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Theoretical Malnutrition and the Realist Therapy in Political Science

Abstract: David Easton's 1953 complaint of political science's 'theoretical malnutrition and surfeit of facts' has been echoed numerous times, recently for instance in the 'Pestroika' movement in American political science. Proposals for rectifying the theoretical deficit have been numerous, but one line of argument has come to predominate: the realist critique of political science's positivism. This essay examines that critique and finds fault both with its characterization of positivism and its proposed remedy for the theoretical deficit. Its diagnosis and therapy are both philosophically inflated, with a dangerous promotion of a transcendental or speculative ontology and concomitant neglect of empirical constraints on theorizing. Yet with a restored but deeper emphasis on the empirical basis of theory, taking the question of the nature and causal mechanisms of 'political culture' as illustrative, and disregarding the inhibitions created by disciplinary boundaries, progress beyond theoretical malnutrition remains possible.

Keywords: positivism, scientific realism, causation, operationalization, covering law

1. Introduction

In 1953, in his 'inquiry into the state of political science,' David Easton famously commented on the discipline's 'theoretical malnutrition and surfeit of facts.' Easton did not mean that political scientists merely collected facts: he ackno-

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wledged that they sought 'to show that variable A is related to variable B and so forth'. But he nevertheless complained that 'most factual research is concerned with singular generalizations, not with a broader type of theory' (Easton, 1953, p. 77). This deficiency, he thought, was reinforced by disciplinary norms: 'when a group of scholars comes together to talk about research, the typical discussion is more likely to move in the direction of improving techniques for gathering facts and identifying new variables or matters suitable for investigation, than of inquiring into the theoretical matrix for research' (Easton, 1953, pp. 66–67). And he found a major source of the problem in premature operationalization: 'Theoretical insight would be too narrowly inhibited if it had to direct its attention at the outset towards immediately verifiable generalization [...]. [W]hile it is true that we ought to be able to state a theory in operational terms, this is the ultimate goal, not the starting point' (Easton, 1953, p. 315).

It is a surprise to find this critique in a book which 'has been characteristically understood as the seminal tract of the behavioral movement' (Gunnell, 1993, p. 236), indeed by the author of a 'credo of behaviouralism' (Easton, 1965, p. 7) – even if Easton's apostasy in his 'credo of post-behaviouralism' (Easton, 1969, p. 1052) is also well known. But the aim here is not to trace Easton's own trajectory, but rather to explore the validity of a critique, like his, of positivist political science that accepts its scientific aspirations while questioning, and seeking to account for, its failure to realize them. Easton is an example of an 'internal' critic of positivist political science, to be distinguished from the better known 'external' critics who reject the scientific aspiration itself, as inappropriate to the political realm or indeed as a covert expression of political interests. Such external critiques are not this essay's concern.

Although Easton's own proposal for filling the gap left by theoretical malnutrition and premature operationalization, 'systems theory', has long passed its peak of popularity, and although political science has diversified considerably in the meantime, the complaint he formulated so succinctly still sounds fresh. It was voiced, for instance, in the debate provoked by the recent 'Perestroika' movement in American political science (e.g. Sanders, 2005). Calls for the promotion of theory in political science have taken various forms: there have been critiques of 'methods driven' research (Shapiro, Wendt, 2005) or 'simplistic hypothesis testing' (Mearsheimer, Walt, 2013), and proposals of 'concrete theory' (Lane, 1990), 'empirical theory' (Monroe, 1997), and 'conceptual theory' (Johnson, 2003). These arguments, despite some differences, have often announced themselves as instances of a 'realist' critique of positivism.² They draw on the emergence of this

² The term 'realism' has of course invited numerous uses among political scientists and political theorists, uses whose detailed relationship with my topic of the realist critique of positivism it would overburden this essay to trace. Joseph Schumpeter's (1975) revision of democratic the-

critique within the philosophy of science (the label ‘scientific realism’ is sometimes used), accusing positivists of failure to keep up with relevant philosophical argument.

This essay will examine and challenge the currently dominant realist expression of the internal critique launched by Easton. It will do this in two stages. In the next section, it will identify some ambiguities in the realist position at its principal, and ostensible, points of contrast with positivism. In the following section the response to realism will go deeper, questioning its account of the key philosophical sources of positivism, David Hume and Carl Hempel, and exploring gaps and contradictions in its own deployment of philosophical argument. The concluding section will offer some pointers towards ways of remedying the deficiencies of positivist political science that do not commit realism’s errors of marginalizing evidence and inviting its philosophical substitution.

