thus the policy-maker will tend to prefer predictions about the way in which nation *x* or *y* will react to a contemplated move on his own nation's part, while the scholar will probably prefer either generalized predictions regarding the behavior of a given class of nations or those regarding the system itself” (1961:89-90). What Singer is disclosing here is a fundamental divergence *within* the academic vocational orientation: Do we stay close to the concerns of the politicians we study, becoming experts who can make recommendations that they might be able to use, or do we depart from their concerns in scholarly ways that allow us to gain a more general view of things?

As a final consideration on the academic character of this debate, consider the paucity of written sources in which policy makers explicitly spell out their notion of what theory means or what kind of knowledge they produce. This should not be surprising if we consider the different incentives, institutions, and professional practices associated with careers in academia and in policy making; the tenure process and peer-reviewed publications on one side, and advancement through the bureaucracy and think-tanks where publication is editor-reviewed on the other, combine to produce distinctly different opportunities for this kind of epistemological self-criticism (Jentleson 2002:179-80). While it is certainly true that “the practices of scholars by which they

deliberate about the status of their knowledge claims are tied to those types of practices by which they link up with their environment” (Büger and Gadinger 2007:105), it is similarly true that scholars are *encouraged* to engage in such debates in ways that policy makers are not. Thus we are faced with a striking situation in which explicit reflection on these combinations of vocational orientations takes place more or less entirely of one side of the diagram – proceeding, perhaps, in more or less splendid isolation from the other side of international relations practice.

The Mind/World Connection

The diversity of perspectives on theory is, however, in no way exhausted by these vocational considerations. As I have argued, within these vocational orientations, we find different notions of what theory is or should be. But the scholarship/politics dichotomy is insufficient to capture all of the varied notions of theory current in the field, since even academics who largely agree that their first goal should be to produce better knowledge about the world do not simply debate the question of whether their scholarship ought to be directed toward informing policy makers or toward refining abstract models. It would not surprise me if the scholarship/politics dichotomy does not exhaust discussions about theory among policy makers either, but those discussions are less likely to appear in a formal printed form for reasons that I have just mentioned. Nor are policy makers particularly likely to spend much time poring over a Compendium such as this. As a result, for the remainder of this discussion I will focus on debates among academics.

There is at least one other set of conceptual dichotomies implicated in those intra-academic debates: a set of dichotomies revolving around positions in philosophical ontology. By “philosophical ontology” I mean not the catalog of stuff that a scholar takes to exist in the world, such as “individuals” or “states” or “international organizations” – that is more properly termed a scholar’s “scientific” ontology, since it contains the list of those objects and phenomena that a given perspective admits into consideration. Philosophical ontology, rather, consists of the logically prior assumptions about how we come to encounter those objects: the conceptual and philosophical basis on which claims about particular objects are formulated in the first place (Patomäki and Wight 2000:215). We are operating here in the realm of assumptions about how observers relate to things observed, how to balance substantive and formal criteria when designing a research project, and the like. In other words, we are operating in the realm of *methodology*, broadly understood.

Unfortunately, broad methodological considerations are often obscured in contemporary academic IR discussions by two related problems. First, the place where methodological issues like this are most explicitly teased out, and where concrete implications for empirical research are most concretely drawn, is in the philosophy of (social) science, and our inadequate familiarity with the philosophy of science in the field of IR does us more harm than good. Philosophy of science is far from a required course in most PhD programs, especially those in the US (Schwartz–Shea 2003), which contributes to the production of IR academics who are not conversant with these philosophical debates. And the only philosophy of science that many IR scholars know involves only three names – Popper, Kuhn, and Lakatos – and employs them in a single heroic narrative whereby Popper establishes “falsification” as the way that science works, Kuhn suggests that scientists adhere to “paradigms” rather than definitively falsifying anything, and Lakatos affects the synthetic moment of this dime-store Hegelian drama by creating “sophisticated methodological falsification,” with its associated conceptual paraphernalia of “hard cores” and “progressive/degenerative” judgments as a way to save falsification in the face of the Kuhnian challenge. (Indeed, I would wager that most contemporary IR scholars are unaware how much of this heroic narrative derives from a partial reading of a single article by Terrence Ball (1976).) Aside from the fact that this reading of Kuhn and Lakatos is seriously misleading (Jackson and Nexon 2009), the major problem here is that the Popper–Kuhn–Lakatos trio represents only a small part of the overall debates in the philosophy of science. But simply bringing in other names – Larry Laudan, Roy Bhaskar, Hilary Putnam, etc. – does little good on its own, since most scholars in our field do not know the proper context within which to appreciate their work – and the received wisdom about Popper–Kuhn–Lakatos serves as something of a deterrent to spending the time required to master that context.

