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# **International Institutions and Global Governance: The Turn to Mechanisms and Process**

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## International Institutions and Global Governance: The Turn to Mechanisms and Process

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### Abstract:

The past decade has seen a sustained move by students of international institutions and organizations to viewing their subject matter as independent variables affecting state interests and policy. Conceptually, this has put a premium on identifying the mechanisms connecting institutions to states; methodologically, there has been a growing concern with measuring process. While this move has produced rich and analytically rigorous studies that demonstrate the multiple roles – good and bad – institutions play in global politics, significant challenges remain. In terms of design, scholars often neglect the problem of equifinality – where multiple causal pathways may lead to the same outcome – and instead conduct process tracing only on their preferred argument. Theoretically, the focus on process seems to reduce the power and generalizability of arguments about institutions. Finally, the potential for process tracing to help combine rationalist and constructivist insights remains largely unfulfilled.

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## International Institutions and Global Governance: The Turn to Mechanisms and Process

### Introduction

In an agenda setting essay first published in the early years of the new millennium, Martin and Simmons argued that the study of international organizations (IOs) and institutions (IIs) had reached an important threshold, focusing less on why they exist and more on “whether and how they significantly impact behavior and outcomes” (Martin and Simmons 2013, 327). Put differently, the past decade has seen a sustained move by students of international institutions and organizations – be they rationalists or constructivists – to viewing their subject matter as independent variables affecting state interests and policy. Conceptually, this has put a premium on identifying the mechanisms connecting institutions to states; methodologically, there has been a growing concern with measuring process.

In this essay, I assess several studies where international institutions are claimed to be influencing state-level action through various processes and mechanisms. The move to process and to the method of process tracing has been salutary, I argue, producing rich and analytically rigorous studies that demonstrate the multiple roles – good and bad – institutions play in global politics.

At the same time, challenges remain. In terms of design, scholars often fail to address the problem of equifinality – where multiple causal pathways may lead to the same outcome – and instead conduct process tracing only on their preferred argument. Theoretically, the power and generalizability of arguments about institutions seem to decrease as the focus shifts to process. Finally, the potential for process tracing to help scholars produce integrative frameworks about international institutions – combining rationalist and constructivist insights – remains unfulfilled.

In terms of criteria for good process tracing (Bennett and Checkel nd, 28–40), recent work on IOs and IIs addresses a number of them. The more general standards (alternative explanations, possible biases in evidentiary sources) are often explicitly invoked. However, those specifically relevant for process tracing – most important, addressing equifinality; and the *a priori* specification of observable implications – are too often left unaddressed. For sure,

discussing the latter will clutter the empirical narrative and story, but the trade-off will be more robust explanations. Thus, an important challenge for future work is to be more explicit, both in its operationalization of process tracing and the evaluative standards behind its use.

The remainder of the paper is structured as follows. I begin with a brief review of work on international institutions and organizations; this provides context for its current focus on mechanisms and process. I then discuss four works that are empirical examinations of the processes and mechanisms through which II/IOs shape state behavior and interests; two are rationalist in orientation (Wallander 1999; Schimmelfennig 2003), while two are broadly constructivist (Kelley 2004; Autesserre 2010). My purpose is not to recount the stories they tell, but to provide a net assessment of their turn to mechanisms and use of process tracing. In a third, concluding section, I argue that students of IOs need to remember that method is no substitute for theory, and that they can strengthen their arguments by combining process tracing with other techniques, such as agent-based modeling.

## **The Study of International Institutions**

By the mid- to late 1990s, research on institutions and IOs had reached a new level of sophistication. Rationalists built upon Keohane's pioneering neoliberal institutionalist work (Keohane 1984; see also Mitchell 1994; Simmons 1994), but applied it to new issue areas (security – Wallander 1999) or new – domestic – levels of analysis (Martin 2000). From a more sociological perspective, constructivists began to document a different, social role for IIs and IOs, where they created meaning and senses of community that subsequently shaped state interests (Finnemore 1996; Risse, Ropp and Sikkink 1999; Barnett and Finnemore 2004). Among all scholars, the focus was shifting away from asking why such institutions existed to a logical follow-on question: Given their existence, how and in what ways did they influence politics within and between states (Martin and Simmons 2013)?

Finnemore's 1996 study exemplifies these achievements and shift in emphasis. In its first pages, she argues that political science has focused too much "attention on the problem of how states pursue their interests," rather than "figur[ing] out what those interests are (Finnemore 1996, ix). She then goes on to develop an argument on IIs and IOs as the source for those state

interests. And it is an argument not simply couched in terms of independent (an IO, in this case) and dependent variables (state policy and interests). Rather, Finnemore is concerned with the intervening process that connects the two.<sup>1</sup> Thus, in her study of state adoption of science policy bureaucracies, UNESCO is not simply some black hole magically diffusing policy; instead, Finnemore theorizes and empirically documents the teaching process behind such diffusion (Finnemore 1996, chapter 2).

At the same time, this turn to process and state properties (policy, interest, normative understandings) raised new challenges. In particular, studying IO influence on states means that, to some extent, one must examine their domestic politics, which requires some theory of the latter. However, at this point (mid-1990s), IR scholars were devoting relatively little attention to politics at the state level (Schultz 2013, 478). Extending these arguments about international institutions to include the domestic level, I argue below, raises additional challenges for those employing process tracing – especially in the absence of any explicit theory of domestic politics.

