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THEORY TALK #44

PATRICK JACKSON ON IR AS A SCIENCE, IR AS A VOCATION, AND IR AS A HARD BOARD

Theory Talks

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PATRICK JACKSON ABOUT IR AS A SCIENCE, IR AS A VOCATION, AND IR AS A HARD BOARD



IR is a scientific discipline, but all too often the 'science' in IR is used to discipline. As a result, many less orthodox approaches to inquiry in IR are—especially in the United States—relayed to the sidelines of the field. Mainstream is separated from 'the rest' by invoking criteria of scientificity borrowed from such philosophers of science as Thomas Kuhn and Imre Lakatos. Patrick Jackson takes a stake with this tendency by engaging heads-on the philosophy of science underpinning the conduct of inquiry in IR. In this *Talk*, he—amongst others—discusses the scientificity of IR, explains how seemingly incommensurable conceptions of science in IR can still talk, and how hard a board the vocation of IR should be.

What, according to you, is the central challenge or principle debate within IR and what would be your position within this debate or towards that challenge?

There are two answers one can give to this question. First, there's the empirical observation: what do I think the actual debate is at the moment? And then, second, the question 'What do I think the debate actually should be?' And given that the philosophy of science stuff that I've been doing is not exactly the central debate, most of my thinking has been about the second answer to that question, particularly since I think that talking more about what we're not talking about—philosophy of science, methodology broadly understood—will make that first discussion richer and better. In terms of what I think the actual debate is right now, I think the big question in IR at the moment is what makes IR a field? I think that the field has gone in so many different directions, there are so many research topics and so many theoretical controversies, it's been a lot harder for people to figure out what makes something 'IR.' And I've noticed this as a book series editor and as a journal editor, as well as just a scholar reading other scholars' papers. I will often see people saying 'well, that's not IR,' but not being very well able to articulate *why*.

So the question of exactly how the field is bound together is, I think, a really significant one. Part of the problem is that the question is not being debated within certain sub-fields and academic cabals, and instead we're getting unsupported assertions. In particular, you don't get those sort of discussions in more mainstream U.S journals because they are much more interested in just doing technical exercises. And that's fine, but they're not posing the bigger question, which is kind of unfortunate.

The two examples that suggest themselves to me most strongly are, on the one hand, you have the school of [Open Economy Politics](#), you have this sort of rationalist study of international and

domestic political economy, and in the recent special issue of [RIPE](#) they talk about this as the ‘American School of IPE.’ What’s interesting there is that if you look through a lot of that work, you get not only a lot of substantive claims about the economy and the relationship between economics and politics, but you get, even though it’s not normally discussed as such, a very strong commitment to a particular way of studying political economy that involves formalization, quantification where possible, hypothesis testing, a preference for large nomothetic generalizations rather than case-specific configurations—even though that is not logically entailed by an approach to political economy that privileges the self-interested action of more or less rational actors. But the two almost always go together as if they somehow were conjoined, and I find that kind of puzzling. The other example that strikes me is the interesting fact that ‘qualitative methods’ folks in IR are also largely neoclassical realists, as if there was something about a certain kind of neoclassical realism that entailed small-n case studies—and I find that equally bizarre. Not that small-n case studies and neoclassical realism are somehow incompatible, but that they would be so necessarily joined together that somehow the only way to study the state interactions from the neoclassical realist perspective would be to conduct several detailed qualitative case studies...those links don’t often get thematized; they don’t get talked about much.

And so we end up with the idea, and I especially see it among graduate students, if I’m interested in subject X, I have to study it in this way. And I just don’t buy that. Because philosophically I don’t think there is that there is an inevitable and necessary logical connection between object of study and methodological way of studying it. And in fact a lot of the interesting scholarly innovations that we’ve had (not just in IR, but in the sciences in general!) come when people combine theory and methodology in novel ways. But because we don’t talk about this, we end up with a field that is subdivided by topics as if somehow everybody talking about a particular topic had to talk about it in a particular way. It really cuts down the variety of perspectives that we can bring to bear on problems, and it cuts down on the kind of insight that we as a scholarly field can generate, I think.

So I think that’s really the big question: what holds IR together as a field of study? The subordinate question to that—if we have some sense of IR is what is, what are we supposed to be as IR scholars?—isn’t one that we can really ask unless and until we talk more about the relationship between theory and methodology. One of my colleagues recently asked how many articles had there been in *International Organization* in the last several years about the global financial crisis, and he couldn’t find a single one. I’m not saying that’s necessarily a bad thing, but that’s a question—should we be interested in this or not? In the work some colleagues and I have done on pop culture and IR, the question is always, this is a big deal globally, Harry Potter sells lots of copies, is this something IR should be interested in? We perpetually wonder about the limits of IR as a field, but we don’t have good or defensible answers.

