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True Nominalism: Referring versus Coding

Jody Azzouni and Otávio Bueno

ABSTRACT

One major motivation for nominalism, at least according to Hartry Field, is the desirability of intrinsic explanations: explanations that don't invoke objects that are causally irrelevant to the phenomena being explained. There is something right about the search for such explanations. But that search must be carefully implemented. Nothing is gained if, to avoid a certain class of objects, one only introduces other objects and relations that are just as nominalistically questionable. We will argue that this is the case for two alleged nominalist views: Field's fictionalism ([1980], [1989a]), and Frank Arntzenius and Cian Dorr's geometricalism (Arntzenius and Dorr [2012]). Central to our competing approach to nominalism is a distinction between terms that refer to objects and ones that instead code empirical phenomena while being referentially empty. We next contrast our approach to nominalism, which uses this term-grained distinction between coding and referring, with approaches (to nominalism) that instead attempt to make a sentence-grained distinction between mathematical and non-mathematical content. We show the latter approach (derived from the work of Kitcher, Maddy, and Sober) fails to be responsive to objections raised by van Fraassen. In the end, only one last approach to nominalism is left standing.

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1 Introduction

Nominalism, in the context of mathematics, denies that mathematical objects exist. Among nominalistically undesirable items are sets, numbers, and functions. Formulated in this way, nominalism is a metaphysical claim about the non-existence of abstract objects, relations, and structures.[1] One central motivation for nominalism, at least according to Hartry Field, is the desirability of intrinsic explanations: explanations that don't invoke objects that are causally irrelevant to the phenomena being explained (Field [1980], pp. 41–6). To quantify over abstract objects in an explanation of certain physical phenomena is to involve objects that can't possibly play any causal role in the resulting account. An intrinsic explanation doesn't involve objects of this sort.

There is something right about the search for genuinely intrinsic explanations. But that search must be carefully implemented. Nothing is gained if, to avoid abstract objects, one only introduces other objects and relations that are just as nominalistically questionable. We will argue that this is the case with two alleged nominalist views: Field's fictionalism ([1980], [1989a]), and Frank Arntzenius and Cian Dorr's geometricalism (Arntzenius and Dorr [2012]). As will become clear, there is something seriously mistaken about the strategy of implementing the search for intrinsic explanations by substituting quantification over regions and/or points of various spaces for quantification over abstracta (objects that are nowhere/nowhen located). A successful defence of

[1] One of the anonymous referees suggests we characterize nominalism epistemically: nominalism is the view that we aren't justified in believing in numbers. Some philosophers would argue that only this can be established, and not the stronger metaphysical claim: that there are no numbers. Field's argument then is (roughly): the only reason one could ever have for believing in something, scientifically, is if it figures in an intrinsic scientific explanation. The question of what metaphysical conclusions we are allowed to draw from what we aren't justified in believing is a subtle one. We're letting the stronger characterization of nominalism stand, which is the one adopted by Field, while acknowledging here the concern the referee has raised.

nominalism can't be implemented this way—certainly, as we will argue, a scientifically acceptable form of nominalism can't be implemented this way.

After showing the problems faced by these versions of nominalism, the remainder of the article is dedicated to exploring what's required to implement a scientifically acceptable form of nominalism by way of considering some case studies, and the conclusions philosophers (Kitcher, Maddy, Sober, and van Fraassen) have drawn from those cases.

One important take-away lesson, we argue, is that it is a mistake to try to identify the ontological commitments of a scientific theory by distilling it (or presuming it can be distilled) into two sets of sentences, where the sentences in the first set quantify over what scientists really take to exist, but the sentences in the second set have quantifier commitments that—for ontological purposes—can be set aside. Field, for example, needs this assumption to show the dispensibility of what he takes to be the mathematics of scientific theories. Sober, for example, needs this assumption to show that what he takes to be the empirical content of a scientific theory can be contrastively confirmed in a way that the mathematical content isn't; and Maddy presumes on this assumption to distinguish indispensable scientific theories that scientists aren't committed to the posits of, from indispensable scientific theories where they are. By contrast, we think the lesson from scientific practice is different: scientists don't focus on theories—true theories—for indications of what exists and what doesn't; they focus on objects and the nature of their instrumental access to those objects. From a scientific language point of view, it's the term (and how it's made to refer to something in the world) that's central to ontological commitment, and not the true existentially quantified sentence. We conclude our article with the case study of Perrin on atoms and molecules to illustrate this point.

