

# *Realism, empiricism, and causal inquiry in International Relations: what's at stake?*

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# Realism, empiricism, and causal inquiry in International Relations – what’s at stake?

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AUTHOR FINAL VERSION

Alexander Wendt argued thirty years ago that a commitment to the reality of ‘unobservable generative structures’ would open up new avenues of causal inquiry in International Relations [IR] (1987: 350). This view has since been further elaborated (see Wendt 1999; Patomäki and Wight 2000; Patomäki 2002; Wight 2006; Kurki 2008; Joseph and Wight 2010; Patomäki 2017) and has recently become quite widespread: the benefits of scientific realism [SR] are now propounded in the methods literature (see Bennett 2013) and even by neorealists (see Mearsheimer and Walt 2013).<sup>1</sup> In fact, it is increasingly common for discussions of causal inquiry in IR to be framed in terms of a contrast between two competing philosophical positions, each with a putative methodological corollary: empiricism is associated with a search for patterns of cross-case covariation, while SR is associated with a search for causal mechanisms.<sup>2</sup>

According to some more recent advocates, SR complements mainstream approaches to causal inquiry. For example, Andrew Bennett argues that ‘common methods in political science – including but not limited to statistical analysis, formal modelling, discourse analysis, and case

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<sup>1</sup> SR has nothing to do with realism in IR theory. The most prominent argument against SR in IR is Chernoff’s (2002).

<sup>2</sup> The same contrast is visible in political science (see Marsh and Furlong 2002).

studies – can all contribute to the development and testing of theories about causal mechanisms’ (2013: 471), while John Mearsheimer and Stephen Walt accept that such theories can be tested by identifying patterns of ‘covariation’ and applying ‘techniques of causal inference’ (2013: 434). For critical realists, however, SR (of which critical realism [CR] is a variant) underpins a profound challenge to approaches to causal inquiry organized around ‘the study of patterns of *regularities* in the world around us’ (Kurki 2008: 6). Critical realists argue that such approaches reflect a misunderstanding of scientific practice. Regularities are significant only when observed under controlled conditions, for example, in experiments, and even then only because they constitute evidence of real underlying ‘causal powers’ or mechanisms (Kurki 2008: 198). In order to make good on the promise of science, viz. to ‘go beyond appearances and provide explanations at a deeper level of understanding’ (Wight 2006: 18), it is necessary to organize causal inquiry around identifying these underlying mechanisms.

Although critical realists are forceful advocates of it, the idea that approaches to causal inquiry which focus on identifying patterns of cross-case covariation are unduly restrictive is not specific to SR. It also underpins a number of recent innovations in philosophically-informed discussions of causality in IR which are not rooted in SR. These include Patrick Jackson’s demonstration that neopositivism is not the only properly ‘scientific’ approach to empirical and causal inquiry in IR (2011), Ned Lebow’s concept of ‘inefficient causation’ (2014), and the investigations, in a recent collection on ‘Problems of Causation in World Politics’ (Humphreys 2017a), into how causal understandings play out in contemporary world politics (see Betts and Pilath 2017; Guzzini 2017; Kurki 2017) and into alternatives to the philosophically discredited covering-law model of explanation (see Humphreys 2017b; Jackson 2017; see also Suganami 2008, Grynawiski 2013). A puzzle therefore emerges. Are critical realists right to contend that

moving causal inquiry in IR beyond the search for regularities requires a philosophical revolution involving the widespread adoption of SR (Patomäki and Wight 2000; Kurki 2006)?

I argue that, on this question, critical realists are wrong. They make it appear that such a revolution is required by depicting SR as the principal alternative to a dominant 'positivist, or ... empiricist, model of social science' (Kurki 2006: 195). This vision of a binary confrontation between two philosophical systems (SR and empiricism), each with a distinctive methodological corollary, gives a misleading impression of what is at stake for IR in philosophical debates about causation. The issue at stake in IR is not the directly philosophical one of whether individual researchers embrace SR for, as Fred Chernoff has argued, this will not necessarily affect the conduct of inquiry (2002; 2009b). What is at stake is the methodological question of whether causal inquiry in IR must be organized around a focus on regularities (or patterns of cross-case covariation) observable in the world around us, that is, in everyday world politics. And what debates about causal inquiry in IR have so far overlooked is that empiricists need not, and perhaps should not, endorse this focus. As I will show, mainstream approaches look just as problematic from the perspective of contemporary empiricism as they do to scientific realists. Far from endorsing competing methodological visions, empiricist and scientific realist philosophers of science share a common understanding of scientific practice, one from which mainstream approaches to causal inquiry in IR depart significantly.

While empiricism (especially as it is linked to positivism) has often been a target for criticism in IR, most notably in the context of the 'third debate' (see Smith 1996), there continues to be a lack of clarity about what empiricism is and hence about which practices of causal inquiry in IR, if any, are distinctively empiricist. Typically, critics of empiricism employ the term either as a

catch-all for methodological approaches to which they object (roughly, those which Jackson (2011) terms ‘neopositivist’) or to denote arguments associated with the logical positivist (and logical empiricist) movement of the early- to mid-twentieth century.<sup>3</sup> The subsequent transformation in empiricist philosophy of science, led by Bas van Fraassen (1980, 1989), has largely been ignored (though Chernoff’s work is a notable exception (see especially 2002, 2009b, 2014)). My aim is therefore to develop a fuller account than has previously been provided in IR of Van Fraassen’s empiricism and how it differs from SR. In light of that, I consider what is at stake in calls for the reconstitution of causal inquiry in IR along scientific realist, rather than empiricist, lines.<sup>4</sup> I focus particularly on what role the search for regularities may play in causal inquiry, both because this is central to critical realist objections to mainstream approaches in IR, and also because critical realists argue that we can make progress on other questions, such as the relationship between agents and structures (see Wight 2006), or whether reasons can be causes (see Kurki 2008), only once we move beyond a narrow focus on regularities.

I show that whereas critical realists suppose that empiricism entails a problematic ‘conception of “scientific methods”’ (Kurki 2006: 211), this is not true of Van Fraassen’s ‘constructive empiricism’ (1980) – his rejection of SR does not lead him to advance a competing model of scientific inquiry. Any departure from the model of scientific inquiry employed in natural science is as much a departure from contemporary empiricism as it is from SR. One lesson

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<sup>3</sup> The same is true of ‘positivism’ (see Johnson 2006: 224-5).

<sup>4</sup> Contemporary empiricist philosophy of science focuses on natural rather than social science. However, scientific realists such as Bhaskar (2008) seek to discredit empiricism as a basis for causal inquiry in the social sciences by showing that it cannot provide a coherent account of natural science. How contemporary empiricist philosophers of science make sense of natural scientific practice is therefore directly relevant to the critical realist case against empiricism in IR.

which emerges from a closer appreciation of Van Fraassen's empiricism is hence that it is not necessary to embrace SR in order to develop a critique, grounded in contemporary philosophy of science, of the pursuit of covering-law explanations and the associated focus on identifying patterns of cross-case covariation which dominates mainstream IR today (see Jackson 2011). A second lesson, which has not been widely noticed in IR, is that approaches such as those articulated by Gary King, Robert Keohane, and Sidney Verba (1994), Stephen van Evera (1997), and John Gerring (2005), which are widely construed as 'empiricist' (see Kurki 2008: 103; Sil and Katzenstein 2010: 419), are troublingly dislocated from the philosophical position with which they are most often identified.<sup>5</sup>

In developing this argument, I proceed as follows. The first section summarizes the argument for SR and against empiricism which is employed by critical realists in IR, distinguishing its methodological and metaphysical components. The second section shows how critical realists identify empiricism with the neopositivist methodology which dominates mainstream IR. The third section introduces Van Fraassen's 'constructive empiricism', showing that, in contrast to earlier forms of anti-realism, it differs from SR principally on a narrow epistemological issue about whether science requires entity realism, an issue which has no clear methodological corollary. The fourth section explores the further metaphysical disagreement over whether science requires causal realism, questioning whether it has any substantive implications for causal inquiry. The final section considers what is at stake in all this for causal inquiry in IR. It argues that scientific realists in IR have failed to make a compelling case for SR as compared to constructive empiricism. Yet insofar as empiricism has been transformed to meet the scientific realist challenge, this tends to reinforce, rather than to undermine, what I consider the critical

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<sup>5</sup> Johnson (2006) characterizes such work as employing a 'positivist' methodology. But, as Smith (1996: 17) points out, this is typically interpreted in IR as involving an 'empiricist epistemology'.

realists' most important argument, viz. that the methodological assumptions which appear to underpin most mainstream approaches to causal inquiry in IR lack philosophical support.

