

Interview – Adam Humphreys

Written by E-International Relations

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Adam Humphreys is Associate Professor and Head of Department in the Department of Politics and International Relations, University of Reading. He joined the University of Reading in 2013, having previously been a British Academy Post-Doctoral Fellow at the University of Oxford (2007–10) and Fellow in Politics at Brasenose College, Oxford (2010–13). His principal research interests are in International Relations theory and metatheory, especially causation and causal explanation, realism and neorealism, the English School, and the relationship between theory and history. He also has research interests in British foreign and defence policy, strategy, and the ethics of war.

Where do you see the most exciting research/debates happening in your field?

I think there is a lot of very exciting research going on in many different areas. In relation to causal inquiry, I am generally impressed by the ingenuity of what Hidemi Suganami and I, following Thad Dunning (2012), term ‘design-based’ approaches to causal inference – approaches which attempt to secure the internal validity of a causal inference through the research design, i.e. through an experiment or natural experiment. In *Causal Inquiry in International Relations*, we examine a couple of examples of this approach in some detail.

However, it is a key contention of our book that causal inquiry should be construed broadly – that many people are doing work that contributes (or could contribute) to the development of causal knowledge, even if they would not necessarily describe themselves as conducting causal inquiries. So, while looking at design-based approaches, we also show, for example, how historical work can plausibly contribute to the development of causal knowledge, whether this work is presented as ‘process-tracing’ or as ‘history’. Speaking personally, I have been delighted to observe the recent resurgence of ‘Historical International Relations’ and also the continued efforts to excavate the history of ‘International Relations’ as a discipline, including its racist and imperialist undercurrents.

That said, one of the frustrations expressed in our book is that causal inquiry is often conceived rather narrowly, contributing to the impression of a ‘divided discipline’. For example, it is a striking feature of historical work in IR at present that it largely avoids causal language. Meanwhile, the ‘causal inference’ literature frequently dismisses the value of historical work, failing to recognize how a ‘single case study’ could generate causal knowledge. It is important to note, of course, that historical work can have many aims and that showing how and why a particular outcome occurred is only one of them. It is also important to recognize that generating causal knowledge is only one aim of research in IR, though an important one. I am convinced, however, that we would be collectively better off if we could recognize the breadth of ways in which causal knowledge can be generated and the potential of so much different work to contribute to this endeavour.

How has the way you understand the world changed over time, and what (or who) prompted the most significant shifts in your thinking?

My early work was very much focused on ‘theory’ and on ‘theories’ of IR. The puzzle which motivated me was the question of what ‘theory’ in IR *is* and what it is supposed to do for us. In short, what kind of knowledge are we seeking to generate in IR and how is this most persuasively done?

A key way in which my work has evolved over time is that I have moved away from approaching this question via

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engagement with ‘grand theories’ in IR. In this respect, my own work has probably moved in lock step with the discipline at large where, as I see it, the focus is more on mid-range theories and the application of particular methods and approaches than on the grand theories which were still in vogue when I first studied international relations.

A key discovery, for me, was that work on causation and causal inquiry in IR offered, as it seemed to me, a much more fruitful avenue through which to pursue the epistemological questions I had always been interested in than work on ‘theory’. I was particularly influenced by Hidemi Suganami’s *On the Causes of War* (1996), not just for its content but also for the careful and thoughtful way in which he sought to navigate the tricky conceptual issues this topic raises. I was also strongly influenced by Milja Kurki’s *Causation in International Relations* (2008) and Patrick Jackson’s *The Conduct of Inquiry in International Relations* (2011) where of course, the title of our book nods to both of these, but for me, the key thing was discovering, in Hidemi, someone who was already thinking about things in the way I wanted to.

How would you describe “causal claims” or “causation” in International Relations?

