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Can the Eleatic Principle be Justified?

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1 Introduction

The Eleatic Principle or causal criterion is a causal test that entities must pass in order to gain admission to some philosophers' ontology.[1] This principle justifies belief in only those entities to which causal power can be attributed, that is, to those entities which can bring about changes in the world. The idea of such a test is rather important in modern ontology, since it is neither without intuitive appeal nor without influential supporters. Its supporters have included David Armstrong (1978, Vol 2, 5), Brian Ellis (1990, 22) and Hartry Field[2] (1989, 68) to name but a few.

Clearly though, if such a principle is to be anything more than just a statement of a certain version of physicalism, it must be argued for. In this paper I will look at the arguments that have been put forward for the principle and suggest some problems for each of these. Of course in such a survey I cannot provide a decisive refutation of the principle (as if anyone *ever* provides such a thing in metaphysics!), but I do hope to show that, despite its intuitive appeal, the Eleatic Principle's main justifications either look *ad hoc* or do not yield a version of the principle that delivers the intuitively correct results about some fairly uncontroversial cases. This is not an entirely negative result though. Once we look at the shortcomings of the motivations for the Eleatic Principle a more general principle suggests itself. This more general principle looks very much like Quine's thesis that we are ontologically committed to all and only the entities that are indispensable to our current best scientific theories.

I should say from the outset that I am not intending to enter into the realist/anti-realist debate in this paper. I will assume realism is true but that some criterion is needed to distinguish real entities from fictional ones. It should also be stressed that any criterion put forward for such a task is a *criterion of acceptance*. As Keith Campbell puts it:

> This search for a criterion for the real must be understood as a search for a criterion *for us to count something as real*; it will be a principle to apply in determining whether to accord that status, given our current stage of

[1] It is called the Eleatic Principle after a passage from Plato's *Sophist*, in which the Eleatic stranger suggests that causal power is the mark of being. (Plato, 1935, 247d–e) David Armstrong cites this passage in *Universals and Scientific Realism* (Armstrong, 1978, Vol 2, 45–46).

[2] Field's criterion is a little different, in that he maintains that acceptable entities must be causally active or, at the very least, be spatio-temporally located.

> epistemic development. There need not be, and probably cannot be, any critical mark of the real itself; the real is what is, period. (Campbell, 1994, 28)

Also a word or two about a precise formulation of the Eleatic Principle. Graham Oddie (1982) attacks the Eleatic Principle by systematically questioning a number of specific formulations of it. Although he does this with considerable success, I will not follow him down this path, since my criticisms of the Eleatic Principle, for the most part, will not depend on any particular formulation. What I take issue with is *the motivation for any formulation of the principle.* In any case, it may be useful to specify a particular version, just by way of example. I suggest the following is as good as any:

Principle 1 *(The Eleatic Principle) An entity is to be counted as real if and only if it is capable of participating in causal processes.*

In one direction a principle such as Principle 1 is reasonably uncontroversial. Most realists agree that causal activity is a *sufficient* condition for an entity to be counted as real.[3] It is more controversial that entities *capable* of participating in causal processes ought to be counted as real (as Principle 1 asserts), for it might be argued that Pegasus would pass such a test. Thankfully we can put this issue to one side: the important question, for our present purposes at least, is whether causal activity is a *necessary* condition for an entity to be counted as real. In what follows I will argue that it is not.

In the next five sections I address what I take to be the four most common motivations for the Eleatic Principle and I raise difficulties with each in turn. In section 2 I discuss an inductive argument sometimes used to justify a causal criterion, while in section 3 I look at a very influential epistemic argument put to the same purpose. Section 4 I devote to an argument which ultimately rests on the claim that only causally active entities can have explanatory power, and this I show is not true. In section 5 I address some counter arguments that the causalists of section 4 may appeal to and in the following section I look at the possibility of motivating the causal criterion by rejecting inference to the best explanation while still remaining a realist. In the final section I sum up and suggest what I take to be the moral to be drawn from the difficulties faced by the Eleatic Principle. The moral is not new—it's just that Quinean indispensability looks more promising as a method of demarcating the real from the instrumental and fictional entities—but it *is* interesting to see how we are led to Quinean indispensability by the shortcomings of the Eleatic Principle.

2 The Inductive Argument

The first motivation for a causal criterion I will consider is an inductive argument from uncontroversial cases of real entities. We start by noting that there are some fairly widely held intuitions (amongst realists at least) about roughly where

[3]Ian Hacking (1983) argues for this claim by applying his interventionist test: those entities that can be manipulated as tools in scientific investigations, as opposed to those that are merely tested for, are to be granted real status.

the demarcation between the real and the instrumental should be. It should include physical objects, including theoretical entities, perhaps fields and hence waves as disturbances in these fields, amongst the real entities, but should not include (concrete) possible worlds[4] and frictionless planes. We thus see an initial motivation for the causal criterion: all the things that we intuitively think of as real are the sorts of things that participate in causal processes (in this world at least), whereas those that we intuitively think of as unreal do not participate in such processes.

Thus motivated, the Eleatic Principle is an inductive hypothesis about the way the world is. We look at the things in the world that are uncontroversially real, such as tables and chairs and notice that they are all causally active. Then, by induction, we conclude that all real entities are causally active. At first glance a causal criterion thus motivated looks as if it is purely descriptive—it lacks the normative force that a criterion of acceptance, such as Principle 1, ought to have. This defect, however, is easily rectified by appeal to some thesis such as naturalism which does make substantial normative claims about what we ought to believe. For example, if all evidence suggested that all real entities are causally active (a purely descriptive claim), then naturalism commands us to *believe* that all real entities are causally active (the corresponding normative claim). It is perhaps important to bear in mind that something like naturalism is required to get from purely descriptive theses to normative ones, but in what follows I will assume that such a move can always be made so I will not dwell upon this distinction any longer.