In addition, although IR as a field has always been somewhat anxious about its “scientific” status, sustained philosophical reflection on the status of IR knowledge has been relatively rare. In part this is probably related to the scholar–expert division, which channels field-wide debate into controversies about how theoretically informed knowledge should relate to concrete political recommendations; to the extent that there have been philosophically reflective debates about IR knowledge, they have tended to take place among scholars, and to be dismissed by experts as irrelevant speculation. The most famous of these exchanges was probably that between Richard Ashley (1984) and Robert Gilpin (1986), in which Gilpin’s rejoinder to Ashley’s meticulous critique of neorealism on conceptual and philosophical grounds was brushed aside in favor of a fairly unreflective assertion of what “everybody” already knew about the role of theory in producing scientific explanation. Echoes of this gesture

persist in the notion that “critical” scholars fail to produce empirical work (Price and Reus-Smit 1998), and in the notion that reflecting on the abstract and formal attributes of a model is insufficient without a concrete, substantive illustration or case study (Johnson 2003).

The resulting bias toward the empirical acts as something of a deterrent toward sustained philosophical dialogues. One consequence is that we often pass right over methodology and fall into debates about *method*: King, Keohane, and Verba can spend the overwhelming majority of their book *Designing Social Inquiry* (1994) on technical issues about case-selection and variable-operationalization, presuming that their readers will already agree with them on methodological questions about the status of knowledge. Another consequence is that opponents of the methodology underlying such recommendations often proceed by grabbing one or another doctrine out of philosophy of science debates and plopping it down in IR as an alternative answer which will finally put an end to these abstract discussions and allow us as a field to get on with “doing IR.” Hence, even philosophically minded scholars like Alex Wendt (Wendt 1987; 2000), Colin Wight (2006), and Fred Chernoff (2005) provide an answer that they defend rather than encouraging a debate *within IR* about philosophical issues related to knowledge and theory. These consequences combine to discourage sustained dialogue about philosophical and methodological matters.

So how might we characterize philosophy of science debates in a way that would give us some leverage over the diversity of positions regarding methodology that presently obtain in the field of IR? Since the goal here is not to resolve the debate between these positions, but to more adequately characterize the diversity of philosophical positions that have an impact on how “theory” is conceptualized in the field, it would seem to make sense to develop a relatively spare set of ideal-typical dichotomies that would parse scholars and scholarship into mutually exclusive categories. This is the same procedure that I used above to distinguish between vocational orientations, but a single fractalized distinction does not seem adequate to the task. Instead, I believe that we need *two* distinctions to make sense of contemporary philosophical ontologies within the field of IR: monism/dualism and phenomenal/transfactual. Both of these dichotomies speak to the nature of knowledge and to the relation of the knower to the known, and as such have a lot to tell us about the nature of theory.

By monism/dualism I mean to pick up something of the hoary old mind/body problem that has been haunting Western philosophy since at least the time of Descartes (Wendt 2006; Pouliot 2007; Jackson 2008): How are knowing subjects plugged into the world they (appear to) know something about? Descartes’ (1993; first published 1641) famous declaration that the only thing of which he could be certain was his own existence, which he knew for certain because of his own act of thinking – *cogito ergo sum* – set up the dualist side of this dichotomy, essentially creating the modern philosophical discipline of epistemology and tasking it with figuring out how an autonomous knowing subject achieved reliable knowledge of an externally existing, mind-independent world. For dualists, valid knowledge must in the end be related to some sort of accurate correspondence between empirical and theoretical propositions on the one hand and the actual character of the mind-independent world on the other, and dualist research techniques are designed to bridge that gap between mind and reality. For monists, on the other hand, the very notion of a fundamental split between mind and world looks like a conceptual mistake; instead, for monists mind and world are continuous, and the problem is not how to get knowledge to correspond to the world as much as it is to explicate the ways that knowledge-production constitutes the world itself. Researchers are part of the object of study, never standing separate from it.

