Mechanisms, Method and the Near-Death of IR Theory in the Post-Paradigm Era

Where did IR theory go? We used to have theories that thought big. Waltz’s structural realism (1979), or Wendt’s (1999) systemic constructivist response to Waltz, or neo-liberal work on international institutions (Keohane 1984) come to mind. But most of us did not like this stuff – too simplifying, too spare, too detached from the world as we know it. So, these theories – and the paradigms to which they gave names - are mostly history. After all, for a good decade and a half, we have been living in a post-paradigm era.

In this new world, our vocabulary for theory development - eclecticism, mechanisms, the middle range, pluralism, bridge building – is symptomatic of an epidemic of think small theory. At this rate - by 2030 if not well before - one may legitimately ask ‘is anyone still a theorist?’ To paraphrase that renowned IR scholar - Austen Powers – we would appear to have lost our theoretical mojo.

This brief note sketches the sources of this theoretical decline, and suggests that current developments in the discipline will likely make matters worse. I begin, though, by providing some evidence for what is wrong with IR theory circa mid-2016.

It would be absurd to argue that IR no longer has theory. This is not true, and anyway not my claim. We actually have little bits of theory everywhere, focusing on a multitude of actors, across a broad range of policy fields, and with grounding in a diverse array of social theories. Much of this is very good – and indeed likely authored by many of the people attending this conference.

Missing from this rich mix, however, are efforts to think in broader theoretical terms. Where, for example, has realism gone? The critique that Legro and Moravcsik articulated over 15 years ago – ‘Is Anybody Still a Realist?’ (1999) - still rings true today. In seeking to make realism more operational and determinate – and middle-range in theoretical terms – scholars, they argued, had turned the theory into a bit of this (domestic politics) and that (ideology) and that (perceptions) and … with no recognizable core.

But we should not just pick on realists. Twenty years after the advent of constructivism, IR is still waiting for a constructivist theory of international politics. In its place, we have arguments about norms, other-regarding behavior, strategic social construction, practices, and social discourses – just to name a few. Again, taken individually, this work is important and fascinating. Yet, this (growing?) fragmentation within
constructivism sets the stage for a reprise of Legro and Moravcsik, but now entitled ‘Is Anybody Still a Constructivist?’

The problem is broader, however; we cannot just blame the realists or constructivists. Three years ago, the European Journal of International Relations devoted an entire special issue to ‘The End of IR Theory?’ (Wight, Hansen, Dunne 2013). While the collection offered no definitive answer to that question mark in its title, it did leave one with the distinct feeling that the end was not too far off.

A look at the journal International Organization (IO) suggests this gloomy portrayal of IR theory is not just some European hang up. IO now rarely publishes think-big theory pieces. Arguably, the last such article was that by Barnett and Duvall on ‘Power in International Politics’ (2005), which was published over a decade ago. Special issues of the journal display a similar trend. Path-breaking collections – say, the regime special issue (36/2, March 1982) – have been replaced by normal-science, let-us-incrementally-push-the-envelope work. Indeed, the last three special issues – ‘The Rational Design of International Institutions’ (55/4, Autumn 2001); ‘The Political Economy of Monetary Institutions’ (56/4, Autumn 2002); and my own ‘International Institutions and Socialization in Europe’ (59/4, Autumn 2005) – could be described in precisely such terms.

My special issue, for example, offered a set of middle-range arguments that theorized socialization by taking existing IR efforts and tweaking these a bit. One might call this an ‘off-the-shelf’ style of theory development in that we developed new theory by grabbing a bit of what was most recently published. However, there are many problems with such an approach – from (blindly) importing into one’s work the biases and assumptions of others to theorizing all too often in a shallow manner. And this off-the-shelf style is not unique to Checkel or outlets like International Organization. When reviewing for journals and the main university presses, I see this is by far the most common approach.

If this is the problem, what has caused it? Three factors are at work, operating on three different levels – methods, philosophy of social science, and professional incentives. On methods, while there is no logical relation, there is often a tendency to couple quantitative techniques with a simplistic view of theory development as little more than hypothesis testing (Mearsheimer and Walt 2013). From this perspective, there is a clear villain to the story: ‘The quants made us do it!’ While there is an element of truth to such a claim, it is only one (smallish) part of the story.