Crudely, my key thought here is that it is probably more helpful to focus on ‘causal claims’ than on ‘causation’. Debates about ‘causation’ tend to be about what causation *is* and, especially, whether it is ‘real’. A key driver of these debates in IR has been a dissatisfaction with the limitations of a so-called ‘Humean’ or ‘idealist’ approach to causal inquiry focused on the search for regularities (or patterns of covariation) and the notion that a shift towards ‘causal realism’ might open new and richer avenues for causal inquiry. However, Hidemi and I are sceptical both about the reading of Hume as a causal idealist and about the idea that in conducting causal inquiry it is necessary either to be an ‘idealist’ or a ‘realist’ about causation. In our view, it is not possible to know whether causation is in fact part of the world as it exists independently of human thought (how could we know that?!) and nor it is necessary. As human beings, we all make sense of the world in part through the idea that it contains causal relations, but as researchers in IR, we can get on with the business of causal inquiry – exploring what we think of as being ‘causal relations’ by developing and evaluating ‘causal claims’ – without having first to resolve fundamentally metaphysical questions.

By contrast, we think there is an enormous benefit to be gained from thinking more carefully about the nature of ‘causal claims’, about how they can be supported by empirical evidence, and about what they do and do not tell us about the world. Adopting this focus is important not only because a key aim of research in IR is to develop causal knowledge, but also because there are some quite pervasive misunderstandings about these issues in IR. In particular, there is a tendency to suppose, wrongly, that statements of the form ‘short circuits cause fires’ or ‘revolutions cause wars’ are in some sense ‘general’ and that if such claims are acceptable then we should be able to observe corresponding patterns of covariation in the world around us – short circuits should be routinely followed by fires and revolutions should be routinely followed by wars. In *Causal Inquiry in International Relations*, we show that statements such as ‘short circuits cause fires’ and ‘revolutions cause wars’ are propensity statements; they tell us that a short circuit will lead to a fire or a revolution will lead to a war *only under the right conditions*. As it turns out, this has some quite profound methodological implications.

In what ways does Scientific Realism (SR) influence mainstream approaches to causal inquiry?

It is worth starting by saying that SR has nothing whatsoever to do with realism in IR. In fact, as Jonathan Joseph, among others, has pointed out, realists such as Waltz are *not* scientific realists! Roughly speaking, to be a Scientific Realist is to believe in the reality – existence – of what is described by our best scientific theories, especially unobservable entities, such as quarks. Put slightly differently: Scientific Realism is a philosophical position to the effect that belief in the reality of whatever is described by our best theories is *required by science*. SR has influenced IR, and the social sciences, mainly as a proposed alternative to positivism/empiricism, which is characterized, from the SR perspective, as ‘instrumentalist’. The basic idea here is that we will not succeed in developing persuasive theories about, for example, the structure of the international system if we treat this structure in an ‘instrumentalist’ fashion and simply say that states behave *as if* it were real. To develop persuasive theories, we need to take seriously the idea that social structures are real and have generative effects. This is roughly the view underpinning

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Alex Wendt's early and influential critique of Waltz.

In relation to causal inquiry in IR, SR is most closely associated with the idea that causal inquiry should seek to identify 'mechanisms'. In *Causal Inquiry in International Relations*, we agree that it is necessary to distinguish sharply between evidence that there is a correlation between two variables and evidence that there is a *causal* relation between them. Another way of putting this might be that there can be no causal relation between events of particular kinds unless there is a 'mechanism' linking them. But we find such language unhelpful; it gives an unjustifiable and sometimes unhelpful 'mechanistic' colouring to our picture of how causal relations unfold. In any case, it is clearly not necessary to embrace SR to make the simple point that 'correlation is not causation' and we are sceptical of whether subscribing to an 'ontology' of powers, mechanisms, and the like adds much to causal inquiry.

How can one establish firm epistemic grounds to provide causal explanations and navigate differing degrees of causal complexity?

The term 'causal explanation' sometimes gets used in quite a broad way. For example, Patrick Jackson, in his new book, *Facts and Explanations in International Studies ... and Beyond* (2024), equates a causal explanation with a recipe for bringing about an outcome of a particular kind. It's also common to hear people talk of 'theories' as containing or providing 'explanations'. In *Causal Inquiry in International Relations*, we defend a narrower but also simpler view: a causal explanation is an account of how an outcome of interest was brought about. The simplest form of such an explanation is the statement '*a* caused *b*'; this statement explains the occurrence of *b* by pointing to one of its causes, *a*.