Monism/dualism parses out quite a number of debates about “theory” in contemporary IR, among them the dispute among constructivist scholars about whether to formulate testable hypotheses or not (Klotz and Lynch 2007:20–2) and the dispute between security scholars about whether logical rigor or empirical accuracy should be the dominant standard of theory-evaluation (Walt 1999; Bueno de Mesquita and Morrow 1999). Testable hypotheses and empirical accuracy fall on the dualist side of the ledger, since they are clearly designed to cover the distance between mind and world by hailing the world into a position of validating or invalidating scholarly speculations; a theory that failed these kinds of tests would run the risk of failing to correspond to the world, and hence generating misleading knowledge. On the other side of the dichotomy, monists are basically by definition unable to use empirical testing as a criterion of theory-evaluation, since the very procedure itself presumes a mind/world separation that they reject. Instead, the proper standards for theory-evaluation are criteria like deductive rigor and pragmatic utility, standards that do not set up a theory against the world and look for representative accuracy. As Kenneth Waltz – an analytical monist often misunderstood in dualist terms (Goddard and Nexon 2005; Wæver 2009) – declared: “a theory is a picture, mentally formed, of a bounded realm or domain of activity ... Theory isolates one realm from all others in order to deal with it intellectually” (1979:8). As such, theory for a monist is not strictly speaking testable – every oversimplification would fail such a test if compared to the empirical domain it abstracts from – but needs to be evaluated in other, more indirect ways.

However, monism/dualism is insufficient to capture the diversity of philosophical ontologies in the field, since it is incapable of distinguishing between fairly significant disagreements *within* each camp. Hypothesis-testers like King et al. (1994) are dualists,

but it is doubtful that they would disagree with a critical realist like Alex Wendt that a theory is “a coherent set of propositions with some correspondence to reality” (1987:343). Instead, the main axis of debate between neopositivists like King et al. (and, for that matter, the contributors to Brady and Collier’s edited volume (2004) that seeks to elucidate “qualitative” inquiry in greater detail) and critical realists like Wendt (and [Colin Wight \(2006\)](#) and [Heikki Patomäki \(2001\)](#)) concerns the somewhat obscure but tremendously important issue of the status of unobservables: neopositivists limit themselves to aspects of the world that they can measure more or less directly, while critical realists maintain that knowledge goes beyond the observable, empirical world and seeks to grasp the deeper structures that give rise to those empirical observations. This obviously has tremendous implications for a definition of “theory,” since a neopositivist theory is limited to an elucidation of the observed correlations of causal factors, while a critical realist theory is free to posit real-but-unobservable mechanisms, such as global class antagonism, and use these in an explanation without having to provide direct evidence of their existence. The role played by such unobservable notions in an explanation is, for a critical realist, justification enough that the notion corresponds to something real – much as quarks are considered by critical realists to be “real” even though they cannot, according to contemporary physics, be directly observed or even exist as individual entities.

To introduce some technical language, the issue of the reality of unobservables defines a dichotomy between phenomenism (the limitation of knowledge to the sphere of experience, and a refusal to maintain the reality of anything that cannot be experienced ([Harré 1985:68–86](#))) and transfactualism (the connection of knowledge with real forces and objects that cannot be directly experienced ([Bhaskar 1998:49–54](#))). Note that in focusing on this distinction, I am eliding an important, but ultimately subordinate, debate among phenomenists about the theory-dependence of observation – a debate that has consumed inordinate amounts of attention in discussions about scientific progress in IR over the past few years. In brief, the difference between classical logical positivism and contemporary neopositivism is that logical positivism was engaged in a search for basic, atheoretical “observation statements” on which to ground knowledge-claims, while neopositivism accepts that even observation statements have conceptual content and are not read directly off of unmediated empirical reality. Popperian falsification, in point of fact, was designed to preserve a notion of scientific progress in the face of this realization; falsification is only required because of the unavailability of atheoretical facts from which knowledge could be reliably derived or deduced. In this respect, although not in most other respects, Lakatos, a philosopher whose works enjoy perennial popularity in IR discussions (e.g., [Elman and Elman 2002; 2003](#)) continues the Popperian agenda –although he adds the important caveat that one can only judge the “progressive” or “degenerating” character of a line of research *in retrospect*. Regardless of the utility –or lack of utility – of Lakatosian criteria for assessing the character of theoretical debates in IR, the fact remains that the theory-dependence of observation in no way challenges or threatens the overall phenomenism of basically everyone involved in that discussion, since nothing in Lakatos justifies attributing real existence to unobservable entities.