Another feature of this work was to accord primacy to international-level factors. At first glance, this makes sense; after all, these were IR scholars studying the causal effects of IIs and IOs. Consider the case of international human rights. The assumption was the real action was at the international level, with international human rights norms diffusing to the domestic arena to bring about change (Risse, Ropp and Sikkink 1999; Thomas 2001). More recently, however, some students of IIs and human rights have reversed the causal arrows, arguing for the primacy of domestic factors (Simmons 2009). At this domestic level and mirroring the thrust of system-level analyses, the emphasis was again on mechanisms and processes (Simmons 2009, chapters 6–7).

In sum, by the early years of the new millennium, arguments about international institutions were becoming more determinate and fine grained. They were also becoming more empirical, with richly detailed case studies and new data sets replacing the illustrative empirics of earlier work. The language might vary – middle-range approach, intervening variables, process, causal mechanisms – but the goal was the same: to theorize and empirically document

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<sup>1</sup> Analytically, she was capturing the workings of what we now call causal mechanisms, or “the pathway or process by which an effect is produced or a purpose is accomplished” (Gerring 2007, 178).

the pathways through which IIs and IOs influenced states. For sure, there were exceptions – for example, work from a principal–agent perspective (Hawkins, *et al* 2006) – but more and more scholars were joining the process and mechanism bandwagon.

## **The Process and Mechanism Turn**

Perhaps the strongest evidence of a clear move among students of IIs and IOs to study process and mechanisms is a non-event: We currently have no grand theories in this area. Among rational-choice scholars, Keohane’s neo-liberal institutionalism serves at best as a starting point for contemporary analyses (Simmons 2009, for example); and there certainly is no single, widely accepted constructivist theory of international institutions. Instead, we have a growing collection of partial, mid-range theories, which are largely the result of the analytic and methodological choice in favor of process and mechanisms.

My analysis in this section is structured around the four books noted earlier, taken in chronological order so as better to assess progress (or lack thereof) in process-based studies of institutions and IOs over time. They cover different issues areas (security, IO membership expansion, human rights, post-conflict intervention), different parts of the world (Eurasia, Europe, Africa), and a variety of IOs (NATO, OSCE, Council of Europe, UN system). While they likely are not a representative sample – there are many other books, articles and chapters produced in recent years that study institutions and IOs from a process perspective – they nicely capture the changes at work over time.

For each of the four books, I begin with a summary of its subject matter and core argument. However, the bulk of the analysis is an assessment of a given author’s success in capturing and measuring process.

### ***International Institutions and the Great Powers***

Published in 1999, the book by Celeste Wallander is quite explicit in its debt to Keohane’s neo-liberal institutionalism (Wallander 1999, chapter 2). However, she moved beyond it in several important ways. First and countering a criticism lodged against Keohane’s early work, she



applied his theory to the study of high politics security institutions – this being in contrast to the low politics / economics emphasis of Keohane (1984) and his early followers (Simmons 1994). Second and as discussed in more detail below, Wallander added elements of process to what was largely a structural approach.

Wallander's topic is security relations between the two most powerful European powers in the Cold War's wake – unified Germany and post-Soviet Russia. The book's empirical core is a careful reconstruction of German-Russian security relations in the immediate post-Cold War period (1991–96). In particular, she explores the role of international institutions in shaping relations between these two European powers in several areas, including the withdrawal of Russian troops from unified Germany, multilateral conventional force reductions, out-of-area missions for German forces, regional conflicts (Yugoslavia and within the former Soviet Union), weapons proliferation, reactor safety and Russian economic reform.

Theoretically, the book develops an institutional argument that goes beyond saying institutions matter. Instead, Wallander theorizes (and then empirically documents) the specific ways and conditions under which international institutions influenced Russian/German relations. Her “stronger institutional theory of security relations” explores how variation in institutional form and function and the layering of international institutions affect the likelihood of states choosing cooperative security strategies (Wallander 1999, 27–40; quote at 28).

Writing in the late 1990s and thus before the more recent revitalization of qualitative methodology, Wallander is – unsurprisingly – not explicit on her methods. However, the underlying approach is clear. Drawing upon a rich set of interviews, she essentially traces the process through which Russian and German interests – post-Cold War – were being formed in interaction with institutions such as NATO. This is process tracing in practice, if not in name.<sup>2</sup> That is, Wallander analyzes “evidence on processes, sequences, and conjunctures of events within a case for the purposes of either developing or testing hypotheses about causal mechanisms that might causally explain the case” (Bennett and Checkel nd, 7; see also Checkel

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<sup>2</sup> Wallander wrote in the late 1990s and published her book in 1999. However, it was only in the early 2000s that the specific term process tracing began to receive considerable attention. Hall (2003); George and Bennett (2005, chapter 10).

2008). While she does not explicitly invoke the language of causal mechanisms, her analysis is very much in this spirit, seeking to fill the gap between the independent variable – European institutions – and the outcome (Russian-German security cooperation).

In contrast to earlier work on international institutions from a neo-liberal perspective, where the empirics were more illustrative in nature (Keohane 1984), Wallander – by focusing more on process – provides a richly documented account of how institutions were shaping state behavior. Her extensive interview material gives the book a sense of ‘history in the making.’ More important, the analysis is not of the either/or type, where either power (realism) or institutions (institutionalism) carry all the causal weight. Instead, Wallander offers a careful both/and argument, where power and interests are refracted through and, in some cases, shaped by institutions.

Put differently, Wallander does not just conduct process tracing on her preferred – institutionalist – perspective. Rather, she takes seriously the possibility there may be alternative causal pathways leading to the outcome she seeks to explain – so-called equifinality. This is a central requirement of good process tracing (Bennett and Checkel nd, 28).

