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**Article** in *Review of International Studies* · December 1998

DOI: 10.1017/S0260210598001028

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# On constitution and causation in International Relations

ALEXANDER WENDT\*

Within the community of academic students of international politics today there is a deep epistemological rift over the extent to and ways in which we can know our subject. Speaking very broadly, on one side stand what have become known as 'positivists', who think we can get closer to the truth about international politics, but only if we follow the methods which have proven so successful in the natural sciences. And on the other side stand 'post-positivists',<sup>1</sup> who think we do not have privileged access to the truth about international politics, and least of all through the methods of natural science. Although it can seem far removed, this epistemological disagreement actually matters quite a lot to our collective efforts to make sense of the real world, since we cannot avoid taking some position on it, and those positions affect the questions we ask, the methods we use to answer those questions, and ultimately the kinds of knowledge that we produce. Perhaps for this reason what may seem more a dispute for philosophers than political scientists has become one of the 'Great Debates' (the Third) in the discipline of International Relations (IR), and, indeed, it arguably underlay the 'Second' Debate between behaviouralists and traditionalists in the 1960s as well.<sup>2</sup>

There are many issues in this debate, most of which I will not take up here.<sup>3</sup> What I am interested in is just one, albeit particularly common, way of framing the debate, perhaps best exemplified by Martin Hollis and Steve Smith's rich and thought-provoking book, *Explaining and Understanding International Relations*.<sup>4</sup> Drawing on

\* The author thanks Mlada Bukovansky for helpful comments on a draft of this paper, and Jennifer Mitzen for conversations that helped clarify some key points.

<sup>1</sup> These labels are far from ideal, since in the strict 'logical' sense 'positivism' has been dead in the philosophy of science for decades, and as such *all* contemporary epistemologies are 'post'-positivist. However, they are the terms in which the current debate in International Relations about the epistemological status of social science is being carried out and as such I reluctantly adopt them here.

<sup>2</sup> For a good overview of the Third Debate, see Yosef Lapid, 'The Third Debate: On the Prospects of International Theory in a Post-positivist Era', *International Studies Quarterly*, 33 (1989), pp. 235–54, and the subsequent commentaries in the same journal. On the Second Debate see, for example, Hedley Bull, 'International theory: The Case for a Classical Approach', in K. Knorr and J. Rosenau (eds.), *Contending Approaches to International Politics* (Princeton, NJ: 1969), and Morton Kaplan, 'The New Great Debate: Traditionalism vs. Science in International Relations', *Ibid.*

<sup>3</sup> For a broader exploration, see 'Scientific Realism and Social Kinds', ch. 2 in Wendt, *Social Theory of International Politics* (Cambridge, 1999).

<sup>4</sup> Martin Hollis and Steve Smith, *Explaining and Understanding International Relations* (Oxford, 1990). The author addressed the ontological aspects of this book in 'Bridging the Theory/Metatheory Gap in International Relations', *Review of International Studies*, 17 (1991), pp. 383–92, which was followed by a reply from Hollis and Smith, 'Beware of Gurus: Structure and Action in International Relations', *Ibid.*, pp. 393–410, and a further exchange in 1992. Here I take up its epistemological aspects, which I had set aside in my 1991 review.

a long tradition in the philosophy of social science, Hollis and Smith argue that social scientists face a basic choice between two approaches to or 'stories' about their subject matter. 'One story is an outsider's, told in the manner of a natural scientist seeking to explain the workings of nature and treating the human realm as part of nature.'<sup>5</sup> The goal of this story is to find causal mechanisms and social laws. This has become known as 'Explaining', and is usually identified with a positivist approach to IR. The other story 'is an insider's, told so as to make us understand what the events mean, in a sense distinct from any meaning found in unearthing the laws of nature'.<sup>6</sup> The goal of this story is to recover the individual and shared meanings that motivated actors to do what they did. Known as 'Understanding', this is usually identified with post-positivism.

Hollis and Smith's intent is pluralistic, since they emphasize that neither story can be reduced to the other and that both are valuable. 'There are always two stories to tell' about international politics. Yet contained within the 'Explanation and Understanding' frame are the seeds of a more conflictual, zero-sum view, which has gripped at least a significant part of the Third Debate. The seeds of conflict lie in Hollis and Smith's assumption, shared with many positivists and post-positivists alike, that natural science is characterized by the outsider's focus on causal explanation, and does *not* include the kind of intellectual activity associated with the insider's focus on actors' understandings. Since the natural sciences constitute our model for 'science', this assumption suggests that the choice facing social scientists is not between two ways of knowing, both seen as part of the scientific enterprise, but between science (as outsider story) and non-science. Given the interest of positivist IR scholars in establishing the epistemic authority of their work as Science, this choice leads them to emphasize that the overriding goal of IR must be Explanation and only Explanation. And given the interest of post-positivist IR scholars in Understanding, this leads them to reject characterizations of their work as science, and some even to reject the possibility as well of Explanation in social inquiry. The belief that the distinction between Explanation and Understanding is one between science and non-science, in other words, is a recipe for the kind of epistemological 'paradigm wars' that have riven the field in the past decade.

On the surface the assumption that natural scientists do not engage in Understanding seems to make sense, since the objects of natural science are not intentional beings and as such not capable of having 'understandings' in the first place. However, in this article I suggest that despite this important ontological difference between the objects of natural and social inquiry, there is no fundamental epistemological difference between the natural and social sciences. The intellectual activities associated with Explanation and Understanding *both*, are, and should be, practised in *both* domains. To the extent that 'Explanation and Understanding' is equated with 'science and non-science', therefore, it is misleading and encourages unnecessarily zero-sum arguments about epistemology. These arguments can only lead to the impoverishment of our collective efforts to make sense of international politics and, given the disciplinary dominance in IR of Explainers, to the professional marginalization of Understanders.

<sup>5</sup> *Explaining and Understanding International Relations*, p. 1.

