The Theory and Practice of Global Governance: The Worst of All Possible Worlds?

ANDREW HURRELL
Oxford University

This article takes the example of global governance in order to reflect on the problematic relationship between theory and practice and on the gap that exists between the academic and policy worlds. That there is a gap between the two worlds is clear. Some insist on the benefits to be gained from trying to bridge the gap, highlighting the contribution that theoretical inquiry can make to the policy world and the responsibility of academics to contribute towards resolving policy challenges. Others argue for the continued importance of a division of labour, stressing that the logic of theoretical enquiry demands analytical and critical distance from power and politics. This article does not examine either of these extreme positions but instead explores the dangers of the middle road.

For academics, insufficient awareness of the problematic ways in which theory and practice are inextricably interwoven makes it more likely that they will fall hostage to the politics and parochial prejudices of both time and place. For policymakers and for those who teach public policy, the danger lies in seeking the authority and legitimacy of academic work that purportedly embodies objectivity and detachment but that in fact merely translates the prejudices and preoccupations of the policy world back into a different idiom. An unreflective and uncritical attitude to the relationship between theory and practice can leave the academic study of International Relations in the worst of all possible worlds.

The relationship between the academic study of global governance and practices of global governance has come under increased scrutiny. In the United States, critics lambast Political Science for being irrelevant to practical and policy concerns (see, for example, Cohen 2009). In Britain, a debate has been raging over the proposal that 25% of the final score for determining university research funding would in future be based on a measure of its “impact”—defined as impact on the UK economy and society, but explicitly excluding impact within academia or within a particular field or discipline. But such questions are by no means limited to the United States and United Kingdom. In India, conferences have been held on the gap that exists between “rising India” on the one hand and the state of academic International Relations on the other. For Amitabh Mattoo, “Interest in India and India’s interest in the world are arguably at their highest in modern times, and yet Indian scholarship on global issues is showing few signs of responding to this challenge” (Mattoo 2009). In Brazil, Fábio Wanderley has raised difficult questions about what he sees as “the problematic character of the relations between relevance (social and political) and the analytical quality of work in the field of political
science,” criticizing, in particular, International Relations for often being little more than “the intelligent reading of newspapers” (Reis 2009). And in China, debates over theories of International Relations have formed one important element of the broader discussion of China’s future role in the world (see, for example, Callahan 2008).

This article takes the example of global governance to reflect on the problematic relationship between theory and practice and on the gap that exists between the academic and policy worlds. That there is a gap between the two worlds is clear. Some insist on the benefits to be gained from trying to bridge the gap: the contribution that theoretical enquiry can make to the policy world; the responsibility of academics to contribute to resolving policy challenges; and the ways in which a more detailed knowledge of the policy process can improve academic understanding of International Relations. Others argue for the continued importance of a division of labor: that the logic of theoretical inquiry demands analytical and critical distance; that the “all things considered judgments” needed by policymakers stand in tension with the theoretical ambitions of academia; and that, as for Stanley Hoffmann, academic work must have distance from power and the temptations of power.¹

I do not wish to examine either of these extreme positions. Instead I would like to explore the dangers of the middle road. For academics, insufficient awareness of the problematic ways in which theory and practice are inextricably interwoven makes it more likely that they will fall hostage to the politics and parochial prejudices of both time and place. For policymakers and for those who teach public policy, the danger lies in seeking the authority and legitimacy of academic work that purportedly embodies objectivity and detachment but that in fact merely translates the prejudices and preoccupations of the policy world back into a different idiom. An unreflective and uncritical attitude to the relationship between theory and practice can leave the academic study of International Relations in the worst of all possible worlds.

In part, such dangers are inherent in the study of politics. As Martin Wight once put it, one of the core purposes of the university is to help escape from “…the Zeitgeist, from the mean, narrow, provincial spirit which is constantly assuring us that we are at the peak of human achievement, that we stand on the edge of unprecedented prosperity or an unparalleled catastrophe; that the next summit conference is going to be the most fateful in history, or the leader of the day is either the greatest, or the most disastrous of all time” (Wight 1991:6). Other difficulties have more to do with place than with time. In the study of global governance, academic International Relations has spent an enormous amount of analytical energy on the question of “governance” but rather less on the meaning and implications of “global.” We tend to throw around the word “global” as if its meaning were stable and obvious and shared across different contexts and cultures. But both of these sets of difficulties can be exacerbated by the pressures of policy relevance and by unreflective demands that academic research should contribute more directly to problems of both national and global public policy.