So that’s kind of the really big question at the moment, which is what does it mean to be doing IR. But that’s the empirical observation about what we are talking about, not my sense of what we should be talking about and how we should be talking about it. And not surprisingly, given the book I just wrote (*The Conduct of Inquiry in IR*), I think we ought to be having a lot more discussions about how we study things, a lot more discussions about methodology, because I feel

like a lot of the time what we do when we're studying world politics is conflate our methodology with our substantive theory in ways that make very little philosophical sense. So what I really think we should be doing is talking about what it *means* to study things like security or political economy. And that is a debate that's not actually being conducted; the reason I wrote the book was to foreground that debate to maybe get a start on it.

So, in short, the debate that I think should be the most prominent is a debate about methodology, a debate about diversity, a more focused and philosophically rich debate about what it means to do IR and what it means to be doing IR. That debate is starting to become more common outside the United States. Within the US, there is an increasing emphasis within IR graduate programs just on kind of technical details of how one does certain kinds of research, especially hypothesis-testing. And so this big question of what exactly is IR is not being as profoundly asked, I think, in the United States as it is elsewhere.

How did you arrive at where you currently are in your thinking about IR?

The hard thing about answering this question is that I'm a relatively middle-class white guy from the United States; I don't have some kind of extraordinary experience fueling my interest... It's an old insight about the United States, if you're a citizen of the US, you can kind of ignore the rest of the world—it's the privilege of empire or hegemony. So international relations never really had a direct impact on me growing up; the rest of the world was simply out there some place, or it was the place that foreign exchange students and British sci-fi shows like *Doctor Who* came from. Living in the US, you don't have to confront the world in quite the same way, particularly not during the time I was growing up, the '70s and '80s; you didn't necessarily have Cold War drills where you're hiding under the desk in case of a nuclear assault, so IR was a distant phenomenon. I didn't start out with world politics; world politics was kind of a consequence of what I became interested in, which was really diversity of knowledge claims and the encounter with the idea that different people and different groups of people know things differently. And some of that for me came from just seeing the variations *within* the United States which, from the outside, look all the same. But within the US, there's a difference between being on the East Coast and being in the Midwest. This struck me as weird! Different things sort of are true in these different places—not necessarily that they contradict each other but it raises the question of translation in interesting ways.

Probably the thing that cemented me in doing IR the most was the fact that while an undergraduate I was a research assistant for a political theory symposium; it was an outfit called the '[Symposium on Science, Reason, and Modern Democracy](#)', run by very [Straussian](#) political theorists, and the year that I was the research assistant for it was the year that they were doing an entire sequence on the 'End of History' debate sparked by the famous—or infamous—Fukuyama [article](#). And so Fukuyama came in to inaugurate the series and then other people came in, Joseph Cropsey, Richard Rorty, and this guy from Harvard by the name of Sam Huntington. And I had not read any of Huntington's stuff at this point and what he presented was the working version of what was going to be the Foreign Affairs piece, 'The Clash of Civilizations?' ([pdf](#)) And I remember listening to him present this and then talking to him about it afterwards and thinking two things simultaneously: On the one hand 'This is wacky! How can anybody

possibly argue that there is one unified western way to do things, or a single Confucian one! But then the other thing that occurred to me was, ‘wow, there’s something really interesting about this.’ Not about his formulation, but the fundamental differences which are not as clear-cut as he makes them out to be in the article or in the book...but there *is* a sort of Wittgensteinian ‘family resemblance’ between things that he puts together in civilizational terms and there’s something profound about that. There’s something fundamental about those issues he identified and how they cut across these state boundaries and other kinds of political arrangements. And I thought, he’s put his finger on something, but I didn’t like his solution at all.

And the other thing that I really liked about what Huntington was doing was that it was broad, it had policy implications—but it wasn’t about that: it was about how we understand the world and how we deal with the fact that, at some level, it’s not all made up of people who see things the same way. In a way, a lot of what I did in my career for the next 10-15 years was to try to figure out a better answer to Huntington’s challenge. My first book is all about the social construction of the notion of Western Civilization and I take apart the idea that civilizations are all discrete. And for me, that’s kind of my backdoor entry into IR. I was never personally concerned with specific substantive issues; for me, IR is a domain in which I can explore certain kinds of philosophical and intellectual issues.

Besides that, I have to mention that the work of Max Weber really had an impact on my understanding of IR. And it’s interesting because my first uses of Weber were actually not as a theorist but as a primary source because I was interested in ways in which different people had conceptualized western civilization and Weber seemed to be one of the important figures in this. But the more I grappled with this formulation, the economic ethics of the world religions and a couple of the different versions of the ‘protestant ethic’ argument, the more I was impressed I was with Weber’s methodological style of singular causal analysis, of trying to explicate case-specific historical configurations rather than being so concerned with nomothetic generalization. And Weber’s way of posing the distinction between science and politics. So when I re-read Weber’s vocation essays in this light, they just kind of floored me. I was like, ‘Wow, someone understood this stuff about the different kinds of callings and the way in which knowledge and practice intersect, and understood this a hundred years ago.’ So I can go back and re-read Weber’s vocation essays and have many, many times.