There is a more substantive objection to this motivation though, which is that there are many other properties that the uncontroversially real entities share and that the uncontroversially unreal entities lack. All the uncontroversially real entities are spatio-temporally located, for instance. Indeed, there is argument on this very issue between protagonists of a strictly causal criterion, such as Brian Ellis, and the likes of Hartry Field, who require causal efficacy *or* spatio-temporal location. The reality of space-time points hangs crucially on whether it's causal efficacy or spatio-temporal location that is the important property. Similarly, we could opt for the property of having a positive rest-mass as the mark of the real and again the demarcation would be different.

Worse still, it seems that such an inductive argument is going to depend on what our set of uncontroversially real entities is taken to be. For instance, if we decide to be fairly cautious about selecting the members of this set, and admit only medium sized solid objects, we might conclude that all real entities are coloured! In light of these considerations, how do we come to decide to pin our hopes on causal efficacy and not on other properties?

One possible answer to this question is to argue that it is by virtue of an entity's causal efficacy that we have epistemic access to it, whereas other properties don't force themselves on us like this.[5] In any case, it seems fairly natural

[4]Even David Lewis grants that modal realism is counterintuitive (Lewis, 1986, 135), which is all that I'm claiming here.

[5]Recall that we seek "a criterion for us to count something as real" (Campbell, 1994, 28), so we may well admit that there are other properties that all the uncontroversially real entities share, but if we don't (or can't) have epistemic access to these entities by virtue of these other properties, then we can hardly use such properties as criteria of existence.

to suppose that we have epistemic access to any entity which we are to count as real. I am thus suggesting that it looks as though it is epistemic access that in fact is the prime motivation for the Eleatic Principle and not the above inductive argument at all. At the very least the above inductive argument needs supplementing and this epistemic argument seems to fit the bill.

3 The Epistemic Argument

The epistemic argument is perhaps the most commonly appealed to motivation for the Eleatic Principle. The argument is simply that even if there were causally idle entities we would have no reason to believe in such entities, since their causal idleness ensures that they don't causally interact with us and, after all, what other way do we have to come to know about entities.[6]

A little consideration, however, reveals that this motivation is also somewhat thin, since the Eleatic Principle, depending on exactly how we formulate it, will either require that entities are causally active, or that they are causally active with humans. The latter alternative, I suggest, looks far too anthropocentric to warrant serious consideration, for surely we ought to believe in stars and planets outside our own light cone[7] even though they are not causally interactive with us. To deny the existence of such entities is to effectively believe that the earth is the centre of the universe. This leaves only the 'causally active (not necessarily with us)' formulation. On this reading though the epistemological motivation is lost altogether, for there may be many perfectly legitimate real entities involved in causal networks, but because they are not causally interactive with us, they suffer the same epistemic worries as causally idle entities. Again stars, planets and so on outside our light cone are the prime examples here. That is, the entities whose existence may be motivated by this epistemic concern form a proper subset of the set of causally active entities.

In fact the Eleatic Principle motivated by epistemic concerns seems to suffer all the same worries that the causal theory of knowledge suffers. In particular, it seems that we have no reason to believe in future objects (whether causally active or not) and even universal empirical facts obtained by induction are likely to be problematic. Colin Cheyne (1998), who defends an epistemically motivated causal criterion, suggests the move to *kinds* of entities in order to overcome some of these problems. He suggests the following principle (Cheyne, 1998, 38):[8]

Principle 2 *We cannot know that F's exist unless our belief in their existence is caused by at least one event in which an F participates.*

He argues for this principle based mainly on evidence from scientific practice—what it takes to convince scientists of the existence of a new type

[6]This argument is, of course, due to Paul Benacerraf (1983).

[7]That part of the universe close enough to us so that there has been sufficient time since the Big Bang for light to reach us from it.

[8]Cheyne offers this principle as a "first attempt" and later in the paper refines it to meet some objections. The objections I have to the principle, though, are not deflected by his later modifications, so I shall be content to deal with his first statement of the principle.

of entity. For example, he cites the discovery of the planet Neptune as evidence for Principle 2 (Cheyne, 1998, 39–40). The existence of Neptune was predicted in 1845–6 jointly by Leverrier and Adams based on Newtonian gravitational theory and anomalies in the orbit of Uranus. It seems that the scientists of the time were unwilling to acknowledge the existence of Neptune until Galle first directly observed the planet in 1846. Cheyne claims that the moral to be drawn from such episodes in the history of science is "[i]nteracting is knowing" (Cheyne, 1998, 40).

Firstly, this example is not entirely appropriate, since we certainly had causal contact with Neptune prior to Galle's visual contact, as Cheyne admits—we had indirect causal contact via its disturbance on the orbit of Uranus. Furthermore, a new planet may not qualify as a new kind of entity since it is of the same kind (namely planet) as the earth.[9] In any case, leaving these points aside, there is another moral to be drawn from this and the other of Cheyne's examples. The moral I draw is "don't settle for indirect evidence if you can do better". Clearly in the Neptune example direct visual evidence was better than orbital disturbances of Uranus, and scientists sought this better evidence because it was possible. Contrast this with the announcement by Wolszczan and Frail in 1992 of one of the first discoveries of planets outside our own solar system. These planets were detected because of the effects they were having on a nearby pulsar, PSR1257 + 12. Here visual contact was out of the question due to their distance from earth and yet it seems there was no reservation on behalf of the discoverers about making full blooded existence claims.

