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Presents

THEORY TALK #32

MIRIAM ELMAN ON LAKATOS VERSUS KUHN AND PROGRESS IN IR THEORY

Theory Talks

is an interactive forum for discussion of debates in International Relations with an emphasis of the underlying theoretical issues. By frequently inviting cutting-edge specialists in the field to elucidate their work and to explain current developments both in IR theory and real-world politics, *Theory Talks* aims to offer both scholars and students a comprehensive view of the field and its most important protagonists.

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MIRIAM ELMAN ON LAKATOS VERSUS KUHN AND PROGRESS IN IR THEORY



How do disciplines advance? Can one still speak of ‘progress’ in a field like IR, consisting of various, competing, explanatory frameworks? Miriam F. Elman has dedicated much of her time to discussing such questions specifically for IR. In this *Talk*, she – amongst others – contrasts the approaches of Thomas Kuhn and Imre Lakatos to progress; she challenges the view that progress in IR theory is primarily inhibited by ‘vested interests’; and she shows how (neo-)classical realism still has a lot to say.

What is, according to you, the biggest challenge / principal debate in current IR? What is your position or answer to this challenge / in this debate?

I’d like to start by saying what I think the principle debates in IR are NOT. The central debates are not whether rational choice or constructivism matters more; whether domestic politics trumps external constraints and opportunities in explaining international outcomes and foreign policy strategies; or whether Realism is or is not relevant in the post cold-war or 9/11 era. I think that these are unhelpful and uninteresting questions that do little to advance the state of the field or our understanding of past or contemporary international relations. Clearly, when we look at any given issue (for example, terrorism; the democratic peace phenomenon; nuclear proliferation), both rational choice and constructivist perspective offer valuable insights. There is no reason to have to choose among these approaches. Similarly, there is no need to pick between ‘second image’ and ‘third image’ variables. In fact, some of the most interesting work in IR today is being done by scholars who combine levels of analysis, looking at how the international impacts the domestic to produce distinct state responses to external challenges (see for example, Etel Solingen’s masterful book *Nuclear Logics: Contrasting Paths in East Asia and the Middle East*, Princeton University Press, 2007). Lastly, Realism remains a vibrant area of study in IR—indeed, classical realist thought is now experiencing a renaissance with the publication of multiple works by self-identified Realists such as William Wohlforth, Randall Schweller, and Christopher Layne (see Steven E. Lobell, Norrin M. Ripsman, and Jeffrey Taliaferro, *Neoclassical Realism, the State, and Foreign Policy*, Cambridge University Press, 2009).

How did you arrive at where you currently are in IR?

I was fortunate to have many terrific teachers and mentors as an undergraduate (at the Hebrew University in Jerusalem, Israel) and graduate student (at Columbia University), but I would say that the scholar that inspired me the most was Robert Jervis. Jervis was (and still is) one of the most important and interesting contemporary scholars writing on IR. Anything Jervis writes—absolutely everything!—is worth reading. His work is extraordinary in its richness, historical breadth, and ability to zero in on the key errors in judgment of both policymakers and theorists. His book, *Perceptions and Misperception in International Politics* (Princeton University Press, 1976), is one of the top 5 books ever published in the field, and a ‘must read’ for every student coming into the field.

Columbia University’s Ph.D. program gave me the tools and skills necessary to think critically about IR. My first publication was a letter to the editor, which was published in *International*

Security (co-authored with my spouse, Colin Elman). The letter originated from some comments that Colin Elman and I made at a seminar presentation by Paul Schroeder at Columbia's War and Peace Institute. Nothing would have ever come of that had Robert Jervis, Jack Snyder, Hendrik Spruyt, and David Baldwin not encouraged us to write our comments up and submit them to the journal. Columbia University's Political Science Department is a special place—nurturing yet demanding; intellectually stimulating yet self-consciously eschewing hand-holding. It has been 13 years since I left, and I have tried to recreate some of that atmosphere for my own graduate students ever since.