2 Troubles for Mathematical Fictionalism

2.1 A distinction

We start with Field's mathematical fictionalism. As part of his argument that mathematics is dispensable to science, Field tries to identify suitable replacements for real numbers. Instead of quantifying over them, as in standard formulations of physical theories, Field suggests that one quantify over space-time regions and/or space-time points (Field [1980], [1989a]). He argues that, as opposed to numbers, we do have epistemic access to space-time regions and points, and that's a reason for thinking such items are nominalistically acceptable (Field [1989b], pp. 68–9).

We will argue that Field is ultimately introducing a mathematical formalism with terms that stand in for (but actually don't refer to) aspects of reality, and that even if space-time regions and space-time points seem to fit the nominalist bill in some contexts (such as in general relativity), they actually don't. For example, the way energy is introduced into the mathematical space-time formalism invites the expectation that space-time itself has causal powers. But we will show that no underlying physical mechanism is established that satisfies this promissory note. And, as we'll also show, this makes mathematical space-time nominalistically unacceptable.

To make good on our claims about this, we argue for a distinction between quantifying over something that plays the desirable Fieldian intrinsic role in an explanation (in particular, a causal role) but is taken to be real, and something that may play a Fieldian intrinsic role in an explanation (including a causal role) but nevertheless is understood to only possess a coding role and to provide no metaphysical license for presuming its reality. A good example of a coding role is the way that energy is (mathematically) stored in space-time; another example is the role of fields in some physical theories. It isn't that there isn't something (a 'we know not what') that's physically behind fields and space-time that makes the coding role of the mathematical formalisms in these cases irrelevant to nominalism as a programme. It's that—regardless of what's metaphysically back there—we have no physical characterization of it. This is indicated both by there being no description of the metaphysical target of the mathematical coding in the physical theory itself and also that no instrumental access to the target is attempted by the physical science. Absence of these crucial items is always an indication that a mathematical formalism is only conveniently coding surface phenomena—including causal phenomena—and not that the mathematical formalism itself involves quantification over something that a nominalist should take to be nominalistically acceptable, that it involves quantification, that is, over something that a nominalist should take to be real.

The failure to systematically heed this important scientific distinction in effect substitutes for the project of nominalism one or another very different (and, we argue, ontologically insignificant) programme: what we'll call 'intrinsicalism' (or alternatively, 'topologism', 'geometricalism', or 'manifoldism').

2.2 Field's programme

Field's nominalistic programme notoriously faces many serious technical hurdles, ones that—in our view—it has never successfully overcome. A brief list: (i) it requires metalogical results that it hasn't been made clear nominalists have any right to; (ii) it characterizes the value of the application of mathematics to empirical science (nominalistically construed) in terms of mathematics being consequence-conservative with respect to empirical science, even though this doesn't seem true of actual applied mathematics; (iii) there are necessary technical restrictions on possible applications of mathematics (in order to guarantee consequence-conservativeness), but these rule out many cases of actual mathematical application in the empirical sciences; (iv) lastly, it's unclear, in any case, that his programme can be generalized beyond the narrow Newtonian case Field applies it to (see, for example, Malament [1982]; Azzouni [2009]; Bueno [2013]). Our aim in the first part of this article isn't to press any of these particular objections further; our concern is solely with Field's justification for his concept of nominalistic languages. In particular, we want to probe his reasons for allowing nominalistic languages to quantify over space-time points and regions.

2.3 Parts of nominalistically acceptable entities are nominalistically acceptable

One apparent argument for Field's view of nominalistically acceptable entities emerges early in his first book, and it's strikingly far ranging in its implications.

