Review – Causal Inquiry in International Relations

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Causal Inquiry in International Relations
By Adam R. C. Humphreys and Hidemi Suganami
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In *Causal Inquiry in International Relations* (2024), Adam Humphreys and Hidemi Suganami have made an outstanding contribution to ongoing debates about causality in our field and in the social sciences in general.[1] This is a book that will change the way a reader approaches the critical task of asking and answering causal questions, which they define as questions about whether and how an event "contributed to the production, or bringing about, of" another event (p. 14). Their central argument is that causal inquiry "hinges on the relationship between concrete causal statements which do refer to specific events and abstract statements which do not" (p. 128), where the latter statements involve *propensities* for one type of event to produce another under the right circumstances. Along the way, they critique major positions in the philosophy of causation and the practice of causal inference, including Bhaskarian critical realism and what they call "the culture of generalization": "the tendency to prioritise the production of 'general' knowledge over knowledge of 'particular'…facts and events" (p. 4).

Humphreys and Suganami begin their analysis with a detailed reading of David Hume's writings on causality. Hume, in many discussions of causality, is typically presented as a defender of the "regularity view" of causation, according to which causality is nothing but an idea formed in the mind when we see a regular conjunction of events — which collapses the distinction between causation and empirical regularity, and makes the observation of empirical regularity in the world the necessary mark of a causal connection. The authors persuasively show that Hume does not actually advance this view, and that even though they themselves are not Humeans, the version of "Humean causation" that is defended by neopositivists and criticised by scientific realists is a thin caricature (p. 41). This, in turn, sets up the authors' discussion of Roy Bhaskar's argument that causality *must be* something *real* in the world, and not just a projection of the mind onto observable empirical regularities. The authors draw on philosopher Bas Van Fraassen's position of "causal agnosticism" — "acknowledging that causal language is intimately bound up with how we, as human beings, relate to and make sense of the world and our place in it, but denying that it is possible to know whether it latches on to a part of the world existing independently of human thought" (p. 104) — to suggest that it is better to leave metaphysics aside[2] and focus on what we actually *do* when we conduct causal inquiry.

If causality is neither a gloss on empirical regularity nor the manifestation of an unobservable-but-real dispositional property of events, what is it? Humphreys and Suganami argue that we should begin by noticing that there are "two kinds of causal statement: statements which relate to specific events, located in space and time, and statements which abstract from specific events and hence possess a broader applicability" (p. 113). Where a concrete causal statement, which they further refine as an *explanatory* statement — "paradigmatically '*a* caused *b*'" (p. 148) — tells us that some specific event *a* brought about some specific event *b*, an abstract statement instead posits a causal relationship between events of types *A* and *B* "such that, if this relationship unfolds without interference, an *A*-type event will produce a *B*-type event" (p. 126). This is the difference between "my dog Zorri did not go on a walk this morning, and this made her unhappy today" and "not going on morning walks makes dogs unhappy."[3] Distinguishing between the two types of statement is important because it is impossible to provide direct empirical evidence for or against the notion that an abstract causal statement expresses; "not going on morning walks makes dogs unhappy" is not a claim about observable *frequency*, but about *propensity*, and as such says not that *most*

dogs who do not get morning walks are unhappy, but that dogs that do not get morning walks are unhappy *unless something else intervenes to prevent that from happening*. A propensity statement can thus be true even if the evidence is inconclusive, because evidence pertains to concrete explanatory statements, and Zorri not being unhappy despite not being walked on a given morning does not mean that the abstract statement is false.

How, then, do we arrive at abstract propensity statements that are in any sense valid? Humphreys and Suganami argue that the key here is the realisation that explanatory statements imply "corresponding abstract statements." If we know that my dog Zorri's unhappiness is caused by her not going on a walk this morning, that knowledge will "indirectly…support a corresponding causal theory," in this instance, the causal theory that not going on morning walks brings about unhappiness in dogs (p. 139). They further argue that evidence that persuasively supports an explanatory statement must rule out "competing explanatory statements concerning specific events which have occurred, and which are located in time and space" (p. 161). It is not enough to merely rule out *alternative* explanatory statements, as those might be complementary to the explanatory statement in question; instead, evidence must be sought that distinguishes between *rival* explanatory statements that could not both be true at the same time. In this instance, we would have to look for evidence that ruled out causes of Zorri's unhappiness that were incompatible with the lack of a morning walk being the cause, e.g., that a change in her diet produced her unhappiness — which would be ruled out by evidence that her diet did not change this morning, but that she did not get a walk and was unhappy. The key is to find evidence consistent with an explanatory statement that invalidates rival explanatory statements.