In terms of inspiring events, I'd have to say it was Israel's 1982 invasion of Lebanon. I was living in Israel at the time and protested the war in downtown Tel Aviv along with tens of thousands of other Israelis. It was a very moving experience and made me realize both the importance of democratic governance (voters can end wars that go sour, or at least compel democratically elected policymakers to change course) and the limits of democratic governance for war and peace decision making (democratically elected leaders can typically initiate wars by circumventing checks and balances, nor are leaders always more bellicose than citizens). Living in Israel brought these issues into stark relief and it is not surprising that one of my first publications was on the Lebanon war, and that I have always been interested in the democratic peace debate. These days I am also doing more work on the Middle East than I have in the past; I have research projects on democratization in the Middle East and the Israeli-Palestinian conflict. So I really feel like I am going back to my roots.

What would a student need to become a specialist in IR or understand the world in a global way?

I'd like to suggest instead that one of the biggest challenges for students doing contemporary IR in general (and security studies more specifically) is to become sufficiently proficient in scholarly work being done outside of the field of IR. This includes having some working knowledge of scholarship being done in comparative and American Politics, and in Political Theory, but also beyond political science and the social sciences more generally. In order to be relevant, both for theory and for policy, IR scholars need to engage with anthropologists, historians, religious studies scholars, psychologists, sociologists, economists, and scholars in other disciplines. This is not to say that we IR scholars all need to be training in joint degree programs or publishing only in interdisciplinary journals. But it is to say that we need to be aware of the work being done in other fields, so that we can draw on the insights from other areas of study and do better in constructing our theories; gathering the evidence for them; and offering policy-relevant answers to the central questions of war and peace. Whenever I go to professional political science conferences (APSA or ISA for example), I am continually struck by how insular so many of us are. But the very best IR scholarship today is being written by people who are intellectually curious about the work being done in cognate disciplines. IR theorists such as Daniel Philpott, Rose McDermott, and Richard Ned Lebow are among those doing this kind of cutting edge, cross-disciplinary work. At the very least, knowing how scholars in other disciplines address similar topics and issues can provide us with a better handle on the guild rules and reward mechanisms that undergird the subfield of IR, and the discipline of political science more generally.

Are we progressing in the study of international politics and how does one 'measure' our progress?

In our book *Progress in International Relations Theory: Appraising the Field* (MIT Press, 2003, second edition 2008), Colin Elman and I, along with a group of distinguished scholars of IR, consider

whether Imre Lakatos' methodology is a useable one for evaluating IR theory. We concluded that it was, although some of the other contributors to the book were less enamored by Lakatos and were more inclined to offer other metrics for assessing whether IR is progressing. I definitely think there are some illegitimate ways of doing science—for example, I don't think that theories should be amended simply in order to salvage them from anomalous evidence. I still think that Lakatos' notion of ad-hocness and his recommendations for how scholars can deal with disconfirming evidence in 'progressive' ways (e.g. by predicting novel facts) is very useful. Scholars should be tenacious in defending their theories—we should not be so fickle that we discard good theories every time a new event unfolds. But we have to do this defending in legitimate ways.

Some of the people I have previously interviewed indicate as a challenge to progress in IR theory the fact that the established people in the field have become over-invested in their intellectual and theoretical positions. They stress the need for a new generation of scholars to open up new paths of inquiry. What's your view on this?

My view is that the scholarship that tends to be rewarded, is the scholarship that takes short declarative positions, ruling out a host of competing explanations, taking only short shrifts at scholarship that came before. The demand in our 'market' is for work that is quite mono-causal and often simplistic in its rendering of the topic or issue at hand. And that tends to be what gets the major exposure. If you think of some of the top work published recently, then you see they don't do in-depth theorizing, but rather only lightly touch upon previously published work on the issue; and the answers they suggest as a response to this earlier work tends to express truncated views of politics and international relations.