Field ([1980]) starts this particular discussion by first tarring his presumed nominalist opponents as people who commit the intellectual crime of having 'finitist or operationalist tendencies'. He notes that philosophers like that wouldn't like his approach, and he writes:

> To illustrate the distinction I have in mind between nominalist concerns on the one hand and finitist or operationalist concerns on the other, consider an example. Someone might object to asserting that between any two points of a light ray (or an electron, if electrons have non-zero diameter) there is a third point, on the ground that this commits one to infinitely many points on the light ray (or the electron) or on the ground that it is not in any very direct sense checkable. But these grounds for objecting to the assertion are not nominalistic grounds as I am using the term 'nominalist', for they arise not from the nature of the postulated entities (viz. the parts of the light ray or of the electron) but from the structural assumptions involving them (viz. that there are infinitely many of them in a finite stretch). (Field [1980], p. 3)

Field, here, is drawing (somewhat implicitly) a distinction between what he calls 'structural postulations' and what he calls 'Platonistic postulations'. The motivation for the distinction is clear: it stems from the importance he places on 'intrinsic' explanations.[2] Nevertheless, the distinction—in Field's

[2] The presumed superiority of 'intrinsic' explanations over 'extrinsic' explanations has been taken up by other philosophers (see, for example, Colyvan [2012]). For the sake of argument, in this article we are accepting the value of intrinsic explanations, as well as the value of a project that attempts to replace extrinsic explanations with intrinsic ones. Later we will give examples from standard science that imply that the value of intrinsic explanations over extrinsic ones—whatever it is—has nothing to do with ontology, and thus nothing to do with debates about nominalism. See our discussion below of continua mechanics.

hands—seems to amount solely to the acceptance or rejection of entities based on their posited locations. If, for example, a collection of objects is postulated to be located nowhere and nowhen—as the traditional Platonist might put it—then by quantifying over such objects, one has introduced Platonist postulations. Furthermore, when an explanation involves objects like that, the explanation fails to be intrinsic precisely because the nowhere and nowhen posits are (by virtue of their nowhereness and nowhenness alone) not intrinsic to the machinations of any items (in space-time). If, however, one posits the objects in question as located in something, then those objects can be part of an intrinsic explanation (for the doings of that something), and so positing such objects is nominalistically acceptable. In this case, because the posits in question aren't nowhere and nowhen but stipulated to actually be 'in' electrons and light rays, explanations about the machinations of light rays and electrons that involve these posits are intrinsic ones—nothing outside the physical entities is involved. Thus, Field takes his nominalistic concerns, therefore, only to induce objections to Platonist postulations. He writes dismissively of possible nominalist opponents:

> I am not very impressed with finitist or operationalist worries, and consequently I make no apologies for making some fairly strong structural assumptions about the basic entities of gravitational physics in what follows. It is not that I have no sympathy whatever for the programme of reducing the structural assumptions made about the entities postulated in physical theories—if this can be done, it is interesting. But as far as I [am] aware, it has not been successfully done even in platonistic formulations of physics: that is, no platonistic physics is available which uses a mathematical system less rich than the real numbers to represent the positions of the parts of a light ray or of an electron. Consequently, although I will make it a point not to make any structural assumptions about entities beyond the structural assumptions made in the usual platonistic theories about these entities, I will also feel no compulsion to reduce my structural assumptions below the platonistic level [...] The reduction of structural assumptions is simply not my concern. (Field [1980], pp. 3–4)

'Structural assumptions' so described are thus nominalistically acceptable.[3] A striking fact, however, is that Field's discussion—so far—doesn't justify the claim that space-time points and regions are nominalistically acceptable. This is because his argument so far amounts only to the claim—roughly speaking—that parts of nominalistically acceptable entities are nominalistically acceptable; and no case has been made, so far, that space-time points and regions are

[3] Notice the strange flavour of the argument Field has implied. It seems to be: opposition based on considerations like it's bad to be committed to infinitely many of something or it's bad to posit entities that are not in a very direct sense checkable aren't acceptable. Thus, structural assumptions are nominalistically acceptable. There must be a hidden premise to the effect of: more sensible reasons for opposing structural assumptions don't exist.