Theory and Practice

Very crudely, we might differentiate between two modes of studying political phenomena. Each involves a particular kind of knowledge and a particular way


of knowing; and each tends to open up different ways in which academics and academic ideas come to influence the practices of politics.

One view stresses the importance of setting individual events within a broader historical narrative, or some historically rooted world view. The goal is not so much to explain, but rather to “make sense” of the big picture and to understand “how things hang together,” including how political life is felt and understood by the agents and actors involved. This kind of knowledge is about how the furniture in the room is arranged, where the room is situated within the house, how the house itself came to be built, and how its particular history affects our understanding of where we are and where we might be going. This kind of knowledge may lay explicit weight on the macroforces and shaping history, but such forces may also form part of what James Joll called the “unspoken assumptions” that shape how policymakers think and act (Joll 1966).

It is not difficult to find examples of where academic ideas have had a profound influence on knowledge of this kind. Take, for example, dependency theory. In Latin America, dependency theory arose in part out of the failures and limits of earlier attempts at national developmentalism and, in part, as a response to US interventionism and to what seemed for many to be the all-too-evident snares and constraints of the global capitalist system. For the Third World more generally, dependency-influenced ideas chimed perfectly with the realization that formal independence had not brought an end, or even a substantial mitigation, to structurally deep-rooted forms of external dependence and to their subordinate position in the global system. A second example, this time from the post–Cold War world, would be Samuel Huntington. Both The Clash of Civilizations and Who are We? are precisely about the grand historicist narrative and about the political logics, the identities, and the meanings that follow from them.

How should we think about the relationship between the power of ideas and the reception of those ideas? Even abstract political theory can have a major impact on public reasoning and political practice (Weale 2010). Think, for example, of the crucial role of Peter Singer’s 1975 book Animal Liberation in helping to shift attitudes to the suffering of animals and to factory farming. It is also the case that writers and theorists can derive their influence from the sheer power of their ideas. Michael Ignatieff gives the example of Hannah Arendt as someone who “created her own authority” (Ignatieff 2003). “She arrived in New York as a penniless refugee and by her death was widely respected as a public intellectual.” And he goes on—usefully for our purposes: “Her example helps us to identify what intellectual authority is, and how it differs from other forms of authority. First, it is unconnected with power. She never exercised power or political influence. But she did exercise intellectual authority. ... An intellectual with authority lacks any possibility of coercing. An intellectual’s characteristic mode is persuasion, argument, rhetoric, and all of these work their effects within a framework of consent, rather than force.” Ignatieff differentiates two ways in which ideas may be related to power: “She was acutely aware of the coercive power of ideas—not just when they are allied to ‘world historical forces’ like Communism or fascism. But ideas can have a malign power of coercion even when they are unsullied by an alliance to forms of political power, when they have only the power of systemization and the apparent promise of piercing through appearances to the eternal truth of the world. These claims for ideas—that they reveal the eternal truth of the world—are a source of coercion and an instrument of tyranny.”

Within International Relations, we can certainly list such ideas as soft power, democratic peace, the clash of civilizations, and the end of history as recent examples of where political scientists have been influential within political practice—even if we should still be rather careful about claims for direct influence.
(as with links between democratic peace theory and the Clinton administration’s doctrine of democratic enlargement). None of this should be surprising, not least given the extent to which the most important single concept in the field, namely power, always rests on a combination of ideas and material factors. As Krieger notes, “...power is a combination of idea and fact that transcends the usual distinction between theory and practice, and one of the characteristic endeavors of history is precisely to show the running connections between theory and practice” (Krieger 1967:14).

At the same time, we in no way lessen the intellectual value of many of these ideas by recognizing the importance of the time, place, and context within which they were expounded and received. The study of the history of political thought has been centrally concerned with the problem of how individual thinkers can be related to their context and to their role within the construction of ongoing traditions and patterns of thought and argument—think of the many debates over, for example contextualism, reception theory, conceptual history, and the construction of traditions. Indeed, it is odd that, in the recent discussion of policy impact and policy relevance, not more attention has been given to these debates and to the unresolved and inevitable complexities that quickly emerge.