### Scientific and Critical Realism

To be a realist about something is to hold that it is real – that it exists (Chakravartty 2011).

Most IR theorists are therefore *empirical* or *common-sense* realists (Joseph 2007: 345-6; Chernoff 2009b: 373): they hold that the observable objects encountered in everyday life are real. The same is true of empiricist philosophers – rejecting SR does not mean rejecting this kind of realism (Chernoff 2002: 191). What is at issue between scientific realists and empiricists is the reality of the *unobservable* entities, properties, and relations (including causality) that feature in ordinary scientific discourse (Chernoff 2007: 400). Scientific realists hold that at least some of these are real, though they disagree about which (Chakravartty 2011) – consequently, SR can take a wide variety of forms (Chernoff 2002: 191). Most scientific realists in IR, however, subscribe to both entity realism and causal realism: they endorse realism both about the *unobservable entities* postulated in our best scientific theories and about the putative *causal powers* of observable and unobservable objects.

As Chernoff points out, most scientific realist philosophers of science are (like their empiricist counterparts) concerned with the natural rather than the social sciences. Moreover, acceptance of SR in relation to well-established natural scientific theories does not entail acceptance of SR in the social sciences (2002: 197-8) – indeed, the relative immaturity of IR theories may provide an additional reason to resist realism about their theoretical postulates (2007: 403-4). The most prominent form of SR in IR is, however, CR, which is a variant of SR

expressly developed in and for the social sciences.<sup>6</sup> It originates principally in the work of Roy Bhaskar, who developed his case for SR in contradistinction to what he variously termed ‘positivism’, ‘empiricism’, and ‘orthodox philosophy of science’ (2008: 12, 14, 15). He contended that the empiricist account of the scientific method is badly flawed and that SR provides the solution – scientific practice is intelligible only on the assumption that what it discovers are the ‘generative mechanisms of nature’, that is real things, with real causal powers (2008: 14).

The fact that most advocates of SR in IR draw on Bhaskar illuminates three features of how they make their case: (i) SR is presented in contradistinction to empiricism (or positivism), which is closely identified with a neopositivist methodology that privileges the search for patterns of cross-case covariation; (ii) SR is presented as the best (or only) means of moving beyond this neopositivist methodology; (iii) SR is presented as involving both entity realism and causal realism. Because my purpose is to examine scientific realist arguments against empiricism *in IR*, I focus in what follows on the case for SR made by critical realists.<sup>7</sup>

Bhaskar’s principal criticism of empiricism is that it subscribes to a ‘Humean theory of causal laws’, according to which ‘a constant conjunction of events’ is a ‘necessary condition for a scientific law’ (2008: 12). This theory, he argues, is inadequate not only for the social sciences, but also as an account of natural science. That is because it fails to distinguish between the

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<sup>6</sup> Chernoff notes that presentations of SR in IR often conflate SR and CR, thereby wrongly giving the impression that scientific realist philosophers of (natural) science would endorse the arguments of critical realists in IR (2007: 400).

<sup>7</sup> Other arguments for SR are available. One prominent example, which has been discussed in IR (see Wendt 1999: 62-3; Wight 2007: 386-7; see also Chernoff 2002: 193-4), is inference to the best explanation. This is rejected by Van Fraassen (1980: 19-23; see also Ladyman, Douven, Horsten, and Van Fraassen 1997).



‘closed systems’ in which constant conjunctions are found and ‘open systems’ in which they are not found; and this distinction is important because the world encountered in everyday experience is not a closed system – in fact, most closed systems are artificially created by scientists in experiments (Bhaskar 2008: 13-14; see also Cartwright 1999: 2-3). Bhaskar’s point is that scientists conduct experiments *because* the world around us is mostly open-systemic: ‘an experiment is necessary precisely to the extent that the pattern of events forthcoming under experimental conditions would not be forthcoming without it’ (2008: 33).

It follows from this simple observation about why scientists conduct experiments that ‘universal empirical generalizations’ are unlikely to be found (Bhaskar 2008: 15). What Bhaskar terms ‘invariances’ are only ‘scientifically significant’ when ‘generated under conditions which are artificially produced and controlled’ (2008: 20). And it follows from this that the idea, which is widespread in IR (and political science), that social scientists should search for regularities of the form ‘if A then always B’, or ‘if A then sometimes B, with probability X’ (Van Evera 1997: 8), must be flawed, at least if such regularities are held to apply both under controlled conditions and also in everyday life.<sup>8</sup> For although we may identify deterministic or probabilistic relationships between particular experimental manipulations and particular outcomes, we do not expect these relationships to hold outside controlled conditions. Consider, for example, the effect of gravity on a falling feather: inside a vacuum chamber, the feather falls at the same rate as a bowling ball but, because of atmospheric resistance, we do not expect this to be replicated in everyday conditions. We could only expect regularities observed under controlled conditions also to hold outside those conditions if the world around us were a closed system – but in that case, it would not be necessary to conduct experiments.

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<sup>8</sup> Cartwright (1983) argues that while fundamental laws in physics do state universal regularities, in doing so they ‘lie’.

As I have presented Bhaskar's position so far, his point is methodological: insofar as empiricism identifies causal inquiry with the search for regularities (or patterns of covariation) that extend outside of controlled conditions, it fails to recognize the crucial role of experimentation within scientific practice. This argument is, however, closely tied to a transcendental argument for SR – Bhaskar holds that the way in which scientists employ experimental results is intelligible only on a scientific realist understanding of what they discover in experiments. In short, his contention that empiricism is methodologically incoherent informs an argument for both entity and causal realism.

He advances this argument by pointing out a commonplace supposition that, he argues, informs scientific practice: that although the precise 'pattern of events' observed under controlled conditions does not extend outside those conditions, the causal relationship of which the pattern is evidence does (2008: 13). Consider, once again, the example of the feather dropped in the vacuum chamber. We do not expect the result observed therein to be replicated in everyday conditions. Yet nor do we infer from this that gravity is only present under experimental conditions. We suppose that gravity is also present in everyday conditions, but recognize that the outcome observed under these conditions will reflect how it contingently combines with precisely those factors, such as atmospheric resistance, that are controlled for in the vacuum chamber.

Bhaskar argues that this supposition (*viz.* causes continue to exert effects outside of the controlled conditions under which they are identified) is intelligible only if the invariances observed under experimental conditions are themselves produced by 'generative mechanisms of nature' (2008: 14). Under experimental conditions, these mechanisms generate invariant

results because they are allowed to unfold unhindered. Outside of such conditions, they continue to act, but the consequences which would be observed under experimental conditions 'are, owing to the operation of intervening mechanisms or countervailing causes, unrealized' (2008: 46). In other words, Bhaskar argues that the 'exportation of explanations' (Clarke 2010: 301) from experimental conditions to everyday conditions is intelligible only if experiments identify something which carries across to open systems. This cannot be regularities, because the invariant results observed under experimental conditions are not reproduced outside those conditions. What is discovered must, therefore, be 'the ways of acting of things', that is, real causal powers, possessed by real observable and unobservable entities, which both account for invariances observed under experimental conditions and also constitute the 'mechanisms' that 'combine to generate the actual flux of phenomena' observed in open systems (Bhaskar 2008: 17).