In sum, Wallander’s book marks an important advance in the study of international institutions. It is theoretically explicit, empirically rich and – central to my argument – begins to add an element of much needed process to its subject matter. Institutions are not magically reducing abstract transaction costs – an analytic claim typically undocumented in earlier work; rather, they are reshaping state strategies in specific and empirically measurable ways.

At the same time, her turn to process is not without weaknesses and limitations. For one, when Wallander uncovers evidence that does not fit within the neat causal arrows of the institutions → state strategy relation, it is either set aside or left under-utilized. Consider two examples. First, the manuscript provides ample documentation that international institutions have not just influenced German strategies, but, at a much deeper level, helped construct its very interests and preferences. Over and over, Wallander’s interviews with high-level German policymakers and analysts show them to be almost reflexively institutionalist. They find it exceedingly hard to conceive or define German interests outside the dense institutional network

within which their country is embedded. Unfortunately, except for a brief mention in the concluding chapter, the author fails to exploit fully such findings.

In addition, Wallander portrays European and other international institutions as passive actors, as a resource to be exploited by self-interested states. They have no sense of agency in their own right. Throughout the book, though, institutions often play a very active and social role, serving as forums for political dialogue, as settings where learning and education occur, or where states are socialized into the ways of the international community. The author has thus uncovered evidence of institutions' constitutive power – as constructivist IR theory might argue (Adler 2013). If Wallander had systematically explored these constitutive dynamics, she would not only have expanded her understanding of process, but would have contributed to the then nascent literature on theoretical bridge building between rational choice and constructivism (Adler 1997, for example).

### ***International Institutions and Membership Expansion***

If Wallander (1999) is suggestive of a greater emphasis on process in the study of international institutions, then Schimmelfennig's study on the post-Cold War enlargement of European institutions is explicit on this score (Schimmelfennig 2003; see also Gheciu 2005; Checkel 2007). Indeed, his central theoretical innovation is to theorize – and then empirically document – the role of rhetorical action as “the mechanism” and “causal link” between rule-ignoring, egoistic individual state preferences and a rule conforming collective outcome: EU and NATO membership being offered to the formerly communist states of East/Central Europe (Schimmelfennig 2003, 6).

Schimmelfennig argues that explaining the enlargement of regional organizations is a neglected area of study, and that post-Cold War Europe offers an ideal laboratory to both theorize and document such processes. This is precisely the task he sets for himself in the book, which begins by conceptualizing Europe as a community environment for state action. He then proceeds to specify the constraints under which states act in such an environment and describes how the rhetorical use of arguments can result in rule compliant behavior (Schimmelfennig 2003, 6).

The concept of rhetorical action, or the strategic use of norms and arguments, serves several purposes for Schimmelfennig. First and theoretically, it moves his argument away from structure and decisively to the level of process, where agents strategically deploy arguments. Second and again theoretically, it positions his argument to bridge rationalist and constructivist modes of social action. After all, arguing, at least before Schimmelfennig wrote, was thought to be a core constructivist concept (Risse 2000). It was part and parcel of following a so-called logic of appropriateness. For Schimmelfennig, however, arguments are strategically deployed by egoistic agents operating under a so-called logic of consequences.

Thus, the book promises a process-based, social-theoretically plural account of the role played by IOs. And it delivers, with Schimmelfennig operationalizing his argument and testing it against rich empirical material. At the time of writing, it was probably the best example of how one theorizes and measures process in the IO/state relation.

The book is especially strong at the level of methods. In particular, Schimmelfennig does not simply assert the central importance of rhetorical action as the causal mechanism linking IOs to outcomes; he also directly addresses the challenge of measuring such mechanisms. Much more so than Wallander – and more explicitly – he carefully thinks through the challenges and possible pitfalls involved in observing mechanisms. Writing several years before George and Bennett (2005) would popularize the term, Schimmelfennig tells his readers how to conduct process tracing or what he calls “looking into the *causal process* that links independent and dependent variables ... in which the researcher explains an event by detailing the sequence of happenings leading up to it” (Schimmelfennig 2003, 13; emphasis in original; quoting in part Dessler 1999, 129).

In conducting the process tracing, Schimmelfennig follows several of the best practices advanced by Bennett and Checkel (nd, 28–40). For example, he increases readers’ confidence in his findings by taking equifinality seriously, theorizing and empirically testing for mechanisms other than rhetorical action that might also explain the outcomes he observes. For all these

various mechanisms, he specifies their so-called observable implications, that is, what one should observe in the empirical data if the particular mechanism is at work.<sup>3</sup>

The result of this theoretical innovation – the turn to and operational use of causal mechanisms – and methodological rigour – the systematic application of process tracing techniques – is a study that significantly advances our knowledge of IOs. For students of European institutions, Schimmelfennig’s argument fills in the gap left by general and hence highly underspecified theories of integration such as intergovernmentalism or supra-nationalism. For the more general study of institutions and IOs, Schimmelfennig – by focusing on mechanisms – demonstrates the value added in taking process seriously, both theoretically and methodologically. Instead of abstract and general theories that at best hint at how institutions matter, Schimmelfennig’s work details the exact causal pathway through which they influence state behavior.