The impact and influence even of such a canonical figure as Grotius has waxed and waned dramatically over the centuries. After a period of relative neglect, his stature grew steadily following Jean Barbeyrac’s translations of De jure Belli ac Pacis first into French in 1724 and then into English in 1738; as Edward Keene has shown, the historical construction of “Westphalia” and of Grotius’s central place within it was closely related to the politics of revolutionary Europe (Keene 2002), and the rise of notions of a “Grotian moment” in 1918/1919 and of a “neo-Grotian moment” in the early post–Cold War years are both clearly related to context and to the particular climate of the times. The history of thought on International Relations also highlights the enormous gap that often exists between intellectual originality and influence. Few writers on international law have ever achieved the impact of Vattel. And yet the impact of his work owed far more to the way in which reflected what generations of state-interest-driven and sovereignty-obsessed statesmen wanted to hear rather than to its intrinsic intellectual quality. On the current measures of policy impact, Vattel would score very highly; Leibniz and Wolff would come nowhere.

Direct impact of this kind is often a source of frustration to political scientists. The ideas that resonate most powerfully are often those that tell simple stories, that explain events in terms of single causes or simple causality, that assign guilt and responsibility clearly to particular people or groups, that distinguish between the good guys and the bad guys, and that tell “us” what “we” should do. It is certainly the case that much classical writing was written for a direct political or polemical purpose—think of Burke or E.H. Carr. Nevertheless, the increased obsession of the media with polemic, with punditry, and with the peddlars of simple stories can have a seriously distorting effect on the sorts of academic work that gets picked up in public life. What counts as professional scholarship can also shape impact and social resonance. Economists, for example, do their professional work in journal articles. If they write books, they are mostly written to be read and are therefore aimed at a general audience. In Political Science, the centrality of the professionally focused monograph tends to work against a broad audience. And one of the reasons why historians sell far more books than political scientists is not just that they usually write to be read but also that they are concerned precisely with providing big-picture narratives and with drawing out explicit judgements about the past and assigning individual and national responsibility.

A second mode of studying politics involves seeking clear causal explanations of particular political phenomena—the very notion of Political Science. Keohane
defines the field in the following way: “I define politics as involving attempts to organize human groups to determine internal rules and, externally, to compete and cooperate with other organized groups; and reactions to such attempts. … I define science as a publicly known set of procedures designed to make and evaluate descriptive and causal inferences on the basis of the self-conscious application of methods that are themselves subject to public evaluation” (Keohane 2009). This is where most of the action is within Political Science. Much of the work within the discipline is driven by the desire to find and answer particular intellectual puzzles. The goal—and the source of professional standing and advancement—is theory development rather than the application of theory to particular “real world” questions, or the generation of empirical knowledge for its own sake. Cases are chosen according to the puzzle and the research design, whether or not the cases relate to issues of policy relevance. To be tractable, these puzzles have to be related to clear sets of methodological tools.

It is precisely this kind of work that exposes the gap between theory and practice. In part, the theoretical puzzles of the academic may simply not speak at all to the immediate problems of the policymaker; in part, the arcane and obscure language of the theorist makes mutual communication hard; in part, theoretically driven puzzle solving can easily degenerate into empty scholasticism and endless differentiation among the cliques, clans, and sects of the faithful; and finally, effort is too often devoted to theoretical point-scoring rather than seeing how the insights of different theories can be combined to shed light on the sorts of messy and complex problems of greatest interest to the policymaker. In other words, the danger is that ever less politically significant puzzles are answered with ever greater theoretical sophistication.

Despite these problems, there are clearly examples of where theoretical work of this kind has had an important impact within the policy world. At the theoretical level, we could place the work of Thomas Schelling and the arms controllers; or, more recently, the work of Elinor Ostrom on common pool resources and decentralized environmental cooperation. At the conceptual level, we could think of the very close relationship between the concepts of neo-functionalism and the practices and politics of European integration. And, in terms of explanatory analysis, we could take the example of Paul Collier’s work on the links between conflict and development (Collier 2008, 2010).