In subsequent work, Bhaskar (2015) built this argument for SR into CR, an approach to causal inquiry in the social sciences which takes seriously the emergent reality of social formations and the need to study the 'persistent relations between the various planes of activity that constitute, and causally impact upon, social life' (Wight 2006: 48).<sup>9</sup> For my purposes, however, it is the basic argument for SR, and how it is contrasted with empiricism which is of principal interest. And my point, at this stage, is that the two parts of Bhaskar's argument are separable. We can coherently accept the substance of his methodological point, viz. the kinds of regularities prized by scientists are mostly discovered through experiment and do not hold outside experimental conditions, while rejecting his claim that *only* SR can render the role of

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<sup>9</sup> Those more recent advocates of SR, such as Bennett (2013) and Mearsheimer and Walt (2013), who regard SR as compatible with, and making sense of, mainstream research practices in IR do not follow Bhaskar and the self-identified critical realists down this path.

experimentation in science intelligible. This is significant because it raises the question of what is really at stake in the scientific realist critique of empiricism in IR – is it a methodological critique of mainstream IR, or a philosophical argument for SR?

### Scientific realist criticisms of empiricism in IR

Critical realists in IR follow Bhaskar in conceiving of empiricism as being unable adequately to make sense of scientific practice. They therefore consider causal inquiry informed by empiricist principles to be methodologically suspect – developing Bhaskar’s claim that ‘empiricism is not neutral in its consequences for scientific practice’ (2008: 42), they associate it with what Jackson terms a ‘neopositivist’ search for patterns of cross-case covariation (2011).

I examine three areas in which they contend that empiricists give an inadequate account of scientific practice: in relation to (i) the objects of scientific inquiry, (ii) the interpretation of scientific theories, and (iii) how science explains. In the following section, I show that while these arguments may hold force against the neopositivist assumptions which shape causal inquiry in mainstream IR, they are powerless against Van Fraassen’s reformulated empiricism, which does not endorse those assumptions. This suggests that what is at stake in philosophical debates around causality in IR is not a philosophical question about the merits of SR as compared to empiricism, but rather a methodological question about the merits of dominant practices of causal inquiry in IR.

Chernoff has previously criticized arguments against empiricism and other anti-realist positions by scientific realists in IR, contending that they often mischaracterize debates in contemporary philosophy of science (2002: 196-9; see also 2007; 2009b; 2014: 34-8). However, he defends

'causal conventionalism' (2009a; 2009b), which combines Van Fraassen's treatment of theoretical terms (2002: 202; 2009b: 374) with positions that Van Fraassen would not endorse, notably instrumentalism (2002; 2009b) and causal realism (2002: 203; 2009a: 177; 2009b: 383). By contrast, I focus more narrowly on Van Fraassen's defence of an empiricism which resists both entity and causal realism, but without embracing instrumentalism.

### *The objects of scientific inquiry*

Like Bhaskar, Milja Kurki identifies empiricism with a 'Humean conception of causation' (2008: 6) which implies that 'causal relations are regularity relations of patterns of *observables*', such that 'when A type of events take place, B type of events can be assumed to follow (at least probabilistically)'. Consequently, causal analysis is tied 'to the study of patterns of *regularities* in the world around us' (2008: 6). Like Bhaskar, moreover, she regards this conception of causation as ontologically impoverished. She insists that 'observed regularities do not constitute causality: causality consists in the *underlying* causal powers' which regularities observed under experimental conditions allow us to identify (2008: 198). In other words, the objects of scientific inquiry are not, as empiricists think, regularities, but rather real causal powers.

According to Kurki, this matters because it has methodological consequences. The impact of the empiricist conception of causation on the social sciences has been the development of an understanding of science as being 'about finding falsifiable, predictive, observation-based regularities' – 'most social scientists are ... adamant that only careful observation of regularities (even if of "localised" regularities) can give us an adequate understanding of human action and society' (2006: 193-4). Kenneth Waltz, for example, claimed that in order to construct a 'theory of international politics' one must first 'conceive of international politics as

a bounded realm or domain; second ... discover some law-like regularities within it; and third ... develop a way of explaining the observed regularities' (1979: 116). Consequently, Kurki argues, empiricists are unable to do justice to causes which are unobservable or which do not show up in stable regularities, such as 'ideas, meanings and reasons' (2006: 203; see also Shapiro and Wendt 1992; Wight 2006). This has led to a methodological divide in IR in which, rather than submitting to the 'straightjacket' which a focus on regularities imposes upon causal inquiry, many interpretive and critical scholars have 'rejected the validity of causal analysis altogether' (2006: 200). Kurki argues, however, that because SR accepts '[r]easons and motivations as well as rules, norms and discourses' as 'legitimate objects' of social scientific inquiry it can 'help forge constructive links between theoretical camps in the "divided discipline" of IR' (2006: 211, 190; see also Patomäki and Wight 2000).

One putative problem with treating regularities as the principal objects of scientific inquiry is hence that it renders empiricism unable to support inquiry into kinds of unobservable factors that are widely encountered in the social world. However, Kurki's characterization of empiricism as searching for regularities *in the world around us* also raises a further question, viz. what is the warrant for believing that such regularities are causally significant? We know that regularities observed in artificially closed experimental conditions are causally significant because when the interference of other factors is ruled out, the factor that is manipulated is the only thing that could have produced the observed outcome (see Bhaskar 2008: 53; Cartwright 1983: 6). This logic can also be applied to randomized controlled trials and to natural experiments.<sup>10</sup> Outside of such conditions, however, we cannot be sure that any

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<sup>10</sup> In a randomized controlled trial, if treatment and control groups are identical, then only the treatment *could have* generated any difference in the observed outcome across the two groups (Holland 1986; see also Cartwright 2010). Similarly, if John Stuart Mill's method of difference is applied to two cases that are 'alike in every way except one', then we have a

regular pattern which may be observed is not a product of chance, or of equifinality (of there being multiple possible causal routes to the same outcome). Consequently, the covariation of two factors does not strictly imply the presence of a causal relationship between them – we can draw *compelling* inferences about causal relationships only from patterns of covariation observed under controlled conditions (whether artificial or naturally occurring). This applies even to probabilistic laws: a pattern of covariation definitively indicates the presence of a causal law which holds probabilistically only if it can be shown to hold probabilistically under controlled conditions.<sup>11</sup>

### *The interpretation of scientific theories*

Critical realists also reject what they take to be an empiricist interpretation of scientific theories. For while Kurki argues that its focus on regularities renders empiricism unable to do justice to unobservable factors, many widely accepted scientific theories do in fact invoke unobservable entities, such as sub-atomic particles. According to Colin Wight (2007), the instrumental fashion in which empiricists make sense of this feature of science restricts the scope of causal inquiry.

Instrumentalism is the doctrine that theoretical claims about unobservable entities should be interpreted instrumentally – as purely theoretical assumptions which (i) do not have truth values (and hence do not make existential assertions) and (ii) are included in scientific theories purely based on their contribution to explanatory and predictive success (Chakravartty 2007:

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natural experiment in which the factor which differs *must be* the cause of the outcome to be explained (Sekhon 2004: 283).

<sup>11</sup> There is therefore a significant difference between a pattern observed under everyday conditions, in which *Bs* sometimes, but not always, follow *As*, and a probabilistic law, in which *Bs* follow *As* with a specified probability, under controlled conditions.