The mechanics of finding such evidence can be tricky, and the authors' gold standard for doing so is a randomized controlled trial (or RCT), although they have a distinctive understanding of such experimental research designs as ruling out competing explanatory statements by controlling for their posited causal factors, instead of the common understanding of RCTs as testing hypotheses derived from theories (pp. 162-163).[4] But developing valid abstract causal statements does not require the use of RCTs; the key is developing the "confidence that the evidence supports a particular explanatory statement," which in turn depends on "our confidence that all plausible competing explanatory statements can be rejected" (p. 225), *regardless* of which methods or techniques are employed in to doing. Discussions in the penultimate chapter of the book engage both experimental and quasi-experimental designs along with "process tracing" and historical reasoning based in archives, examining how they all rely on the kind of abductive inference that seeks to provide good reasons "to accept a [concrete] proposition based on how well it explains certain observed facts" (p. 219).

So instead of looking at many dogs who did or did not did or did not get morning walks and ascertaining whether or not they were happy, Humphreys and Suganami would have us look at *concrete* dogs to see if their *concrete* unhappiness can be explained by the lack of a morning walk — a process which involves eliminating rival explanations of their unhappiness *in each case* — and only *then* advancing an abstract causal statement about the relationship between the lack of morning walks and unhappiness. This, in turn, means paying heightened attention to context, and detailing the conditions under which the lack of a morning walk brings about unhappiness.[5] The authors therefore do not recommend eliminating or controlling for context so as to generate the most robust and general estimates of average causal effects, differing here with virtually all of the neopositivist and some of the "qualitative methods" strategies for causal inference. Instead, Humphreys and Suganami insist on the slow and painstaking process of moving from concrete explanations of causes and effects in context to nuanced claims of any broader applicability, without being able to take the shortcut supposedly enabled by empirical generalisation and its associated, mainly quantitative, and formal techniques. Against the "culture of generalization" that advocates such tactics, they advocate a more grounded immersion in case-specific details as the foundation of solid causal inquiry.

This is where I diverge somewhat from the authors' analysis of causality and causal explanation. To my way of thinking — elaborated most recently in *Facts and Explanations in International Studies…and Beyond* (Jackson 2025)—it is far from clear what it would even *mean* to consider an explanatory statement to be purely "concrete" in the first place. The authors do not argue that a causal relationship is directly observable — what we can observe are inputs and outputs, and even the vaunted RCT approach only shows us that the evidence is *consistent with* some *a* bringing about *b* and *inconsistent with* factors other than *a* doing so, rather than giving us causation "directly" — and they, like basically everyone else working on causation who isn't a neopositivist, distinguish sharply between

observed covariations in the world and causation per se. But this means that in the very heart of every "concrete" explanatory statement, there is something "abstract": the very notion that *a* caused *b*, rather than the notion that *a* only preceded *b* or *b* only succeeded *a*. Without this "abstract" notion, all the detail in the world about Zorri's lack of a morning walk cannot possibly be evidence that this "caused" anything.

The problem seems to be that Humphreys and Suganami start their analysis from a hypothetical situation of an investigator operating with *no* causal theories whatsoever, and certainly with no causal theories that the investigator already takes to be valid. While they do suggest that it is possible to generate a causal explanation "by applying a well-supported causal theory," this pathway takes a secondary place in their account, behind the approach of "identifying evidence which directly supports an explanatory statement that is to be offered as an explanation" (p. 168). But this is a highly tendentious account of how we actually operate in producing causal explanations. Whether we are talking about professional scientific researchers or about people in their everyday lives, John Dewey's classic pragmatist analysis in *How We Think* (1933) still rings true to me: people start with habits that have been established and are retained precisely because they work well enough in practice, and when those break down, inquiry begins. We *never* start from zero; we enter a situation with a set of pre-existing, pre-vetted notions which form the basis of our investigation. We are in no way limited to re-stating or confirming those notions, but the point is that we start *from somewhere* and make modifications and improvements accordingly. So, the situation that the authors posit — with an investigator approaching a situation without preconceptions, needing to find indirect evidence for propensity statements that they only generate *after* validating explanatory statements — never occurs.