1 Recently, I have sought to correct the latter problem by returning to work on socialization and theorizing in more foundational terms (Checkel 2017). Instead of IR theorists, I now draw upon the people who actually think, theoretically, about socialization - sociologists and anthropologists.
In fact, a more significant factor has been at a deeper level. For the better part of two decades, philosophers of social science, political scientists and IR theorists have been hunting for a better understanding of cause. Dissatisfied with constant conjunction, Humean, and correlational conceptions of the term, scholars have increasingly invoked a mechanismic, processual understanding of cause, typically captured with phrases like causal mechanism or social mechanism.

Today, virtually everyone talks mechanisms. Rational choice scholars endorse them (Elster 1998), as do constructivist social theorists (Wendt 1999); empirical constructivists theorize and measure mechanisms (Risse, Ropp and Sikkink 2013), as do their interpretive counterparts (Guzzini 2011; Pouliot 2015; Norman 2016); neo-liberal institutionalists call for more attention to them (Martin and Simmons 2013, 344). Mechanisms seem to have become ‘the mother of all isms’ (Bennett 2013b). Entire methodologies have been developed to measure them – process tracing, most obviously (Bennett and Checkel 2015). As I write – mid-2016 – mechanism has achieved the (notorious) status of being our latest buzzword: A term many feel a need to use, but often with little operational sense of how actually to build theory based on it. As I tell my undergraduates, mechanisms are cool, hot and sexy. This is a problem – for my undergraduates and for us.

Mechanisms are popular among qualitative IR scholars – and for good reason. Theorizing in terms of mechanisms gives us more determinate, empirically accurate pictures of the social world. Yet, the problems with mechanism-based theories are numerous. If they are built on more than one mechanism – which is often the case - the theories quickly grow complex and become over-determined. Relatedly, it is not clear how they cumulate to larger bodies of knowledge (Checkel 2015). Indeed, in many studies, a list of mechanisms is the extent of the theoretical ambition on offer.

Thinking – theoretically – in terms of mechanisms has also pushed American IR scholars to reconsider their meta-theoretical framework for theory development. Positivism is inadequate, as it adopts a Humean, correlational understanding of cause (Wight 2013). However, there is no consensus on what should replace it. Some argue in favor of pragmatism (Johnson 2006; Katzenstein and Sil 2010), while others favor scientific realism (Bennett and Checkel 2015, ch.1).

Cutting across these differing meta-theoretical positions, however, is a unifying thread – a vision of theory and theory development cast in more modest terms. The goal should not be grand or general theory – a la Waltz (1979), say – but theories one level down; more formally, the goal is theories of the middle range. Different theorists invoke differing terms –
eclecticism (Katzenstein and Sil 2010), pluralism (Checkel 2013), conversation (Fearon and Wendt 2002, 68), dialogue (Caporaso, Checkel, Jupille 2003) – but they all endorse some kind of mid-range theory. And guess what? After mechanisms, middle-range theory is running a close second in the buzzword contest.

Indeed, in too many cases, invoking it is more a badge to be worn than a carefully elaborated analytic approach. The result is proliferating lists of variables and causal mechanisms. Put differently, the middle range and eclecticism are not without costs (Parsons 2015). To give one example, there is a tendency with middle-range approaches to adopt a micro-focus, where one theorizes causal mechanisms in some temporally or spatially delimited frame (Haas 2010, 11). The danger is then to miss the macro level where material power and social discourses may fundamentally shape and predetermine the mechanisms playing out at lower levels. More generally, and as Nau has argued, middle-range theories “inevitably leave out ‘big questions’ posed from different or higher levels of analysis” (Nau 2011, 489-90).