10-11). Its potential appeal is that it appears to make sense of the explanatory power of scientific theories in which assumptions about unobservable entities appear without requiring a commitment to entity realism. Scientific realists argue, however, that instrumentalism has a significant methodological cost. Because instrumentalists deny that theoretical assumptions about unobservable entities refer to anything real, when they invoke theories containing those assumptions in explanations they must reason in an 'as-if' fashion (Wendt 1999: 61) – they can explain only that what happened was *as if* the theory's assumptions were true. Consequently, the unobservable entities which those assumptions postulate never themselves become objects of investigation. Whereas scientific realists will question whether such entities are indeed as described in scientific theories, instrumentalism forecloses this avenue of inquiry (Wight 2007: 381, 383, 393-4). In short, instrumentalism makes science 'conservative': by exempting knowledge claims about unobservables 'from criticism', it limits the scope of scientific inquiry (Bhaskar 2008: 42-3).

Waltz's conception of the structure of the international political system constitutes the most widely discussed example of this problem in IR. Waltz argued that theoretical 'assumptions are not assertions of fact. They are neither true nor false. Theoretical notions find their justification in the success of the theories that employ them' (1979: 6). Scientific realists contend that the deficiencies of this instrumentalism emerge in the substance of neorealist theory. Wendt (1987) argues that Waltz's definition of structure in terms of the distribution of capabilities among functionally equal units in an anarchic system (see Waltz 1979: 88-99) effectively reduces it to a property of states, viz. their capabilities. This is adequate for Waltz's purposes insofar as it enables him to distinguish between bipolar and multipolar systems and hence to articulate his central theoretical claim: that the former are more stable than the latter. Wendt argues, however, that Waltz's definition leaves neorealism unable to illuminate



how structure might help to *generate* states as actors (1987). In short, because Waltz treats structure as a theoretical instrument the incorporation of which into neorealist theory is justified purely by its contribution to the prediction of system-wide outcomes, he fails to explore its generative potential.

### *How science explains*

A third aspect of empiricism which is widely criticized on methodological grounds is its approach to explanation. This is typically assumed to be some version of Carl Hempel's covering-law model, according to which we explain an event, B, by showing that its having occurred is logically deducible from the premises (i) that there is a well-established (covering) law of the form *if A, then B* and (ii) that A occurred, such that the occurrence of B was 'to be expected in the circumstances' (1966: 48; see also Wendt 1999: 79; Suganami 2008: 330). In IR, for example, James Lee Ray asserts that to explain a particular event it is necessary to show it to be 'an example of an established general pattern' (1995: 138).

Wendt identifies two ways in which this model of explanation 'can negatively affect the practice of social science'. First, social scientists may prioritize the search for 'the behavioural laws ostensibly needed for causal explanations' over other forms of inquiry (1999: 80). This corresponds to Kurki's concern that prioritizing the search for regularities places causal inquiry in a methodological straitjacket. Second, social scientists may 'turn to false, "as if" assumptions as substitutes for the laws which we have not yet discovered' (1999: 80). This corresponds to Wight's concerns about the dangers of instrumentalism. Yet Wendt also objects to the covering-law model itself. He observes that 'subsumption under a law is not really explanation at all, in the sense of answering *why* something occurred, but is simply a way of saying *that* it is an instance of a regularity' (1999: 81; see also Suganami 2008: 331). In

short, the covering-law model of explanation does not tell us what we want to know – it tells us only why something might have been expected to occur, not what caused it to occur. In order to explain why something occurred, Wendt argues, ‘we need to show how a causal process works, which depends on knowing mechanisms’ (1999: 81).

### Van Fraassen’s ‘constructive empiricism’

If construed as being directed against particular methodological practices and assumptions in mainstream IR, these arguments are, I contend, very important. Focusing on identifying regularities in the world around us and treating unobservable entities instrumentally are indeed liable to restrict the scope of causal inquiry, while the covering-law model of explanation cannot be sustained.<sup>12</sup> It is, however, a further question whether this tells us anything about the intrinsic limitations of empiricism as a philosophical position. Critical realists’ contention that it does underpins their advocacy of a philosophical revolution in which empiricism is replaced by SR – if empiricism has these methodological corollaries (and if SR is the only plausible alternative), then broadening the scope of causal inquiry in IR will require empiricism to be abandoned in favour of SR. Yet this contention is dubious. Indeed, such support as critical realists provide for their claim that empiricism entails the methodological practices and assumptions they criticize involves identifying empiricism with now largely defunct positions such as ‘logical empiricism’ (see Shapiro and Wendt 1992; Wendt 1999).<sup>13</sup>

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<sup>12</sup> My point is not that there is *no* merit in searching for regularities in the world around us, but rather that limiting causal inquiry to the search for such regularities closes down other potentially productive avenues of inquiry. Even if we accept that patterns of cross-case covariation may constitute powerful evidence of causal relationships, it does not follow from this that all causal relationships show up in such patterns, nor that such patterns constitute the only possible evidence for causal relationships.

<sup>13</sup> Bhaskar acknowledged that his arguments depicted an idealized form of empiricism to which, even if the 1970s, ‘few, if any, modern philosophers’ would unambiguously subscribe (2008: 26).

Such characterizations of empiricism are not widely challenged in IR – as in the so-called ‘third debate’, empiricism (like positivism) continues to be a term employed principally by critics of mainstream IR to denote positions they reject. Both mainstream scholars and their scientific realist critics have ignored the transformation in empiricist philosophy of science wrought since the 1970s, principally by Van Fraassen (cf Chernoff 2002). His key work remains *The Scientific Image* (1980), which seeks to identify what form a coherent empiricist account of science must take and how it differs from the forms of SR then being defended in the philosophy of science. More recently, Van Fraassen has explored the nature of philosophical stances (2002) and the nature of scientific representation (2008). However, the arguments which are most relevant to the critical realist characterization of the empiricist approach to causal inquiry in IR are to be found in *The Scientific Image* and his subsequent defence and development of that position (see 1985, 1989, 2001).

When mentioned in IR, Van Fraassen is typically identified as a positivist (Wight 2007: 388) or an instrumentalist (Wendt 1999: 61; Jackson 2011: 80). This characterization is, however, inaccurate. Van Fraassen explicitly acknowledges the deficiencies of previous forms of anti-realism, including phenomenalism, conventionalism, fictionalism, logical empiricism, logical positivism, and instrumentalism. He also accepts that many of the ‘realist arguments’ against those positions were ‘correct and successful: the positivist picture of science no longer seems tenable’ (1980: 2, 10, 41). The form of empiricism which he presents in its place does depart in important respects from SR. Yet these differences do not have the methodological consequences which scientific realists in IR expect (see also Chernoff 2007: 404-7; 2009b: 380; 2014: 256). The reader may therefore wonder what significance they have for IR. If so, then I beg patience – it is precisely the *similarities* between SR and constructive empiricism which I

consider significant, for they bring into the question the critical realist claim that it matters for causal inquiry in IR whether those conducting the inquiry are scientific realists or empiricists.

*Constructive empiricism and the objects of scientific inquiry:*

Van Fraassen asserts that science aims to 'save the phenomena' and that 'regularities in the observable phenomena' are 'the scientist's basic topic of concern' (1980: 4, 70). This makes him sound as if he holds the kind of view critical realists in IR reject. However, he joins with scientific realists in philosophy of science by rejecting what he, following Wilfrid Sellars, terms the 'levels picture' – a picture of science which 'pervades positivist writings' and in which 'singular observable facts ("this crow is black") are scientifically explained by general observable regularities ("all crows are black") which in turn are explained by highly theoretical hypotheses'. 'We do not really expect [scientific] theories to "save" our everyday generalizations', Van Fraassen observes, 'for we ourselves have no confidence in their strict universality'. In everyday life, 'we are liable to find only putative laws heavily subject to unwritten *ceteris paribus* qualifications': 'all crows are black – except albinos; water boils at 100°C – provided atmospheric pressure is normal; a falling body accelerates – provided it is not intercepted ... and so forth' (1980: 32; see also Cartwright 1983: 89).