2. Realism versus Positivism

Auguste Comte’s coinage of the term ‘positive science’, in part because it is so provocative, remains a helpful starting point for a discussion of the realist critique. Distinguishing three stages ostensibly traversed by all branches of knowledge, ‘the Theological, or fictitious; the Metaphysical, or abstract; and the Scientific, or positive,’ Comte asserted that in the positive stage, ‘the mind has given over the vain search after Absolute notions, the origin and destination of the universe, and the causes of phenomena, and applies itself to the study of their

ory, for instance, which became the basis of the ‘empirical theory of democracy’ and its associated positivist (behaviouralist) method, was formed of propositions he repeatedly dubbed ‘realistic’, as opposed to fanciful or wishful, illustrating the fact that realism and positivism or empiricism often are taken to be the same thing. Still, the realist position in the philosophy of social science clearly rests on this antithesis, which it is the essay’s aim to investigate.

In the case of the subfield of international relations, an inconvenience arises from the (scientific) realist critique in view of the fact that the field already contains a substantive explanatory paradigm with that name. Mearsheimer and Walt are major contributors to that realist paradigm, while Wendt is a leading opponent of it: all have contributed to the realist critique of positivism, however. This terminological inconvenience is arguably also a logical inconvenience, if we agree that substantively realist IR is ‘rational choice theory applied at the level of the state system’ (Hay, 2002, p. 17), and that rational choice ‘stand[s] proudly under the positivist aegis’ (Lane, 1996, p. 376). But if so, it is an inconvenience for Mearsheimer and Walt, not for the present argument.

An exception to the explicit deployment of the philosophy of scientific realism against positivism among the critics I will consider is James Johnson’s invocation of the pragmatist tradition (see below). But as I will show later, the substantive criticism and the proposed therapy Johnson presents closely match those emanating from the realist school.

laws – that is, their invariable relations of succession and resemblance’ (Comte, 1959, p. 75). This foundational statement already expresses clearly three features of positivism to which the realist critique has taken exception: its understanding of causation, its emphasis on the observable, and its rejection of ‘metaphysics.’ Let us inspect these (overlapping) lines of criticism in more detail.

2.1. Causal Laws and Causal Mechanisms

Shapiro and Wendt’s critique of the two dominant positivist variants of political science – rational choice theory (which they call ‘logicism’) and post-behaviouralism (in their account a merely less ambitious form of the earlier behaviouralist school) – finds its defects to lie in the empiricist philosophy of Hume, in particular his theory of causation, and in the elaboration of this as a philosophy of science by Hempel. For ‘Hume and his followers,’ they say,

the limits of what can be said to exist – ‘the real’ – were thought to be exhausted by what is given or deducible from direct sensory experience [...] [and] it is not legitimate to infer from putative effects the existence of entities, be they quarks or utilities, that cannot be directly observed (Shapiro, Wendt, 2005, p. 23).

This radical empiricism, Shapiro and Wendt (2005, p. 23) argue, entailed the view that ‘the commonsense idea that causal mechanisms exist in the world, producing the regularities and other phenomena we observe, was no more than metaphysical superstition.’ And on the basis of this denial of ‘common sense,’ scientific explanation was instead understood in terms outlined by Hempel as the ‘deductive-nomological model’ which maintained, in Shapiro and Wendt’s words, that ‘scientists try to subsume particular causal claims under more general lawlike theories, so that the relation between particular instances of lawlike arguments and the arguments themselves can be conceived of deductively.’

The dispute over causation has been mainly conducted in terms of an opposition between causal laws, or laws of nature, and causal *mechanisms*. Comte put the point bluntly: laws of nature, according to him, consist only of ‘invariable relations of succession and resemblance.’ He gives the example of the theory of gravitation, which ‘explains’ the motions of planets and other bodies by subsuming them under a mathematical generalization (Newton’s inverse square law): ‘As to what weight and attraction are, we have nothing to do with that, for it is not a matter of knowledge at all.’ (Comte, 1959, p. 76). These matters with which we have nothing to do are, in the contrary view of realists, the very crux of causal explanation, since they embody causal mechanisms.

Jon Elster (1998, p. 48; emphasis removed) expresses the same stark opposition: ‘the antonym of a mechanism is a scientific law.’ The idea of mechanism has been an appealing component of the realist critique, and has indeed become so widely invoked as to dilute some of the specific meaning that realism attached to it (Gerring, 2010, pp. 1500–1501) – a telling outcome, which suggests that the idea is not necessarily incompatible with a positivist conception of causation, as I will argue below. In any case, for the realists, ‘theories provide a causal story;’ ‘a theory explains why a particular hypothesis should be true, by identifying the causal mechanisms that produce the expected outcome(s)’ (Mearsheimer, Walt, 2013, pp. 431–432).