In addition to Weber, I would say, there’s a little book by a discursive psychologist named John Shotter called *Cultural Politics of Everyday Life*; it was a book I read in grad school when I was kind of struggling to figure out some way to express myself—I was interested in post-structural critique of the Ashley/Walker variety, but I wasn’t happy about either the full version of that critique or Wendt’s ([Theory Talk #3](#)) sublimation of that critique into something that was a little more normal science-y.... And then I read Shotter, and Shotter’s way of talking about the problem of social order and social meaning just absolutely grabbed me. And from Shotter, I went on to read more [MacIntyre](#) and Wittgenstein, and they became very important for my way of conceptualizing stuff. It wasn’t until later on that I discovered that Onuf and Kratochwil and Alker were there already, so by the time I really encountered their work it was more like finding unexpected allies than anything else.

In your opinion, what would a student need in order to become a specialist in IR?

Well, I think the first thing they would need would be about scholarship and about academia, because the advice that I always give my students is the same that my wife got when she entered seminary: if you will be happy doing something else, then go do it. Because the extrinsic rewards of being an academic compared to other fields of human endeavor are somewhat ephemeral. We're not particularly well-paid for our level of education, the work conditions are not as good as they could be in many ways, the workload is odd, you know Weber has that allusion about politics being the strong and slow boring of hard boards and I think scholarship is the even stronger and slower boring of even harder boards! Because you can work all day, all week, all month on something and not get anywhere. I remember Nick Onuf mentioning to me at one point: wash the dishes every evening, because then you can at least say you accomplished something that day. Which I think is great advice! Because I think you need, in order to be able to do this kind of work, that temperament of the longer term, of not being so wrapped up with immediate gratification, because, let's face it, most of the immediate feedback is negative, because you get things rejected. We're trained as academics, as scholars, as disciplinary intellectuals, we're trained to take each other's arguments apart so most of the exchanges we have are 'let's point out flaws in other people's claims.' So you really have to focus on the longer term, on the broader horizon. And you have to be okay with that. That's just not for everyone, and I think the most important prerequisite for being able to survive doing this is that kind of academic vocation—and I use the word 'vocation' very deliberately. This is that argument that Weber makes ([Pdf](#)), this is the kind of calling that kind of comes on you, and you discover it in yourself. And the parallel to the seminary education is, I think, quite appropriate because seminary is also not for everyone either. And there's no shame in it, it's not a good or a bad thing, it's just—not everybody is content to do these kinds of things, so I think that's the first thing that one has to grapple with.

But on a more positive, or perhaps less negative, note, I think that probably the most important thing for a budding IR scholar is to look more broadly than whatever the methodological tradition of your own context is: realize that there are these places like China and the IR there doesn't look like the way you do IR. My colleague Amitav Acharya in one of the earlier *Theory Talks* ([#42](#)) talked about this quite a bit. Although, I think he's placed more on the substantive diversity of IR across different kinds of politics, whereas for me it's more about the methodological diversity of what truth is. But I think that these are certainly complementary positions. So I think that what one needs to do to get the intellectual background for this kind of IR is read more broadly, read broadly in history, in sociology and social theory – and be familiar with, at least, the major debates and philosophies of science and what we do and what the implications of that are for how we ought to communicate claims that appear to be contradictory or at least in tension to each other. That's important stuff.

And then I guess the other thing I would say, if I were designing my ideal PhD training program, I would make sure that people go to actual conferences. There's nothing quite like the dislocating experience of a conference; the first conference you go to outside your local or national setting, everything is different. Particularly a conference in another country. Even just the basic, basic things like how registration works and how the conference itself is organized is not quite the same thing as you are used to and that's really interesting. You go, 'Wow! Wait a minute! I'm in a

different place now and this is very strange!’ That feeling of being a little bit outside of one’s self is where I think the interesting scholarly and intellectual insights actually come from. If you’re too comfortable, you’re just reproducing stuff. You don’t want to be comfortable; you want to be challenged.

I think that this existential insecurity is particular for IR, because other fields of scholarly inquiry have a much clearer sense of their own self-identity and their differentiation from other things. The puzzle or the challenge that’s posed in being in those fields is more about dialoguing with an accepted canon or about filling in gaps in existing sets of explanations—it’s a different kind of scientific work where you can do sociology, you can do anthropology without incessantly worrying about what sociology and anthropology are. You can do economics without incessantly worrying about what economics is. It’s a lot harder to do that in IR. It’s interesting because in U.S. Political Science, you can sort of do political science without worrying too much about what political science is as long as you’re basically doing American politics, you’re doing studies of electoral returns and more or less rationalist models of interest bargaining and so on. And then you can basically bracket these sorts of field identity questions. But when you deal with IR, you can’t really ignore it too long because IR purports to be global and it purports to be in that sense universal, so that means there are, by definition, a huge variety of voices. And IR at least globally purports to be interdisciplinary, and that opens an much larger can of worms where other kinds of ways of thinking about things can show up. So I think in order to be able to survive in IR, one also needs to be comfortable with that kind of existential messiness and realize that whatever kind of solution you come to is probably not going to be universally shared by other people who call themselves IR scholars—and you’re probably going to have this discussion of what IR is for the rest of your scholarly career.