In short, science does not principally consist in a search for regularities in the world round us. In order to discover the *kinds* of regularities scientists are interested in, Van Fraassen argues, 'one needs experimentation' (1980: 73ff). He notes, moreover, that these kinds of regularities 'are exceedingly subtle and complex, so experimental design is exceedingly difficult'. Consequently, 'the very search for new and deeper empirical regularities becomes couched in theoretical language' (1980: 73). His point is that even if we conceive of science as searching for regularities, this often requires developing and testing theories which make assumptions

about unobservable entities and forces – ‘[s]cience presents a picture of the world which is much richer in content than what the unaided eye discerns’ (1980: 59).

Consequently, although Van Fraassen characterizes science as aiming to discover ‘regularities in the observable part of the world’ (1980: 73), this does not have the implications that critical realists might imagine. Because Van Fraassen does not endorse the search for regularities in everyday life, questions about their significance for causal inquiry do not arise. And while he is not directly concerned with the social sciences, there is no *prima facie* reason to suppose that unobservable factors in the social sciences are further beyond the scope of scientific inquiry than are those in the natural sciences. That said, Van Fraassen’s acknowledgement that scientific theories often refer to unobservables raises the question of how he interprets such theories, a question which gets to the heart of constructive empiricism and how it differs from SR. Yet his position is not what might be supposed given the frequent claim, in IR, that instrumentalism is the principal alternative to SR (see Wendt 1987: 350; Wight 2007; Mearsheimer and Walt 2013: 432-4).

#### *Constructive empiricism and the interpretation of scientific theories*

Van Fraassen characterizes the core of SR as follows:

*Science aims to give us, in its theories, a literally true story of what the world is like;  
and acceptance of a scientific theory involves the belief that it is [approximately] true.*  
(1980: 8)<sup>14</sup>

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<sup>14</sup> I follow Chakravartty (2007: 4-8) in qualifying ‘true’ as ‘approximately true’ because, as Van Fraassen acknowledges (1980: 7), SR does not imply that our current theories are in fact correct or even that ‘science will arrive in due time at theories true in all respects’ – the key realist claim is that science is an ‘enterprise of discovery’ *aimed at* revealing ‘what there really is’ in the world.

He points out, however, that while all anti-realists must reject some part of this position, they have two options: they can either reject the idea that scientific theories should be literally construed or reject the idea that accepting a literally construed theory requires belief that it is true (1980: 10). Whereas the first option is instrumentalist, on Van Fraassen's view 'the apparent statements of science really are statements, *capable* of being true or false' – if a theory's statements include 'There are electrons', then the theory implies the existence of electrons (1980: 10-11). He therefore pursues the second option.<sup>15</sup> He agrees with scientific realists (against instrumentalists) that scientific theories should be construed literally, that is, as making existential assertions about unobservable entities, but insists that construing a theory literally

relates not at all to our epistemic attitudes towards theories, nor to the aim we pursue in constructing theories, but only to the correct understanding of *what a theory says ...* After deciding that the language of science must be literally understood, we can still say that there is no need to believe good theories to be true, nor to believe *ipso facto* that the entities they postulate are real. (1980: 11-12)

In other words, scientists can accept a theory and construe it literally while at the same time "bracketing" its ontological implications' (1980: 81).

On first inspection, this may sound unpersuasive. Most scientists, we may suppose, are entity realists – they believe (some of) the theories they accept to be true and hence believe that (some of) the unobservable entities to which they refer exist. Yet Van Fraassen would accept this! He is not concerned with what scientists in fact believe, but rather with whether it is

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<sup>15</sup> Whereas Chernoff defends instrumentalism, but denies that it has the methodological consequences scientific realists in IR expect (2002, 2007, 2009b), Van Fraassen rejects instrumentalism – his point is that empiricists need not (and should not) be instrumentalists.

*necessary*, for science, that scientists believe the theories they accept to be true in what they say about the unobservable parts of the world. His view is that the extent of belief *required* for theory acceptance is less than scientific realists suggest – scientists can coherently resist entity realism by remaining ‘agnostic about the existence of the unobservable aspects of the world described by science’ (1980: 72).<sup>16</sup>

Van Fraassen therefore presents an alternative account of theory acceptance. Accepting a theory must, he notes, involve belief that it is ‘*empirically adequate*’ – that it is true in ‘what it says about the observable things and events in the world’ (1980: 12). No scientist can accept a theory that they do not believe at least to this extent. Accepting a theory also involves ‘a commitment to confront any future phenomena’ with the ‘conceptual resources’ which that theory provides (1980: 12). This commitment compels scientists to ‘immerse’ themselves in the ‘world-picture’ the theory constructs and also in its language, including language which indexes unobservable entities (1980: 81) – scientists will therefore respond to questions about the world in the language of and by reference to the picture given by the theories they accept. Van Fraassen insists however, that scientists can believe that a theory is the best available (out of those which are empirically adequate), and hence commit to it and employ its language in talking about the world, yet still maintain an open mind about whether it is true in what it says

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<sup>16</sup> Some parts of the world which cannot be perceived by our senses may nonetheless be detected with the aid of scientific instruments (and theories for interpreting what they show). Consequently, scientific realists argue that we *can* be certain about the existence of *some* unobservable entities, viz. those which are ‘detectable’ (Chakravartty 2007: 14; see also Jackson 2011: 85-7). Van Fraassen insists, however, that detection is not observation (1980: 16-17; 2001: 154), implying that scientists can coherently remain agnostic about the existence even of ‘detectable unobservables’.

about the unobservable parts of the world.<sup>17</sup> He therefore offers the following summary statement of constructive empiricism, which is intended to bring out the contrast with SR:

*Science aims to give us theories which are empirically adequate; and acceptance of a theory involves as belief only that it is empirically adequate. (1980: 12)*

The difference between SR and constructive empiricism is therefore subtle – it concerns the extent of belief required for theory acceptance in science. Van Fraassen argues that acceptance *requires* belief only that a theory is empirically adequate and not, as scientific realists argue, belief that the theory is true *simpliciter*. In other words, acceptance of a theory which makes truth claims about unobservable entities does not require a commitment to entity realism – agnosticism about the reality of the unobservable entities to which many scientific theories refer is permitted. He also notes one important reason for thinking that agnosticism is sufficient for science, viz. if a theory *is* true in what it says about unobservable entities, that makes no difference to its *empirical* content, as compared to it being empirically adequate – ‘we can have evidence for the truth of a theory only via evidential support for its empirical adequacy’. Consequently, he argues, belief that a theory is not only empirically adequate, but also true in what it says about unobservables is ‘supererogatory’ – scientists *may* believe what theories say about unobservables, but it makes no difference for, and hence

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<sup>17</sup> An anonymous reviewer suggested that while Van Fraassen may not be an instrumentalist about the assumptions within scientific theories (because he regards them as having truth values) he is, nonetheless, an instrumentalist about theories themselves (insofar as he holds that they should be evaluated on grounds other than their truth), which makes him a kind of pragmatist. Van Fraassen acknowledges that there is a ‘pragmatic aspect’ to theory acceptance, because it comes with a commitment ‘to seeing nature’ through a ‘theory’s eyes’ – an accepted theory is a ‘guide both to theoretical and practical life’ (2001: 164; see also 1980: 87-8). His key claim, though, is that while scientific theories *do* make truth claims about the unobservable entities to which they refer (which rules out instrumentalism), we cannot directly evaluate them – consequently, we can accept theories which make such claims as guides while withholding belief that those claims are true.



is not necessary for, science (1985: 255).<sup>18</sup> Significantly, it follows from this that there is no theory which a realist-inclined scientist can accept which an agnostic cannot accept. This is because a theory which is (believed to be) true and is hence acceptable to the former is also, therefore, (believed to be) empirically adequate and is hence equally acceptable to the latter.