> [W]e demonstrate that [...] the pulsar is orbited by two or more planet-sized bodies. The planets detected so far have masses of at least 2.8 $M_\oplus$ and 3.4 $M_\oplus$, where $M_\oplus$ is the mass of the Earth. Their respective distances from the pulsar are .47AU and .36AU, and they move in almost circular orbits with periods of 98.2 and 66.6 days. (Wolszcan and Frail, 1992, 145–147)

Perhaps a better example is that of the discovery of the element germanium. In 1871 there had been no (known) causal contact with this element and, in fact, causal contact wouldn't come until Winckler isolated the metal in 1887. However, because of the "gap" in Mendeleeff's periodic table corresponding to the position of germanium, much was known of its chemical behaviour. Cheyne claims that:

> If [Mendeleeff] believed, prior to 1887, in the existence of germanium, that belief, although true, would not count as knowledge. It could only be a lucky guess, unless it was actually caused, in an appropriate way, by events in which germanium atoms participated. (Cheyne, 1998, 36)

Even if I were to agree that if Mendeleeff believed in the existence of germanium prior to 1887 it would not count as knowledge, it seems extremely harsh

[9] This point illustrates a difficulty with the move to kinds of entities—the difficulty of deciding whether a new entity is of a different kind to other entities already accepted. I will not pursue this point though, because I think Cheyne's approach has other, more serious problems.

to call such a belief "a lucky guess". After all, it's not as though he would have had *no* reason to believe that germanium existed, for there was surely reason to believe that something ought to fill the relevant gap in the periodic table. (Perhaps because of some argument from past predictive success of the table or some appeal to symmetry.) It's not a lucky guess in the same sense that someone may guess a winning lottery number. I claim that, at the very least, Mendeleeff would have had *reason to believe* in germanium prior to 1887, which is all that we are interested in for the present purposes. If you accept this claim, then this example, while perhaps not a counterexample to the causal theory of knowledge, looks like a counterexample to an epistemically motivated Eleatic Principle (such as that which Principle 2 would motivate), since Mendeleeff had justified belief in a novel substance without the causal contact that Principle 2 requires.[10]

It seems clear that appeal to the causal theory of knowledge (which is, after all, what is at the bottom of the epistemic justification) and all its notorious difficulties, is not the right approach for a justification of a causal restriction on ontological commitment. For one thing, an Eleatic Principle thus justified leaves out too many uncontroversially real entities (stars and planets outside our light cone) and secondly, even if one were to accept the causal theory of knowledge, there is no reason to insist that such acceptance implies that causal contact is necessary for *justified belief*, as the last example illustrates.[11] Furthermore, Cheyne's move to *kinds* of entities does not save the Eleatic Principle from these objections.[12]

4 The Argument from Causal Explanation

In this and the next section I will address what I take to be the most important argument for the Eleatic Principle. This argument, in its most compelling form at least, is due to David Armstrong.[13] Armstrong has defended the Eleatic Principle in various places. For example, in *Universals and Scientific Realism* he proposes the following dilemma. "Are these [abstract] entities capable of *acting upon particulars*, or are they not" he asks (Armstrong, 1978, Vol 1, 128). He then raises difficulties for the first horn of the dilemma, since typically

[10]Of course germanium is *capable* of participating in causal processes, so the Eleatic Principle as I set it out in Principle 1 would not rule out belief in germanium prior to 1887. But this just serves to highlight the gulf between the Eleatic Principle and an epistemic motivation for it.

[11]It might even be reasonable to argue that many of our basic justified beliefs require causal contact, but this does not mean that inferential beliefs require causal contact. For example, if I am justified in believing P (because I have had causal contact with the truthmaker of P, say) and I am justified in believing that P implies Q, then surely I am justified in believing Q, whether or not I have causal contact with Q's truthmaker. I am indebted to Peter Forrest and John Bigelow for this point.

[12]In fairness to Cheyne, though, he is interested in a causal criterion of *existence*, whereas I am interested in a causal criterion of *justified belief*.

[13]In the discussion following a presentation of this paper, David Armstrong denied that his argument for the Eleatic Principle relies on all explanation being causal (despite the textual evidence I present below). Be that as it may, the argument I present in this section, whether or not it is the argument Armstrong intended, has considerable plausibility and is worthy of careful attention. For convenience I will continue to attribute the argument (as I read it) to Armstrong

causation involves one change bringing about another, and yet here we have unchanging abstract entities presumably bringing about changes by "some sort of steady, unchanging, pressure" (Armstrong, 1978, Vol 1, 129). He concludes that "[s]uch a notion is perhaps barely possible, but it is impossible to see how such alleged causal operation could ever be identified" (Armstrong, 1978, Vol 1, 129). The other horn of the dilemma is simply that "Occam's razor ... enjoins us not to postulate them" presumably because causally idle entities have no role to play in science (Armstrong, 1978, Vol 1, 130). As Graham Oddie puts it, "[r]espectable entities work for their living, and there is no social security in Armstrong's universe" (Oddie, 1982, 285–286).