However, some realists construe the relationship between laws and mechanisms somewhat less exclusively. Mario Bunge comments about Elster’s formulation, ‘Mechanisms without conceivable laws are called *miracles*’ (Bunge, 2004, p. 196). The idea here is that mechanisms need to be understood, in order to serve their explanatory purpose, in terms of regular sequences of events. For Bunge (2004, p. 199), ‘any mechanism-free account must be taken to be shallow and therefore a challenge to uncover unknown mechanism(s). By the same token, any mechanism unsupported by some law(s) must be regarded as ad hoc and therefore equally temporary.’ Even so, Bunge rejects Hempel’s nomological model of explanation on the grounds that it ‘does not involve the notion of a mechanism;’ ‘to explain is to exhibit or assume a (lawful) mechanism’ (Bunge, 2004, pp. 202, 203; emphasis removed).

2.2. The Observed and the Unobservable

While, for Hume, a sceptical view of causation was a paradigm case of his more general empiricism, for realists the supposedly alternative idea of causal mechanism amounts to a paradigm of anti-empiricism, for their point is that mechanisms lie hidden beneath the empirical surface of event regularity. But since mechanisms exist, there must for realists be a dissociation between reality and the empirical.

Positivists have dealt with the question of unobserved mechanisms using what Kolakowski has designated a ‘rule of nominalism.’ He writes that while positivism admits a role for general or theoretical concepts, it requires that ‘we do not forget that these abstractions are no more than means, human creations that serve to organize experience but that are not entitled to lay claim to any separate existence’ (Kolakowski, 1972, p. 15).

Against this, we find realists insisting that it *is* in fact ‘legitimate to infer from putative effects the existence of entities, be they quarks or utilities, that cannot be

directly observed.' Shapiro and Wendt deploy this position in critique of recent 'post-behaviouralist' political science, which in its positivist denial of 'unobservables' is too ready to embrace as evidence whatever is immediately apparent or easily measured, such as 'the views of everyday agents and the institutional practices they support' (2005, p. 29). Their example is the 'faces of power' literature, in which the claim of Robert Dahl and other pluralists that community power was dispersed among competing, but accountable, groups of elites was challenged by a more critical view emphasizing hidden 'structural power' such as the power of agenda-setting. Positivists, they argue, were compelled to reject the critical account: 'the empiricist's characteristic skepticism about unobservables has the effect of privileging everyday explanations of consent by default [, and] these reduce to the claim that if one wants to know whether an agent's evident agreement was coerced in a given situation, one should ask that person' (Shapiro, Wendt, 2005, p. 29). Mearsheimer and Walt too say that causal mechanisms are 'often unobservable' (2013, pp. 431–432), and hence are neglected by positivism's operational measures.

Ruth Lane goes further, endorsing realist Rom Harré's suggestion that for positivism, strictly speaking, 'the back of the moon did not exist before the Apollo fly-pasts' (Lane, 1996, p. 371). But with this example we can detect an overstatement of the realist position somewhat parallel to the one Bunge identified in Elster. No positivist ever spoke so strictly. And by the same token, we might ask whether positivism is bound to eschew the search for evidence that, like evidence of 'agenda-setting power', might be *de facto* hidden but obtainable in principle. Conspiracies are, after all, sometimes exposed. About the 'necessary connection' between events that Hume denied, his argument was that it could not be detected by *any* investigation: 'Contemplate the subject on all sides; you will never find any other origin of that idea [of necessary connection]' (Hume, 1975, p. 75). The idea that the moon is a sphere, or that hidden scheming contributes to municipal politics in New Haven, is surely not an idea of the kind Hume meant to exclude. Positivism merely asks that these ideas be substantiated by observation.

2.3. The Turn to Ontology

The problem of evidence arises at its starkest in the philosophical contrast made by realists between the epistemology-centred doctrine of positivism and the turn to ontology executed by realism. It has been influentially stated by Roy Bhaskar, the originator of 'critical realism', a branch of realist argument specifically aimed at the social sciences and seeking to restore an emancipatory potential

said to have been snuffed out or at best deferred by the positivist preoccupation with measurement and correlation (a hint of which is provided by Shapiro and Wendt's discussion of the faces of power literature). His work, initially appreciated largely in Britain, has been increasingly adopted by American social scientists too. Bhaskar writes of the 'epistemic fallacy' committed by positivism and empiricism, which 'consists in the view that statements about being can be reduced to or analysed in terms of statements about knowledge, i.e. that ontological questions can always be transposed into epistemological terms' (Bhaskar, 2008, p. 36).