So I'm not even sure if this is going to be solved by a generational shift. I think that there is a more profound issue here about what gets published in the major research journals and by the major academic presses. And that in turn might be a consequence of publishing pressures: the pressure to go smaller, shorter, less foot notes, less detail, shorter in word length... That lends itself to a certain kind of writing that I think is not open to the sort of eclectic, multi-level, multi-paradigmatic theorizing attentive to history and complexity which one would ideally have. It's hard to find books that do such sound theorizing nowadays.

Let me give you an example: I've talked to a lot of scholars recently about criticisms they've received on their published works, and they all indicated that they *had* these criticisms covered in the earlier manuscripts of their published work, but that editors forced them to remove that nuance because they didn't have space. Another example is that your standard journal article has to look as follows: you start with an existing explanation for a problem, explain why according to your new approach it is wrong, show two or three cases and sum the argument up in the conclusion. So that means that some of the reasons why theoretical innovation is lagging might be far more mundane than one thinks at first.

But still, one could agree with for instance Alexander Wendt (*Theory Talk #3*) that it is difficult for established scholars to let go of their 'big ideas'.

Yes, but I don't see that as something necessarily bad: you're tapping into the tenacity of theorists and their theories here. I think that resilience is something good: the fact that someone is reluctant to give up his or her pet theory is not only good for theory development but also logical from a more human point of view: if you've worked on something for a long time, you wouldn't want to give it up that easily.

Theoretical tenacity is a central issue in the work of Lakatos, which we linked to IR in the previously mentioned volume. Thomas Kuhn would say that theories persist because not enough anomalies have pervaded for scientific revolution to take place, or because alternative theories and their proponents haven't been persuasive enough; so it's a matter of a radical break between one theory and the other, a sweeping and permanent change. Some people looked at post-Cold-War IR theory as reflecting such a kind of shift, when many in the field moved away from neorealism towards other, more domestic-policy sensitive approaches.

For Imre Lakatos, by contrast, this kind of prescriptive way of seeing science is very dangerous, because new ideas might come from demagogues and it's simply not a very pluralistic or open way of speaking about science, to say that it's *either* the old theory *or* the new one. So he suggested an alternative way to measure progress in scientific research, and I think that his approach is intuitively appealing for many IR scholars, simply because we try not to throw out our theories with each challenging real-world event but rather we try to improve our theories as a response in order to explain what's happening. Unlike Kuhn, who saw science as dominated by single monopolistic paradigms at any given time, Lakatos's MSRP is far more tolerant in anticipating that at any time a given science will have several scientific research programs. I think this more accurately describes the field of IR. Lakatos envisioned a pluralistic science and he was justifiably worried about the 'destructive effect' that naïve falsification strategies could have on budding scientific research programs. The notion of Kuhnian 'paradigm wars' is totally foreign to Lakatos's approach.

Yet a Lakatonian view on progress in IR doesn't support relativism; I would take a stand against scholars holding on to 'their' co-existing theories which both fail to explain what's happening in international politics. Lakatos would see that as very dangerous, and I concur that we have a serious issue in IR with this. You can see it in the way new theoretical debate positions get framed: scholars refer to other theories as 'having a point', but then again, their own, new theory also does. Everybody is kind of 'right', as if it's all the same, as if there are no incorrect assertions concerning facts. Yet I think there are facts, and that there are right interpretations and wrong interpretations. That's linked to my conception of progress in IR. I think there is a lot of theory that just isn't moving forward, that is sort of stuck in its own pond, but there are also advances that are moving us forward, like the neoclassical realist agenda. While some say it is regressive, I think that it moves forward the thinking of classical realists in a more systematic way, in a way that helps us address some of the important aspects of IR today and I think it is a vibrant progress.

Earlier, you've examined how traditionalist diplomatic and military historians and qualitative case study methodists relate to one another. What are the main differences between their approaches and how should a student recognize and pick one or other?