Yet for someone interested in theory, this inherent messiness has a positive side as well. One of the things that’s interesting about IR is that so many of the fundamental issues are much closer to the surface; we are still pondering and we are still leaving them open. It’s a lot harder to go into physics and biology and say ‘What is science, anyway?’ because that’s not really a live question. But you go into IR and ‘should we be studying the world scientifically?’ is a very easy question to pose because that’s actually really what [William James](#) might call a live concern, that’s actually something that’s really available to us. And that’s a great benefit to be able to engage those kinds of issues. The other great benefit is the fact that, because IR has a set of claims about being global, about being open to the whole world, about basically encompassing everything within itself to some extent, it allows the possibility of thinking some of these big questions that other fields might have a hard time grappling with. So where else can one think about humanity as a whole? Where else can one think about big questions about history and the way in which global politics works itself out, or politics has global effects? There’s not really a lot of other places you can grapple with these really big questions. And I think that’s a huge benefit. The challenge is to hold open the space for that and avoid being dragged into micro-level policy commentary and forsake the soul of what it means to be an IR scholar. So there’s a temptation to forsake that particular calling, and we have to be vigilant in not giving into that temptation.

What separates good IR theory from bad IR theory?

Oh boy, that's a hard one, and it's particularly hard because I think the answer varies an awful lot based on how one tries to cash out what theory means—and since that's not a settled question within IR, there are a plurality of different answers based on what we think theory is for, what we think theory is supposed to do. But let me back off that for a second to answer your question sort of obliquely. The kind of IR theory that I like, the kind of IR theory that speaks to me, is IR theory that has a real sense of the tragic gap between what is intellectually pure on the one hand and what is practically possible and enacted on the other.

I think of politics as a concrete struggle in which actors try out or situate themselves between different types of forces. That to me is what we should have, that to me is what IR theory sort of should be—to try to foreground and inform these sorts of configurational studies of how provisional solutions to thorny perennial questions get worked out in practice. The rather facile generalizations that sometimes pass for IR theory bore me; I'm not too sure what the point of yet another correlation is. I think what's interesting in IR, in politics, is not the approximate covariation between factors, but the very interesting and unique ways that actors find solutions to things; the way they kind of muddle through and shape and structure—maybe 'envison'—their worlds.

So I guess that leads into the other thing that I look for in a piece of IR theory: that it leaves space for human creativity and agency. So theory that is too deterministic sort of depresses me, and it depresses me in a kind of Nietzschean way. I am referring here to that line of Nietzsche's that if there were gods, there would be no freedom... What Nietzsche tries to get at there, is the insight that when we have something completely solid and irrevocable that limited what people could do even in principle, concepts like 'agency' and 'creativity' would be very problematic, because the fact that there was one limit would spill into others and you would have a situation where you would just have a deterministic set of social relations and that just does not strike me as a particularly useful way to think about things.

And so Nietzsche's understanding of apparent generality as being imposed by a sort of epistemic will to power captures much better what's going on with a lot of that stuff. And that's not a project that I'm particularly interested in... But when I first read Nietzsche, I thought: good theory is leaving space for that kind of creative agency, instead of trying to establish fixed constraints—which is not to say that good theory is indeterminate, but that good theory is about disclosing historical possibilities in their specificity instead of being about erecting untransgressable barriers. And theory that doesn't do that, regardless of the methodological style in which it's articulated in, strikes me as problematic.

I think that even a cursory examination of work in the philosophy of science and the philosophy of inquiry suffices to demonstrate that there are multiple different kinds of ways of producing knowledge; and different ways have different standards. There's no universal answer to what constitutes good theory or good IR scholarship except for certain formal criteria which are the ones that I build into my broad definition of science, things like internal consistency and rigorous connections between premises and conclusions. The idea that a scientific claim has to be public, that it has to be something that not just my own internal introspection, it has to actually be phrased in a way that is common to a relevant group so then that group can engage with it and critique it hopefully for the purpose of improving it in some way. And then science has to be

worldly in the specific sense that it has to be about the world, not about things that are kind of trans-worldly and only accessible through contemplation or revelation or something similar. Which is not to say that those things don't exist and that they are not important—but, you know, the contemplation of higher planes of existence is not really the same thing as trying to make claims about empirical cases of stuff that is, in some sense, measurable or able to be tangibly grasped. (I'm trying to avoid the word 'observable' here because there's such a weird ambiguity about the world 'observable ... critical realists claim there are unobservables, but when you try to break that down they're actually conflating several different meanings of what it means to observe. The critical realist claim is true for certain kinds of things that are not observable in principle; it's not true for things that are not observable because we haven't seen them yet. There's no special epistemic problem presented by a planet that's too far away to be seen with the naked eye that can be seen with a telescope; you don't need a deep theory of unobservable deep structures to talk about that, you have to just say we didn't have the right tools yet.)