<sup>6</sup> *Ibid.*

In my view, the essence of the insider's story is not a focus on actors' understandings, but an interest in a particular kind of *question*. That question, speaking broadly for the moment, is 'how are things in the world put together so that they have the properties that they do?'. This question transcends the natural/social science divide; it is routinely asked by researchers in both domains. But the ways in which they answer it must differ because the objects of their respective inquiries are made of different kinds of stuff. Things in the social world (what philosophers call 'social kinds'), like money, the state, and international society, are made largely of ideas. This means that if we are interested in the question of how social kinds are put together we will have to engage in an interpretive recovery of actors' private and shared beliefs, just as proponents of Understanding have long argued. However, the same question can be asked, and routinely *is* asked, of 'natural kinds' like dogs, elm trees, and DNA. Natural kinds are made of physical substances, not ideas, which means that if we want to know how *they* are put together we will have to study their genetic, chemical, or other material structure. This shows that an interest in actors' beliefs is not intrinsic to the insider's search for Understanding, but contingent on the nature of the properties by which the objects of this activity in social science are constituted. Put another way, the Third Debate has tended to conflate two distinct issues—what things are made of (ontology) and what questions we should ask (part of epistemology). Disentangling these issues enables us to see that Explanation and Understanding are not only not mutually exclusive, but mutually implicating.

If Explanation and Understanding are distinguished not by science vs. non-science but by the kinds of questions that they are asking, then we need some way of characterizing those questions. There are different ways to do this, but I shall take Explainers to be asking 'causal' questions, and following David Sylvan, to whom much of my thinking on this score is indebted,<sup>7</sup> Understanders to be asking 'constitutive' ones. The differences between these two kinds of question are not well understood, and they are often conflated; it is increasingly common in IR scholarship, for example, to see the phrase 'mutually constitutive' used to describe relationships that are in fact causal, and relationships that are in fact mutually constitutive described in the causal language of 'interaction'. In the first half of this article I explore the differences between causal and constitutive questions, with particular reference to the latter since causal questions are relatively well understood.

A question-oriented approach to the Third Debate in one sense leaves us in a position similar to that advocated by Gary King, Robert Keohane, and Sidney Verba in *Designing Social Inquiry*, which has rapidly become a canonical 'positivist' text on social scientific method.<sup>8</sup> King, Keohane, and Verba interpret the distinction

<sup>7</sup> First as a student of his in graduate school and then through several unpublished manuscripts on the logic of constitutive inquiry which he co-authored with Stephen Majeski, most recently 'Modeling Theories of Constitutive Relations in Politics' (1998); for an earlier approach, see David Sylvan and Barry Glassner, *A Rationalist Methodology for the Social Sciences* (Oxford: Oxford University Press, 1985). Two other key sources for thinking about constitutive theory are Friedrich Kratochwil, *Rules, Norms and Decisions* (Cambridge, 1989) and Nicholas Onuf, *World of Our Making* (Columbia, SC, 1989). And although using other terminology, much the same kind of argument has long been advanced by Hayward Alker; see *Rediscoveries and Reformulations* (Cambridge, 1996). Use of the term 'constitutive' to describe this kind of theory seems to be gaining ground, as evidenced by Steve Smith's 'The Self-images of a Discipline: A Genealogy of International Relations Theory', in K. Booth and S. Smith (eds.), *International Relations Theory Today* (Cambridge, 1995), pp. 26–8.

<sup>8</sup> Gary King, Robert Keohane, and Sidney Verba, *Designing Social Inquiry* (Princeton, 1994).

between Explanation and Understanding as one between ‘causal inference’ and ‘descriptive inference’, which although not put in these terms seems equivalent to saying that the two kinds of inference answer different questions. An important virtue of their book is its emphasis on the fact that descriptive inference is a crucial part of science, both natural and social, which traditional positivists often neglect, and as such like Hollis and Smith, their view attempts to be methodologically inclusive and pluralist. But they specifically reject as ‘confusing’ the idea that the non-causal inquiries associated with Understanding might be ‘explanatory’, which means that for them, as for Hollis and Smith, Explanation equals causal inference.<sup>9</sup> This I think understates the contribution of the ‘insider’ approach to knowledge, and—albeit unintentionally—both reinforces its second-class status in social science and contributes to the reification of the social world. The distinction between Explanation and Understanding is not one between explanation and description, but between explanations that answer different kinds of question, causal and constitutive. In the second half of this article I attempt to dispel King, Keohane, and Verba’s confusion by clarifying the explanatory role of constitutive theory and showing that this matters for how we think about international politics.

### Causal and Constitutive Theories

In the philosophy of science a common way to characterize the differences between kinds of explanations is in terms of the kinds of questions which they answer.<sup>10</sup> It seems useful to apply this approach to the distinction between causal and constitutive explanations.

Causal theories answer questions of the form ‘why?’ and, in some cases, ‘how?’. ‘Why did the Protestant Reformation occur?’, ‘why did Gorbachev move to end the Cold War?’, ‘why does the sun rise in the east?’, and ‘why does salt corrode metal?’ are all questions about the causes of things. Certain how-questions are also inquiries into causes: ‘how did the Germans conquer France in 1940?’, ‘how did Christianity displace paganism?’, ‘how are babies made?’, and ‘how does the AIDS virus work?’. Causal researchers disagree among themselves about the importance or distinctiveness of how-questions, which may to some extent reflect competing conceptions of causality. Those who subscribe to the logical empiricist view that we have explained something causally only when we have subsumed it deductively under a law tend to concentrate on why-questions, while those who subscribe to the scientific realist view that we have explained something causally only when we have described the mechanisms or process by which it is generated tend to attach more importance to how-questions.<sup>11</sup> These disagreements are important, but for present purposes they can be downplayed because they are within the family of requests for causal explanations, and so I shall not pursue them further here.

<sup>9</sup> *Ibid.*, p. 75, footnote 1.

<sup>10</sup> See, for example, Charles Cross, ‘Explanation and the Theory of Questions’, *Erkenntnis*, 34 (1991), pp. 237–60.

<sup>11</sup> For discussion of these competing views of causal explanation, see Russell Keat and John Urry, *Social Theory as Science*, 2nd edn (London, 1982), and Ernan McMullin, ‘Two Ideals of Explanation in Natural Science’, in P. French, *et al.* (eds.), *Midwest Studies in Philosophy*, vol. 9 (Minneapolis, MN, 1984), pp. 205–20.