In *A Combinatorial Theory of Possibility* Armstrong has more to say about this second horn of the dilemma.

> To postulate entities which lie beyond our world of space and time is, in general, to make a speculative, uncertain, postulation. The postulation may perhaps be defended if it can be presented as *explaining* some or all of the spatio-temporal phenomena. But if the entities postulated lie beyond our world, and in addition have no causal or nomic connections with it, then the postulation has no explanatory value. Hence (a further step of course) we ought to deny the existence of such entities. (Armstrong, 1989, 7–8)

Here Armstrong explicitly cites the role entities play in explanations as the key to justifying the Eleatic Principle. This is not at all surprising, since explanation often plays a key role in justifying scientific realism[14] it would seem only fitting that it should also provide some guide as to the extent of that realism.

While I agree with Armstrong's rejection of the atypical causal action of the first horn of the dilemma, I don't think that the postulation of causally idle entities has no explanatory value. There are many instances of causally idle entities playing important explanatory roles in scientific theories and I will now give a couple of examples of such cases.

4.1 Antipodal Weather Patterns

We discover that at some time t_0 there are two antipodal points p_1 and p_2 on the earth's surface with exactly the same temperature and barometric pressure. What is the explanation for this coincidence?

Notice that there are really two coincidences to be explained here: (i) Why are there *any* such antipodal points? and (ii) Why p_1 and p_2 in particular? The first explanation I will offer is a causal explanation (i.e. featuring only causally active entities) and it addresses the second question. This explanation, presumably, will trace the causal history of the current weather patterns, to arbitrary fine detail if necessary, to account for the weather patterns at p_1 and p_2. In particular, the temperature and pressure readings at p_1 and p_2 at time t_0

[14]For instance, J.J.C. Smart's cosmic coincidence argument which, very crudely, is that the best explanation for the world behaving *as if* there were theoretical entities, is that the entities in question actually exist (if not it would be a remarkable coincidence) (Smart, 1963, 39).

will be accounted for. Notice that an explanation such as this does not explain why p_1 and p_2 have the *same* temperature and barometric pressure, just why each has the particular temperature and pressure that they have, and that these *happen* to be the same. Thus an important part of the original phenomenon is left unexplained.

This case looks similar to that of explaining why there were 11 fatalities on New South Wales roads over the 1995 Easter break. The causal story will give the causal history of each fatality but will not explain why, in particular, there were 11 fatalities. This does not seem like such a deficiency in the road fatalities case, since it seems as though there is nothing (significant) left to be explained above and beyond what the causal story tells us. The case of the antipodal weather conditions though is entirely different.

The difference is due to a theorem of algebraic topology which states that for any time t there are antipodal points on the surface of the earth which simultaneously have the same temperature and barometric pressure.[15] This theorem, or more correctly the proof of this theorem, provides the missing part of the causal explanation. It guarantees that there will be two such antipodal points at any time and, furthermore, the explanation makes explicit appeal to non-causal entities such as continuous functions and spheres.

Notice, though, that this explanation also has its limitations—it does not explain why it is p_1 and p_2 in particular that have the same temperature and pressure. So we see that for a complete explanation of the phenomenon in this example, we require both causal and non-causal elements in the explanation.

4.2 The FitzGerald-Lorentz Contraction

The special theory of relativity tells us, amongst other things, that a body in motion, relative to some inertial reference frame $\mathcal{F}$, suffers a FitzGerald-Lorentz contraction. This is a reduction in the length of the body in the direction of motion, as measured by an observer stationary with respect to $\mathcal{F}$. What is the explanation for this contraction?

Minkowski's great contribution to relativity was in offering an elegant explanation for the Lorentz transformations (including the FitzGerald-Lorentz contraction). This explanation appeals to the now familiar concept of space-time, that is, a three-plus-one-dimensional manifold, which consists of three spatial dimensions and one temporal. Minkowski realised that one of the key assumptions of special relativity, the constancy of the speed of light, could be formalised as the satisfaction of the equation:

$$(\Delta x_1)^2 + (\Delta x_2)^2 + (\Delta x_3)^2 - c^2(\Delta t)^2 = 0 \tag{1}$$

in any inertial frame. Here x_1, x_2 and x_3 are the spatial coordinates, t is the temporal coordinate and c is a constant (the speed of light in a vacuum). Minkowski then introduces the imaginary time coordinate

$$x_4 = ict$$

[15] This theorem is a corollary of the Borsuk-Ulam theorem, combined with some minor structural assumptions (i.e. that the earth is approximately spherical in shape and that temperature and pressure change continuously across its surface) (Kosniowski, 1980, 157–159).

where as usual $i = \sqrt{-1}$. So (1) becomes:

$$(\Delta x_1)^2 + (\Delta x_2)^2 + (\Delta x_3)^2 + (\Delta x_4)^2 = 0 \tag{2}$$

and (2) will be satisfied in every inertial frame if the quantity

$$\sigma^2 = (\Delta x_1)^2 + (\Delta x_2)^2 + (\Delta x_3)^2 + (\Delta x_4)^2 \tag{3}$$

is invariant under Lorentz transformation. This, says Einstein, "shows that the Lorentz transformation so defined is identical with the translational and rotational transformations of Euclidean geometry, if we disregard the number of dimensions and the relations of reality" (Einstein 1967, 31). That is, the FitzGerald-Lorentz contraction is nothing more mysterious than the apparent shortening of a ruler, say, (conceived of as a two-dimensional object) when it is rotated. The relevant invariant quantity is not its 2-dimensional length, but its 3-dimensional length. This latter thesis we might call the invariance of (3-dimensional) length under translation and rotation, and is expressed mathematically as the invariance of the quantity

$$s^2 = (\Delta x_1)^2 + (\Delta x_2)^2 + (\Delta x_3)^2 \tag{4}$$

under linear transformations with determinate $|1|$ (i.e. the transformations are neither contractions nor expansions).