I think that what we end up with is a variety of different kinds of work. Each kind has its own set of criteria for what makes it good. I think that that kind of internal validity is absolutely essential for any kind of social science, but at the same time, you also have to accept the inevitable diversity of claims: they don't all necessarily go together. I don't think those claims would contradict each other as much as people schooled in the badly misleading version of Thomas Kuhn we are familiar with in IR would suggest, so I just don't understand that claim that there's no way to communicate between perspectives. [Donald Davidson](#) made this argument a long time ago: there's no such thing as a frame of reference in a loose conversational sense because that conversational sense of a conceptual frame of reference is that you have a set of concepts, and somehow they're self-contained. So my set of concepts exhaustively defines my world and your set of concepts exhaustively defines yours and there's no overlap. This is bad Kuhn. I have to point it out because it's kind of silly, and the reason this is silly is because, if I can make a statement in my framework which is well-verified within my frame, and if I can translate those terms into your frame of reference, the claim is still going to be well-verified in your frame of reference. Two scheckels plus two scheckels is still four scheckels, even if you're not a Babylonian. As long as we can translate, it's fine. Where things get interesting is when you have untranslatable; when you have ways of worlding that are so different from one another that I can't take what you've said and re-code it in terms that make sense to me.

This is what Thomas Kuhn's later work is *actually* all about. He threw out the idea that what he called paradigms were hermetically sealed. But if you actually look back through the history of science, you find these moments of untranslatability (strictly speaking) and zeroing in on those moments is insightful: Aristotle uses the word 'motion' in a very different way than modern physicists use 'motion,' which doesn't mean you can't understand Aristotle, but you can't understand Aristotle in terms of modern physics... Does that mean in Aristotle's physics you can fly and in modern physics you can't? No. It just means that there are statements that you can make in one approach that you cannot make in others, which doesn't mean such statements are false out of their context, but that they are meaningless out of that context.

I think that happens in the study of IR sometimes: you look at the debates between neo-positivist and critical feminist security scholars about security issues and often those debates look to me to revolve around fundamentally different understandings of different concepts and terms, not that

they're contradicting each other, but that there's a certain incomprehensibility. A fundamental 'Wow, that statement you just made doesn't make any sense to me.' And those are the best and most valuable moments because if I as a scholar wanted push the limits of knowledge I need to look for a moment where a piece of work is well-valued within a different research tradition but I can't make heads or tails of it... well, that's fascinating! I think if we're really precise about what the different standards are, that's what gives us as a scholarly community the ability to really zero in on those moments, those points of tension. There's a way in which the whole scholarly field, organized by having diverse modes of inquiry and everybody equally sharp about what their precise understandings of validity are, allows us as a whole to disclose those moments, those sticking points, those kinds of fundamental dilemmatic commonplaces that we keep wrestling with but we don't come to a firm or final understanding of.

And that's, I think, where some of the most interesting scholarship comes from, is the attempt to translate, knowing you're going to translate badly, to fail in the attempt to translate perfectly and therefore gain insight (a perfect translation, after all, would just preserve the original insight, not add to it). Paul Feyerabend commented that you need 'disproven theories' floating around because otherwise, how are you going to come up with discrepant evidence against which other things can be evaluated? You need to have this mass of discredited stuff. I think that's true. So I guess—it's a long answer—but my sort of final answer is to what makes a really good theory is theory that acknowledges the limitation of its own standards and tries to push beyond those standards without losing sight of the demand for internal rigor. I think that's the most interesting work, to play around in that tension and say 'What sense can I make of this' without throwing your hands up and saying 'there are no standards.' Trying to mediate between the tension between those two claims is what makes for interesting theory, in IR or otherwise.

In your latest book, you argue in your context of inquiry in IR, you argue that the claim of scientificity has a disciplining function. What do you mean by that?

That kind of disciplining function, which is wrapped up with the word 'Science', is... It's interesting, we see it very rarely in print, which is why I think that the *ISQ* exchange between Tickner and Keohane ([Theory Talk #9](#)) is such a good source for this (read Tickner's response to Keohane [here](#), pdf). Where you more often see it is at the level of dissertation committees and article reviews when articles will come back and the reviewer will say 'This isn't science' or 'If you want to make a scientific claim, you have to...' and then promptly elaborate a whole variety of things about hypothesis testing. So the term 'science' gets used as a disciplinary commonplace in those situations a lot more often. And it will show up in mission statements and in statements of editorial intent. But I think where it gets mobilized is exactly these moments where in effect, what is being said is, 'The only way I know how to engage with you is if you do things on my terms. And if you don't do things on my terms, I don't have to acknowledge what you're doing as a legitimate enterprise.' And it saddens me that that is way 'science' gets used—not just in IR, but in the social sciences more generally. Because the whole endeavor of doing science was precisely to be an antidote to 'You have to do it this way' due to certain power structures or certain types of traditional standards or whatever, and saying no, we're not going to do it that way! And the closing up of those potentials by the really firm definition of what it means to be scientific is a

disciplinary move that I don't find particularly healthy. I don't think we should *ever* be using a term like 'science' as an excuse not to deal with somebody's work. You want to take somebody's work and say it has problems, please! Tell me what its problems are! But in the first instance, you should at least *read* it, you should try to look at it on its own terms, and say 'What is it actually trying to do? What is the epistemic origin for these claims?' It may not be your epistemic origin, but what is going on in this article? What are they trying to do? And then, did they do it well? And only afterwards can you ask the question, 'and is it useful that they did this? What's the value of this endeavor afterwards?'