Bhaskar's mode of argument is transcendental: he derives an account of reality and its structure by asking what must be the case in order for science to be possible. Briefly stated, three things must be the case. We must have empirical experiences; this much is common ground with empiricism. These experiences must in general be experiences of actual events. Realism here does not claim anything unacceptable to positivism (which has never denied the existence of an external world), but places far more stress on it. And, crucially, the observed regularities of events must instantiate enduring causal mechanisms, even though these are themselves not observed. From this argument, Bhaskar derives his 'stratified' conception of the three domains of science: the *empirical* domain of experiences, the *actual* domain of events of which those experiences *are* experiences, and the *real* domain of the causal structure of the world which we inescapably posit by doing scientific work upon it (Bhaskar, 2008, p. 56). The 'epistemic fallacy' collapses these three domains into one.

Among the most enthusiastic importers of Bhaskar's 'transcendental realism' into political science has been Colin Hay, who has subtly recast Bhaskar's 'irreducibility of ontology to epistemology' into a 'priority of ontology over epistemology'. As he puts it, 'We cannot know what we are capable of knowing (epistemology) until such time as we have settled on (a set of assumptions about) the nature of the context in which that knowledge must be acquired (ontology)' (Hay, 2006, p. 84). As a 'set of assumptions', ontology is, for Hay, entirely unempirical: to admit empirical considerations would be to reverse the 'directional dependence' of epistemology upon ontology. How then does one arrive at an ontological assumption? Hay's answer is that 'sustained ontological reflection' leads him and a 'remarkable consensus' of ontologists to a 'post-naturalist, post-positivist approach to social and political analysis premised upon the acknowledgement of the dynamic interplay of structure and agency and material and ideational factors' (Hay, 2006, p. 94).

Hay's method of reflection seems to come down either to mere ontological 'plumping', or more likely to the covert reintroduction of empirical criteria despite their explicit proscription. He says (Hay, 2002, p. 163), for instance, that

‘Our preference for [...] [various] perspectives on social and political change may [...] reflect our preference for parsimony or theoretical complexity,’ seemingly giving the latter ‘preference’ free rein; but he also finds rational choice theory wanting for its lack of ‘complex, credible, and realistic analytical assumptions,’ where ‘realistic’ seems to mean empirically plausible. Hay argues explicitly against Wendt and Shapiro’s (1997, p. 167) view that ‘Although from a realist perspective it may be legitimate to argue that unobservable entities and structures exist, realism does not in itself establish the existence of any particular unobservable phenomenon.’ Regarding Marxism, a position many critical realists embrace, Wendt and Shapiro (1997, p. 174) say that its concept of a universal ‘emancipatory interest’ requires empirical demonstration ‘via the methods of science, not via philosophical speculation;’ and regarding Hay’s topic of ‘realist’ models of agency and structure, they say, ‘We can settle them only by wrestling with the empirical merits of their claims about human agency and social structure’ (Wendt, Shapiro, 1997, p. 181).

Bhaskar (2008, p. 36) warns against a view of ontology as the study of ‘a world apart from that investigated by science. Rather [he continues], its subject matter just is that world, considered from the point of view of what can be established about it by philosophical argument.’ It is a mistake to think of ontology as ‘treating of a mysterious underlying physical realm;’ this idea has ‘prevent[ed] metaphysics from becoming what it ought to be, viz. a conceptual science.’ Hay ignores this warning, whereas in contrast Wendt and Shapiro advance a more limited and prophylactic argument whereby realism merely protects a theory against premature rejection when the evidence is not yet available. As already noted, this position is not radically opposed to positivism.

3. Beyond Realism's Caricature of Positivism

Realism’s critique of positivism is in the spirit of Hamlet: ‘There are more things in heaven and earth, Horatio, / Than are dreamt of in your philosophy’ (*Hamlet*, Act 1, Scene 5). However, the survey just offered reveals that the antithesis of realism and positivism somewhat fragments on closer inspection: realists have differing views about the place of natural laws in scientific explanation, the role of observational evidence, and the nature and relevance of philosophical or metaphysical reasoning. In this section I pursue the critique of realism’s critique of positivism at a deeper philosophical level, by investigating the arguments of the alleged sources of positivism’s deficiencies, Hume and Hempel, and by placing realism’s own philosophical basis under scrutiny.