At the same time, the book’s overall argument and approach raises a troubling issue, one that would only become more apparent in the years following its publication. Simply put, what is the theoretical take away, in a broader sense? Yes, Schimmelfennig demonstrates the important role of rhetorical action. However, what more general theory of IOs emerges? It would seem there is none. Instead, one gets a middle-range argument (George 1993), where a set of factors interact to produce an outcome, but where that very complexity limits the generalizability of the argument. Today, ten years after the publication of Schimmelfennig’s book, do we have a research program on rhetorical action and the study of international institutions? No, we do not. The time of the general isms in the study of IOs – Keohane’s (1984) neo-liberal institutionalism, say – may have passed; what, though, has replaced it?

A second theoretical dilemma/limitation is less troubling but still worth highlighting. Despite Schimmelfennig’s claim to be utilizing insights from both rational choice and constructivism (Schimmelfennig 2003, 281–87), the book essentially offers a clever rational-choice argument where central elements of constructivism – social structure, recursivity,

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<sup>3</sup> Schimmelfennig’s interest in the application of process tracing to the study of IOs is reflected in later work as well – Schimmelfennig (2005) and, especially, Schimmelfennig (nd). For a superb application of process tracing in a different issue area – civil war – see Bakke (2013).

interpretation, holism (Adler 2013) – are notable only by their absence. Yes, there is some recognition of the importance of theoretical pluralism in the book, but it is quite limited in the end. However and to be fair to Schimmelfennig, such limited efforts at building theoretical bridges have become the norm (Checkel 2013a), despite the optimism of its early proponents (Adler 1997; Katzenstein, Keohane and Krasner 1998; Fearon and Wendt 2002).

### ***International Organizations and Minority Rights***

Like those by Wallander and Schimmelfennig, Judith Kelley's (2004) book examines the role of international organizations in Europe. Like these other books, she is also seeks to theorize and empirically measure the mechanisms linking IOs to state behavior. However, in at least two ways, her study advances the research frontier in work on international organizations. First, she explores possible IO influence in a policy area – the rights of ethnic minorities – with enormous implications for state sovereignty and identity. Second and more important, she addresses a neglected point: IOs ultimately matter and shape state behavior only when they work through the domestic politics of particular countries.

The latter is perhaps Kelley's central contribution. For over two decades, there have been persistent calls for IR theorists to take domestic politics seriously. Kelley does this in a theoretically plural way that seeks to combine elements of rational choice and constructivism – and the payoffs are high. Her 2004 book is essential reading not because she shows us that international institutions matter – others had by that time made and documented such claims. Instead, by thinking systematically about the mechanisms – cost/benefit calculations and incentives as well as normative pressure – that connect the international with domestic politics, Kelley shows us how this occurs. She can thus explain domestic implementation dynamics and ultimate policy outcomes ignored by virtually all other scholars studying IOs at that point in time.

The danger – or, better said, challenge – in modelling the interaction between IOs and domestic politics is the enterprise can get messy. In social science terms, the result may be over-determined outcomes, where a host of causal factors are in play, but it is difficult to parse out

which matter most. Kelley mostly avoids this problem, by careful, upfront attention to design and methods.

Kelley's focus is post-Cold War East Europe and the efforts by regional organizations to influence state policy on ethnic minorities. Like the other books assessed here, her concern is to connect these institutions to states by theorizing and empirically measuring the causal mechanisms at work. Theoretically, her aim is to combine previously separate compliance schools – enforcement and management – to explain why and how states might abide by IO injunctions. To do this, Kelley focuses on two specific causal mechanisms – membership conditionality and normative pressure. With the former, rationalist mechanism, states respond to incentives and sanctions imposed by IOs; with normative pressure – the constructivist mechanism – IOs rely solely on the use of norms to persuade, shame or praise states into changing their policies (Kelley 2004, 7–8).

If Kelley were to stop here, her study would resemble others – specifying the mechanisms between independent (IO) and dependent (state policy) variables. However, she goes an important step further, introducing domestic politics into the analysis, basically as an intervening variable, with the degree of domestic opposition (high or low) affecting the likelihood of one or the other of the two mechanisms having effects on state policy (Kelley 2004, 32 – Figure 2.1). This analytic move is to be applauded for it highlights the important point that international actors affect states by working through, shaping and influencing their domestic politics.

The argument is tested through a combination of quantitative and qualitative methods, as well as carefully executed counter-factual analysis. On the qualitative techniques, process tracing is again explicitly invoked as playing a key role (Kelley 2004, 23–24, 67). Its operationalization – by neglecting issues such as the observable implications of her mechanisms, and equifinality – is less clear, though, than in the case of Schimmelfennig (2003).

Overall, the book makes an important contribution. It illuminates the specific conditions and mechanisms that allow regional organizations to influence policy on highly sensitive issues (policy on ethnic minorities). For students of IOs, it offers a nuanced understanding of when conditionality is likely to work, which is a welcome contrast to broad-brush critiques asserting

that it is rarely if ever effective (Kelley 2004, 9). Moreover and in a fashion similar to the other two books discussed, Kelley demonstrates that a focus on process and mechanisms is fully consistent with theoretical and methodological rigor. Finally, with a greater emphasis on policy, she demonstrates how a mechanisms/process approach can and should play a key role in designing policy interventions by the international community (Kelley 2004, 189–91).

At the same time, Kelley's argument can be criticized on three grounds. First and at the risk of sounding like a broken record, what is the broader contribution to theory? Her nuanced, mechanism-based argument is not easily generalized; moreover, it may only work in post-Cold War Europe (see also Kelley 2004, 192–93).

Second, while Kelley's turn to the domestic level is an important and progressive theoretical move in the study of IOs, it can nonetheless be criticized for being rather simplistic. One gets no theory of domestic politics here, be it one emphasizing institutions, interest groups, elites, or the like. Instead, we are told that high levels of domestic opposition make it harder for the international community to influence policy. This is surely no surprise.