The explanation for the FitzGerald-Lorentz contraction is seen very clearly when one realises that the quantity s^2 in equation (4) is not invariant under Lorentz transformation in Minkowski space (although it *is* under rotation and translation in $\mathbb{R}^3$, as we have seen). The relevant invariant in Minkowski space is σ^2, as given by equation (3). I also stress the obvious here, that this is a purely geometric explanation featuring such non-causal entities as the Minkowski metric and geometric properties of Minkowski space.[16]

5 Causal Relevance

In this section I investigate another reply that supporters of the causal criterion are liable to make. This is to deny the causal idleness of the entities in examples such as those presented in the last section. One plausible way which this can be done is to claim that the entities in the explanation are *causally relevant* but not *causally efficacious*. Frank Jackson and Philip Pettit give a good account of this approach in 'Program Explanation: A General Perspective' (Jackson and Pettit, 1990). Although Jackson and Pettit don't specifically put the notion of causal relevance to work salvaging the causal criterion, none the less, their program could be used for this purpose.

Consider a case of trying to fit a square peg of side length ℓ into a round hole of diameter ℓ. Clearly it will not go. The first reason is non-causal: because of the squareness of the peg (and the roundness of the hole). The second is causal: the resistance offered by the overlapping portion of the peg. Furthermore, it seems that someone in possession of the squareness explanation knows

[16] I am indebted to Jack Smart for drawing my attention to this example in both discussion and in his paper 'Explanation' (Smart, 1990).

more than someone who knows only the overlapping explanation. Jackson and Pettit suggest, and I agree with them here, that although the abstract property of squareness did not *cause* the overlapping, nor did it combine with the overlapping to produce the blocking, it is certainly true that the squareness was efficacious only if the overlapping was. They conclude that the abstract property of squareness is not causally efficacious (at least in this example). There is a sense, though, in which it is not causally irrelevant either. It is not irrelevant in the way in which, say, the colour of the peg is. On this, Jackson and Pettit have the following to say:

> Although not efficacious itself, the abstract property was such that its realization ensured that there was an efficacious property in the offing. (Jackson and Pettit, 1990, 116)

That is, the property of squareness *programs* for the efficacious property of overlapping portions.

While I think there is much to be said for the causal relevance approach, in the end it won't save the causal criterion, for as I see it there are two serious difficulties facing this approach. Firstly, I don't think that this defence will work for all explanations making use of non-causal entities. It will work only for those in which a fully causal explanation (i.e. one in which *all* the entities in question are causally efficacious) is on offer as well as the non-causal one, or where there are non-causal elements in a largely causal explanation. Thus, this strategy won't work for the FitzGerald-Lorentz contraction case, for instance, where only one explanation is on offer and it is non-causal, in that it makes no appeal to causally active entities (only to the geometric properties of Minkowski space). Secondly, although it enables supporters of the causal criterion to classify many apparently non-causal entities as causal, this is done at a fairly high price: significant blurring of the distinction between the causal and the non-causal. This blurring, if serious enough, is just the sort of thing that antagonists of the causal criterion would welcome. After all, if the property of squareness can enter into causal explanations, albeit in a subsidiary role (i.e. as causally relevant rather than causally efficacious), it seems that the causal requirement lets in too much. I am more inclined to admit that causally idle entities can have explanatory power than to fiddle with the definition of 'causal' in this way.

One final move is left open to the supporter of the claim that only causally active entities can have explanatory power, and this is to argue that the geometry of space-time, for instance, while not being causally efficacious, nor programming for causally efficacious properties, may predetermine the range of possibilities. Space-time is thus seen as a *structuring cause* in Dretske's language (Campbell, 1994, 36). This move will allow the supporter of the causal criterion to classify the remaining recalcitrant explanations I've presented as fully causal explanations. But now I think that the difference between such a position and my own is merely verbal. After all, what is the difference between allowing that there are causally idle entities with explanatory power, on the one hand, and, on the other, maintaining that only causally active entities have explanatory power but that some of those entities might be structures that are not directly involved in causal chains? I suggest that whether one classifies such structures as causal or not, the important point is the recognition of the importance of

such structures in scientific explanations.

Where does this leave us then? Either there are causally idle entities with explanatory power, such as the geometry of space-time, or only causally active entities have explanatory power but they may include structural elements such as the geometry of space-time and programming properties such as continuous temperature distribution functions. Clearly it is the former conclusion I have been arguing for, but the latter will do as well. If the causal criterion is motivated by a notion of causally active entity which must include geometric properties, continuous functions and the like, then it is ill equipped to make the demarcation required of it. Geometric properties and mathematical entities are just the sorts of entities the causal criterion is usually appealed to to eliminate. Perhaps this is not a terribly damaging argument against the causal criterion. After all, you could just bite the bullet and accept that the causal criterion does not rule out mathematical and geometric properties as is commonly thought. I have no disagreement with a causal criterion thus construed except that I fail to see what the word 'causal' is doing in its name, since surely such entities are the paradigm cases of non-causal entities. It looks as though the causal criterion is preserved in name only.