3.1. Hume, Constant Conjunction and Causal Mechanisms

A first sign that the realists' identification of Hume as a source of positivist error might itself be an error is the emergence, since the 1980s, of a substantial revisionist current in Hume scholarship that has declared Hume himself to be a realist, though a 'sceptical' one.³ Social scientific realists have not paid attention to this current. The revisionists suggest that Hume was prepared to allow that 'real' causal connection existed, while insisting that we could not know its true nature: it was a 'secret connexion', in Hume's phrase. They cite passages such as this: 'nature has kept us at a great distance from all her secrets, and has afforded us only the knowledge of a few superficial qualities of objects; while she conceals from us those powers and principles on which the influence of those objects entirely depends' (Hume 1975, pp. 32–33).

If correct, this interpretation would put the authority of Hume on the side of the realist critics of social scientific positivism, leaving them only the problem of identifying a different philosophical source. But the support would be quite meagre, since Hume's scepticism would remain: his insistence on the *secret* character of the necessary connection, if it exists. Hume makes it clear that suppositions as to its qualities would be scientifically useless:

I am, indeed, ready to allow, that there may be several qualities both in material and immaterial objects, with which we are utterly unacquainted; and if we please to call these *power* or *efficacy*, 'twill be of little consequence to the world. But when, instead of meaning these unknown qualities, we make the terms of power and efficacy signify something, of which we have a clear idea, and which is incompatible with those objects, to which we apply it, obscurity and error begin then to take place, and we are led astray by a false philosophy (quoted in Beebee, 2006, p. 112).

The implication of this passage is that, whatever Hume might have thought about the reality or otherwise of unobservable powers or mechanisms, he opposed giving a specific account of them that exceeded the evidence with which we are 'acquainted'. His empiricism and scepticism are clearly on display.

But the corollary of this view is that mechanisms and causal powers would *not* be 'of little consequence to the world' *if we could observe them*. In other words, mechanisms per se are not precluded. It is important to note that Hume was a keen observer of the progress of the physical sciences, writing at length about it in his *History of England* (Wertz, 1993). He knew about the discoveries

³ A key contribution to this revisionist literature is Strawson (2014). For overviews, see Read and Richman (2007) and Beebee (2006).

that better measuring instruments, such as the microscope and telescope, could yield, even if his knowledge was not that of a practising and mathematically competent scientist (Force, 1987, p. 178).⁴ When he wrote that ‘nature [...] has afforded us only the knowledge of a few superficial qualities of objects’, he did not of course mean that we cannot ever discover further qualities by looking more closely, perhaps with the help of a microscope, telescope or other instrument. He could not plausibly have meant that *a scientist* cannot learn anything more about a causal relation by investigating its instances more closely and thus discovering an underlying mechanism.

A derogation of mechanism on Hume’s part is admittedly suggested by his observation that ‘While Newton seemed to draw off the veil from some of the mysteries of nature, he shewed at the same time the imperfections of the mechanical philosophy; and thereby restored the ultimate secrets to that obscurity in which they ever did and ever will remain’ (quoted in Force, 1987, p. 177). But the ‘mechanical philosophy’ he here rejects, and takes Newton to reject, is the substantive claim, made for example by Descartes, that causal relations all have a literally mechanical character, involving ‘corpuscles pushing against each other’ (Glennan, 1996, p. 51). No present-day realist, or scientist, would embrace this claim. For Hume and, on Hume’s account, Newton, such a view exceeds what the evidence will allow. Consistently with positivist nominalism, he writes:

When we call [the inertia of a body according to Newton’s natural philosophy] a *vis inertiae*, we only mark these facts, without pretending to have any idea of the inert power; in the same manner as when we talk of gravity, we mean certain effects, without comprehending that active power (quoted in Force, 1987, p. 172).

Thus, it is ‘occult’ mechanisms that Humean empiricism objects to, not mechanisms of whose workings we can discover evidence.

Hume’s theory of causation therefore insists that evidence is needed in order for claims about causal mechanisms to be valid (which does not rule out that such claims might be instrumentally useful when made prior to the collection of evidence by stimulating the search for it). But in addition, his theory requires that such evidence must take the form of ‘constant conjunctions’, generalizations applicable to all cases we have observed, observations which trigger our habit of recognizing causation. Mechanisms are themselves regularities, and even though we can explain a visible regularity in terms of a mechanism we might

⁴ James Force (1987, pp. 170–177) quotes, as well as examines, all eleven of Hume’s published references to Newton.

have to work hard to observe (by using a microscope or other measuring instrument), that mechanism itself will be manifest to us as a regularity. The existence of mechanisms is therefore compatible with, and indeed actually entails, the view that causation is regularities *all the way down*.