In providing answers to causal questions, in saying that 'X causes Y', we assume three things: 1) that X and Y exist independent of each other, 2) that X precedes Y in time, and 3) that but for X, Y would not have occurred. The first two conditions should not pose anything more than a conceptual problem for the causal researcher and as such do not generally receive much attention. It is essential that the effort to establish causal connections make sure that X and Y are independently existing and temporally separated, but having done so the causal researcher's main problem is to satisfy the third, counterfactual condition, which involves distinguishing causation from mere correlation, or necessary from accidental association. This is no easy task, since our inability to experience causation directly means that 'we can never hope to know a causal effect for certain',<sup>12</sup> and as a result much methodological advice has been devoted to improving the quality of the inferences that we inevitably have to make. But in thinking about the difference between causal and constitutive questions it is the first two assumptions which are crucial, since constitutive stories do not make them. These assumptions reflect the central objective of causal stories, which is to explain *changes* in the state of some variable or system. To highlight this objective, Robert Cummins refers to causal theories as 'transition' theories.<sup>13</sup> In order to explain transitions it is necessary that the factors to which we appeal be independent of and temporally prior to the transitions themselves; hence the terminology of 'independent' and 'dependent' variables that is often used in causal theorizing.

Constitutive theories have a different objective, which is to account for the properties of things by reference to the structures in virtue of which they exist. Cummins calls such theories 'property' theories.<sup>14</sup> Unlike transition theories, which explain events through time, property theories are static. Their goal is to show how the properties of a system are constituted. The systems whose properties they explain may be dynamic, and indeed *all* systems, natural and social, are always in process, continually being reproduced through time even if they do not change. But constitutive theories abstract away from these processes and take 'snapshots' instead, in an effort to explain how systems are constituted.

As such, constitutive questions usually take the form of 'how-possible?' or 'what?'. 'How was it possible for Stalin, a single individual, to exercise so much power over the Soviet people?' 'How is it possible for Luxembourg to survive in an anarchic world next door to Great Powers like France and Germany?' 'How is it possible for a gas to have a temperature?'<sup>15</sup> And 'how is it possible for the Earth to keep the moon in its orbit?' are all requests for information about the conditions of possibility for natural and social kinds. A related logic underlies what-questions: 'What kind of political system is the European Union?'. 'Was Serbian behaviour during the Bosnian Civil War "genocide"?'. 'What are comets made of?'. And 'what is ball lightning?'. What we seek in asking these questions is insight into what it is that instantiates some phenomenon, not why that phenomenon comes about.

Adequate answers to how-possible and what-questions must satisfy different truth conditions than answers to causal questions. As with the latter, the answers to constitutive questions must support a counterfactual claim of necessity, namely that

<sup>12</sup> King, Keohane, and Verba, *Designing Social Inquiry*, p. 79.

<sup>13</sup> Robert Cummins, *The Nature of Psychological Explanation* (Cambridge, MA, 1983).

<sup>14</sup> *Ibid.*, pp. 14–22.

<sup>15</sup> The example is Cummins', *ibid.*, p. 15.

in the absence of the structures to which we are appealing the properties in question would not exist. But the kind of necessity required here is conceptual or logical, not causal or natural. The relationship between the factors constituting the social kind 'Cold War' and a Cold War is one of *identity*, in the sense that those factors define what a Cold War *is*, not one of causal determination. And this in turn means that the answers to constitutive questions will necessarily violate the first two assumptions of causal explanations, independent existence and temporal asymmetry. The factors constituting a Cold War do not exist apart from a Cold War, nor do they precede it in time; when they come into being, a Cold War comes into being with them, by definition and at the same time. This means that the 'independent variable/dependent variable' language that characterizes causal inquiries makes no sense, or at least must be interpreted very differently, in constitutive inquiries.<sup>16</sup> The effects of constitutive structures might be said to 'vary' with their constituting conditions, but the dependency reflected in this variation is conceptual rather than causal. When constituting conditions vary, then so do their constitutive effects, *by definition*.

However, although they have different truth conditions, this does not mean that causal and constitutive theories imply different 'epistemologies'. Both kinds of theory are true or false in virtue of how well they correspond to states of the world. This is perhaps more obvious in the case of causal theories: a theory of what caused the Cold War which made no mention of competing ideologies, mutual distrust, and rough material parity is unlikely to be true. But correspondence to states of the world is also an important goal for constitutive theories: a theory of how the state is constituted that made no mention of sovereignty, territoriality, or a monopoly on the legitimate use of organized violence would not reflect what are in fact the conditions of possibility for that social kind, no matter how satisfying such a theory might be on other grounds (internal coherence, parsimony, political correctness, or whatever). Not all constitutive theories are true any more than all causal theories are true, and the test for both is ultimately their relationship to how the world works.

Post-positivists might object that a correspondence test of truth for constitutive theories is problematic because human beings do not have direct access to how the world is constituted. What we see in the world is always and necessarily mediated by the background understandings we bring to bear on our inquiries. I agree that all observation is theory-laden, and this means that we can never test our theories directly against the world, but only indirectly via other, competing theories. But this is equally true of causal and constitutive theories (and, note, it also compatible with what is today called 'positivism' in IR). Moreover, for both kinds of theory the 'scientific' solution to the problem is the same, namely to rely on publicly available, albeit always theory-laden, evidence from the world, which critics of our theoretical claims can assess for relevance, accuracy, and so on. The importance of such evidence is not lost even on the most hardened post-positivists who, while perhaps overtly rejecting the correspondence theory of truth, in their empirical work routinely try to find accurate and relevant evidence from the world to support their claims. Why limit themselves in this way if the external world does not somehow set the truth conditions for their theories? Why not be creative and choose arbitrary 'evidence'? One answer may be that post-positivists at least implicitly realize that

<sup>16</sup> For a good discussion of how behavioural methods and language relate to constitutive theorizing, see David Sylvan and Barry Glassner, 'Is Statistical Inference Appropriate to Structuralism?', *Quality and Quantity*, 17 (1983), pp. 69–86.

they would not be saying anything interesting about the external world, even if they might still be saying interesting things in the form of art, poetry, or revelation. At the end of the day, in other words, most post-positivists are ‘tacit realists’ (a form of positivism) in their empirical research, which is to say that they are guided by the desire to make their theories correspond to how the world works.<sup>17</sup>

Doubting that the truth of constitutive theories depends on correspondence to the world is one road that post-positivists take to get to the conclusion that their work requires a different epistemology than causal theorizing. However, there is another pathway to this conclusion as well. This road starts out by highlighting the ontological differences in what the natural and social worlds are made of—physical substances and ideas respectively—and then associates materialist ontologies (and thus natural science) with causal theories and idealist ontologies (social science) with constitutive theories. If this association makes sense then that might imply different epistemologies for the natural and social sciences, because the ways in which we would ‘know’ whether causal and constitutive theories are true would be radically different in the two domains, observing physical facts on the one hand and interpreting meanings on the other.