Collier’s work is instructive. It has had enormous resonance in the policy world on both sides of the Atlantic and is by some way the best-selling work on development. But three points are worth noting. First, it illustrates the lack of cross-over between sub-fields within social science, with Collier acknowledging very little of the work on similar issues undertaken within Political Science. Second, it is hard to avoid the conclusion that policy impact and success have rested on the apparently scientific nature of the analysis and on the staggering lack of doubt involved in the policy recommendations. And third, it is even clearer that this is a mode of analysis which fitted glove-like with many of the unspoken assumptions about governance dominant in the post–Cold War period. To be told that conflict is the result of internal factors and that it has little or nothing to do with politics or with political grievance delegitimizes many forms of political violence and shifts the locus of responsibility away from the global. (For critical and alternative views see Keen 2008 and Mamdani 2009).

It is with these problems in mind that we can turn more directly to the problems involved in the study of global governance.

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1For an illuminating account of how authority and meaning are created by means of the canon and the study of canonical texts see Halbertal (1997).
Global Governance and the Zeitgeist

Much of most influential and successful US and European writing on global governance and order over the past couple of decades has been rationalist in method and technocratic in character. Analytically, this approach saw rationality and rational bargaining as offering, if not an escape from anarchy, self-help, and conflict, then at least the potential for mitigation and for a degree of cooperation. The very notion of “governance” as opposed to “government” has been expressed largely in these terms. The analysis of global governance came to be dominated by a dual liberal hegemony: a historicist hegemony that all too easily assumed that history was moving down a one-way street; and an analytical liberal hegemony that tended to work with a narrow notion of agency; with too little room for the historical analysis of the structures within which supposedly ahistorical logics of rational choice and collective action play out; and still less room understanding their temporal and geographical rootedness. The financial crisis turned the critical spotlight on the failures of economists. But the global governance industry within International Relations also missed a great deal of what was going on. Too often, it reinforced rather than questioned the conventional wisdom of the day with its trite notions of “flat earth” and “end of history” and “transforming globalization.”

Instead of a Kojevian Hegelianism pointing to the end of history, we might have more profitably resorted to old-style dialectics or, more modestly, to a consideration of how responses to economic and political challenges in one period can have ongoing and unanticipated consequences in another—thereby bringing back in both structure and history. After all, a central part of Western responses to the crises of capitalism and the decline of US hegemony in the 1970s was to foster and encourage an aggressive phase of globalization, especially of financial globalization. And yet, it was precisely the particular character of liberal economic globalization that helped to create the conditions for the successful emerging economies of today and for the current challenges to US power and authority. The other central feature of the US policy in the 1970s was to revive a policy of highly active intervention across the developing world. While this may have been a successful element in the victory of the West in the Cold War, it also helped to foster, or deepen, or shift the character of many of the conflicts that are proving so intractable to Washington today, especially in relation to the Islamic world with Afghanistan the most notorious example. Seen from both perspectives, the 1970s become more important and the end of the Cold War rather less so. As Arne Westad has suggested, such a conclusion appears more evident when we look back at the Cold War in a global rather than US-centric perspective (Westad 2006). This leads, then, to considering the politics of place.

The Politics of Place

There is no view from nowhere. All theories and concepts are bounded by time and place: they draw their relevance from the temporal sequences and particular contexts in which they are developed and deployed. And yet this is a central part of the intellectual challenge involved in studying global governance or global order. It is about an attempt to imagine what the patterns of order and governance look like—analytically and normatively—when we stand back from the parochial preoccupations of Washington, or Berlin, or Beijing, or Delhi. But if this is the case, what is the policy audience for whom we could be writing? An extraordinary amount of writing on global governance in the United States is not really about global governance at all. It is about what “we” should do. Even recent accounts of the “post-American world” and the “rise of the rest” can end up profoundly US-centric—far more about “us” than about “them” (for example, Zakaria 2009).
Even within a particular theoretical approach, place and position play an important role in how the theory is understood and taken up. Let me pick up the example of dependency. In 1977, Cardoso famously complained about the “consumption of dependency theory” in the United States. His point was that US intellectuals had picked up and stressed those external aspects dependency theory (such as US imperialism and the role of multinationals) that reflected their own analytical and political concerns (Cardoso 1977). Twenty years on, Cardoso the president of Brazil and the practitioner is often seen as having rejected his earlier views, now stressing the need for Brazil to integrate into a fast-changing global capitalist system. And yet it was precisely the continuity of many of intellectual beliefs and premises that led him to this particular policy conclusion (Hurrell 2010).