Third and similar to Schimmelfennig, Kelley's theoretical bridge building is biased and thus ultimately weak. In particular – and in keeping with Kelley's strong positivist commitments – if she cannot carefully measure and operationalize a concept, it falls by the wayside. Thus, while she claims in the book to be speaking to constructivist social theory, she in fact does this in only a very minimal sense. For example, Kelley invokes the concept of socialization (Kelley 2004, 7–8, 31, 34–35), whose sociological core is all about processes of internalization. Yet, she shies from measuring the latter and instead searches for (weak) proxies as the observable implications of it.

Thus, for someone who argues that a central goal of her research is to promote conversation across theoretical traditions (Kelley 2004, 9, 187–88), Kelley comes up short, especially on constructivism. The only constructivism that works for her is that measurable in a way (pre-) determined by her positivist epistemological starting point. This limits her theoretical creativity and also her ability to use the book to push forward the debate over bridge building.



### ***International Institutions and Post-Conflict Interventions***

In her recent study, Severine Autesserre uses a focus on mechanism and process to demonstrate that IOs need not always be a force for good, helping states cooperate or promoting global governance (Autesserre 2010; see also Autesserre 2009). The three other books reviewed in this essay – by Wallander, Schimmelfennig and Kelley – all highlight the role of IOs in fostering interstate cooperation (Wallander) or in promoting normatively good outcomes (enlargement of European institutions; fair treatment of ethnic minorities). There is nothing wrong with such a focus, which has clear roots in Keohane’s (1984) original formulation of neo-liberal institutionalism as well as the normative commitment by many of those studying IOs to improve world order.

At the same time, it is entirely plausible – once one grants IOs some degree of autonomy and agency – that they may perform sub-optimally and even pathologically. These latter outcomes need not be caused by member states, but may arise because of processes and mechanisms at work within the organizations themselves (Barnett and Finnemore 2004).

Picking up on this line of reasoning, Autesserre’s book explores the role of international organizations in post-conflict interventions.<sup>4</sup> In the post-Cold War era, this has typically meant IO efforts to promote/preserve peace in states where a civil war has occurred. Her specific focus is sub-Saharan Africa and the international community’s efforts to intervene in the long-running internal conflict in the Democratic Republic of Congo (DRC). These interventions, in Congo and elsewhere, typically do not succeed; in nearly 70% of the cases, they fail to build a durable, post-conflict peace (Autesserre 2010, 5). Why?

The answer, Autesserre argues, lies not in the national interests of states or specific organizational interests (Autesserre 2009, 272–75; Autesserre 2010, 14–23), but in a powerful framing mechanism that shapes the understanding and actions of intervening organizations. This peacebuilding culture – as Autesserre calls it – establishes the parameters of acceptable action on

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<sup>4</sup> Autesserre’s focus is actually the broader set of international interveners, including diplomats, NGOs, and IOs. Given my concerns in this essay, I consider only the IO-part of her argument.

the ground by UN peacekeepers; it “shaped the intervention in the Congo in a way that precluded action on local violence, ultimately dooming the international efforts” (Autesserre 2010, 10–11).

This is an argument about how process – framing dynamics first theorized by sociologists – shape what IOs do and the effects they have. To make it, Autesserre conducts multi-sited ethnography, semi-structured interviews (over 330) and document analysis, spending a total of 18 months in the field – mainly in Congo (Autesserre 2010, 31–37). While she never explicitly cites process tracing, this is in fact a central technique she employs. In the article version of the study, Autesserre makes clear her concern is less to capture the relation between independent and dependent variables, and more “to document a dispersed process, where social objects have multiple sources, and where ideas, actions and environmental constraints mutually constitute each other” (Autesserre 2009, 255, note 21).

Despite its implicit application, the process tracing is carefully executed. For example, while she does not use the language of observable implications, Autesserre does just this throughout the book’s empirical chapters, exploring what she ought to see if the dominant frame/peacebuilding-culture is at work (Autesserre 2010, chapters 2–5). She also carefully triangulates across multiple data streams to increase confidence in the validity of her inferences (Autesserre 2010, 32–33) – another of the process-tracing best practices that Bennett and I develop elsewhere (Bennett and Checkel nd, 35–36).

Bringing it all together – the innovative sociological theory, the carefully executed methods, the rich empirics – Autesserre develops a powerful, process-based argument that helps scholars better understand the role (good and bad) that IOs can play in rebuilding war-torn societies. This is not just my assessment, as the book won the highly prestigious Grawemeyer Award for Ideas Improving World Order this past year.<sup>5</sup>

At the same time, this study of IOs, mechanisms and process will leave readers with lingering questions and concerns; I raise three here. First, does the argument about frames and the failures of peace building travel? Does it explain anything but the – clearly very important –

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<sup>5</sup> See <http://grawemeyer.org/news-updates/analysis-of-what-went-wrong-in-congo-wins-grawemeyer-award>, last accessed 19.05.13.

case of Congo? Are there certain scope conditions for the operation of the framing mechanism – that is, when it is likely to affect the behavior of different IOs seeking to intervene in other conflict situations? It would appear not.

As Autesserre makes clear from the beginning (Autesserre 2010, 14–16), her argument about framing supplements existing explanations based on material constraints, national interests and the like. Such both/and theorizing is appealing as it captures the reality of a complex social world where it really is a ‘bit of this and a bit of that factor’ that combine to explain an outcome. At the same time, it makes it difficult – in a more social science sense – to parse out the exact role played by framing in the Congolese case. If we cannot determine its precise role here, how can we apply it elsewhere?