It is certainly the case that the natural and social worlds are at least in part made of different kinds of stuff, and that these ontological differences require different methods and data for their study. We simply can’t study ideas in exactly the same way that we study physical facts because ideas are not the kinds of phenomena that are even indirectly observable. However, this does not imply different epistemologies for the natural and social sciences, since it is wrong to think that material conditions imply causal theorizing and ideas imply constitutive theorizing. Both kinds of stuff have both causal and constitutive effects. Ideas have constitutive effects insofar as they make social kinds possible; masters and slaves do not exist apart from the shared understandings that constitute their identities as such. But those shared understandings also have causal effects on masters and slaves, functioning as independently existing and temporally prior mechanisms motivating and generating their behaviour.<sup>18</sup> The same dual role is found in the case of physical substances. The human brain is a causal mechanism generating intelligent behaviour, but it is also a condition of possibility for being human. Indeed, some of the most important theories in the natural sciences are constitutive rather than causal: the double-helix model of DNA, the kinetic theory of heat, and so on.<sup>19</sup>

The paired examples provided above of causal and constitutive questions from the natural and social sciences show that the differences between those questions cannot be reduced to the differences between physical substances and ideas. Both

<sup>17</sup> The phrase ‘tacit realism’ is Mario Bunge’s in ‘Realism and Antirealism in Social Science’, *Theory and Decision*, 35 (1993), pp. 207–35. A similar argument is made by Linda Alcoff about Foucault’s work in ‘Foucault as Epistemologist’, *The Philosophical Forum*, 25 (1993), pp. 95–124.

<sup>18</sup> For perhaps the definitive statement of how ideas, in the form of reasons, can have causal effects on behaviour see Donald Davidson, ‘Actions, Reasons, and Causes’, *Journal of Philosophy*, 60 (1963), pp. 685–700. In saying that reasons can be causes I am taking one side in a debate about what remains a controversial issue; for an opposing, Wittgensteinian, view see David Rubenstein, ‘The Concept of Action in the Social Sciences’, *Journal for the Theory of Social Behaviour*, 7 (1977), pp. 209–36. On the causal role of ideas in international politics, see especially Judith Goldstein and Robert Keohane (eds.), *Ideas and Foreign Policy* (Ithaca, NY, 1993).

<sup>19</sup> See John Haugeland, ‘The Nature and Plausibility of Cognitivism’, *The Behavioral and Brain Sciences*, 2 (1978), p. 216, and Cummins, 1983, p. 15.



kinds of question get asked in both domains. Although the kinds of evidence that we need to answer these questions in the two domains may be different, there is no essential difference in the questions themselves; they are isomorphic. Things get caused in society just as much as things get constituted in nature.

### **Constitutive Theory as Explanation**

In *Designing Social Inquiry*, King, Keohane, and Verba accept that Understanding or constitutive theory is a distinct intellectual activity, but consider that activity to be descriptive inference. Thinking about constitutive theory in this way is in some respects attractive, since constitutive analyses do have a substantial descriptive component. Also, by emphasizing inference, King, Keohane, and Verba highlight the fact that description is not just a matter of reporting observations, but requires theory to unite and make sense of them. In the end, however, I think this interpretation of constitutive theory falls short. Constitutive theories provide *explanations*. These explanations are not causal, but they are explanations just the same. The burden of this argument, therefore, must be to dispel the ‘confusion’ that King, Keohane, and Verba feel attends the idea of non-causal explanation. Before trying to do so, however, it may be useful to say something about why we should care about what seems to be merely an issue of semantics.

At least two things are at stake. One is rhetorical but has real consequences for the sociology of knowledge. Despite King, Keohane, and Verba’s call to social scientists to treat descriptive inference as an important end in itself, in disciplines worried about their epistemic status as ‘science’ the incentive to distinguish one’s work as explanatory, and to distinguish it in particular from history—often seen by social scientists as being ‘mere’ description—will be a powerful force. As long as such worries exist, scholars, and perhaps especially younger ones, will face strong disciplinary pressures *not* to treat descriptive inference as an end in itself, but to go ‘beyond’ description to causal inference. The latter is also important, of course, and gives us insight into the world that description does not. But in the social sciences today the connotations of having one’s work characterized as ‘descriptive’ are so negative that almost all scholars want to be seen as engaging in ‘explanation’, as even a cursory survey of most dissertations and our leading journals will attest. Even if coupled with an effort to change these perceptions, therefore, at least in the short run treating constitutive theory as descriptive inference will inevitably have the effect of reinforcing the prejudice that it is second-best, inferior, and not fully ‘science’.

That the rhetoric of descriptive inference might contribute to the marginalization of constitutive theory may be unfortunate, but that doesn’t necessarily mean it is misleading with respect to our substantive inquiries. The other issue at stake here centres on that possibility, and specifically the kind of knowledge that constitutive theory generates and thus the uses to which it can be put. Treating constitutive theory as descriptive rather than explanatory contributes to the ‘reification’ or ‘naturalization’ of social kinds, in the sense that it obscures the extent to which they are ongoing social constructions and encourages us instead to see them as if they were like natural kinds, the character of which is independent of what human beings

think and do.<sup>20</sup> The language of descriptive inference does this by the way in which it implicitly characterizes the relationship between intersubjective understandings—shared ideas—and the social kinds they constitute: in saying that ideas ‘describe’ the Cold War we do not get a sense that they generated or produced the Cold War, that but for certain shared ideas the Cold War would not have existed, in short, that they ‘explained’ the Cold War.