The influence of place may often be unconscious. For many, it is intellectually clear and normatively proper that stressing effectiveness, efficiency, and incentive-compatibility is the best way to design a regime for climate change. But assumptions about the constraints of place quickly creep in: carbon trading is needed because it can generate resources for the South but without financial flows having going through formal multilateral institutions in a way that that US political and public opinion would never accept. Such arguments may seem simply a matter of common sense. But so too is the position of those in the developing world who believe that a forward-looking, incentive-based, sunk-costs view of the climate change regime means abandoning the legal and moral principles embedded in the Kyoto framework, especially those that maintain some residual notion of historic responsibility on the part of the North and the idea of common but differentiated responsibilities.

Theory should be a central part of the answer. It should provide us with the tools to stand back from the particular case and from the particular place—through asking of what is this particular event a more general instance, and through opening up a much broader range of potential explanatory mechanisms than may be evident in a particular national or regional context. And yet theory can also be a snare. The US-centered nature of much International Relations and the perceived link between International Relations (IR) theory and western or US practice remain major subjects of debate and contestation in many parts of the South: how best to engage with US theory despite its normative blindspots and its particularist concerns (see, for example, Tickner and Waever 2009).

The problem is certainly not exclusive to US political science. The study of regionalism provides a very good example where it has been enormously difficult to escape from the shadow of the experience of the EU and from the theories that were developed within the context of the European case. This is even more problematic because of the immense prescriptive and normative quality of so much of the integrationist literature and because of the current political role of the EU in taking the message of Europe to other regionalist schemes. Particular countries become elevated to the status of models (as in the case of Spain in the transitions literature) and those that were perceived to work in one context are assumed to be intrinsically superior.

Even the most abstract political theorist remains part of a bounded context. Rawls’ influence came both from the sheer power of his ideas but also from the extent to which he reflected and refracted the particular anxieties and perplexities of American liberalism. Habermas’s work is inexplicable outside of the social, political, and historical consciousness of Germany. Although the very

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*For a general discussion see Castiglione and Hampsher-Monk (2001). Jannik and Toulmin’s account of the importance of context for understanding Wittgenstein remains illuminating: on one side, he was taken up by the English-speaking world in Cambridge as an abstract philosopher of language; on the other, his ideas and preoccupations were profoundly shaped by the political and philosophical anxieties of the Vienna in which he grew up Jannik and Toulmin (1996).*
generality of normative theory can seem to invite travelling with ease across the
globe, it is precisely here that the issue of the “global” comes into sharpest focus.

The study of global international society has found it ever harder to exclude
the contested politics of global justice. But what is the stock of ideas that we can
draw on to make sense of these problems? And who is the we doing the drawing?
Looking back at the history of western political thought, it is worth noting the
extent to which much recent work has stressed the close linkages between the
international and the domestic and between the imperial center and the domi-
nated periphery. Some have stressed the role of empire and conquest in the very
construction of such core political concepts as property, social order, or political
rule. Others have shown how thinking about somewhere such as apparently
particular as Haiti can tell us a great deal about Hegel and the idea of universal
history (see Buck-Morss 2009).

Looking forward, it seems abundantly clear that this must involve a careful
analysis of “non-western” sets of ideas and practices. But the problems soon start
to mount. The study of comparative political thought on a global level is in its
infancy (Goto-Jones 2010). It is certainly a major advance to move to a view of
“civilizations” that stresses their complexity and their multiple traditions (see
Katzenstein 2010). But getting beyond the “non-West” remains a major chal-
lenge. First, there is the need to consider the processes by which different ideas
of international order were transposed into different national and regional con-
texts and to the mutual constitution of ideas and understandings that resulted
from that interaction. Norms do not travel unproblematically. Second, as postco-
lonial writing has suggested, European thought is both indispensable for under-
standing non-European political modernity but also deeply flawed because of the
way in which the categories of European thought are implicated in the produc-
tion of a world of hierarchy and domination. But, third, postcolonialism has its
own limits: it works best at the level of the local and the particular; and much
postcolonial writing itself plays off a curious kind of Occidentalism and an often
one-dimensional view of the “West.”

Direct Pressures

Hedley Bull once argued, in typically robust fashion, that policy and policy sci-
ence was the enemy of academic enquiry: “[But] it is just this prostitution of aca-
demic inquiry to practical ends that is the foremost obstacle to the development
of a science of politics” (Bull 1959:587).