Despite this concern, in the book’s concluding chapter, Autesserre nonetheless claims the “scope of this argument is not limited to the international intervention in the Congo. The approach ... is valuable to understanding peacebuilding success or failure in many unstable environments around the world” (Autesserre 2010, 247). The following pages provide a number of empirical examples broadly suggestive of the generalizability of her approach. However, it is difficult to see the analytic role – if any – played by framing and the peacebuilding culture in these illustrations.

Second, given her process and mechanisms focus, Autesserre must address equifinality, which means to consider the alternative causal pathways through which the outcome of interest might have occurred. However, it is not sufficient simply to consider alternative explanations for the observed outcome, failed interventions, in this case – which Autesserre nicely does (Autesserre 2010, 14–23). Rather, one needs to theorize the mechanisms suggested by alternative accounts, note their observable implications, and conduct some process tracing on them – likely based on secondary sources (Bennett and Checkel nd, 28–31). In Autesserre’s case, this would have involved taking the most plausible alternative (that based on national interests?) and demonstrating that, at key points in the process, it generated observable implications different from what she found.

Third, Autesserre fails to offer a broader, integrative framework that combines different theoretical schools, which is a missed opportunity. After all, this is a book with a firm grounding

in constructivist ontology (culture and frames creating meaning, making action possible) but one that simultaneously recognizes the importance of rationalist factors (material constraints, strategic action). That is, the building blocks for such a framework are there. Moreover, it is precisely a focus on mechanisms and process – as seen in Autesserre – that makes it easier to identify points of contact between different theories (Checkel 2013a).

### **Mechanisms and Process Tracing Are Not Enough**

For students of international organizations, the move to process and mechanisms and to the method of process tracing has been salutary. As the books reviewed here attest, the result has been rich and analytically rigorous studies that demonstrate the multiple roles institutions play in global politics. We now know much more about how these organizations really work and shape the behavior and interests of states. The embrace of a mechanism-based understanding of causality has reduced reliance on ‘as if’ assumptions and thus heightened theoretical/empirical concern with capturing better the complex social reality of IOs.

Yet, as my many criticisms suggest, there is no such thing as a free lunch, even in the study of IOs. There are tradeoffs, opportunity costs and limitations to a process/mechanism-based understanding of the IO–state relation. And to be clear, my (hopefully constructive) criticisms here are only possible thanks to the pioneering work of scholars like Wallander, Schimmelfennig, Kelley and Autesserre. By taking mechanisms seriously, carefully operationalizing concepts and methods, and then applying them to extensive empirical data, they have demonstrated the tremendous advantages of such an approach. These facts along with the transparency of their methods and designs make it easier to see what is working – and where challenges remain. On the latter, I see three issues of theory and three regarding method that deserve further attention.

### ***Theory***

Despite or because of the focus on mechanisms and process over the past decade, one recent agenda-setting essay on IOs concluded that “more attention to the causal mechanisms advanced ... would greatly enhance our ability to explain the world around us” (Martin and

Simmons 2013, 344). Given the results achieved to date, such an endorsement makes sense – and is consistent with the move to mechanism-based theorizing in political science and other disciplines more generally (Johnson 2006; Gerring 2007; Hedstroem and Ylikoski 2010).

In almost all cases, however, there is a trade-off. Mechanisms provide nuance and fine-grained detail, filling-in the all-important steps between independent and dependent variables, but do so at the expense of theoretical parsimony. More general theories of IOs – Keohane’s neo-liberal institutionalism, say – have been replaced by a growing collection of partial, mid-range theories. This might not be a problem, especially if it was clear what was replacing the general theories.

Unfortunately, it is not clear. Mind you, we have a name for the replacement – middle-range theory – which is repeated with mantra-like frequency by a growing number of graduate students and scholars. Missing, however, is an operational sense for how such theory is constructed and critical self-reflection on its limitations.<sup>6</sup> For sure, the very name tells us something: Middle-range theory is in-between grand, parsimonious theories and complex, descriptive narratives. Typically, it brings together several independent variables and causal mechanisms to explain an outcome. The ideal is that the resulting framework will have some degree of generalizability – in a particular region or during a particular period of time, say (Glaser and Strauss 1967; George 1993; see also Checkel 2007). More recently, prominent scholars have endorsed such theorizing as the way forward for the IR subfield as a whole (Katzenstein and Sil 2010).

However, middle-range theory has three potential drawbacks about which students of IOs should be aware. For one, it will often be over-determined. That is, with several independent variables in play, it is not possible to isolate the causal impact of any single factor. One way to address and minimize this problem is by emphasizing research design at early stages of a project. This may sound like Grad Seminar 101 advice, but it needs nonetheless to be stressed (see also Martin and Simmons 2013, 344).

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<sup>6</sup> The reaction of students at a recent [PhD seminar](#) co-taught by the author was typical. After finishing an assessment of mid-range theory that explicitly discussed its weaknesses, most students noted this was the first time they had ever been exposed to such criticism.

In addition, when large parts of a research program are characterized by mid-range approaches, the production of cumulative theoretical knowledge may be hindered. Specifically, for work on IOs and institutions, the various middle-range efforts – including those surveyed above – are not coalescing into a broader theoretical whole. Instead, we have proliferating lists of variables and causal mechanisms. Contrast this with neo-liberal institutionalism – a paradigm-based, non-plural body of theory on the same topic (Keohane 1984). Here, there has been theoretical advance and cumulation, as later efforts build upon earlier work – for example, by adding process and domestic politics variables while still keeping a rational-choice core (Mitchell 1994; Simmons 1994; Wallander 1999; Martin 2000, for example).