parts of nominalistically acceptable entities.[4] This aspect of Field's ([1980]) justification for the nominalistic acceptability of space-time points and regions involves a lacuna, as Malament ([1982], p. 531) notes—one that Field subsequently deals with in (Field [1989c]). For Field takes his nominalistic justification of space-time regions and points to presuppose a substantivalist view of space-time itself. He writes:

> There are, to be sure, certain views of space-time according to which the quantification over space-time points or space-time regions really would be a violation of nominalism. I'm speaking of *relationist* views of space-time, as opposed to the *substantivalist* view. According to the substantivalist view, which I accept, space-time points (and/or space-time regions) are entities that exist in their own right. (Field [1980], p. 34)

Field ([1980], p. 35) notes—but only in passing—his belief that relationism is untenable. Later, in his paper (Field [1989c]), he provides a detailed technical discussion of the various manoeuvres open to the relationist, and draws varying and tentative conclusions about their tenability.

It's important to realize that the logical space inhabited by nominalists and Platonists isn't quite the way Field presents it above, and in his paper (Field [1989c]), he aligns nominalist opposition to the nominalist acceptability of space-time points and regions to the relationist position about space-time. And, along these lines, the discussion in (Field [1989c]) fills the lacuna Malament ([1982]) notes by evaluating various technical maneuvers for implementing relationism. These maneuvers, however, are various techniques for implementing bits of physics without quantifying over space-time regions and space-time points. That is, and strictly speaking, (Field [1989c]) is a discussion of whether or not (and in what ways) quantification over space-time points and/or space-time regions is indispensable to the physical theories Field is examining.

This is more than a bit odd because a proof of the quantificational indispensability of space-time regions and points (for physics) may show that the relationalism is untenable, but it can't show—without further assumptions—anything about the appropriate attitude nominalists should have about space-time, space-time regions, or space-time points that are indispensable to physics. Somehow, Field takes it to be already established that space-time points and regions, if they exist, are nominalistically

[4] Actually, putting it this way is to over-state the particular intuitions being pumped by Field's illustrations. The illustrations at best suggest only that positing continuum-many points in physically acceptable entities are nominalistically acceptable. This doesn't indicate whether or not those weird non-measurable parts of light rays (that the axiom of choice invites us to believe in) are nominalistically acceptable; it doesn't even tell us that light rays are nominalistically acceptable. We're presuming, on Field's behalf, that he's indicating generalizations with his illustrations. To some extent, we're speculating about what generalization(s) these are by the conclusions Field draws.

acceptable—that they aren't abstracta. The argument for this, however, can't be the one we rehearsed from (Field [1980]) about parts of nominalistically acceptable entities being nominalistically acceptable because the application of that argument presupposes what still needs to be established: that space-time is itself a nominalistically acceptable entity.

There are two other arguments that Field offers against the view that space-time points and regions aren't nominalistically acceptable—more accurately, arguments that each of two reasons for why some nominalists don't like no-where and nowhen abstracta aren't reasons to similarly dislike space-time regions and points. One argument concerns the worry about the absence of causal connections between us and nowhere and nowhen abstracta. Field writes:

> There are no causal connections between the entities in the platonic realm and ourselves; how then can we have any knowledge of what is going on in that realm? And perhaps more fundamentally, what could make a particular word like 'two', or a particular belief state of our brains, *stand for* or *be about* a particular one of the absolute infinity of objects in that realm? It seems as if to answer these questions one is going to have to postulate some *aphysical connection*, some *mysterious mental grasping*, between ourselves and the elements of this platonic realm. (Field [1989b], p. 68)[5]

This problem is not faced by space-time points and regions, according to Field ([1989b], p. 68), because there are 'quite unproblematic physical relations, namely, spatial relations, between ourselves and space-time regions, and this gives us epistemological access to space-time regions'. He has in mind that (some of these) regions fall within our visual field, and that we have access to some of these regions indirectly, for example, by indirect epistemic access to a space-time region a chair occupies via our direct epistemic access to that chair.