More light can be cast on this question by inspecting Glennan's (1996) much cited critique of Hume's 'regularity theory of causation.' Advancing as a supposed alternative a 'mechanical view of causation,' Glennan defines a 'mechanism underlying a behavior' as 'a complex system which produces that behavior by [...] the interaction of a number of parts according to direct causal laws' (Glennan, 1996, p. 52). He discusses as examples the 'strictly mechanical' case of the float valve in a toilet cistern and the 'non-mechanical' electronic mechanism of a voltage switch (the basis of a transistor or amplifier).

To be considered as a valid objection to Hume's account, Glennan's first example would have us imagine Hume denying that explanation of the filling of a toilet cistern would be advanced by removing the lid and inspecting the arrangement of parts inside. This is of course implausible. As for the second example, the mechanism Glennan describes is itself just a specification of the lawlike relations of the inputs and outputs of the switch – a position resembling that of Bunge already mentioned, but again nothing that taxes Hume's framework.

Glennan furthermore agrees that laws describing the behaviour of electrons within the switch cannot themselves be explained by a lower-level mechanism: they are in his terms 'fundamental.' It is indeed, according to prevailing interpretations, precisely the hallmark of quantum phenomena like the behaviour of individual electrons that their 'reality' can be expressed only in probabilistic terms – a feature of quantum mechanics that, as Arthur Fine (1996) has discussed in detail, proved highly problematic to a critic from the realist direction such as Einstein. It would doubtless be disconcerting to Hume too to learn that 'fundamental' regularities such as those involving subatomic particles might be no more than stochastic. Yet the mystery to which Einstein objected – the construal of an electron not as an object but as a probability function – would nevertheless be quite consistent with Hume's theory of causation. Arguably it is indeed the ultimate vindication of it.

3.2. Hempel and Explanation by Covering Laws

The second key philosophical target of the realist critique is, as we have seen, Carl Hempel, who is invoked particularly in connection with the alleged error of being satisfied with scientific laws, or mere generalizations, in the formulation of explanations, when it is mechanisms that are needed. One objection to Hem-

pel, that ‘invariant laws’ are not plausibly invoked in any explanation of human behaviour, can be set aside easily, as it neglects the fact that Hempel’s ‘covering law’ account of scientific explanation specifies two models, the ‘deductive-nomological’ and the ‘inductive-statistical.’ The presence of the second one is often simply overlooked by realist critics, such as Wendt and Shapiro cited earlier, who object to the ‘deductivism’ of Hempel’s account. Hempel (e.g. 2001b, pp. 284–286) is indeed quite clear that the ‘covering laws’ that are invoked in historical explanation are likely to be of the statistical kind, representing tendencies rather than invariant event sequences.

A more substantial objection is that positivists fail, and must by virtue of their Humean definition of causation fail, to distinguish between correlation and causation: as Russell Keat and John Urry (Keat, Urry, 1982, p. 28) put it, ‘on a regularity theory of causation, many [...] cases of non-explanatory regularities will qualify as causal relations.’ Mario Bunge similarly displays his realist credentials by criticizing Hempel’s covering law model – represented as the formula ‘Law & Circumstance \Rightarrow Fact to be explained’ – as not being explanatory, because it does not specify a mechanism.

It is indeed not an explanation of an event to say ‘that is what always happens under those circumstances’. But this is not what the covering law model says. Hempel’s own example makes this clear (1959, p. 232). The cracking of a car radiator overnight is explained by the conjunction of the (major premise) laws governing the brittleness of the metal of which the radiator is constructed and the law describing the anomalous expansion of water with the (minor premise) fact of the night’s sub-zero temperature. The reasoning is explanatory because of the difference in descriptive level between the phenomenon to be explained and the laws cited in its explanation. It is not a ‘law of car radiators’ that is cited in explanation but laws about metals, water, volume and temperature.

Part of the problem is the label ‘covering law model’, first used by one of Hempel’s critics, William Dray (1957), which Hempel (e.g. 2001a, pp. 69, 83, note 2) was perhaps unwise to adopt as his own. It suggests that the law ‘covers’ the event to be explained by actually *referring* to events of this type. But Hempel is not implying, for example, that the French Revolution would be explained by constructing a ‘law of revolutions’ and conjoining with it the particular circumstances of France in early 1789. Neither for car radiators nor for political revolutions are the relevant laws formulated at the same descriptive level as the event to be explained. They are indeed precisely the lower-level or deeper, but also broader and more abstract, generalizations which Bunge and Glennan say that positing a mechanism involves, and that scientific explanation requires.