Finally, there is a tendency with middle-range approaches to adopt a micro-focus, where one theorizes (interacting) 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 – say – fundamentally shape and predetermine the mechanisms playing out at lower levels. This is precisely the trap into which Checkel and collaborators fell in their project developing theoretically plural, middle-range theories of European IOs and socialization. A global search of the resulting volume reveals virtually no hits for either power or discourse (Checkel 2007, *passim*). More generally and as Nau has argued, middle-range theories “inevitably leave out ‘big questions’ posed from different or higher levels of analysis”; they may thus “not get rid of ‘isms’ [but] just hide them and make it harder to challenge prevailing ones” (Nau 2011, 489–90).

One promising possibility for addressing these problems is typological theory, or theories about how combinations of mechanisms interact in shaping outcomes for specified populations. Compared to middle-range approaches, this form of theorizing has several advantages. It provides a way to address interactions effects and other forms of complexity; stimulates fruitful iteration between cases, the specification of populations, and theories; and creates a framework for cumulative progress. On the latter, subsequent researchers can add or change variables and re-code or add cases while still building on earlier attempts at typological theorizing on the phenomenon (George and Bennett 2005, chapter 11). For example, in a recent project on civil

war (Checkel 2013b), it was demonstrated that typological theorizing is one way to promote cumulation, even in the hard case of mid-range, theoretically plural accounts (Bennett 2013).

A second theoretical issue upon which IO scholars might reflect is their efforts at theoretical pluralism and bridge building. As argued above, these have been limited. This is odd, especially for accounts of IOs and institutions built on causal mechanisms. As the philosophy of science literature reminds us, the latter are “quite compatible with different social theories of action” (Mayntz 2004, 248) – rationalist or constructivist, say.

An excellent example of a bridge-building effort in the area of international institutions and IOs is a project on human rights led by Thomas Risse, Stephen Ropp and Kathryn Sikkink (1999). However, it is the exception that proves the rule. Actually, it proves two rules: (1) that very few other students of IOs have shown this degree of theoretically plural ambition; and (2) even the bridge building of Risse, *et al*, is quite limited.

Risse and collaborators sought to develop a model explaining the process through which international norms have effects at the national level; it was conceived from the beginning as a plural one integrating insights from both rational choice and social constructivism. To accomplish the latter, they employed a temporal-sequencing bridge building strategy. That is, it was the combination of different theoretical approaches – first, rational choice; then, constructivism – working at different times, that explained the outcome.

Using this theoretically plural frame, the volume’s empirical studies provide structured and rich evidence that compliance with international prescriptions is not just about learning new appropriate behavior, as many constructivists might argue. Nor, however, is it all about calculating international or domestic costs. Rather, by combining these insights, Risse and collaborators provide scholars with a richer picture of the multiple causal pathways through which norms matter.

All this said, it is important to note an important limitation in the book’s self-consciously plural theory. If we continue with the bridge-building metaphor, then it is about a bridge not crossed – that of epistemology. Positivism or its close relation scientific realism is the philosophical starting point for the project – exactly as we saw for the Schimmelfennig (2003)

and Kelley (2004) books discussed above. It would thus appear that theoretically plural accounts of IOs can include the whole spectrum of rationalist scholarship, but only that part of constructivism with a foundation in positivism. This seems unfair as constructivism is a rich theoretical/meta-theoretical tradition with equally strong roots in interpretative social science (Adler 2013).

One possibility is that interpretive constructivism is missing from these accounts because it is structural and holistic, while the IO work reviewed here is about mechanisms and processes. However, this is not true. Over the past decade, interpretive constructivists have added a strong element of process to their accounts (Neumann 2002). They have done this through the concept of social practice, where “it is not only who we are that drives what we do; it is also what we do that determines who we are” (Pouliot 2010, 5–6). This has not been an abstract exercise, as the concept has been operationalized and rigorously applied – including to the study of IOs (Pouliot 2010, *passim*). Moreover, these scholars are now actively developing an interpretive variant of process tracing, thinking in concrete terms about how to do it well (Guzzini 2012, chapter 11; and, especially, Pouliot *nd*).

So, the concepts and tools are there to allow for a bolder form of theoretical bridge building – one that crosses epistemological boundaries – when studying IOs. However, it has for the most part not happened. Perhaps to combine (positivist) rationalism with (interpretive forms of) constructivism just cannot be done; it is an apples and oranges problem. The former is about cause, linearity, and fixed meanings, while the latter is about recursivity, fluidity, and the reconstruction of meaning. Yet, these black and white distinctions blur into ‘bridgeable’ grays when the research is applied and empirical. Thus, in two important books, Hopf combines the interpretive recovery of meaning with causal, process-tracing case studies (Hopf 2002, 2012; see also Holzscheiter 2010). These books are about Soviet/Russian foreign policy and the origins of the Cold War; however, the basic interpretive-positivist bridge-building design could just as easily be applied to the study of IOs.

My point here is straightforward. Research on IOs has gained considerably by focusing on mechanisms and process over the past 15 years. It has also gained by integrating insights from both rational choice and constructivism. It may gain even more if – depending upon the question



asked – it integrates practice and discourse into its accounts. And by gain, I simply mean it may acquire even more practical knowledge about why IOs do what they do in global politics (see also Checkel 2013a, 235–36).