In order to have firm epistemic grounds on which to advance such an explanation, we need to be sure that *a* did in fact contribute to the bringing about of *b*! Conceptually speaking, this is obvious, but practically speaking it will often be very difficult to achieve. To be sure that *a* contributed to the bringing about of *b* we will have to be sure that *b* was not brought about by a combination of events which does not include *a*. There will often be many such competing explanations to rule out and limited evidence on which to draw. This is why experiments, if they can be deployed, are so powerful: if we can show that the introduction of *a* was the only thing that changed, leading to the occurrence of *b*, then we will be very confident in ruling out competing explanations of how *b* was brought about and hence we can also be very confident in advancing the claim '*a* caused *b*'. Unfortunately, there are many important research questions in IR which are not conducive to experimental investigation. In any case, there is an important difference between establishing, by means of an experiment, that *A*-type events cause *B*-type events and showing, in the specific case of interest, that *b* was caused by *a*.

If this gives some sense of the *practical* difficulties involved in developing epistemically secure causal explanations, there is also a *conceptual* complication which is often overlooked, viz. in evaluating a causal explanation it is necessary to pay attention not only to the epistemic grounds on which it is advanced but also its pragmatic orientation. In other words, it is necessary to consider whether an explanation in fact answers the question to which it is offered as an answer. Suppose you were to ask for an explanation of what caused World War I and someone were to say 'Austria's ultimatum to Serbia following the assassination of Archduke Franz Ferdinand caused the war'. It would be quite reasonable to respond by saying something like 'yes, I know all about the immediate lead up to the war, but I was wondering what the deeper causes were'. This response indicates that the original explanation was inadequate because it was insufficiently detailed or complex.

Notice that in order to think clearly about the importance of 'complexity' in relation to causal explanation it is necessary to distinguish the two quite different issues I have touched on here. One issue concerns how easy it is to show, epistemically, that one event, *a*, caused another event, *b*. This will be easiest in an artificially simplified experimental setting and will typically be much harder in the everyday world where there are multiple simultaneous and interacting causal processes in play, any or all of which may have contributed to the bringing about of *b*. A second issue concerns whether an epistemically justified explanation, '*a* caused *b*', is 'complex enough' to satisfy its audience. Much of the existing discussion of causal explanation and causal complexity in IR fails to distinguish these two issues clearly.

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How would you explain Weber’s ‘ideal types’ and its application to the idea of ‘concerts’ in world politics? What challenges might an ideal type concert face in the context of 21st-century global governance?

I explored ‘concerts’ and ‘concert governance’ as part of a multi-year, multi-national project run out of the Peace Research Institute Frankfurt on security arrangements suited to a multipolar 21st-century world. In my contribution to this project, I drew on Weber’s concept of an ‘ideal type’ to address the challenge of how to define a ‘concert’ given the desire, on the one hand, to learn lessons from the 19th-century Concert of Europe and the obvious fact, on the other hand, that any 21st-century concert arrangement would have to differ considerably from its 19th-century predecessor.

Roughly speaking, a Weberian ‘ideal type’ (as articulated in ‘The “Objectivity” of Knowledge in Social Science and Social Policy’) is a conceptual representation or abstraction which gives expression to the underlying ideas which we draw upon, often implicitly, to classify real-world phenomena. In this case, I was interested in the underlying idea of ‘concert governance’ by reference to which we classify certain actual, historically-situated sets of institutional arrangements as either being or not being, ‘concerts’. The advantage of this approach is that it allows us to think of somewhat different institutional arrangements, in different historical contexts, as sharing a kind of ‘family resemblance’ sufficient to justify labelling them all as ‘concerts’. This, in turn, allows us to scrutinize the ways in which various overlapping sets of institutional arrangements might all facilitate the pursuit of ‘concert governance’ in a 21st-century context.