The phenomenal/transfactual dichotomy helps make sense of disagreements between neopositivists and critical realists over the proper way to build theory: Because of their phenomenism, neopositivists hold the more or less direct comparison of theory and experience to be the gold standard of theory evaluation, while critical realists are far more tolerant of elaborate theoretical scaffolding as long as that scaffolding eventually helps to make sense of observed empirical reality. Hence neopositivists often look askance at notions of “social structure,” preferring measurable quantities like the distribution of power (e.g., [Mansfield 1993](#)) or supposedly directly-observable entities like human beings and the social groups into which they are organized (e.g., [Moravcsik 1997](#)), while critical realists are perfectly comfortable with constitutive norms (e.g., [Reus-Smit 1999](#)) and social processes (e.g., [Bieler and Morton 2008](#)) that give rise to observed entities and patterns. The definition and role of theory thus vary directly with these two different philosophical ontologies.

The transfactual/phenomenal distinction can also shed some light on debates within the monist camp. Monism plus phenomenism gives us an analytical or instrumental view of theory, where the role of a good theory is to capture important dynamics rather than to exhaustively or exactly mirror the world; this stance is characteristic of rational-choice theory and Waltzian structural realism, and also of much of the recent “pragmatist” literature in IR ([Bellamy 2002; Festenstein 2002](#)). Not going beyond experience, and in particular not dignifying one’s theoretical assumptions with the ontological status of “real,” produces a view of theory akin to Max Weber’s notion of an ideal-type: a deliberately oversimplified model the value of which is in its use, not in its correspondence with anything. In fact, the brief analysis of vocational orientations that I conducted above is an example of what the application of ideal-typical theory looks like; the value of that analysis is whether it gives us any insight into the dynamics of knowledge-production in the field of IR, and not whether it accurately describes or mirrors anything. Rational-choice theory and structural realist theory, along with pragmatic theories like practice theory ([Pouliot 2008](#)) and field theory ([Guzzini 2000](#)), despite their technical differences, are on the same page when it comes to methodology – it would be as inappropriate to “test” a rational actor presumption as it would be to “test” the presumption that social action arises from a

practical context.

The remaining combination – monism plus transfactualism – has the least presence in IR scholarship, although the volume of this literature appears to be growing. The basic idea driving such a stance would be that even though there is no firm mind/world split, it is still possible to go beyond experience to achieve knowledge of actually existing broader or deeper structures of social action. Such a stance would appear to be characteristic of psychoanalytic work, especially when that work is conjoined with a feminist sensibility about the performance of identities (Weber 1998; Tickner 1997), or a postcolonial sensibility about the continued presence of historical circumstances in the present (Inayatullah and Blaney 2004). Under such conditions, the role of theory would be something more than a useful oversimplification, but something other than a picture of a mind-independent reality – theorizing would be, rather, part of the process of bringing trauma to light and beginning to confront it. This process would also be reflexive, inasmuch as the trauma thus revealed would be as much the knower’s own trauma as anything characteristic of the population under study. Along these lines, Judith Butler suggests that the role of theory is to make us aware of our vulnerabilities – both as individuals and as collectivities – and help us to recognize that “we are not separate identities in the struggle for recognition but are already involved in a reciprocal exchange, an exchange that dislocates us from our positions, our subject-positions, and allows us to see that community itself requires the recognition that we are all, in different ways, striving for recognition” (2006:44). Moments of widespread disaster, such as the 9/11 terrorist attacks, provide an occasion for such reflection – an occasion that has to be seized by the theorist in order to be actualized. This is radically different from the definition and function of theory upheld by other philosophical ontologies, but is explicable as a combination of transfactual and monist commitments.

Putting these two dichotomies – monist/dualist and phenomenal/transfactual –together generates the 2 × 2 table shown in Figure 2.