3.3. Evidence and Philosophical Argument

The question of mechanism is often conjoined with that of observability, as in Mearsheimer and Walt's claim that positivists 'dismiss the idea that theories contain causal mechanisms that reflect what is actually happening,' and suppose that 'science is all about measuring observables, which in turn encourages hypothesis testing' (Mearsheimer, Walt's, 2013, p. 433). But as noted previously, realist critics construe positivism's requirement for observational evidence very narrowly, so that even minor impediments to observation (like the lid of a toilet cistern) are taken to frustrate the requirement and put a stop to positive science. Yet it is more consistent with the arguments of positivist philosophers, and of course with the practice of scientists, to construe its evidential requirement in terms of 'observability in principle.' Unacceptable to positivism is only the permanent and insuperable 'occultness' of existential claims. It is, of course, not always easy to tell what possibilities of observation might arise in the future – although in many cases, such as theories of political conspiracy or of the topography of the dark side of the moon, a highly plausible guess as to its future possibility can be made. For this reason, a permissive standard for 'observability' is sensible, so that potentially useful avenues of research are not foreclosed. But one does not need realist philosophy to justify this.

Furthermore, realist philosophy, precisely because of the key role it gives to philosophical argument and in particular to transcendental ontology, supports a standard of evidence that errs in the opposite direction, of being *too* permissive: admitting those 'occult entities' of which Hume complained. Not only is it tempting for realists to smuggle in substantive empirical claims like those of 'historical institutionalism' under the cover of philosophical ontological reasoning, but the transcendental form of argument itself, which 'asks what the world must be like for science to be possible' (Bhaskar, 2008, p. 36), is not so obviously free of empirical considerations as is claimed. At the most basic level, we might ask whether an argument of the form 'For Y to be the case, X must be the case; and Y is the case' necessarily escapes the need for evidence in support of its major premise, while of course its minor premise is wholly empirical.

Justin Cruikshank makes the related point that transcendental argument must admit its own fallibility, since it derives its conclusion from the existence of a practice of science that might itself change (Cruikshank, 2007, p. 276). Bhaskar admits this in principle, but insists that his ontological inference 'is (at present) uniquely consistent with the historical emergence, practical presuppositions and substantive content of the sciences' (quoted in Cruikshank, 2007, pp. 277–278). Cruikshank's concern that an 'ontological pluralism' of competing transenden-

tal accounts of the hidden features of the world might potentially emerge is in fact realized by the fact that Bhaskar's is not the only contender. Stephen Clarke (2010) notes that Nancy Cartwright has reached the entirely different conclusion of a 'dappled world' in which causal mechanisms are very limited in their scope and cannot be inferred to apply universally on the basis of successful laboratory experiments. Clarke seeks not to decide the issue, but simply to show that transcendental argument is not unique in its conclusion – contrary to Bhaskar's assertion. Of course it might be that further discussion among philosophers will resolve this disagreement, but this is hardly a basis for supposing that philosophical argument per se has an infallible capacity to yield a clear and unique conclusion. It has seldom done so.

I am of course treating these philosophical debates with undeserved brevity: the point, however, is not to resolve them but simply to show that they exist. That is already enough to cast serious doubt on the realists' claim, supposedly philosophically authorized, to speak of things in heaven and earth not recognized by positivism. As social theorist Isaac Reed has put it, realism 'proposes a framework in which going beyond evidence is warranted and indeed necessary' (Reed, 2011, p. 41). It 'use[s] theory to go beyond the facts, but remain responsible to those facts' (Reed, 2011, p. 63). But the nature of this responsibility to evidence which nevertheless goes beyond it is obscure. It is in this way that the realist diagnosis of the problem of theoretical malnutrition points towards a highly counter-productive therapy.

4. Conclusion and Prospect

The realist critique as a response (knowing or unknowing) to Easton's diagnosis of theoretical malnutrition in political science errs both in diagnosis and in therapy. Its diagnosis is based on a caricature of the philosophical sources of positivist political science, and with equal over-reliance on philosophy its therapy either risks legitimating a speculative deployment of abstract reasoning in place of a search for evidence of mechanisms, or more likely has simply reintroduced empirical claims under the cover of transcendental reasoning, and hence with inadequate support. But with this conclusion we do not quite return to our starting point, an undiagnosed and untreated condition of theoretical malnutrition and premature operationalization. We have at least gained some sensitivity as to how things can go wrong in responding to this condition, implying some pointers towards better diagnosis and therapy. An example borrowed from one of the internal critics of positivist political science – the example of political culture research – can illustrate.