This view does not have the negative methodological consequences which scientific realists attribute to instrumentalism. Recall that both Bhaskar and Wight regard empiricism as conservative. As Wight puts it: the ‘realism principle (“is it really the case?”) provides the motor which keeps science running’ – science progresses in part by developing new theories and exposing the entities they postulate to critical scrutiny (2007: 383; see also Bhaskar 2008: 15-16; Patomäki and Wight 2000: 218). Van Fraassen notes, however, that ‘there have always been reasons to doubt the empirical adequacy of extant theories’ (1980: 93; see also Chernoff 2007: 402). One reason for this is that a theory which is claimed to be empirically adequate is claimed to be true of *all* the observable parts of the world, including parts not yet observed (1980: 72).<sup>19</sup> A science which aims to develop theories which are empirically adequate will therefore not, as Wight suggests, prematurely ‘come to an end’ (2007: 383), but will rather be perpetually committed to devising new and ever more sophisticated tests of the empirical adequacy of its theories.

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<sup>18</sup> An empiricist *may*, therefore, believe in the reality of (some) unobservables, so long as she acknowledges that this belief is supererogatory.

<sup>19</sup> As van Fraassen acknowledges, this means that accepting a theory requires an epistemological commitment that extends beyond the evidence. He denies, however, that this opens the door to SR, for ‘it is not an epistemological principle that one might as well hang for a sheep as for a lamb’ (1980: 72).

These tests may, moreover, include tests of assumptions about unobservable entities. Making the case that causal inquiry in IR should focus on causal mechanisms, and hence should embrace SR, Bennett notes that theories involving unobservable entities

generate implications on what should be true in the observable world if the posited mechanisms and our instruments of observation operate in the manner that we theorize. We can test these observable implications ... even though we cannot directly observe mechanisms. (2013: 466)

This could have been written by a constructive empiricist, rather than a proponent of SR, for to test the *observable* implications of a theory that postulates an unobservable entity is precisely to test its empirical adequacy. For reasons I examine below, Van Fraassen is reluctant to embrace the term 'causal mechanism' to characterize such entities. Constructive empiricism nonetheless endorses scientists making unobservable entities objects of inquiry in the only way that is possible – by constructing, and testing the empirical adequacy of, theories which invoke them.

#### *Constructive empiricism and how science explains*

Whereas scientific realists in IR associate empiricism with the covering-law model of explanation, Van Fraassen rejects it on grounds similar to Wendt's. He notes that, according to the covering-law model, an explanation must afford 'good grounds for believing that the phenomenon [to be explained] did, or does, occur', but objects that this 'does not always amount to explanation' (1980: 104). On his own account, by contrast, 'science gives us a picture of the world as a net of interconnected events, related to each other in a complex but orderly way', such that we can conceive of an event we are seeking to explain as being

‘enmeshed in a net of causal relations’.<sup>20</sup> In order to explain that event, it is necessary to pick out the most ‘salient factors’ from within this ‘causal net’ – these constitute ‘(what are ordinarily called) the *cause(s)* of that event’ (1980: 123-4).<sup>21</sup> Van Fraassen therefore conceives of (causal) explanations as drawing on the world-picture provided by our best scientific theories to provide an account of why something occurred – and if those theories invoke unobservable entities, then such entities may, on Van Fraassen’s account, be identified as causes of the outcome to be explained.

There is, however, a difference in the inferences that scientific realists and constructive empiricists believe can be derived from explanations that refer to unobservable entities. Nancy Cartwright is a causal realist whose arguments speak directly to the case against empiricism advanced by critical realists in IR. She argues, against Van Fraassen, that ‘causal explanations have truth built into them’ – to the extent that we find a causal explanation acceptable, ‘we must believe in the [reality of the] causes described’ (1983: 91, 5).<sup>22</sup> In other words, if we accept an explanation which invokes an unobservable entity, then that implies a commitment to the reality of that entity. Van Fraassen disagrees. He points out that explanations are always relative to some theory and argues that the fact that a theory can explain some outcome is not a reason to believe it to be true (1980: 100). In other words, we

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<sup>20</sup> Van Fraassen in fact questions the ‘adequacy of the terminology of cause and causality’ to describe the picture science gives us (1980: 123-4). He does, however, accept that use of this terminology constitutes a normal part of science. I discuss this further below.

<sup>21</sup> Van Fraassen’s analysis of explanation focuses on how we determine which are the salient factors; he argues that an explanation is best understood as ‘an answer to a why-question’ (1980: 134) and hence that salience is pragmatically determined. On the significance of this insight for the evaluation of competing causal explanations in IR see Grynaviski (2013), Chernoff (2014), and Humphreys (2017b).

<sup>22</sup> On the utility of Cartwright’s distinction between identifying causes and putting them to work in causal explanations for thinking about the nature of causal explanation in IR see Jackson (2017).

can accept an explanation which invokes an unobservable entity while remaining agnostic about whether that entity exists. This difference does not, however, lead constructive empiricists to offer different explanations than scientific realists do (cf Chernoff 2014: 256) – they differ over the extent of belief science requires in the theories on which explanations draw, not over the world-picture those theories generate.

### Regularities and causal powers

The preceding analysis suggests that constructive empiricism differs from SR far less strongly than is suggested by scientific realist arguments against empiricism in IR. Van Fraassen describes scientists as (i) immersed in theories which (ii) typically go beyond what is observable and (iii) are developed largely through experimentation. He also notes (iv) that these theories provide the scientific world-picture that is put to work in explanations. He agrees with scientific realists (v) that any claims such theories make about unobservable entities must be construed literally, but notes (vi) that belief that such theories are not only empirically adequate but also true in what they say about unobservable entities makes no difference to the evidence which counts in favour of them and hence argues (vii) that such belief is supererogatory. What makes this a distinctively empiricist position is neither the scientific method nor the theoretical world-picture it commends, but rather its epistemological modesty (see 1985; 2002) – Van Fraassen interprets science as requiring less belief for theory acceptance than scientific realists do, and hence denies that it requires a commitment to entity realism. Like SR, however, constructive empiricism is ‘non-revisionary’ with regard to scientific practice – it aims to render that practice philosophically intelligible, but does not seek to change it (Rosen 1994: 156). Consequently, constructive empiricism and SR offer competing interpretations of scientific practice, not competing prescriptions for it.

Nonetheless, a puzzle remains. Van Fraassen's characterization of science as seeking to discover regularities may not have the methodological implications that critical realists suppose, but it contrasts sharply with Bhaskar's argument that science is intelligible only if the invariances discovered in experiments are construed as evidence of underlying causal powers. Van Fraassen argues, in effect, that science may be understood as *merely* identifying regularities – that we are *not* compelled to construe these regularities as the manifestation of underlying causal powers. In other words, he seeks to resist not only entity realism, but also causal realism (see 1980; 1989).

I do not seek to resolve this debate. Rather, the question I consider is how much of a difference for causal inquiry these competing metaphysical stances make, when considered in their own right. This question has received hardly any attention in IR, for the presumption, among critical realists, has been that the metaphysical disagreements between empiricists and scientific realists will be accompanied by methodological differences, differences which will handicap an empiricist approach to causal inquiry. Yet Van Fraassen argues that we can, without cost, sidestep metaphysical issues. In other words, whereas Bhaskar argues that we can only make sense of the practice of science on the supposition that causal powers are real, Van Fraassen argues that we can do without this supposition and still get on with the business of science. If so, then this further undermines the contention that embracing SR will open up new avenues of causal inquiry, as compared to empiricism.