6 Rejection of Inference to the Best Explanation

In the previous two sections I discussed, at some length, the motivation for the Eleatic Principle that rested ultimately on the claim that only causally active entities can have explanatory power. It might be useful at this stage to make explicit one other assumption of that argument. This assumption is that we have ontological commitment to the entities in our best scientific explanations, which is simply to say that inference to the best explanation is a justified form of inference—it's just that supporters of the argument of section 4 are committed to all explanation featuring only causally active entities. Once the argument is put that way, another defence of the Eleatic Principle presents itself. This defence is to accept that there are non-causal entities with explanatory power, but to reject inference to the best explanation in its most general form. This position has been defended by Nancy Cartwright in her book *How the Laws of Physics Lie* (Cartwright, 1983) in which she argues for inference to the most likely cause instead of the more general inference to the best explanation. Brian Ellis argues for a similar position in *Truth and Objectivity* (Ellis, 1990) in which he accepts that science makes extensive use of non-causal explanation but that only fully causal explanations carry ontological commitment.[17] If some restriction on inference to the best explanation to causal explanations can be sustained then the Eleatic Principle is justified trivially. I consider Cartwright's and Ellis's arguments for such a restriction in this section.[18]

[17] Of course there are other issues on which Cartwright and Ellis have substantial disagreement. For instance, Cartwright is anti-realist about scientific theories whereas Ellis is not.

[18] In fact, it's more appropriate to see Cartwright, at least, as arguing for inference to the best causal explanation as a principle *in its own right* rather than as a restricted form of inference to the best explanation. Moreover, she does not explicitly argue for the Eleatic principle. Nevertheless, her position may be put to such a purpose, which is why I discuss it here.

6.1 Ellis's Argument

Ellis is a scientific realist, and like many other realists, is so largely because of J.J.C. Smart's cosmic coincidence argument (which I mentioned in section 4). There is one difference though. Ellis does not accept inference to the best explanation as Smart does. Ellis claims that "[o]ntological commitment can derive only from causal process explanations" (Ellis, 1990, 22). The latter is enough for a restricted version of Smart's argument for scientific realism to go through. The resulting realism is restricted to causally active entities.

> The ontology does not admit abstract entities like propositions and sets, unless these can somehow be reduced to entities of other kinds. For such entities have no causes or effects, have no location in space or time, and cannot influence any causal processes. It is argued that while such entities may have a role in model theoretic explanations, acceptance of such explanations carries no ontological commitments; only the acceptance of causal explanations carries any such commitment to the entities involved. The entities occurring in our model theories should generally be regarded as fictions. (Ellis, 1990, 5)

His reason for restricting inference to the best explanation in this way is apparent once we distinguish between two quite different types of scientific explanation. The first is the causal explanation which, on Ellis's account of causation, will typically involve a story about exchanges of energy between physical entities. The second type of explanation is the more general model theoretic explanations, which typically idealise away from real situations. These latter explanations are used as backgrounds for causal explanations. For example, Newton's first law provides the background for a causal explanation of why some moving object comes to rest. These model theoretic explanations typically feature such obviously fictional entities as frictionless planes, non-turbulent, laminar flow and inertial reference frames, so we should not accord existence to the entities that feature in such explanations.

While I agree that this argument presents good reason to be suspicious of entities in such model theoretic explanations, it says nothing of abstract entities that feature in causal explanations. For example, in a fully causal account of a billiard ball collision (i.e. with frictional forces etc.) we will find reference to vectors.[19] Ellis acknowledges as much in the following passage.

> The main argument for realism about theoretical entities is also, apparently an argument for the existence of forces, fields, numbers, sets, spatio-temporal relationships, possible worlds, and many other kinds of things. (Ellis, 1990, 60–61)

But he has another reason for insisting that abstract entities do not exist.

> The basic reason for resisting abstract particulars is that the world we can know about would be the same whether or not they existed. (Ellis,

[19] It is this feature of mathematical entities that Putnam so forcefully argued for in his realist days. He argued that mathematical entities feature indispensably in *the very same explanations* that lead realists to believe in theoretical entities (Putnam,1971).

1990, 79)

The key phrase here is 'the world we can *know* about'. Clearly some causal theory of knowledge is alluded to here, for otherwise the statement is patently false.[20] So in the end Ellis's restriction of the application of inference to the best explanation to causal process explanations will not provide a justification for the Eleatic Principle (by his own admission—see second last quote), so we are back to the epistemic justification which I discussed and dismissed in section 3.

6.2 Cartwright's Argument

Nancy Cartwright is also a realist of sorts. She sums up her view rather nicely in the following passage.

> I believe in theoretical entities. But not in theoretical laws. Often when I have tried to explain my views on theoretical laws, I have met with a standard realist response: 'How *could* a law explain if it weren't true?' Van Fraassen and Duhem teach us to retort, 'How could it explain if it *were* true?' What is it about explanation that guarantees truth? I think there is no plausible answer to this question when one law explains another. But when we reason about theoretical entities the situation is different. The reasoning is causal, and to accept the explanation is to admit the cause. (Cartwright, 1983, 99)

In her rejection of inference to the best explanation she aligns herself more with anti-realists such as Bas van Fraassen[21] but accepts theoretical entities that feature in causal explanations for the same sorts of reasons as Ian Hacking.[22] So whereas Ellis is a realist who rejects inference to the best explanation in its most general sense, it's perhaps more appropriate to see Cartwright as an anti-realist who accepts inference to the most likely cause. Hacking's arguments ensure that causal activity is a sufficient condition for ontological commitment, whereas general anti-realist considerations ensure that it is also a necessary condition. Once put this way it is clear, I think, that for me to reply to Cartwright would involve entering into the realist/anti-realist debate, which I said at the outset was too large a task to tackle in this present paper. None the less, I feel obliged to say something in reply to Cartwright, but before I do this I need to clear up an ambiguity in Cartwright's position.