If we grant this claim, we can still worry about how far it can be extended. After all, the set of visual regions we can reasonably take ourselves to have—even indirect—epistemic access to is a pretty impoverished class. Not only does it exclude points, it also excludes a huge number of regions that never-theless fall within our visual field—smallish ones as well as many of those that are fairly large, but are indistinguishable from the ones to which we can be presumed to have indirect access. And this is not to mention all those regions that happen not to have any convenient landmarks that our epistemic access to them can be indirectly anchored in. These epistemic re-strictions can be circumvented to some extent, of course, if Field again invokes the claim that (arbitrary) parts of nominalistically acceptable entities are

[5] Field alludes to (Benacerraf [1973]) at this point.

nominalistically acceptable. As long as very large regions of space are nominalistically acceptable, then so are all of their spatial parts.

It's also worth pointing out that Field's invocation of indirect epistemic access to spatial regions serves primarily to rhetorically cover a morass of complications. First off, of course, there is the tacit (unstated but nevertheless present) implication that any nominalist who resists indirect epistemic access to entities has operationalist tendencies. But this charge is inappropriate. A nominalist, after all, could be worried that the so-called indirect epistemic access to spatial regions is just theoretically driven descriptions that are imposed on what we have visual epistemic access to. A nominalist who simply tries to rule out indirect epistemic access to something on the grounds that it goes beyond visual confirmation probably does have operationalist tendencies. But the alternative isn't having to accept theoretical impositions (of any sort at all) on visual impressions. Rather, a subtle analysis of exactly how a (scientific) theory is being deployed to connect a phenomenon that we do see with one that we then descriptively infer from what we see (the way that the chair that we see is used to descriptively infer the existence of a spatial region that chair is in) is needed to determine that no questions are being begged. It's possible, after all, for someone to claim that we have indirect epistemic access to the number two by gazing at a pair of shoes. But this would patently beg the question against the nominalist. One needs to know that the relationship between a chair and the spatial region it occupies is epistemically different from the relationship that the number two bears to a pair of shoes. This isn't made obvious by simply noting that (as we speak) a chair is in the space it occupies. After all (and just as obviously, one would think), a pair of shoes is two shoes.

Field's second argument is directed towards those nominalists who dislike nowhere and nowhen abstracta because they are presumed to be acausal. Field ([1989b], p. 70) objects that field theories (in physics) are 'most naturally' construed as theories that attribute causal properties directly to space-time points. This is probably Field's most important argument for our having an epistemically informed causal relationship with spatial points and regions, precisely because visuality just is sensitivity to electromagnetic radiation (within a certain range, of course) and that radiation (Field thinks) is to be most naturally directly attributed to spatial regions and points. We will table this argument for now; our discussion of it is connected to the distinction (that we'll argue for) between quantifying over something that plays a coding role, as opposed to quantifying over something that's taken to be metaphysically real.

Meantime, the preliminary upshot is this: Leaving aside the second polemical response Field makes to nominalists who distrust acausal abstracta, we have found that some version of the parts-of-nominalistically-acceptable-entities-are-nominalistically-acceptable principle to be crucial to Field's claims about the nominalistic acceptability of space-time regions and points. It plays a crucial role, for example, in extending our epistemic grasp of a meagre number of spatial regions to the required richer class of these regions; and it also plays a crucial role in committing the nominalist to the parts of nominalistically acceptable entities, such as light rays and electrons. We go on to probe this principle (or a descendent version of it). It needs at least a moderately memorable name. We call it the 'acceptable parts principle'. We first note that Field's apparently quite broad version of this principle is scientifically unacceptable. We then cut this version down to a much weaker (but scientifically acceptable) principle that we'll show is unable to do what Field needs it for. Then we return to Field's claim that the most natural way of construing field theories is as attributing causal properties to space-time points (and thus, derivatively, to space-time regions).

2.4 Structural assumptions: How far can these be pushed?

It's important to realize that without further constraints of some sort, the acceptable parts principle coupled with a practice of avoiding nowhere/nowhen abstracta isn't particularly exclusive. Any kind of Platonistic abstracta-structure—no matter how rich—can be embedded in ordinary space-time as structural postulations. We'll take a moment to illustrate this point before turning to the question of what other constraints Field might (implicitly) have in mind for his nominalistic programme.