I think this is in many ways what was at stake when Weber tried to re-articulate the notion of objectivity in [that famous 1904 piece](#) of his that obviously also gets so badly translated into English that people somehow presume that he's talking about objectivity in the classical sense, when he's not. Weber's really arguing in a lot of ways in that piece that there are ways in which we can appreciate the technical correctness of other arguments without buying their substantive core. So I have a set of value orientations and I turn them into an ideal-type and I go forth I use them to do something, but you don't have to buy my normative commitments in order to appreciate the value of my results.

I feel this a lot when I read Marxist work because I, quite honestly, I just don't buy the starting point of a lot of Marx class analysis stuff. Part of me not buying the starting point I'm sure has to do with the fact that I'm American and famously, there's no classes in America, and everybody in America's middle class. We can sociologize why the notion of class doesn't grab me, but it's sort of not relevant for the purposes of this example; the point is it doesn't. But I can still look at a good piece of class analysis—Van der Pijl's ([Theory Talk #23](#)) analysis of the trans-Atlantic bourgeoisie, for example—and say 'Wow! There's a neat set of insights here!' Then I look at that and go ok, I wouldn't cash it out that way - I'd probably talk about it more in terms of professional socialization networks and circulation; then I'd reach back to [C. Wright Mills](#) and okay, fine! Now I'm in a different place in thinking about this issue which isn't the same place that the Marxist analysis started, but those insights were only sparked for me if the analysis was good on its own terms, even though I don't buy its initial terms.

That to me is kind of what the endeavor of social science ought to be about: That we can say 'I don't think you should start there but given that you've started there, I think you did a good job, and that sparked me thinking about something that you may not necessarily agree with, but give me a minute to take it through my version and I can have an insight and we can sort of talk about the tensions or the possible complementarities between those different types of insights.' In many ways, what that requires us to think less in terms of persuading of other people and more in terms of generating insight about the world because I'm never going to persuade a committed Marxist, and I'm not going to try. What I'm going to say is, in this instance that you've done this, I don't understand what the analytical value is of the apparatus of class struggle and alienation that you've brought to bear on this problem. If you can explain that to me, that's cool and then I can understand your work, but I'm not going to convince you to give that up because that's part of what you are.

The great ethnographic insight is that in some sense, the researcher is the research instrument; it's not just true for people who travel to villages. People who do statistical work, who *really* do

statistical work, have a kind of inner feeling for the rectitude of that work. And if they didn't, then they wouldn't do it very well. Just because some methodology is enlightening to you doesn't mean it's going to be enlightening to me, and I object to the claim that there is no other way for me to generate any valid insight unless I do it your way. And I think that's the disciplining use of science, the vague use of science that I wrote the book against—so that we can say 'No, we can still be scientists.' And parenthetically, there are IR scholars that aren't scientists by my definition, and I don't think they should be run out of town, but I do think that there's a difference between social science and, say, normative critique of existing social institutions. Which I try to make a case for in the book.

The field of IR is often characterized as composed of a set of Kuhnian paradigms or like Lakatosian research programs (see [Theory Talk #32](#)). But that's a very common part of many of the social sciences and you take stake with that issue. Why?

As I describe in the book, there are two problems that I have. One of them is just a question of technical language. Both Kuhn and Lakatos have some pretty clear criteria in mind for what constitutes a research program or a paradigm; arguably Lakatos' research program criteria are more clearly spelled out than Kuhn's, because as I said Kuhn kind of changes his mind as his career goes on. But regardless, even in the first edition of [Structure of Scientific Revolutions](#), Kuhn still has pretty definite understandings of what a paradigm is; it's just that there are 30 of them or whatever it is... The problem is that none of those criteria actually apply to IR because you need a level of internal consistency and a level of everyday normal science practice that we don't have. Neither Kuhn nor Lakatos wrote anything designed to make an aspiring science more scientific. There's a reason their work is mostly about physics; it's taking an existing science that nobody really doubts the scientificity of or the value of or the insight gained through it, and coming up with transcendental conditions under which it works. Kuhn's puzzle in *SoSR* is not how do we be a science; his puzzle is, given the old linear account of progress doesn't seem to match up with the history of science, how the hell does science work? And then Lakatos's problem is, given that the old version of scientific progress doesn't work, but also given that Kuhn's version of things is oversimplified and causes its own problems, how do we simultaneously have internal diversity of ways of understanding and something like progress? So then you have this ingenious solution of Lakatos's involving retrospective reconstruction, which is great if you have successes to retroactively reconstruct rationally. The thing about IR (and as far as I know, the first person who made this claim publicly was [Fred Chernoff](#)) is that IR doesn't have this problem! We don't have outcomes that have to be explained in terms of scientific progress. In physics, airplanes fly and Skype works; in IR we don't have that. We don't have the equivalent of airplanes flying and computers working, so we don't need that apparatus. So I think that part of my objection to it is just on those technical grounds: this is a mis-application of what these guys were up to.