A third and final factor reducing the level of theoretical ambition in IR is professional incentives. We socialize graduate students to get their work published fast and in the best IR journals – and for very good reason. Increasingly, some such publications are needed to make the cut for tenure-track positions at major universities. Of course, writing articles is important, but their size, the nature of the review process and the need to write oneself into the current debates and literature encourage a pull-theory-off-the-shelf approach.2

For qualitative IR graduate students (and scholars), there is an additional – emerging – incentive to think even less about theory. Recall it is these researchers who have bought into (and invested in) the mechanism turn. Empirically testing mechanism-based theories is a time-intensive affair, as one needs training on and knowledge about context and process, and the special methods required to measure mechanisms. This is all well-known and nothing particularly new, so why do I emphasize it? The answer - in a word - is DA-RT, or data access and research transparency. This movement and now at least one policy – the Journal Editors’ Transparency Statement (JETS) – will significantly raise the bar in terms of what is expected – methodologically – of qualitative IR theorists.3

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2 In Europe, such incentives are being further exacerbated by a structural change: the introduction of article dissertations in political science. Such a PhD, as the name implies, consists solely of 4-5 articles of ‘publishable quality.’ There is no requirement for an integrated manuscript, where one has the space and time to think – theoretically – in broader terms.

3 On DA-RT, see Symposium 2014, 2015, 2016, as well as the ‘Dialogue on DA-RT’ (http://dialogueondart.org/) and ‘Qualitative Transparency Deliberations’ (https://www.qualtd.net/) websites. On JETS, see http://www.dartstatement.org; the policy has been adopted by over 25 top political science and IR journals.
Already – the policy became effective on January 15 of this year – JETS requires of qualitative researchers: digital archiving and making publicly available your data (‘production transparency’); and delineating clearly the analytic procedures upon which your published claims rely (‘analytic transparency’). While the former raises important ethical issues, the latter may require significant time and resources to implement. However, in a world of finite time and resources, there is a real danger that full implementation of these standards will relegate theory to second-class status, further incentivizing us to put method before theory.

To sum up, I have argued that a combination of methodological choice (quantitative techniques for testing our theories), developments in social-science philosophy (cause understood as a process) and incentives (currently in the form of DA-RT) have led us to think small and not very ambitiously in theoretical terms. Even if my diagnosis is correct, some may question what the problem is in the first place. Most of us did not like the era of grand theory and isms, so why should IR go back to that kind of scholarship?

To be clear, while I am not advocating a return to the ‘good old days’ of isms, I do worry that IR theory has swung too far in the other direction. We all plug away in the middle range, do the normal science, and abstain from big disputes. For those of us who operate in this space, we can and should think harder about its downsides and how they might be addressed. One clear weakness is the limited ability of middle-range theory to generalize. Some have proposed typological theory (Bennett 2013a) as a way to aggregate a bit more; this deserves further attention.

We also might think more ambitiously about theoretical terms and constructs, perhaps leaving existing theory ‘on the shelf,’ as it were. Here, Snidal and Wendt are spot on to argue – in the opening article in the first issue of International Theory (IT) - there should be more emphasis in IR theory development on ‘original contributions … [that] add new theory rather than … test old theory’ (Snidal and Wendt 2009, 9). Implementing this sound advice, theorists might benefit by re-reading Rosenau’s (1980) ideas on ‘thinking theory thoroughly,’ where he advances nine pre-conditions for creative theorizing. Some seem less important from today’s perspective, but one still stands out. ‘To think theoretically one must be playful about international phenomena … to allow one’s mind to run freely … to toy around’ (Rosenau 1980, 35). The implication is to think outside the box, to get outside your comfort zone.

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4 See, for example, the good faith but rather daunting operationalization of these principles by the American Political Science Review: http://www.apsanet.org/PUBLICATIONS/Journals/APSR-Submission-Guidelines-2016-in-Brief.
5 Prior to founding IT, Wendt had left the IO Board of Editors, partly out of concern at the very theoretical trends described here.
Beyond the middle-range, we should welcome and reward efforts to theorize more broadly and creatively, yet in ways that still help us understand and explain the (complex) social world. One example is the article – already mentioned – by Barnett and Duvall (2005) on power in IR. Another might be Moravcsik’s (1997) attempt to recast liberal IR theory. The point is not to replace the middle range, but to complement – and perhaps challenge it – from additional theoretical vantage points.

References