Consider the effects of such an assumption. If shared ideas do not explain the Cold War, then policymakers could not end the Cold War by changing their ideas. The Cold War becomes seen as something external to how they think, in the same way that rocks and trees are external, and as such can only be dealt with in the manner in which we deal with nature, i.e. through the manipulation and control of objects for whose dispositions we are not ourselves responsible. Such reification in fact characterized the Cold War for many years; each side thought the conflict was caused by the intrinsically aggressive, implacably hostile nature of the Other, not by the behaviour of the Self. Their own ideas about the conflict were therefore nothing more than necessary reflections of an externally existing fact, not constitutive of that fact in the first place. Social science and specifically the language of descriptive inference were of course not responsible for this situation, but to the extent that social scientists took the existence of the shared ideas constituting the Cold War as given, failing to problematize the role of those ideas in generating the conflict, social scientists participated in the naturalization of the Cold War and by extension were not helping to empower policymakers to end it, just to manage it. It took the ‘New Thinking’ of the Gorbachev regime, with its realization—prompted, to be sure, by changing material conditions—that the Soviets’ own behaviour helped sustain the Cold War, to get the two sides out from under the seemingly inexorable, externally imposed logic of their situation. Such a realization, embodying an attitude of ‘reflexivity’ rather than reification toward social kinds, is more likely if constitutive theory is seen as explanatory than merely descriptive.

This example gives us a sense of what is at stake in how we interpret constitutive theory, and also a suggestion of how constitutive theory might be seen as explanatory, but more needs to be said about the nature of this kind of explanation, and especially about its non-causal character. To do so I shall deal separately with the two characteristic questions of constitutive inquiries—what? and how-possible?—since their answers explain in somewhat different ways.

What-questions seem on the surface to be wholly concerned with description. When a small child asks her parent, ‘what’s that?’, she might be satisfied with the response, ‘a dog’. That answer doesn’t seem to involve much in the way of ‘explanation’. If the child persists and asks ‘what’s a dog?’, the parent may say ‘man’s best friend’. This too seems descriptive, since it doesn’t seem to do more than point to the role that dogs play in human society, although it also sends the implicit message to the child that dogs are not dangerous. Yet, the English language permits an explanatory interpretation of the parent’s answer as well: it would be equally valid to say that the parent was ‘explaining’ the role of dogs in society, and ‘explain-

<sup>20</sup> On this conception of reification, see Douglas Maynard and Thomas Wilson, ‘On the Reification of Social Structure’, in S. McNall and G. Howe (eds.), *Current Perspectives in Social Theory*, vol. 1 (Greenwich, CT, 1980), pp. 287–322, and also Peter Berger and Thomas Luckmann, *The Social Construction of Reality* (New York, 1966), p. 89.

ing' the fact that dogs are not dangerous by reference to their being our friends. Neither of these explanations is causal. Semantics perhaps, but it suggests that already at this very simple level the answers to what-questions might not be purely or unambiguously descriptive, but have varying degrees of explanatory content.

The explanatory potential of what-questions becomes clearer when dealing with unobservables. If we ask an astronomer, 'what's that dark spot in the telescope around which stars swirl in a downward spiral?', she might say 'a black hole'. In addition to its descriptive qualities, this answer explains in the sense that it accounts for certain capacities (to absorb light, suck in stars, etc.) by positing a structure in virtue of which those capacities exist. Once we have classified the dark spot as a black hole, in other words, we have made an *inference* that makes sense of phenomena. An emphasis on the inferential quality of some descriptions is one of the important contributions of *Designing Social Inquiry*. Yet, inferences are always based on *theories*, and one of the essential features of theories seems to be that they 'explain'. Granted, it does not violate any grammatical rules to say that 'theories describe', but this is not how we normally use the term; normally we say that theories 'explain'. Again, perhaps only semantics, but I am trying to build a case here that, at least on an intuitive level, non-causal explanation may not be as confusing as King, Keohane, and Verba think.

These examples could be multiplied indefinitely with social kinds. 'What was the mass of disparate interactions between the U.S. and Soviet Union between 1947 and 1989?' Answer: 'an instance of the social kind known as Cold War'. 'What are the behaviours of the individuals at the border crossing who are going through my luggage?' Answer: 'the legitimate actions of the state'. 'What was the downturn in Western economic activity from 1929 to 1933?' Answer: 'the Great Depression'. In each case the answer to our question is in part descriptive, since our disparate observations are subsumed within it, but it is also explanatory, since it *classifies* those observations as a such-and-such and *unifies* them as parts within some coherent whole.

This combination of classification and unification is characteristic of what William Dray, a philosopher of history, called 'explanations-what'.<sup>21</sup> Explanations-what explain by subsuming observations under a *concept*—as opposed to a law, as in the logical empiricist model of causal explanation—and as such they are also sometimes and perhaps less awkwardly called 'explanations by concept'. In a recent review and extension of Dray's idea, Steven Rappaport points out that unification is increasingly seen as a key, distinguishing feature of scientific explanation more generally.<sup>22</sup> He quotes Wesley Salmon's discussion of the Newtonian synthesis in physics, which Salmon thinks is explanatory in part precisely because it unified a variety of otherwise discrete laws and regularities under more general laws.<sup>23</sup>

<sup>21</sup> William Dray, "Explaining what" in history', in P. Gardiner (ed.), *Theories of History* (Glencoe, IL, 1959), and *Philosophy of History* (Englewood Cliffs, NJ, 1964). For a contemporary critique of Dray's position, see Richard Reiner, 'Necessary conditions and explaining how-possibly', *The Philosophical Quarterly*, 43 (1993), pp. 58–69.

<sup>22</sup> Steven Rappaport, 'Economic Models and Historical Explanation', *Philosophy of the Social Sciences*, 25 (1995), pp. 421–41. For a good overview of the role of unification in scientific explanation more generally see Philip Kitcher, 'Explanatory Unification and the Causal Structure of the World', in P. Kitcher and W. Salmon (eds.), *Minnesota Studies in the Philosophy of Science*, vol. 13 (Minneapolis, MN, 1989), pp. 410–505.