		Relationship between knowledge and observation	
		Phenomenalism	Transfactualism
Relationship between the knower and the known	Mind/world dualism	neopositivism (hypothesis testing)	critical realism
	Mind/world monism	analyticism (rational choice, pragmatism)	reflexivity (feminism, postcolonialism)

Figure 2

Viewed in this way, surprising affinities appear between the different philosophical ontologies. For instance: the similarity between neopositivism and analyticism on the limitation of knowledge to the realm of experience helps to explain their shared skepticism about the trans-experiential notions found in critical realist and reflexive theories, and the dismissal of those notions as ideology or politics or both. Likewise, the similarity between critical realist and reflexive theories in embracing trans-experiential notions helps to explain their shared criticism of the neopositivist and analyticist reluctance to go beyond what is in order to elucidate hidden potentials implicit in the present. Neither neopositivist nor analytical social science has as much of a “critical” dimension in the sense of going beyond the present to explicitly articulate empirically plausible alternate futures (Wendt 1987:370), as they are generally more concerned with explaining and understanding what exists instead of directly disclosing another way that things could be.

Contra Robert Cox, however, this does not make all phenomenalist theories “problem-solving” theories. Cox (1996:88-90) is deliberately ambiguous when he suggests that the alternative to taking “institutions and social power relations for granted” is to “call them into question,” since there are at least two different meanings of this latter injunction. One is to question how those relations came about, and the other is to question whether or not we ought to continue to have those relations at all. For phenomenalists these are distinct intellectual operations, with the separation between them underpinned by something like Weber’s fact/value distinction properly understood (Jackson and Kaufman 2007); for transfactualists, the fact/value dichotomy can be bridged or even abrogated through the creation of adequate knowledge (e.g., Bhaskar 1998:62-5). This debate happens

periodically in the field, but the different positions are not particularly clearly stated; an analytic of philosophical ontologies can help to clarify what is going on at such moments.

One could repeat this exercise with other combinations of positions, and (for example) take critical realists to task for conflating the neopositivists and reflexive poststructuralists under a generic “anti-realist” label (as in [Patomäki and Wight 2000:216–19](#)); while both camps reject aspects of the critical realist position, they each reject a *different* aspect, while sharing the other aspect in common with critical realism. The only complete rejection of critical realism belongs to the analyticists, and hence if any perspective is going to be labeled “anti-realist” it should be only the lower left-hand box. Space prevents me from teasing out all of the implications of the alliances and oppositions signaled by the dichotomies I have proposed, so I will leave that as an exercise for the reader – and perhaps for the field as a whole in the future.

Conclusion: Engaged Pluralism

Throughout this discussion I have steadfastly avoided doing two things: providing a single and concise definition of “theory,” and attempting to adjudicate between the various claims I have explicated. These two avoidances are quite deliberate, inasmuch as I have no wish to legislate the meaning of “theory” for the field as a whole, and inasmuch as a plurality of definitions obtains in the field at present and arguably has for generations. Rather, I have sought to bring some order to the diversity by suggesting a pair of analytically autonomous but practically intersecting typologies for organizing debates and discussions about “theory” in the field. The typology based on vocational orientations is an effort to retain some of the insights of the literature on the relationship between academic scholarship and policy-making activity, without falling into facile “theory vs. practice” distinctions. In so doing it directs our attention to the diversity of ways that “theory” functions both inside and outside of the academy. The typology of philosophical ontologies is an effort to group recent debates about the philosophy of science and its implications for concrete IR scholarship into meaningful categories. In so doing it directs our attention to the diversity of ways that “theory” functions in academic debates. In both cases, the aim is not to resolve this diversity into some kind of synthetic unity, but to sharpen and focus our debates such that we might disagree about the proper things – “proper” in the sense of carrying actual implications for how we do IR.