A final theoretical issue concerns domestic politics. Kelley (2004) shows the clear benefit of beginning to incorporate the domestic level into explanations of IO effects on states. More recently, Simmons (2009), while again having no theory of domestic politics, goes a step further – systematically linking the nature of domestic legal systems (common vs. civil law) to state receptivity to IO norms and rules.

Beyond the theoretical rationale of offering more complete and thus determinate explanations, there is a real world reason for bring domestic process into our theories about IOs. Simply put, for many IOs, what they decide and do have become the subject of deep and intense domestic (and transnational) political contestation. Consider the most powerful IO in the world – the European Union (EU). After decades when theorists of the EU ignored domestic process, national politics and politization now occupy an increasingly central place in their arguments (Hooghe and Marks 2009; and, especially, Risse nd).

At the same time, care should be exercised in not taking domestic politics and process too seriously, where system-level influences from IOs (and other actors) are ignored. This is precisely the (flawed) theoretical move taken by an increasingly influential branch of international political economy in the US – open economy politics (Oatley 2011 for an excellent review). It is possible that for some IOs on some issues, what they do is driven entirely by the domestic politics of a particular member state. However, this should be a matter of empirical discovery and not theoretical diktat.

### ***Method***

Given the centrality of causal mechanisms in the books reviewed, I focus here on a central method for measuring them – process tracing – and should begin by reiterating my earlier praise. All the authors – and especially Schimmelfennig (2003) – do a good job. This is all the more notable because they were mostly writing well before the recent literature seeking to

systematize and establish good standards for process tracing (Collier 2011; Beach and Pedersen 2013; Bennett and Checkel nd).

Despite this excellent start, future work on measuring the causal mechanisms of the IO–state relation could improve on three counts. First, the process tracers need to carefully and fully theorize their mechanisms. The more care at this stage, the clearer will be those mechanisms’ observable implications. As Gates has argued “[t]heory must take primacy over method. Theory offers the perspective through which we can interpret empirical observation . . . . [T]he interpretation of events in a process-tracing case study are shaped by theory” (Gates 2008, 27). The bottom line is that “[t]ightly specified theories with detailed mechanisms can substantially enhance the discriminating power of process [tracing] analysis by generating crisp and unique empirical predictions” (Jacobs nd, 3).

This was precisely the strategy Checkel and collaborators pursued in work on European institutions and socialization. We took the mechanism of socialization, disaggregated it into three sub-mechanisms (strategic calculation, role playing, normative suasion), theorized scope conditions for each, and specified their differing observable implications (Checkel 2007, chapter1). This allowed us not only to avoid “lazy mechanism-based storytelling” (Hedstroem and Ylikoski 2010, 64), but to advance the process-based research program on IOs and socialization.

Second and again for process tracers, scholars studying IOs from a mechanism based perspective must address fully the challenges raised by equifinality, where multiple causal pathways may lead to the same outcome. This means to specify these other candidate mechanisms, identify their observable implications and conduct some process tracing on them. It is not sufficient to carry out process-tracing on one’s preferred mechanism, or to run through alternative explanations. Done properly, this takes time (and resources) and should thus be integrated into research designs at an early stage.

Moreover – and to link back to my first point – full, robust theorization of these various mechanisms will only facilitate this task. The point is not to eliminate equifinality; that is not possible given the complex social world we (and IOs) inhabit. Rather, by explicitly addressing it,

the researcher increases readers' confidence in the validity of the mechanism-process story he/she relates. This is precisely the strategy pursued by Schimmelfennig (2003), with considerable success. Unfortunately, his work is more the exception than the rule at this level.

Third, process tracing should not be viewed as the only way of capturing causal mechanisms. One promising strategy is to employ computer techniques known as agent-based modeling to explore the logic and hypothesized scope conditions of particular causal mechanisms. For example, in recent, process-based work on civil war, scholars have used agent-based modeling to analyze the transnational diffusion of social identities as a key process underlying the spread of civil conflicts. They disaggregate – and thus better specify – diffusion as occurring through two possible causal mechanisms: social adaptation in a transnational context, and transnational norm entrepreneurship. Their simulations – the computer modeling exercise – indicate that norm entrepreneurship is the more robust mechanism of diffusion, which is an important confirmation of a finding in the qualitative, process-tracing work (Nome and Weidman 2013).

## **Conclusion**

If one returns to this essay's title, 'International Institutions and Global Governance,' then it is clear that 'The Turn to Mechanisms and Process' has delivered. We now have a more complete, causally more robust understanding of how IOs and institutions contribute to – and detract from – governance at the global and regional levels, and of their relation to states. This has helped spur exciting new work on what one might call comparative regional organizations (Acharya and Johnston 2007).

However, this new knowledge has come with some unwanted baggage. The methodological challenges highlighted above are relatively easy fixes; basically, scholars need to do a bit better what they are already doing. The theoretical issues, though, are of another magnitude. Mechanism-based theorizing raises a number of challenges – most important, how to scale up from explanations grounded in mechanisms to larger bodies of theory. We can and should do better than generating endless lists of case-specific causal mechanisms.

The days of paradigm wars and grand isms may be mostly past, which are surely good things. However, for students of IOs – and the broader discipline – their replacements are not clear. Terms and concepts such as mid-range theory, analytic eclecticism and pragmatism open up exciting, more plural theoretical (and epistemological) vistas, but are less clear in telling us how to get there in a way that maintains some degree of intellectual coherence.

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