<sup>23</sup> *Ibid.*, pp. 430–1.

Although Salmon is talking about causal explanations, Rappaport argues that the same principle of achieving explanatory power through unification applies by analogy to explanations by concept.

An interesting illustration of how this principle functions in IR concerns the current debate about ‘what is the European Union?’. Is it an emerging federation? An international state? A post-modern state? A confederal consociation? An international regime? Governance without government? Neo-Medievalism? Each of these proposals is an attempt to classify and unify a diverse and complex set of phenomena under a single concept. The concepts are partly descriptive, but they seek to be explanatory as well; indeed it is hard to imagine that people would care so much about what the EU is if the answer to the question were *not* explanatory. Yet the kinds of explanations which these concepts offer are constitutive, not causal. Each tries to make sense of the properties of the EU and in so doing provide insight into its dispositions. Dispositions are propensities to behave in certain ways under certain conditions. If the EU is an emerging federation then it will have a disposition to centralize authority; if it is an international regime then it will have a disposition to preserve the sovereignty of states; if it is a neo-Medieval structure then it will have a disposition to disperse and duplicate authority; and so on. These claims tell us something about the EU’s ‘laws of motion’, but for our purposes here a key feature of dispositions is that if the relevant activating conditions are not present then they will not be actualized in behaviour. Salt has the disposition to dissolve in water, for example, but if it is not placed in water then it will not dissolve. When dispositions *are* actualized then we are in the domain of causal rather than constitutive theories, since then we are dealing with changes of state—‘transitions’ rather than ‘properties’. Gaining explanatory leverage on transitions is of course often one of the main reasons that we try to explain properties, and it is here that causal and constitutive theories abut one another, but that does not mean that the two kinds of theory are equivalent. Explaining dispositions is one of the main objectives of explanations by concept, and is worth doing even if those dispositions have not been manifested in a given case.<sup>24</sup>

Explanations by concept pervade the social sciences. Dray argues that they, rather than causal explanations, are the dominant mode of explanation in history. Interestingly, given the positivist sensibilities of most economists, Rappaport argues that much of what economics offers is actually also explanations by concept, since much of economic theory—and by extension rational choice theory in IR—is concerned *not* with finding lawlike generalizations or testing hypotheses against data, the activities we normally associate with science, but with investigating the properties of *models*.<sup>25</sup> Those models are thought to ‘explain’ insofar as they capture the properties and dispositions of the systems they represent, even if they do not relate effects to independently existing causes. Turning to psychology, although he does not use Dray’s terminology, Cummins argues that many of the theories in that discipline are best seen as property theories rather than transition theories. And as the examples above indicate, explanations by concept are also common in IR. ‘Explanations-what’ are not the *only* kinds of explanations found in these disciplines, nor am I arguing that they should replace causal explanations. The point, rather, is

<sup>24</sup> Cummins, p. 18.

<sup>25</sup> Rappaport, ‘Economic Models and Historical Explanation’.

simply that answering what-questions should be recognized as a valuable and distinct kind of theorizing in its own right, and that, properly understood, it can have explanatory as well as descriptive pay-off.

Sometimes an answer to a what-question will offer a satisfactory constitutive explanation by itself. But other times we may want to know more about the structure in virtue of which a social or natural kind has certain properties or dispositions, to know how it is possible. Answers to how-possible questions explain not by telling us how or why a thing came about, or what it is, but by telling us how its elements are composed and organized so that it has the properties that it does. John Haugeland calls these 'morphological' explanations,<sup>26</sup> and they come in two forms, reflecting the two kinds of structures, internal and social, that can constitute kinds.

By an internal structure I mean the structure of a thing as such. Water is constituted internally by the atomic structure H<sub>2</sub>O; human beings are constituted internally by their genetic structures; doctors are constituted internally by the self-understandings necessary for someone to play the role of 'doctor'; states are constituted internally by organizational structures that give them a territorial monopoly on organized violence. In each case, appeals to internal structures do not explain the properties associated with them in a causal sense, because structure and properties are not independently existing, and those appeals also contain a substantial descriptive element. But they are nevertheless more than descriptions, because the analyst is using one set of facts (the nature and organization of genes, the character of self-understandings, etc.) to account for another (being human, being a doctor, etc.). This something 'more' is the sense in which they are constitutive explanations.

When we account for a thing by referring to its internal morphology we are engaged in what might be called 'reductionism'<sup>27</sup> or 'essentialism'. We are hypothesizing an internal core or essence to which a thing's outward properties can in some sense be reduced. In the natural sciences this is the dominant, perhaps even sole, way to answer how-possible questions, since it is by internal structures that natural kinds are in fact constituted; few, if any, natural kinds are constituted by relationships to other entities. As such, it may be that constitutive explanations in the natural sciences are inevitably essentialist.<sup>28</sup>

Essentialism is more problematic in the social sciences. Some social scientists seek constitutive explanations in the essentialist, natural science manner by reducing the properties of social kinds to their internal structures. On this view, usually identified with the doctrine of methodological individualism, the properties of social kinds like gender or the state should be reducible to the attributes and interactions of independently existing agents. The properties of those agents, like identities and interests, might in turn be caused by their relationships with other agents (in a process of socialization for example), but they are not constituted by them. If that

<sup>26</sup> Haugeland, 'The Nature and Plausibility', pp. 215–26.

<sup>27</sup> This usage is different to Kenneth Waltz's in *Theory of International Politics*, but compatible with the idea of 'structural' explanation. See Ernan McMullin, 'Structural Explanation', *American Philosophical Quarterly*, 15 (1978), pp. 139–47.

<sup>28</sup> See Jarrett Leplin, 'Is Essentialism Unscientific?', *Philosophy of Science*, 55 (1988), pp. 493–510. For doubts about the claim that an essentialist strategy is necessary in the natural sciences see Paul Teller, 'Relational Holism and Quantum Mechanics', *British Journal for the Philosophy of Science*, 37 (1986), pp. 71–81, and for a thoughtful discussion of essentialism in the social sciences, see Andrew Sayer, 'Essentialism, Social Constructionism, and Beyond', *Sociological Review*, 45 (1997), 453–87.

were true then the *only* type of constitutive structure we would need to explicate to answer how-possible questions about a social kind would be its internal structure.