The other problem is the sociological function of paradigm-speak. Because of the way that Kuhn articulates the notion of paradigm and because of the way, especially in *SoSR* although not in his later work, he sometimes talks about paradigm shifts as incommensurable jumps between two inconsistent things, it opens itself up to a plausible but I think ultimately indefensible mis-reading which would say that to have two different paradigms is to not be able to speak to one another at

all. I don't think that's actually what Kuhn is saying; I think it's a lot more subtle than that, having to do with particular terms and contexts that he analyzes later in his career, but the sociological function in the field of IR of the notion of 'paradigm' does something very similar to what the notion of 'science' does when sort of deployed vaguely, which is to say 'I don't have to deal with what you're saying because it's different, and I'm doing what I'm doing and it's ok for me just to do what I do.' Ok, it's true, you don't have to deal with every single argument, but to say that everybody should live in their own little 'paradigmatic' boxes doesn't help anyone. But that's the way that Kuhn gets deployed a lot: as an excuse for ignoring counterarguments and inconvenient contrary evidence.

So I think my other objection (besides the technical/philosophical one) to the use of 'paradigm' and 'research program' language in IR is that it further sub-divides the field and relieves us of the awful burden of actually reading work that's outside of our comfort zone. If I can claim the high ground of 'I'm just doing normal science within my paradigm,' I can basically work in a vacuum. The people who say this, of course, tend to be neopositivists and can say things like 'I don't have to read *Alternatives*, I don't have to read *Millennium*. They don't do my paradigm! You can go and do your whatever-it-is over there but I won't read your stuff, and I sure as hell won't hire any of you into my department. It will disturb my students greatly if they start trying to do the kind of work that you people do.' So I think that the real problem—it's a real problem for students, because most graduate students entering graduate school don't have a clear enough sense of the intellectual landscape to know exactly what they're going to be disciplined into in what they choose to study—is that paradigm-speak insulates researchers from one another.

And my two objections are related. There's the philosophical problem, which is what I try to address in the book by demonstrating that no approach to the producing knowledge is so universally accepted that you would be on solid ground using that approach to knock out everything else and saying this is the only way to do things, particularly but not exclusively in a social science where we lack great accomplishments that have to be explained transcendentally. So nobody can claim the philosophical higher ground. This in turn means that it's not philosophically legitimate to wall ourselves off into our 'paradigm' secure in the notion that eventually we'll make scientific progress, which in turn means that this language may have a sociological and disciplining function but it doesn't have a philosophical leg to stand on. Maybe by surfacing it explicitly we can advance a discussion about these things.

I also think it's deeply problematic because the world faces a huge number of global challenges, and it's the height of arrogance to assume that a particular approach is going to be sufficient to meet those challenges, particularly when we're talking about something like the neopositivist analysis of inter-state inter-actions; that's a particular tool that's used for particular thing, and it would be a real tragedy if we cut ourselves off from another kind of tool that is out there because we had a diluted understanding or misunderstanding of 'paradigm' and said 'Oh, they're just working in their tradition, which I can safely ignore because it's not mine.'

Now, there's a caveat to this. There's certainly an awful lot of very good scholarship that ends up just working within its particular tradition and I don't want to say that that work is not valuable; it is. But the expectation that that should be the kind of work that we all aspire to is what reduces our scholarship—I can put it in more grandiose, almost theological terms, as I do in the

conclusion of the book, but I won't, I'll just put it in personal terms—it's *boring*. I'm struck by Robert Keohane talking a little bit about how he's looking for things that are more interesting. Robert Keohane, the central figure in academic IR for the past several decades. A lot of the really specific detail-oriented work that fleshes out of some particular hole of well-established way of doing things, that can be useful technical work, but it's not the most exciting stuff and it's ... to say that somehow *that's* the kind of work we should all be doing really means that it's a lot harder to generate any kind of knowledge that dialogues with the wider world in any way. So, I really worry that either the disciplining of 'science', vaguely understood, or the disciplining of 'my paradigm', is going to cut that stuff down.

When I teach, certainly when I teach undergraduates, I'll talk about 'isms', I'll talk about realism and liberalism and constructivism and feminism and post-colonialism, because it's a useful pedagogical move: you say 'Ok, here's some sort of core assumptions about what these arguments mean and there's a lot of stuff in the debate around the edges, but this is the basic stuff that people adhere to, albeit not perfectly.' But that's only appropriate for people's first entry into the dialogue. I don't know that that's the best way for us to organize our actual scholarship. Those kinds of simple theoretical typologies strike me as a pedagogical exercise that you pass through and say okay, this theoretical tradition is helpful, but there's no reason to stay over here in this box exclusively and if my insights pass over *there*, I have to go follow where the investigation leads. Weber has this great claim in 'Science as a Vocation' that to do science is 'to serve only the thing,' to only be true to the subject-matter itself. And it leads you to all sorts of directions and you have to trust yourself and go with that, no matter which kind of boundaries you end up traipsing over. The attempt to gate-keep and hold these things intact strikes me as really short-sighted: philosophically illegitimate, pedagogically harmful, and stifling to thinking.