It follows from this approach that a 21st-century ‘concert’ would not precisely match the ‘ideal type’, but would rather depart from it in ways appropriate to its context. However, thinking in a Weberian vein can allow us to identify some of the generic strengths and weaknesses of ‘concert governance’ and to think through how these might manifest in a 21st-century concert. In brief, the strengths of ‘concert governance’ might be said to include informality and adaptability, the capacity to accommodate great power interests and to satisfy their status aspirations, and the capacity to reduce violent disorder. Its weaknesses would likely include concerns around legitimacy, inclusion, and justice. In short, ‘concert governance’ might be said to value *a certain conception of order over a certain conception of justice*. Arguably, institutions such as the UN General Assembly do the opposite, but then this is also, arguably, what makes them ineffectual!

One of the main aims of your book is to bridge the gap between philosophical debates on causation and methodological practice. How do you see these two areas—philosophical reflection and empirical methodology—interacting, and why is it important to integrate them more effectively in IR scholarship?

One of the problems we seek to address is that philosophical and methodological debates have become quite detached. At the philosophical end, this is because debates about whether those conducting causal inquiry should embrace SR are primarily a question of belief or commitment, not a question of method. At the methodological end, this is because the focus on relatively narrow questions about how particular methods or techniques are best applied in the service of causal inquiry inevitably brackets deeper philosophical questions.

In our book, we have sought to show how precise, philosophically-informed thinking can be productively applied not only to metaphysical questions but also to methodological questions. In particular, we focus attention on the nature of causal statements (or claims – see above) and the question of how different kinds of statements can be empirically supported. We show that although the aim of causal inquiry is typically to establish support for causal claims with broad applicability, such as ‘revolutions cause wars’, in fact, the only kind of causal statement that can be tested against empirical evidence is a case-specific claim about an event or set of events which has already occurred. This helps to reveal what we call a ‘deep logic’ to causal inquiry, a logic which has some significant methodological corollaries. For example, we demonstrate the innate dependence of causal inquiry on knowledge of specific, past events, something that has been entirely missed in existing methodological discussions in IR, which tend to privilege cross-case studies and the application of quantitative methods.

In short, the reason that it is important to integrate philosophical and methodological thinking more closely in IR is

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that doing so can generate important methodological insights. Although our approach is philosophically-informed, we are writing in and for those conducting causal inquiries in IR, not (or not primarily) for philosophers!

You suggest that causal inquiries are not only methodological but also have significant political implications. How can scholars conducting causal research remain reflexive about the political stakes of their findings, especially when those findings might influence policy or global power dynamics?

It is likely that causal inquiries in IR will quite often have political implications insofar as they will reveal how outcomes of particular kinds can potentially be brought about or avoided. Indeed, developing this kind of knowledge is a key purpose of causal inquiry. In this context, part of remaining reflexive is to recognize that while we have an obligation to be scrupulous about how we conduct research and how we report our findings, we have real and important choices to make about which issue to focus on in our research. It is quite proper that such choices should be informed by ethical and political judgements about what is significant and valuable either to us as researchers or to those who may draw on our research.

One of the aims of our book is to demonstrate how a wide range of methodological approaches can be used to generate causal knowledge and to show how causal inquiry can complement normative, interpretive, critical, and historical inquiry. In our view, it is important to recognize the truly wide range of questions that causal inquiry can potentially help to answer and not to restrict our causal investigations only to those questions which are answerable through the application of a particular set of methods. However, it is for individual scholars to make their own judgements about what they wish to study and why.

How can scholars in IR stay ethically aware when their research might have political consequences? Do you think that paying more attention to the politics of causation could change how research is done in the field?

I do not think there is a 'politics of causation', but only a politics to the kind of questions we ask (see above). However, there is perhaps a kind of 'disciplinary politics' around causal inquiry, in which it is convenient both to some scholars who wish to focus causal inquiries around the application of particular methods and to some scholars who reject the very idea of causal inquiry to propagate an overly narrow account of what causal inquiry consists in. Our book follows in the footsteps of both Kurki's *Causation in International Relations* (2008) and Jackson's *The Conduct of Inquiry in International Relations* in rejecting this kind of 'disciplinary politics'.

What is the most important advice you could give to young scholars of International Relations?

Try to find a balance between two important but sometimes competing goods. First, pursuing your intellectual interests fervently, wherever they may lead, and continually fighting to make space for the possibility of thinking differently, or at least independently. Second, seek out a community of scholars working in an area you can contribute to and among whom you can imagine feeling valued and supported.