It is beyond the scope of this article to criticize methodological individualism.<sup>29</sup> Suffice it to say that there are theoretical reasons to doubt that social kinds can always be reduced to their internal structures, and political reasons to worry that the effort to do so will obscure the role, and therefore responsibility, of society in making social kinds what they are. Consider the social kind known as 'rogue state'. Part of what makes states rogues is their rejectionist attitude toward the norms of the international community; to that extent states are constituted as rogues by their internal structure. But rogue states are also constituted by social relations to other states in the form of the representational practices of the international community (and of the Great Powers in particular). This opens the door to the possibility that one state may violate the norms of the international community without being constituted as a rogue (Israel?), while another state doing so is (Syria?)—with all the differential consequences for the life chances of these states and their populations that follow. Some scholars might take this as evidence for the proposition that *any* essentialist answer to a how-possible question about social kinds is wrong or politically incorrect. However, we do not have to accept that radical conclusion to think that many social kinds are constituted not only by internal structures, but by social ones as well.

By a constituting social structure I mean the set of relationships with other actors that define a social kind as such. A familiar example is the master and the slave. Each is a social kind with certain properties and dispositions: ownership and the enforcement of obedience on the one hand, and chattel and wanting to escape on the other. Individuals acquire these properties through a causal process of accidents of birth, socialization, coercion, and so on. And they are constituted with them in part by their internal, self-understandings; it is hard to be a master (and perhaps even a proper slave) if one does not see oneself as such. But if we want to explain how a master can sell his slave then we need to invoke the structure of shared understandings existing between master and slave, and in the wider society, that make this ability to sell people possible. This social structure does not merely describe the rights of the master; it *explains* them, since without it those rights by definition could not exist. By way of contrast, even if a parent in the antebellum American South had the physical capability and desire to sell their child, they could not do so because the structure of that culture did not recognize such a right. These explanations are not causal. It's not as if the social structure of slavery exists independent of the master's right to sell his slave and causes that right to come into being. Rather, the master's right is conceptually or logically dependent on the structure of slavery, such that when the latter comes into being so does the former, by definition. Hegelians call this kind of relationship an 'internal relation', by which they mean that the properties of a relation's elements are internal to the relation itself, and so do not exist apart from it.<sup>30</sup> (Note that this is in effect the opposite of what I referred to as an 'internal structure' above). In the terminology of this essay, when Hegelians appeal to internal relations to explain the powers of masters and slaves they are offering a constitutive explanation.

<sup>29</sup> See Wendt, *Social Theory of International Politics*, ch. 4.

<sup>30</sup> For a systematic development of this idea, see Bertell Ollman, *Alienation* (Cambridge, 1971).

Although the social structure of international politics is thinner and simpler than that of domestic politics, there are many examples in that domain as well of the properties of social kinds being made possible by social structures. The case of rogue states has already been mentioned. I have argued elsewhere that the deep structure of anarchy varies as a function of whether states constitute each other in the role of enemy, rival, or friend.<sup>31</sup> The properties of an 'enemy', for example, are in part made possible by his self-understandings (internal structure), since he will not be able to properly fill that role unless he sees himself as your enemy. But being enemies is also about being in a particular social relationship with an Other, like master and slave, that defines who both of you are.

However, the most widely acknowledged example of how a state's properties can be explained, constitutively, by social relations is the property of state sovereignty. How is it possible for Luxembourg to survive in an anarchic world surrounded by states with thousands of times its military power? The answer is that other states recognize its sovereignty as a *right*—in effect, to 'life, liberty, and territory'—and therefore do not try to conquer it. In order to acquire such a right an entity must have the kind of internal structure that constitutes it with 'internal' sovereignty, namely exclusive political control and a territorial monopoly on the legitimate use of organized violence. Collectivities lacking this internal structure like football teams and churches don't get recognized as sovereign in today's international system. States that have a lot of military power may be able to survive in an anarchic world with nothing more than this internal structure, but in the modern international system, military power is in fact not crucial to state survival, since states recognize each other's internal sovereignty as a right (what is sometimes called 'external' sovereignty). This means that today sovereignty is not only an internal or essential property of states, but also an *institution* between states that constitutes them with social capacities—rights—that they would not otherwise enjoy. These rights make it possible for even very weak states like Luxembourg, or 'failed' states like Somalia, to survive in an anarchic world. To that extent the institution of sovereignty *explains* state survival even though it does not exist independent of or prior to the rights to life, liberty, and property which make that survival possible.

It is important to see this constitutive story as an 'explanation' in part just because that is what it in fact is; it is not merely a description, nor is it a causal explanation. As Charles Cross concludes after reviewing a variety of answers to how-questions (both causal and constitutive), 'explanations of these sorts are widely regarded by scientists as *explanatory* (and as scientific)'.<sup>32</sup> However, returning finally to the larger issue of what is at stake in this semantic battle, it is also important to see the story as explanatory because denaturalizing sovereignty increases the ability of international society to make progressive changes in it. In saying this, it should be emphasized that reification is sometimes good, even essential, to the maintenance of social order. As presently constituted the institution of sovereignty plays a vital role in reducing the incidence and severity of war and in helping weak states survive. Since these are presumably normatively positive outcomes we should *want* states to take the social relations that make them possible for granted, want states to treat them as natural, because that will make them harder to undermine. However, the

<sup>31</sup> *Social Theory of International Politics*, ch. 6.