Applied to causal explanation, Dewey's account gives us a picture that is in some ways distinct from the one that Humphreys and Suganami provide. For the authors, causal explanation is "shaped, in part, by pragmatic considerations" (p. 180 fn 11). In contrast, for Dewey, as for me, explanations including causal explanations are *entirely* pragmatic, responding to particular problem-situations and aiming to resolve particular perplexities. Certainly, causal explanations cannot do so without empirical evidence to support them, but they are best understood as efforts to build on *previously-vetted* propensity statements to build a set of instructions that can resolve the perplexity and solve the problem. The act of explaining and the act of vetting an abstract propensity statement should be kept separate, precisely because we always enter a situation with a pre-existing stock of such propensity statements from which we construct both our novel explanations and potential rivals to them.[6] If we did not have such a pre-existing stock, how would we even begin to determine what rival explanations to seek disconfirming evidence of? And how would we begin to suspect that some factor *caused* a specific outcome, unless we had prior reason to believe that it at least might have?

I very much agree with Humphreys and Suganami that we should keep clear the distinction between causal explanations and the abstract propensity statements that inform them. But if we do not also keep the process of vetting abstract propensity statements separate from the process of explaining specific outcomes, we end up with either a mystery concerning how we ever pass from empirical observations to abstractions, or with a sleight of hand whereby an abstract notion is smuggled from the beginning into what purports to be merely an empirical observation. It is implausible to me that in suggesting that Zorri's unhappiness is caused by her lack of a morning walk, I was not drawing on abstract notions and propensity statements derived from prior experience. Only if we *already* thought that the lack of a morning walk has a propensity to produce an unhappy dog would we ever be able to causally explain Zorri's unhappiness by seeing whether she had a morning walk that day.

All of that said, the practical advice that Humphreys and Suganami offer about looking for empirical evidence to distinguish between rival explanatory statements is solid, despite my disagreements with them about the precise relationship between abstract propensity statements and concrete acts of causal explanation. I heartily concur with their argument that issues of causal inquiry in international studies are not going to be resolved by metaphysical speculation or debate. The methodological aspects of causal inquiry deserve the kind of attention that the authors give them, and the conversation that Humphreys and Suganami are advancing is critical to the evolution of the field. They should be commended for a splendid statement and acknowledged for the richness of the discussion about their analysis that will hopefully ensue.

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[1] By way of full disclosure, I will point out here at the outset that I have been in conversation with the authors about these issues for many years, and that I participated in the workshop leading to their 2017 co-edited special issue of the *Journal of International Relations and Development* on causation in world politics (in which I had an article published). My own recent book, *Facts and Explanations in International Studies…and beyond* (2025), is also in part an outcome of these ongoing conversations.

[2] This is what causal agnosticism does, even as it leaves room for the realist claim about causation being part of the mind-independent world to be potentially true. The agnostic simply does not have to take a position on whether the realist claim is true or not (p. 109).

[3] In this example, *a* is "Zorri did not go on a walk this morning" and *b* is "Zorri is unhappy today" while *A* is "not going on morning walks" and *B* is "unhappy." "Dog"/"dogs" is a scope and domain condition for both claims. The authors specifically point out the importance of the use of different tenses in the two types of causal statement, such that explanatory statements in English are typically in the past simple tense, while abstract causal statements are in the present simple tense (pp. 128-130).

[4] Humphreys and Suganami's understanding of RCTs is thus rooted in what some philosophers call IBE — inference to the best explanation (p. 224) — or what a layperson might refer to as the Sherlock Holmes investigatory approach of eliminating impossibilities in order to arrive at the truth. On parallels and connections between detective fiction and the hunt for valid causal explanations, see (Kern 2006).

[5] Indeed, it may even be better to say that the formula for an abstract causal statement is "A causes B in context C," which has the added advantage of paralleling John Searle's (1995) formula "X counts as Y in context C" for social and institutional facts.

[6] A lot of what we sometimes call "theorizing" in scholarly work is, I would say, a form of "vetting": not the same as conducting a laboratory experiment, but like such an experiment, intended to elucidate the propensity—the causal tendency or power—of some element and render it plausible to the reader.

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