So, to sum up: If IR is about real-world events out there, traditionally the relations between states, then why should we pay attention to philosophy?

Well, I think that the thing that philosophy does for us—and by 'us' I mean IR scholars broadly understand, those of us who are in some sense interested in global affairs—we're interested in producing knowledge of global affairs that is in some sense valid. I think that's a really important qualifier because there are lots of people that are interested in global affairs primarily so they can go out and change it. I have lots of students like this, who want to study (for example) what's going on in sub-Saharan Africa so they can go out and improve people's lives, which is excellent work and they should go do that, and if they do it well they'll make an excellent near-term impact. But if they're interested in knowing things and generating knowledge about global politics that is in some sense valid, that's another matter. A lot of things are packed into the phrase 'in some sense' because there's diversity in things can be valid. And I don't think this is what philosophers find useful in the philosophy of science. What the social sciences should find useful in the philosophy of science, or in philosophy in general, is that the exercise of elaborating the logical structures and the preconditions of the assumptions of particular modes of knowing can provide some useful clarity for those of us that are mostly engaged in our everyday work in grappling with the *stuff* of the social world. Philosophy allows you to pull back from that *stuff* a little bit, reflect on exactly what it is that you're doing. There's a way in which the study of

philosophy or the reading of philosophy can serve as a moment for methodological and theoretical reflection.

Now I know this is not what philosophers of science think they're doing, because they're not particularly interested in providing moments of reflection for IR scholars or other social scientists. Ok, fine, but we're not in philosophy, we're here in IR, so we have to just sort of operate from where we are. On that basis I think that it's useful to read philosophy—it's useful for any sort of social-scientific field, but it's *particularly* useful for IR to have that kind of moment of reflexivity—methodological reflection—precisely because in our very subject matter itself, which is global, there are diverse answers to those questions. This is not to say that we necessarily have to always adopt the perspective of people we study or to say that we have to ignore the perspective of people we're studying, but it's to say that we should need to probably confront the question of what we're doing when we make sense of the world and how it relates to what the people we're studying are doing in making sense of the world.

This is not a question that has to be posed quite as rigidly in other fields: most physicists do not spend an awful lot of time worrying about what quarks and gluons think they're doing. Some philosophers do: [Alfred North Whitehead](#) did, [Karen Barad](#) does in her feminist approach to physics. But for the most part, they don't do that, but we do; it's a more pressing issue for us.

[Richard Feynman](#) has a great line that goes something like 'Scientists need philosophy of science like birds need ornithologists' —and he's right. Perhaps scientists don't need to know the philosophical criteria of what they're doing, they just kind of need to do what they're doing. But the difference is, as far as we know, birds don't carry around a theory of ornithology in their heads or in their praxis. They might, but we don't know that. But we do however know that those of us in the social sciences *do* carry a philosophy of science either in our heads or our practices (arguably natural scientists do too, but that's material for a longer discussion elsewhere). More likely than not, given the way that the field works now where most people are not trained in philosophy of science, more likely than not we carry those things tacitly in our practices. And I think there's value to making those tacit assumptions explicit so we can reflect on them and say exactly what their value is.

But it is definitely the case, at least for me, I have no interest in turning IR into a field which is all about philosophy and philosophizing about world politics. We're social scientists, we're making claims about the world. I think philosophy is useful toward that end and helping us figure out what we mean by 'claims' 'about' 'the world', about all the terms of those claims about the world... and what it means to make a *good* claim. This is all too often talked about as if we're a nascent science, as though it's too bad we have to wrestle these issues all the time. Actually, I think it's a benefit: this stuff is closer to the surface, we haven't resolved it yet. We have the great privilege of getting to wrestle with these issues, unlike within the strains of academic philosophy in which primarily what you have to do is relate what you're doing to what other people wrote and compare footnotes to footnotes. We get to relate them to actual ongoing empirical pieces of research, which is pretty damn exciting! There, I think, that's one of the great benefits of doing IR: precisely the fact that those issues are closer to the surface. They're still live, possible things we get to grapple with. If I wanted to do philosophy, my career would have been very different.

But I wanted to use philosophy to inform an endeavor of knowledge-production, which makes IR a great place to dwell.

Patrick Thaddeus Jackson is currently Director of the General Education Program, and Associate Professor of International Relations in the School of International Service, at the American University in Washington, DC. He previously taught at Columbia University and New York University. He received his Ph.D. in Political Science from Columbia University in 2001. In 2003-4, he served as President of the International Studies Association-Northeast; in 2012-2013, he will do so again. He is presently Editor-in-Chief of the *Journal of International Relations and Development*, and Series Editor of the University of Michigan Press' book series *Configurations: Critical Studies of World Politics*.

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