Before considering the differences, it is important to note the similarities. Diplomatic historians and qualitative IR scholars are bound to be very sympathetic to each other. They have a shared subject matter, similar methodological leanings, and mutual views that their scholarship is underrepresented or undervalued in their discipline. There really is no such thing as an a-theoretical historian or an a-historical political scientist—this is a stereotype that pervades our thinking about the two disciplines but it really is a misnomer. I think the main difference is in the aesthetics—history simply looks and reads differently than political science. If you look at the books in political science/IR published by the top university presses (for example, Cornell, Princeton, Cambridge) they have a certain style that is followed fairly religiously: introduction; theory chapter; case study chapters (all distinct in time and place); concluding chapter with policy recommendations. Historians would never write international relations this way. For historians,

causal arguments are embedded in narrative, and arguments don't float freely across temporal and spatial contexts.

What's the most important lessons both approaches can learn from one another?

Robert Jervis (*Theory Talk #12*), Paul W. Schroeder, Richard Ned Lebow, John Lewis Gaddis and the other contributors to my book (co-edited with Colin Elman), *Bridges and Boundaries: Historians, Political Scientists, and the Study of International Relations* (MIT Press, 2001), offer different and compelling answers to this question. In the last paragraphs of their respective essays in the book Schroeder says that he could not conceive of being a political scientist and Jervis says that he could not be a historian, but both acknowledge the importance of a continued dialogue and conversation, one in which historians and political scientists can “be friends and, in some cases, allies”.

Last question. You've examined the famous challenge to realists, who see states as 'black boxes' caught up in an inescapable balance of power, that democracies don't fight amongst each other about a decade ago. Does this still hold? And is democracy cause, or effect, of peace?

Much of my earlier work has been critical of the democratic peace theory, but I have never thought that critiquing the democratic peace proposition requires us to throw the baby out with the bathwater by rejecting consideration of the domestic sources of foreign policy or the normative influences on state behavior. Democracy matters, it is just that most (not all) democratic peace theorists get democracy wrong—they don't operationalize it right, and so the theory tends to be more applicable to understanding American foreign policymaking than it is for explaining the behavior of other democratic states. Most democratic states simply do not distinguish friend from foe on the basis of regime type alone. Threat perception is much more complex than that. So the theory does not travel very well outside of the US context. (I have a host of other critiques of the theory that can be found in my edited book *Paths to Peace: Is Democracy the Answer?*, MIT Press, 1997).

I am currently working on a project that looks at how war and peace impact democratic political development (the second image reversed). “Democracy as effect of peace” has been relatively understudied, certainly compared to the cottage industry that looks at how democracy impacts war and peace outcomes. When states are under the gun (particularly when they are new, fledgling polities), democratic governance tends to work out differently than when external conditions are more benign. It is particularly interesting to look at how institution builders use a threatening international environment to persuade others about the optimality of particular rules and structures. This tends to be a pattern that I have found across multiple historical and contemporary cases.

As an aside, I would amend your question slightly. Realists do not argue that states are ‘black boxes’. On the contrary, realists acknowledge that domestic politics (as well as ideology and identity) matters, and indeed often dominates state foreign policy making. What realists argue is that rational states should not allow these things to matter, and that those that do will eventually suffer the consequences (In Kenneth Waltz's parlance, they will “fall by the wayside”). Your claim that realism ‘black boxes’ the state is common in the literature, and often found in the writing of realists themselves. But I think it is fundamentally incorrect. Indeed, realists from Hans Morgenthau to contemporary writers like Kenneth Waltz, Stephen Walt, John Mearsheimer, and Christopher Layne have always looked within the state, but what they found there they did not like, and they proscribed that smart states should instead follow the ‘national interest’, and base

policy on strategic (read: external) constraints and opportunities. Realism is far more of a normative position on IR (and democratic politics too) than most people recognize.

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- Read Colin and Miriam Elman's *Lakatos and Neorealism: a reply to Vasquez* (The American Political Science Review, 1997) [here](#) (Pdf)