I am certain that this (non-)resolution of the various debates I have sketched and analytically reordered will frustrate some readers, who might be more interested in hearing that the field has achieved some sort of consensus about theory, theorists, and theorizing. But this is simply not the case. While relative consensus reigns within certain groups of scholars, or in some cases within certain subfields, when we look at the field as a whole the situation is much different. IR academicians in general do not agree on what theory is and what it is for, and academicians and policy makers have a divergence of views that runs orthogonal to the academic debates. Under such circumstances, I think that the best response is something like the “engaged pluralism” that [Yosef Lapid \(2003\)](#) calls for. This is not a silent tolerance of other points of view operationalized mainly in ignoring those authors and claims with which one disagrees, but is instead a serious effort to construct dialogues across fundamental differences. The goal here is not the idealistic hope of achieving a unified consensus, but the perhaps more plausible aim of clarifying similarities and differences by continually placing them into dialogue with alternatives. In the end, the most significant thing about the IR field's approach to theory may be that it hasn't settled those fundamental questions, and doesn't appear to be about to do so; this makes the field of IR one of the best places to *have* those discussions, and thus keep the questions and the questioning attitude toward “theory” alive.

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Online Resources

Global Security Matrix. At www.watsoninstitute.org/globalsecuritymatrix/, accessed August 20, 2009. Features efforts by IR theorists to identify and conceptualize security threats; draws on a diverse set of theoretical perspectives.

Theory Talks. At www.theory-talks.org/, accessed August 20, 2009. An archive of interviews and speeches by prominent IR theorists. Covers a variety of approaches.

The IR Theory Website. At www.irththeory.com/, accessed August 20, 2009. Wiki-like collection of thumbnail definitions of different IR theories. Focused mainly on scholarly accounts, and on theories about particular phenomena.

Institute for the Theory and Practice of International Relations. At <http://irththeoryandpractice.wm.edu/>, accessed August 20, 2009. A set of materials trying to bridge gaps between scholarship and policy making, and also a comprehensive set of comparative evaluations of academic IR programs.

International Relations Theory wikipedia entry. At http://en.wikipedia.org/wiki/International_relations_theory, accessed August 20, 2009. Even if you don't go here, your students will – so if something is lacking on this page, update it.

International Relations Theory. At <http://internationalrelationst...stheory.googlepages.com/index.htm>, accessed August 20, 2009. Another wiki-like archive of various approaches in the scholarly field.

IR Rumor Mill. At <http://irrumormill.blogspot.com/>, accessed August 20, 2009. The closest thing to a comprehensive real-time account of hiring in academic positions in International Relations. US-focused.

The Duck of Minerva. At <http://duckofminerva.blogspot.com/>, accessed August 20, 2009. An ongoing set of efforts to apply the insights of IR theory to current events.

About the Author

Patrick Thaddeus Jackson is currently Associate Professor of International Relations in the School of International Service at the American University in Washington, DC; he is also Director of General Education for the university. He previously taught at Columbia University and New York University. He received his PhD in Political Science from Columbia University in 2001. In 2003–4, he served as President of the International Studies Association–Northeast. He is presently the Editor-in-Chief of the *Journal of International Relations and Development*.

Jackson's research interests include culture and agency, international relations theory (particularly the intersection of realism and constructivism), sociological methodology, the role of rhetoric in public life, the concept of 'Western Civilization', and the political and social theory of Max Weber. He teaches courses on IR theory, political philosophy, research methodology, the philosophy of social science, and the intersection of popular culture and international politics. He has three times received university awards for the innovative use of technology in the classroom, and was the AU Honors Program's Faculty Member of the Year for 2004–5.

Among his recent publications are "Bridging the Gap: Towards a Realist–Constructivist Dialogue" (forum editor and contributor), *International Studies Review* 6:2 (2004), 337–52; "Glocal Hero: Harry Potter Abroad" (co-authored with Peter Mandaville), in *Harry Potter and International Relations*, ed. Daniel H. Nexon and Iver B. Neumann (2006); "Twisting Tongues and Twisting Arms: The Power of Political Rhetoric" (co-authored with Ronald R. Krebs), *European Journal of International Relations* 13 (1) (2007), 35–66; and "Foregrounding Ontology: Dualism, Monism, and IR Theory," *Review of International Studies* 34 (1) (2008), 129–53. His book *Civilizing the Enemy: German Reconstruction and the Invention of the West* was published in 2006, and he is the co-editor of *Civilizational Identity: The Production and Reproduction of 'Civilizations' in International Relations* (with Martin Hall; 2007). His book on the philosophy of science and its implications for IR scholarship will be published in 2010.

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