Recall, that Cartwright admits theoretical entities that are causes of some phenomena which require explaining. The ambiguity revolves around what constitutes an event (or phenomenon) in need of explanation. For example, suppose that all entities that are causally active are the cause of some event or other. Then Cartwright's inference to the most likely cause may warrant belief in these entities as the most likely causes of their respective events. Notice that nothing here ensures that the entities in question are causally active with us. On the

[20] Elsewhere Ellis is more explicit about his endorsement of a causal theory of knowledge (Ellis, 1990, 7).

[21] See van Fraassen (1980) for details of van Fraassen's rejection of inference to the best explanation.

[22] See footnote 3 of this paper or Hacking (1983) for full details.

other hand, one could argue that what she takes to be an event in need of explanation must be an event which *we know about*. That seems uncontroversial enough, but now depending on how we spell out the 'we know about' claim, it looks as though Cartwright cannot admit causally active entities which are not causally active with us.

As it turns out I think that it is the latter position which Cartwright is committed to. This can be best seen by considering a case where there is some event, e, which we have no causal contact with and asking the question: what reason does Cartwright have to believe that such an event occurred? One way would be if we directly observe e or observe a result of e, but this would mean that we have causal contact with e, which is ruled out by construction. The important question is whether, for Cartwright, there can be any other way of knowing about e. It seems not, given what she says about inference to the best explanation. Recall, that inference to the best explanation is not an admissible inference for Cartwright, so it can't be that e explains some other event or phenomena, unless of course e is the cause of that event or phenomena, and this is also ruled out by construction. What other reason can we have to postulate a causally isolated entity on Cartwright's account? I can think of none, so I must conclude that Cartwright is indeed committed to only those entities which are causally active with us.[23] I will now use this reading of Cartwright's position on some examples I have considered previously. These examples will show how Cartwright's position seems unable to give the intuitively correct result in what are fairly uncontroversial cases.

The first example is that of the stars and planets outside of our light cone. These are theoretical entities that are not the cause of anything (that we can observe), so it seems on Cartwright's account they ought not be granted "real" status. As I've mentioned previously, in relation to the epistemically motivated Eleatic Principle (cf. section 3), this seems like the wrong answer. Note, however, that I am not claiming that Cartwright is committed to this view, just that to avoid this conclusion will require some additional argument. In the absence of such an argument the undesirable conclusion does seem to follow from inference to the most likely cause alone.

The other example I'd like to reconsider is the case of belief in the existence of germanium prior to 1887. Recall that I assume there had been no known causal contact with germanium until 1887 but that it was postulated in 1871 on the basis of a "gap" in Mendeleeff's periodic table. It seems Cartwright must deny any good reason to believe in germanium at that time since its causal isolation guarantees that it couldn't have been the cause of anything (that we knew about) and the only appropriate inference she allows is inference to the most likely cause. Again I trust that your intuitions suggest that this is the wrong answer!

[23] Nancy Cartwright has pointed out to me that she could appeal to some other form of inference besides inference to the best explanation to justify belief in causally isolated events and entities. Nothing in her position rules out such a move. Such a move, however, is not open to defenders of the Eleatic Principle interested in restricting the admissible, relevant inferences to inference to the most likely cause. Such a move clearly undermines the Eleatic Principle by (presumably) allowing belief in entities without any restriction on their causal histories.

Much more could be said about Cartwright's ontology and her inference to the most likely cause, but in the end I find examples such as those above give us good reason to suspect that her project will not work as a defence of a causal constraint on existence claims.

7 Conclusion

In this section I wish to show that there is a positive message to be gleaned from what looks like the complete lack of support for the Eleatic Principle.[24] In section 2 we saw how the inductive argument for the principle is in itself inadequate but, leads rather naturally to epistemic considerations. These, in turn, suffer from the same sorts of objections as the causal theory of knowledge (which is, after all, what underlies the epistemic motivation). In particular, we saw how this motivation yields some undesirable consequences in some fairly straightforward cases; for instance that we ought not believe in stars and planets outside of our own light cone. Such a position is surely unpalatable to even the staunchest defenders of the principle.[25] In section 4 I examined what I take to be the most important argument for the Eleatic Principle, due to David Armstrong. Armstrong claims that causally idle entities have no explanatory value and hence should not be considered to be real. This argument was seen to rely on the assumption that only causally active entities have explanatory power, and this was show to be a mistake. Finally, in section 6 we saw that motivating the Eleatic Principle by appeal to a restriction of inference to the best explanation to causal explanation also faces serious problems.