While styling his critique a pragmatist rather than a realist one (Johnson, 2003, p. 110), James Johnson nevertheless sounds many realist themes in his discussion of the example of positivist research into political culture. He suggests it ‘not only neglects conceptual resources on offer in other research traditions, it also trades upon poorly specified explanatory mechanisms’ (2003, p. 95). It is, he says (2003, p. 97), ‘driven by neither theory nor data. It is driven by technique,’ in particular the technique of the attitude survey, and as a result neglects questions about the measurability by this technique of ‘tacit “typically unstated” orientations’ (2003, p. 99). It offers ‘no plausible account – causal, functional or otherwise – of how political culture “works,” of how it motivates individual action or generates persistence or change in aggregate political action or economic behavior’ (2003, p. 103).

Johnson’s therapy is ‘conceptual theory,’ a term borrowed from philosopher Larry Laudan, and certainly promisingly named. But his implementation of conceptual theory soon takes a wrong turn, which we can now easily recognize. A turn to the interpretive anthropology of Clifford Geertz, while it is a well-trodden path (see Welch, forthcoming), is a blind alley if the purpose is to discover ‘how political culture works,’ for such a project of causal inquiry collides with Geertz’s widely cited dictum that ‘culture is not a power, something to which social events, behaviors, institutions or processes can be causally attributed; it is a context, something within which they can be intelligibly – that is, thickly – described’ (Geertz, 1975, p. 14). It is true that Geertz once entertained the investigation of the causal mechanisms of culture (see Welch, 2013, pp. 55–56). But it is his later turn away from causal theory and explanation, where his ideas were mainly programmatic, towards interpretation that constitutes Geertz’s main legacy, with its damning conclusion that ‘calls for “a general theory” of just about anything social sound increasingly hollow, and claims to have one megalomaniac’ (Geertz, 1993, p. 4). Only a marginalization of responsibility to evidence, typical of realism, could allow this embrace of what I have called theoretical denial (Welch, 2013, pp. 4–5, 37, 57–59) in the search for ‘conceptual theory.’

Johnson pinpoints a more promising starting point for therapy when he notes the vagueness of the specification of political culture itself. Gabriel Almond’s initial definition was hardly illuminating: ‘Every political system is embedded in a particular pattern of orientations to political action. I have found it useful to refer to this as the *political culture*’ (Almond, 1956, p. 396). And it was rapidly succeeded by operationalization in the form of the attitude survey (Almond, Verba, 1989), a technique of undoubted productivity in revealing the distribution of attitudes but casting no light on their nature, which it takes entirely for granted.

Where might one look for an improvement on this conceptualization? Social psychology is an obvious candidate. It too has been much engaged in field

surveys, prompting the complaint by Herbert Blumer (1986, p. 92) that 'Despite the vast number of studies of attitudes that have been made over the years, I am unable to find that they have contributed one iota to knowledge of the generic nature of attitudes.' Yet recent experimental work in the discipline has moved beyond such operationalist nominalism. For example, a theory of 'dual attitudes' and the 'adaptive unconscious' has been developed by Timothy Wilson and associates (Wilson, 2002; Wilson et al., 2000). Its implications are radical: a hint of them is provided by the suggestion that what attitude surveys measure is what is psychologically accessible to the respondent at the time of the survey rather than what actually motivated action (Wilson et al., 1996). Still more radical implications are contained in psychologists' studies of consciousness and free will (e.g. Wegner, 2002), and in arguments about 'dual process' decision-making advanced recently by Nobel Prize-winning economist Daniel Kahneman (2012). Developing these implications in political science has potential for showing how political culture works without exceeding the bounds of empirical evidence (for an attempt, see Welch, 2013).

Many other concepts in political science whose conceptualization has been rapidly superseded by operationalization would merit similar investigation. The very productivity of operationalization, in terms of the yield of data, has tended to obscure this possibility. Furthermore, a defensive deployment of positivist precepts, as when Harry Eckstein (1996, p. 473) said of political culture that it 'is not some "real thing out there" that may be characterized correctly or incorrectly [...] [, but] should be taken to mean what the patent holders intended it to mean,' renders the task of improving conceptualization impossible.

I have tried to give a glimpse of such an exploration, as illustration of what is involved in that phase of theory which Easton pointed to when he complained of operationalization becoming the starting point of science instead of its ultimate goal. His own therapy, systems theory, in fact prefigured realism by being abstract and unempirical. This kind of false turn (though not that specific example), now bolstered by a large apparatus of philosophical argument, is now widely defended by the realist critique of a discipline that is accused of not keeping up with philosophical trends (Lane, 1996, pp. 362–365). Reaching instead not into philosophy, but into adjacent disciplines, for empirical accounts of 'real things out there' beyond the operationalizations that have been productive and have become proprietary in political science, would be a sign of our discipline's maturity.

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