<sup>32</sup> Cross, 'Explanation and the Theory of Questions', p. 245; emphasis in the original.

social relations constituting sovereign rights also have less satisfying consequences, like enabling states to repress their people, to keep out refugees, to wage war, and so on. If we want to eliminate these consequences by changing the terms of sovereign rights it does us little good to think about the shared understandings that constitute those rights as merely 'describing' them, as if they existed independent of what states think. That kind of reification is disempowering, and suggests that the only solution to the problems of sovereignty is to destroy the state, in the same way that the solution to AIDS is to destroy the AIDS virus. In contrast, if we can show that the negative effects of sovereignty are not independent of—are constituted by—what states think it should mean, then the possibility emerges of reforming sovereignty by getting states to change their minds about those meanings. Whether states should do so is of course another question, but if we come to see that sovereign rights are *explained* rather than merely described by certain shared understandings then we come to recognize our collective authorship and responsibility for the maintenance of those rights in their present form, and it becomes possible to have a more fully reflexive debate about whether that is what we want.

## Conclusion

As a community, we in the academic study of international politics spend too much time worrying about the kind of issues addressed in this essay. The central point of IR scholarship is to increase our knowledge of how the world works, not to worry about how (or whether) we can know how the world works. What matters for IR is ontology, not epistemology. This doesn't mean that there are no interesting epistemological questions in IR, and even less does it mean that there are no important political or sociological aspects to those questions. Indeed there are, as I have suggested above, and as a discipline IR should have more awareness of these aspects. At the same time, however, these are questions best addressed by philosophers and sociologists of knowledge, not political scientists. Let's face it: most IR scholars, including this one, have little or no proper training in epistemology, and as such the attempt to solve epistemological problems anyway will inevitably lead to confusion (after all, after 2000 years, even the specialists are still having a hard time). Moreover, as long as we let our research be driven in an open-minded fashion by substantive questions and problems rather than by epistemologies and methods, there is little need to answer epistemological questions either. It is simply not the case that we have to undertake an epistemological analysis of how we can know something before we can know it, a fact amply attested to by the success of the natural sciences, whose practitioners are only rarely forced by the results of their inquiries to consider epistemological questions. In important respects we *do* know how international politics works, and it doesn't much matter how we came to that knowledge. In that light, going into the epistemology business will distract us from the real business of IR, which is international politics. Our great debates should be about first-order issues of substance, like the 'first debate' between Realists and Idealists, not second-order issues of method.

Unfortunately, it is no longer a simple matter for IR scholars to 'just say no' to epistemological discourse. The problem is that this discourse has already contamin-



ated our thinking about international politics, helping to polarize the discipline into 'paradigm wars'. Although the resurgence of these wars in the 1980s and 90s is due in large part to the rise of post-positivism, its roots lie in the epistemological anxiety of positivists, who since the 1950s have been very concerned to establish the authority of their work as Science. This is an important goal, one that I share, but its implementation has been marred by an overly narrow conception of science as being concerned only with causal questions that can be answered using the methods of natural science. The effect has been to marginalize historical and interpretive work that does not fit this mould, and to encourage scholars interested in that kind of work to see themselves as somehow *not* engaged in science. One has to wonder whether the two sides should be happy with the result. Do positivists really mean to suggest that it is not part of science to ask questions about how things are constituted, questions which if those things happen to be made of ideas might only be answerable by interpretive methods? If so, then they seem to be saying that the double-helix model of DNA, and perhaps much of rational choice theory, is not science. And do post-positivists really mean to suggest that students of social life should not ask causal questions or attempt to test their claims against empirical evidence? If so, then it is not clear by what criteria their work should be judged, or how it differs from art or revelation. On both sides, in other words, the result of the Third Debate's sparring over epistemology is often one-sided, intolerant caricatures of science.

One of the important virtues of *Explaining and Understanding International Relations* and *Designing Social Inquiry* is that they both seek to get us out of this dead end and toward a more pluralistic and tolerant—because more confident—conception of our common endeavour, all too often forgotten, of making sense of international life. Despite my complaint about the baleful influence of epistemology talk on IR, if more such discourse can help purge its influence then it is all to the good. Yet in different ways both of these books frame the issues in ways that subtly help reinforce the very walls they seek to overcome. Hollis and Smith identify natural science with the outsider's perspective of causal theorizing, which suggests that natural scientists don't tell insider stories and therefore that social scientists who do tell them are not doing science and, by implication, can't be positivists. King, Keohane, and Verba assimilate Understanding to the positivist, scientific project, but see it as a form of descriptive inference and reduce Explanation to causal inference. Given the sociology of knowledge in our discipline that condemns Understanders to second-class status, and also obscures the role of ideas in producing the structures of international life, contributing to their reification and thus to a lack of perceived responsibility and control by the actors whose ideas constitute them.

In this article I advanced two arguments. First, against Hollis and Smith I argued that the difference between Explanation and Understanding should be seen as a difference between two kinds of question, causal and constitutive, which are routinely asked in both natural and social science. The data and methods appropriate to answering these questions will differ in the two domains to the extent that natural and social kinds are made of different kinds of stuff, but this does not imply an epistemological difference. As King, Keohane, and Verba emphasize, in the two domains there are similar problems of theory construction, of inference to unobservables, and of adjudicating knowledge claims against publicly available evidence. This fact is not lost on most post-positivists, who in their empirical

research are at least tacit positivists. As such, there seems little reason not to make this explicit and consider constitutive inquiries or Understanding as part of a positivist approach to social inquiry.

Second, against King, Keohane, and Verba I argued that the answers to constitutive questions are explanations rather than simply descriptions. Since these explanations are not causal this requires a broader conceptualization of 'explanation' than the one with which King, Keohane, and Verba operate, but this is fully in line with our ordinary, everyday understanding of what it means to 'explain'. That doesn't mean that there is no such thing as description, but rather that the description/explanation distinction cuts across the causal/constitutive distinction. Causal and constitutive theories alike both describe and explain. The effect of this argument is to elevate constitutive theory to an autonomous, co-equal status relative to causal theory, worth doing in its own right, rather than treating it as something we have to go beyond before we will really be doing science. And the argument also highlights the role of shared ideas in producing social kinds, which denaturalizes them and thereby expands the potential for progressive change.

Rather than engaging in gate-keeping against each other as the purveyors of false epistemological gods, positivists and post-positivists in IR would do better instead to adopt a rule of 'mutual recognition' toward each other's preferred questions, and focus on the respective logics, explanatory potentials, and truth conditions of those questions. A reflexive, critical science of international politics needs every kind of knowledge it can get.