Space-time is typically conceptualized as a standard manifold with a standard cardinality. That is, the cardinality of the set of its points is taken to be $2^{\aleph_0}$. Of course, nothing about the topological and metrical properties of standard manifolds requires them to be restricted to this cardinality.[6] Notice that it would miss the point to complain that space-time regions are to be understood as restricted by the cardinality of their points, so that to treat them as having higher cardinality would be to introduce entities that aren't space-time regions. For we are—at this point—only engaged in showing how weak the mere constraint is that additional postulations not be Platonic. To embed additional structure in space-time isn't—by definition—to introduce Platonic entities. It's clear, of course, that Field doesn't think it would be acceptable to posit space-time as having a higher cardinality structure. The question, then, is what other assumptions are in play.

There is one possible constraint that's not quite explicitly stated by Field, but implicit in his structure-postulation practice. This is that the mathematical

[6] Proof: First-order set theory coupled with (first-order) topological and metrical axioms for manifolds is consistent (assuming that set theory is). Then apply the (upward) Löweinheim–Skolem theorem.

structure that's allowed to be embedded in space-time (or in any physical entity, for that matter)—the structural postulations—aren't to go beyond the already in-place mathematical characterization of the entity in question. The structural postulations can't go beyond the in-place mathematical characterization of entities, that is, what is already part of science. We think Field probably has a strong constraint like this in mind[7]; unfortunately, at least from a philosophical point of view, it's rather *ad hoc* (and it begs the question against his nominalist opponents as well). The constraint in question would amount to this: the mathematical structures allowable in space-time can only be as rich as contemporary physics allows them to be. A principle like this would certainly justify attributing standard cardinality and manifold assumptions to space-time and nothing more, just as Field does in his work ([1980]; [1989a]): the space-time manifold is to be posited as no richer than the then contemporary physics portrays it.

We described this second constraint as *ad hoc*, and we accused Field of begging the question against his nominalist opponents; this needs elucidation. The first point is that, as contemporary physics (and the history of physics) makes clear, physicists aren't above postulating an arbitrarily rich mathematical structure to space-time (and other related theoretical structures) if their theorizing succeeds because of it. We need only note, for example, the multi-dimensional complex space that is the basis for quantum mechanics. There is nothing in scientific practice that stops the structure of the entire set-theoretic universe from being deployed as part of the structure of one or another physical entity, if applying that mathematics works empirically.

The reason is straightforward: Mathematical theories themselves don't come with metaphysical assumptions built into them that their entities are nowhere and nowhen. Mathematical theories (and mathematical practice) are instead studiously neutral about this. In particular, knot theory, Turing-machine mathematics, rigid-body dynamics, and Euclidean geometry (for that matter) are all about items that seem to come embodied in space and time; nevertheless, these are all topics of pure mathematics. This means that there are no constraints, in pure mathematics, for how mathematical posits are to be deployed empirically.[8] The second point is related: physicists (naturally) have the habit of borrowing useful mathematics and employing that

[7] In what we've quoted, Field writes that he won't make structural assumptions that go beyond 'platonistic formulations of physics'—but such platonistic formulations of physics are simply physics as usual, and they employ, in particular, mathematics as usual. However, as we note below, we don't think that the mathematical theories that are used in applications (or anywhere else in mathematical practice for that matter) come with metaphysical assumptions regarding abstracta built into them.

[8] We aren't attempting an argument against Platonism by making this point—in particular, against that Platonism that takes mathematical entities to be acausal and nowhere and nowhen. We are merely pointing out what pure mathematical posits look like before physicists adapt them for empirical purposes. The point is that what pure mathematical posits look

mathematics in physical theories in any way at all that's empirically valuable—specifically, with respect to mathematical characterizations of physical entities.