The moral to be drawn from all this is best illustrated by considering the deficiencies of the arguments for the Eleatic Principle and how they might be overcome. For instance, the epistemic justification failed because it ruled out entities such as germanium prior to 1887 and stars and planets outside our light cone, which seem reasonable to believe in. There is a way of getting around these problem cases though, and that is to appeal to some sort of "rounding out" principle. Thus, a causalist may argue that even though an epistemically motivated Eleatic Principle rules out stars and planets outside our light cone, none the less, such entities may be included on the basis of this "rounding out" principle.[26] But now one wonders what the purpose of the Eleatic Principle is, if it is so easily overruled when it gives the wrong answer. Why not try to

[24] I should qualify this remark by noting that Jody Azzouni has recently provided a motivation for a version of the Eleatic Principle (Azzouni, 1997a, 1997b) and that this motivation doesn't seem to fall foul of any of the objections I raise for the Eleatic Principle in this present paper. Azzouni's argument deserves careful attention and I leave a discussion of it for another occasion.

[25] It may well be possible to arrive at a formulation of the principle that overcomes many, if not all, of the objections that I discuss in this paper. Keith Campbell, another defender of a causal test, addresses this issue (Campbell, 1994) and concludes that this is indeed possible. It seems to me, however, that a criterion so motivated is left looking *ad hoc* in the extreme. Its only motivation is that it ought to give the right answers to all the problem cases and still look something like a causal test. So whereas Campbell and I agree that the criterion must be framed in such a way that items lying outside our light cone are not excluded, I do not agree with him that "such adjustments do not call the causal test into serious question" (Campbell, 1994, 32). Such adjustments deprive it of any reasonable motivation.

[26] For instance, Alan Musgrave has suggested this in conversation.

be more explicit about what the "rounding out" principle amounts to and just appeal to it in the first place?

It seems this rounding out principle, if it allows belief in stars and planets outside our own light cone, is going to do so for reasons of symmetry or, more generally, for reasons of theoretic virtue. That is, the astronomical theory that posits stars and planets outside our own light cone is a better theory than its counterpart that does not posit such entities. (For one thing, the latter theory is stuck with the problem of explaining why the earth is apparently at the centre of the universe.) Now notice that most entities which are causally active are likely to be needed in (causal) explanations of certain phenomena.[27] Thus, using this "rounding out" principle, at least as I've outlined it here, will effectively subsume one direction of the Eleatic Principle—causally active entities will gain admission to our ontology because they are needed for our scientific theories. I suggest that this principle looks more promising than the Eleatic Principle, since it seems to avoid the more obvious pitfalls of the epistemically motivated Eleatic Principle and yet, because in many cases both principles yield the same results, it retains much of the Eleatic Principle's intuitive appeal. In effect, I'm suggesting that the Eleatic Principle may be a good ontological "rule of thumb" but may not be the final arbiter on such matters.[28]

Alternatively, let's cast our minds back to the Armstrong argument for the Eleatic Principle. This argument placed the responsibility for questions of ontology firmly upon the explanatory power of the entities in the theories in which they occur. My point of disagreement was that we ought not confine our attention to causally active entities. While I agree that explanations featuring causally active entities are often very good explanations, and perhaps we ought to seek such explanations when appropriate, it's just pointless to restrict our attention to these explanations, since they do not exhaust the acceptable explanations of science. However, I think there is an important insight in Armstrong's argument. This is the move to deciding questions of ontology by looking at the explanatory power of the entities in question. If we heed this advice, but do not restrict the acceptable explanations of science to those featuring causally active entities, we find ourselves once again with a very general principle for deciding ontology, not unlike the "rounding out" principle we arrived at in the last paragraph. In both cases the principle places the responsibility for ontology on theoretic virtue (symmetry in the former case and explanatory power in the latter) and in both cases the principle goes well beyond the scope of the Eleatic Principle.[29]

[27] For example, the stars and planets inside our own light cone will be needed to explain why we see points of light when we look up at the night sky.

[28] Indeed, there is some evidence to suggest that even some of the Principle's supporters may agree with me here. For instance, in the following passage David Armstrong suggests that the causal criterion is a heuristic device for isolating those entities for which it is hoped that a reductive analysis may be given.

> The argument from lack of causal power is simply intended as a reason for thinking that the research programme [of giving an account of the truth conditions of numbers, classes, propositions etc. in terms of particulars and their properties and relations] is a promising one. (Armstrong, 1978, Vol 2, 5)

In other words, he suspects that causally idle entities are dispensable to science but admits that the real work is the required reduction of such entities to more respectable entities.

[29] For both these principles it will be an open question whether mathematical entities will

This I find very interesting. In fact it becomes even more interesting when one observes the similarities between the above suggestions and Quine's criterion of ontic commitment: We are ontologically committed to all and only the entities that are indispensable to our current best scientific theories.[30] This is not intended as an argument for the Quinean ontic thesis, for I'm sure there are other ways to avoid the difficulties faced by the Eleatic Principle.[31] However, the fact that consideration of the difficulties faced by the Eleatic Principle, and making rather natural responses to those difficulties, has led us to a position not unlike Quine's is rather telling. It suggests that the Quinean thesis (or something like it) is better equipped to embark on the difficult and important task of deciding which entities we allow entry to our ontology.[32]

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be granted real status, whereas the Eleatic Principle rules them out *a priori*. Indeed, this may count as another point in favour of the more general principle. Any criterion which claims to settle such long standing problems as the ontological status of mathematical entities in such a straightforward manner ought to be treated with some suspicion.

[30]See Quine (1953), for instance. Quine actually speaks of existential quantifications in theories when in canonical form. The difference (if any) is not important here.

[31]Nor is it intended to suggest that the Quinean position is problem free. See Maddy (1992), Maddy (1995) and Sober (1993) for some recent objections to Quinean indispensability theory and Resnik (1995), Hellman (199?) and Colyvan (1998) for recent defences of it.

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