Van Fraassen recognizes, of course, that 'when scientists describe the world they do so in causal discourse' (1993: 438). When explaining what their theories show, for example, scientists often use causal terminology, describing theoretical models as containing 'causal mechanisms' (1980: 80) and describing the picture of the world given by science and put to

work in explanations in causal terms (1980: 123-4). But Van Fraassen questions whether this reveals anything about the world as it is independent of science. He identifies an alternative possibility – scientists’ causal discourse might describe ‘features of our models, not features of the world’ (1989: 214). In other words, scientists’ causal discourse may be *just a way of talking* about what science shows, a way of talking that does not in fact latch on to any feature of the world as it is independent of science. However, this is not an argument for causal idealism – Van Fraassen does not *deny* that causal powers are real (1993: 435).<sup>23</sup> Rather, he defends an agnostic position, withholding judgement on whether scientists’ causal discourse latches onto features of the world as it is independent of science.<sup>24</sup>

In defending this agnosticism Van Fraassen advances two arguments which speak to Bhaskar’s position. First, whereas Bhaskar argues that invariances observed under experimental conditions are explained by the operation of underlying causal powers, Van Fraassen rejects ‘the demand for an explanation’ of such regularities, ‘by means of truths concerning a reality beyond what is actual and observable, as a demand which plays no role in the scientific enterprise’ (1980: 203). He is happy simply to say: ‘that the observable phenomena exhibit these regularities ... is merely a brute fact, and may or may not have an explanation in terms of unobservable facts “behind the phenomena”’ (1980: 24). He points out that if we *do* ask what explains these regularities, then in order to avoid an infinite regress (viz. what explains what explains the regularities?, etc.) it will be necessary to postulate, behind the regularities, some natural properties which do not themselves require explanation. But in that case, he observes, there will *always* be *something* left unexplained (1980: 205-7, 213). In short, causal realists and agnostics stop in a different place – whereas causal realists seek to explain why there are

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<sup>23</sup> For a defence of causal idealism in IR see Lebow (2014).

<sup>24</sup> He is therefore not guilty of what Bhaskar (2008: 36) terms an ‘epistemic fallacy’.

regularities but consider it unnecessary to explain why there are causal powers, Van Fraassen considers it unnecessary to explain why there are regularities.

Second, whereas Bhaskar holds that the exportation of experimental results to everyday settings is intelligible only on the supposition that experiments identify real causal powers, Van Fraassen denies that science's success in manipulating the world requires explaining. He is happy to observe simply that '[o]nly the successful theories survive – the ones which *in fact* latched on to actual regularities in nature' (1980: 40). Consider, once again, the case of the feather when it falls under everyday conditions. In such cases, Clarke notes, most 'scientists don't hesitate to claim that the universal law of gravity continues to apply', even though they are 'unable to incorporate the force of gravity and the forces of wind in an accurate model' (2010: 302).<sup>25</sup> Yet this does not reveal anything about the world as it is independent of science (see Clarke 2010: 313-4). If scientists do suppose that regularities hold outside the conditions under which they are identified, this may amount to no more than a promissory note, indicating an expectation that science will one day be able to model how forces combine in open systems.<sup>26</sup> Even if the supposition is based on a belief that the world consists of real causal powers, Van Fraassen regards such beliefs as supererogatory. At the heart of his position is a contention that observable phenomena cannot adjudicate the truth or falsity of claims about causal powers (1980: 2) and hence that we can remain agnostic about such claims.

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<sup>25</sup> Clarke is actually discussing Neurath's example of a falling bank note, but the idea is the same.

<sup>26</sup> Cartwright (1999) identifies yet another possibility – that scientific laws only hold locally (see also Cartwright 2010).

Whether persuaded by Van Fraassen's arguments or not, readers may wonder how this somewhat abstruse metaphysical debate is relevant to causal inquiry in IR. If so, then, once again, that is my point – whereas critical realists argue that an embrace of SR will open up new possibilities for causal inquiry in IR, it is not clear that embracing causal realism rather than remaining agnostic will make this kind of difference. Consider an example of a scientific explanation provided by Van Fraassen: of why the conductor in a power station was warped following a short circuit. He shows how, given information about the momentary current produced by the short circuit, the size and orientation of the conductor, and the size of the earth's electro-magnetic field, the 'theory of electro-magnetism allows us to calculate the force exerted on the conductor at the time' of the short circuit (1980: 102). In his example, doing this calculation reveals that the surge in current generated a significant increase in the downward force exerted on the (horizontal) conductor by the earth's electro-magnetic field. This leads him to argue that the warping is explained by the short circuit.<sup>27</sup> Expressed in everyday terms, he argues that the short circuit *caused* the warping. The question I am posing is: what difference does it make if we construe this language of causes and forces as latching on to real causal powers, as compared to remaining agnostic on this matter?

This question may be illuminated by contrasting Van Fraassen's position to that of Nancy Cartwright, a scientific realist who has been particularly prominent in pushing the case for causal realism, especially against Van Fraassen. She argues that causal claims about forces such as electro-magnetism 'are best rendered as ascriptions of capacity' (1989: 141). She would therefore treat the claim that the short circuit *caused* the conductor to warp as invoking a causal capacity (or power) in nature which was responsible for the outcome – a capacity of

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<sup>27</sup> This is conditional on certain pragmatic assumptions about what kind of explanation is being sought (1980: 141-2; see also above).



electro-magnetic fields, under the right conditions, to produce the kind of warping observed in the conductor. This is consistent with Wight's stipulation that an explanation should provide 'an account of how the underlying mechanisms work' (2006: 32). Van Fraassen, by contrast, would be more modest. He would interpret the proffered explanation as accounting for the observed outcome by reference to, and in the language of, a widely accepted scientific theory which makes particular claims about the properties of electro-magnetic fields. He would, however, remain agnostic as to whether those properties reflect underlying causal capacities in nature.

This suggests that in thinking about the difference that causal realism makes it is helpful to distinguish two kinds of question. The first concerns what explains (or caused) a specific outcome – in this case, the warping. On this question, Van Fraassen and Cartwright will give the same answer: it was the short circuit, given the earth's electro-magnetic field. The second concerns why the regularities identified by the theories put to work in such explanations hold. On this question, Van Fraassen and Cartwright will give different answers. Whereas Cartwright will say that regularities in electrical phenomena hold in virtue of the causal capacities that produce them, Van Fraassen will acknowledge that *some* scientists certainly *describe* things in these terms, but will prefer to remain agnostic about whether such capacities are features of the world as it is independent of science.

As with their epistemological difference over the extent of belief required to accept a scientific theory, this metaphysical difference between constructive empiricism and SR is *philosophically* significant. However, insofar as Van Fraassen and Cartwright accept the same theories and put them to work in relation to the same problems, they are likely to offer the same explanations. It is, in other words, far from obvious that embracing causal realism will make a substantive

difference for causal inquiry. Certainly, scientific realists in IR have not demonstrated that it does make such a difference, for their presumption has been that empiricists are unable to accept the same theories and offer the same explanations as scientific realists. This is not true of the kind of empiricism defended in contemporary philosophy of science.

### What's at stake for causal inquiry in IR?

Critical realists in IR construe empiricism as being differentiated from SR both by the kinds of theories it can accept and also, consequently, by the scientific method it endorses, viz. the search for regularities in the world around us. This makes it appear that the difference between the two positions has substantive implications for causal inquiry in IR. However, in contemporary philosophy of science empiricists and scientific realists do not systematically disagree either about which theories should be accepted or about the practice of scientific inquiry. Their epistemological and metaphysical differences lead them to offer competing *interpretations* of scientific practice (specifically: of the aims of science, of the extent of belief required to accept scientific theories, and of what science reveals about underlying causal powers), but they do not offer competing *prescriptions* for it. Viewing the realist-empiricist debate in philosophy of science as a binary confrontation between philosophical systems, each with a distinctive methodology corollary, is therefore misleading.

Wendt appears to recognize this when he observes that 'realist and anti-realist physicists disagree about the ontological status of quarks, but this does not affect their research'. He argues, however, that this kind of disagreement *does* affect research in the social sciences. This is because 'social scientists are less confident than physicists about what their practice should look like, and have often turned to philosophers for methodological guidance' (1999: 48). However, he describes social scientists as turning to outdated forms of anti-realism,

notably logical empiricism (see also Shapiro and Wendt 1992; Chernoff 2002: 199). Whatever problems this generates do not argue *for SR, as compared to empiricism*, but only against outdated forms of anti-realism which Van Fraassen also repudiates.