Newton treated space-time as having all the mathematical structure that seemed needed to enable the geometrical and analytical reasoning (of his time). It's true, of course, that Newton and many philosophers have subsequently grappled with the question of whether space-time itself—the empirical object within which we live—has this (mathematical) structure. It should not be assumed that the answer from physics is 'yes' on the mere grounds that the mathematics the physicist has borrowed to do physics involves treating the empirical object as having that mathematical structure.[9]

Nominalists who oppose this claim—who balk at the idea that space-time (in particular) has such a rich structure—are on a par with similar philosophers who would deplore treating minds as really being Turing machines simply because Turing-machine mathematics turned out to be a useful, perhaps even an indispensable, way of framing our descriptions of mental activity. The objections of such nominalists shouldn't irresponsibly be labelled 'operationalist' or 'finitist'; the concern is different. It's a worry about attributing metaphysical structure to an aspect of the world just on the basis of the way that branch of indispensable mathematics is applied—for example, structurally instead of in some more detached way.

Put this way, a worry about so-called Platonistic assumptions—a worry about nowhere and nowhen entities—is a relatively superficial construal of nominalism. The nominalist concern, rather, is with when it's appropriate to treat the terms of an applied mathematics as really referring (metaphysically speaking) to something merely because posits of that mathematics are construed as parts of physical entities, and when it isn't so appropriate. The nominalist denies that it automatically follows that a mathematical term refers to something nominalistically acceptable simply because it's convenient (even indispensable) to treat that term as referring to a part of a physical entity in order to apply that mathematics.

2.5 Cases of indispensable theories where the mathematical posits are known not to be real

There are many empirical applications of mathematics where we know that the mathematical posits can't be understood as referring to something real.

like invites the embedding of them into what is otherwise described as physical in all sorts of open-ended ways.

9 Because it doesn't affect our objection, we are leaving aside the point that, strictly speaking, the space-time structure needed for Newtonian physics is less than Newton realized. See, for example, (Stein [1967]; Sklar [1976], Chapter 3).

More accurately, there are many cases where the mathematical posits are spatial ones (with topological, metrical, and geometrical structure) that are treated as parts of objects that we otherwise take to be real; nevertheless, those parts are known not to be real. We give two easy examples.

The first is the simple application of Euclidean geometry with a metric to a chalkboard. The theory quantifies over chalky diagrammatic figures, it successfully characterizes their areas and angle properties, and attributes useful numeric properties to these things. We know that the implicit assumptions about the space these figures live in (and the implicit assumptions about the figures themselves, for that matter) aren't true. A continuum structure that we know is false is imposed on them and on the space itself: we know that the underlying physical reality is instead granular. The best way of describing the situation is by saying that the chalk figures (which are real) are given an internal structure that isn't real by the imposition of a branch of applied mathematics to them. (The contours of the figures, in particular, are described as lines and curves with a continuum point structure.) This internal structure doesn't correspond to anything real—the entities posited (points and regions of curves and lines) don't exist. They do stand for the (ultimately quantum-mechanical) internal structure that these things actually have. And, at the level that we interact with such figures, the properties they manifest are nicely captured (that is, within measurement thresholds) by a theory that imposes these mathematical structural postulations onto the chalk figures. The structural posits thus successfully code these properties.

Notice that it isn't that all the geometrical part-posits, which this application of geometry attributes to chalky figures, aren't real. Rather, there are a moderate number of sub-regions of the chalky figures that can be treated as real, just as the chalky figures themselves are. But the applied mathematics doesn't license a Field-style claim that all the 'structural postulations' imposed by this mathematical application are real (or nominalistically acceptable). That would be seen as a bizarre conclusion.

The second example is a related family of examples. Consider the rather rich and important branches of fluid dynamics and rational continuum mechanics.[10] Here, too, substantial (but known to be false) geometric and topological assumptions are made about various materials—specifically, about the topological, metrical, and geometrical properties of their posited parts. These assumptions are indispensably invaluable for a family of empirical sciences: the needed physical concepts deployed (arising from various physical continuity and differentiability conditions) presuppose that the substances studied are smooth—apart from additional approximation assumptions. The resulting physical theory must be applied to the phenomena in

[10] See, for example, (Malvern [1969]; Truesdell [1991]; Truesdell and Rajagopal [2000]).