There are clearly problems with such a view. First, whether there is a direct
responsibility to influence public policy, there is surely a responsibility to com-
municate beyond the often very tribal groups that form at academic conferences
and the arcane and specialized dialects in which they speak. Second, it is legiti-
mate that those who provide funding, especially public funding, should demand
some kind of public accountability, however, indirect and mediated it might be.
And third, as Philippe Schmitter has argued, the real world can be an important
reality check on the analytical utility of the concepts and ideas deployed within
the world of theory.

…Beware of concepts that fail to “resonate” beyond the academic audience to
which they are originally addressed. They are probably based on unrealistic
assumptions or unrealizable principles. [Dahl’s “polyarchy” is an interesting mar-
ginal example…] The dialogical process of acceptance and rejection by other
(sub-) disciplines and wider publics that tends to surround the reception of con-
ceptual innovations constitutes an important instrument of “self-correction.” …
This point also suggests the proper place on the “ladder of abstraction,” namely
on the highest rung upon which one can communicate to both specialists and
And yet pressures for policy impact and the idea of tightening the links between theory and practice can have real dangers, especially when set against the broader difficulties discussed in the earlier parts of this paper. The UK research funding debate is obviously parochial. But it has brought out many of the problems involved in trying to assess relevance and impact (see, for example, British Academy 2009 and Bekhradnia 2009): the problem of poor work having a major impact; the difficulties of time lags, attribution, and corroboration; the problem of what timeframe to use; the mismatch of focusing on national impact in a global world and within a global academic community; the fact that much successfully “applied” work has resulted from “basic or fundamental” research rather than work that has aimed directly at impact; the mismatch between pressing academia to have “impact” when we know that the receptivity of governments to research or even “evidence-based research” is often deeply flawed or non-existent; the problem of giving a major role to “users” and “stakeholders” in the formulation of research priorities when they are precisely those who are likely to be most beholden to the conventional wisdom.

**What to Do?**

Across a range of different national contexts, it is all too easy for the academic study of International Relations to reinforce rather than to challenge the status quo and the conventional wisdom. In doing so, academics run the risk of living in the worst of all possible worlds, neither providing real analytical depth and distance nor escaping from the interests and prejudices of one part of the world. As I have suggested, the intellectual failings that stand behind the present economic crisis should make us all think hard, and not just the economists. Too often in the 1990s and the early years of this century, research programmes and calls for proposals have started from the conventional wisdom rather than challenging it: that globalization was obviously transforming world politics; that the traditional study of war should obviously be replaced by the analysis of new security challenges; that transnationalism had eroded the role of the state; that the new religious terrorism was obviously both new and obviously connected with religion.

So, in the first place, there is an overriding need to encourage counter-cyclical research, to support academic bloody-mindedness, and to maintain a pluralism of approaches—a pluralism of theories, a pluralism of different cultures and mind-sets, a pluralism of methods of analysis, and a pluralism of academic disciplines.

Second, it is important to keep fighting against the traditional hostility of many parts of academic International Relations toward Area Studies, and to find new ways of conceptualizing the relationship between disciplines and regions—and certainly going beyond the still prevalent belief that the discipline provides the theory and the regional specialist the empirics. This matters both because of the need for secure empirical knowledge on which to theorize but also to achieve genuine non-parochialism in the process of concept formulation and theory development. It is also a crucial part of the relationship with practitioners. As we are constantly reminded by events in Iraq or Afghanistan, detailed area-specific knowledge in the policy world is often lamentably thin.

Third, there is the need to insist on the inner logic to academic enquiry that requires a willingness to ask and pursue unfashionable questions and to be highly skeptical of all purveyors of conventional wisdom. Yes, doctoral students need to be inculcated into the discipline; but an excessive homage to existing work and
to the leading figures, excessive citations of articles from within an extraordinarily narrow range of journals, and an excessive focus on theory and on theory development can easily become counterproductive. Even if they do not lead to distance between theory and practice within a national context, they are certainly likely to do so in terms of the global study of global governance.

And fourth, we might encourage the idea that the best work is likely to be that which draws together the two modes of studying politics outlined earlier: careful theoretically driven explanatory work that tells us something explicit and meaningful about some aspect of the big historical stories that both shape the political landscape and influence how the field of International Relations is itself conceived. And we might even hope that moves in this direction will encourage a more productive mediation of theory and practice.

References


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