This undermines the case for a philosophical revolution in IR in which empiricism is replaced by SR. On the one hand, because critical realists base their rejection of empiricism on positions that empiricists also repudiate, they have failed to make a compelling case that whatever problems they identify in mainstream approaches to causal inquiry are problems of empiricism *per se*. As my discussion of Van Fraassen's constructive empiricism has shown, the kind of 'neopositivist' methodology to which critical realists object is not a necessary counterpart to an empiricist philosophy of science. On the other hand, because critical realists in IR tend to suppose that the epistemological and metaphysical differences between SR and empiricism have a clear methodological corollary, they have failed to show that such differences matter in their own right, when this methodological corollary is absent. In short, they have failed to show that SR would make a difference as compared to the kind of empiricism defended in contemporary philosophy of science.<sup>28</sup>

My objections to scientific realists' calls for a philosophical revolution in IR do not, however, call into question the substance of their critique of mainstream practices of causal inquiry. Quite the contrary! Indeed, a key insight that emerges from engaging with contemporary empiricist philosophy of science is the following: to the extent that the critical realist argument against mainstream practices of causal inquiry in IR is based on the ways in which such practices deviate from the model of scientific inquiry employed in natural science, it is a

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<sup>28</sup> Critical realists might argue that their critical orientation enables them to ask questions that empiricists cannot, but they have not established this case against constructive empiricism.

critique that empiricists can and should join them in advancing. For if SR and constructive empiricism share a common understanding of scientific practice, then any departure from that practice in IR constitutes a challenge to empiricism as much as to SR. What is at stake in critical realist arguments against empiricism in IR is therefore not whether those conducting causal inquiry are scientific realists.<sup>29</sup> What is at stake is a methodological question – to what extent do the presuppositions which underpin mainstream approaches to causal inquiry depart from the practice of natural science as understood by both scientific realists and empiricists, why, and with what consequences?

Although I can, in the space available, only begin to sketch the issues this raises, a key concern arises in relation to Kurki's observation that mainstream approaches to causal inquiry in IR are organized around the search for regularities in the world around us, that is, in everyday world politics. As noted above, Van Fraassen agrees with scientific realists (i) that experimentation is crucial for identifying scientifically significant regularities (which may not be observable in everyday life), (ii) that scientific theories (may) make existential assertions about unobservable entities, and (iii) that in order to explain something it is insufficient simply to subsume it under a regularity. By contrast, although mainstream approaches to causal inquiry in IR are often thought of as empiricist, they diverge from this philosophical consensus on all three counts.

I have already provided examples of mainstream theorists treating regularities observed in everyday world politics as causally significant. Demands that the search for such regularities should constitute a central component of causal inquiry are also prominent in the research methods literature. For example, Gerring contends that 'a mechanistic argument without an

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<sup>29</sup> This is confirmed by the fact that scholars such as Bennett (2013) and Mearsheimer and Walt (2013) endorse both SR and the kind of neopositivist methodology that critical realists reject.

appeal to covariational patterns between *X* and *Y* does not make any sense ... to talk about mechanisms is also, necessarily, to talk about covariational patterns (“correlations”)' (2005: 166).<sup>30</sup> Meanwhile, although King, Keohane, and Verba describe the view that ‘unobservable concepts’ have ‘no place in scientific research’ as ‘untenable’, they nevertheless argue that we ‘should choose observable, rather than unobservable, concepts wherever possible’. They hold, moreover, that ‘[e]xplanations involving concepts such as culture or national interest or utility or motivation are suspect unless we can measure the concept independently’ (1994: 41, 109-10; see also Johnson 2006: 228). Although they do not defend an explicit model of explanation, this suggests, in turn, that causal explanations must be rooted in observable patterns of cross-case covariation.

These methodological demands are, moreover, made in the name of ‘scientific research’ (King, Keohane, and Verba 1994: 7). Jackson exposes the potential problem with such claims – appeals to ‘science’ in IR tend to serve ‘a *disciplining* function’, privileging particular modes of inquiry in the name of ‘science’ (2011: 9-10). He argues, therefore, that the problem with King, Keohane, and Verba’s invocation of ‘science’ is that they prescribe a logic of inquiry which they claim is applicable to ‘all good research’ (1994: 4), whereas really what they are prescribing is a logic of inquiry for ‘all good *neopositivist* research’ (Jackson 2011: 67). Yet the reality is starker than Jackson suggests, for a comparison of their prescriptions with Van Fraassen’s understanding of scientific inquiry reveals the extent to which their prescriptions depart even from contemporary *empiricist* philosophy of science. We are, in short, confronted with a hegemonic practice which departs considerably from the closest supporting arguments

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<sup>30</sup> At best, this holds only in relation to closed systems.

in philosophy of science, but with very little explicit defence of that departure – indeed, very little acknowledgment of it.<sup>31</sup>

It seems reasonable, therefore, to ask those who employ and defend mainstream approaches to causal inquiry in IR to consider how Van Fraassen's reformulation of empiricism might speak to their own practices. In light of the preceding discussion, three areas stand out as deserving closer consideration:

1. How do the regularities sought in mainstream approaches to causal inquiry in IR (viz., those observable in everyday world politics) differ from the kinds of regularities sought in natural science and with what consequences for causal inquiry?
2. Given Van Fraassen's acceptance that unobservable entities are routinely incorporated into scientific theories and that references to them should be construed literally, what is the rationale for seeking to avoid incorporating into social scientific theories factors which cannot be directly measured, such as 'intentions, meanings and understandings' (Wight 2006: 60)?
3. Given the established deficiencies of the covering-law model of explanation, what account of casual explanation is endorsed, and how do patterns of cross-case covariation feed into it?

In posing these question, I do not mean to imply that powerful answers cannot be provided. It is, though, pertinent to note that these are all areas in which there has been innovative work by philosophically-oriented scholars in IR, both from a critical realist perspective and from alternative perspectives (see, for example, Patomäki 2002; Wight 2006; Kurki 2008; Jackson

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<sup>31</sup> King, Keohane, and Verba explicitly sidestep philosophical issues (1994: 3, 6; cf Johnson 2006).

2011; Grynaviski 2013; Lebow 2014; Humphreys 2017b; Jackson 2017; Patomäki 2017). I have shown that recognizing the extent of the challenge this work provides to mainstream approaches to causal inquiry in IR does not require the adoption of SR and repudiation of empiricism. It is not open to those who defend mainstream approaches to say that such considerations do not apply to them because mainstream approaches are empiricist. In all three areas, contemporary empiricist philosophy of science has more in common with SR than with the presuppositions which appear to underpin mainstream practices of causal inquiry in IR.

## Conclusion

To many, SR may be intuitively attractive. Many scientists will believe that (some of) the unobservable entities postulated in their theories are real and that scientific theories identify real causal powers in the world as it is independent of science. From the point of view of the debate between SR and constructive empiricism, however, this is beside the point. What is directly at stake in that debate is not what scientists in fact believe, but rather (i) the extent of belief *required* to accept a scientific theory, that is, whether scientists are rationally compelled to be entity realists and (ii) whether scientists must be causal realists, that is, conceive of science as identifying real causal powers. The question for IR is what difference, if any, the divergent stances taken on these issues by scientific realists and empiricists make for the conduct of causal inquiry.

I have argued that the critical realist call for a philosophical revolution in which SR replaces empiricism as the basis for causal inquiry in IR is misleading as to the methodological significance of the philosophical differences between SR and empiricism. One reason this has not been recognized in IR is that the extent to which empiricist philosophy of science has been

renewed to meet the scientific realist challenge has been neglected, not only by critical realists but also, more importantly, by those who advocate most strongly that causal inquiry should seek to identify patterns of cross-case covariation and develop covering law explanations. To the extent that scientific realists draw on philosophy of science to challenge such demands, it is a challenge that empiricists can and should join them in advancing.



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