an autonomous way, relatively independent of more fundamental sciences (for example, quantum mechanics) because of the massive mathematical intractability of the latter sciences, and because the specialized concepts of these branches of macro-physics are wedded so thoroughly to particular applications of the mathematics of analysis and topology. Notice, crucially, that it's exactly the supposed structural postulations of these materials that are falsely mathematicized in this way. This is how mathematical abstracta are empirically applied in these cases: not as detached nowhere and nowhen posits, but instead as items—for example, topologically continuous regions of stuff possessing such-and-such properties—to be treated as structurally located in items we otherwise take to be real (pieces of wood, bodies of water, steel beams undergoing stress, and so on). Notice also that causal predictions about how these materials shift under various pressures are a crucial part of these sciences. These causal predictions are enabled by structural postulations known to be false about these materials, and yet the predictions are impossible without them.

Two important points: As with our first example, some but not most of the structural postulations are taken to be real. Second, the causal properties of the substances—metal deforming under pressures, for example—are derived from the continua structural postulations that are recognized nevertheless to be unreal. Thus, there are intrinsic explanations in the sense that Field understands them. Nevertheless, the posits involved in such explanations aren't taken (by scientific practitioners) to be suitable postulations for what's really in such materials; and this is leaving aside the question of whether such things would be nominalistically acceptable if anyone happened to think they were real. Clearly, Field's broad formulation of the acceptable parts principle is unacceptable on scientific grounds (additional considerations to this effect are also provided below).

2.6 How does science distinguish between the indispensable structural presuppositions taken to be real and those that aren't?

One way to try to draw the distinction between the cases Field is interested in (where the structural postulations are to be taken as real as opposed to the many cases where they aren't) is to notice that the structural posits of an otherwise indispensable 'middle-level' science are belied by a quite different underlying description (provided by an alternative 'more fundamental' science). So the thought might be: Field's conditions apply to sciences that occur—as it were—at the 'ground floor', and not further up the scientific hierarchy. This is somewhat vague as a characterization, but it's stated clearly enough to see why it won't work. One would like a stronger reason for taking structural posits seriously than the mere fact that (at the moment) there isn't a 'more fundamental' description of the internal structure of a class of entities that contradicts the one supplied by a higher-level branch of physics coupled with a branch of applied mathematics. It would be a bit shocking if metaphysical claims were based on such a superficial (historical) factor as whether a science (at a certain point in time) was a ground-floor science—this is assuming (of course) that such a hierarchical idea can be made out successfully. And, indeed, physical science itself draws a distinction between posits of a theory that are bits of mathematics from posits that aren't in a way that doesn't involve any issues about the fundamentality of science. We turn to how this is managed in a moment.

But first we must dispense with a legalistic manoeuvre that seems open to Field at this stage of the debate. Field can accept everything we've claimed, but respond that (strictly speaking) it's irrelevant to his differences with his nominalist opponents. After all, Field can argue, what's been shown is that certain nominalistically acceptable posits are nevertheless not taken by scientists to be real. But that's irrelevant to the real issue: if these posits are taken to be real, should they be unacceptable to nominalists? So the real complaint against Field being made here isn't that the structural postulations he's willing to entertain aren't nominalistically acceptable, but instead that they shouldn't be accepted by anyone as real.

We reject this reconstrual of the debate (even though accepting it wouldn't gain Field any debating points—it would just allow us to label his position 'non-naturalistic nominalism'). The reason is the one we gave earlier: the real nominalist concern isn't (or shouldn't be) just about commitments to metaphysically suspicious entities (ones that are acausal, atemporal, aspatial, and so on), but instead that it's metaphysically gullible to accept the existence of something merely because it's part of a package of indispensable mathematics that a physical science helps itself to. On our view, if acausal, atemporal, aspatial entities were posited by physics and, assuming it was possible, if the existence of these entities were established on good physicalistic grounds (on the kinds of grounds we are going to momentarily describe), then they should be acceptable to the nominalist. It is indiscriminate metaphysical commitment on the sheer basis of applied mathematics that nominalists should object to. Our nominalist credo in place, we now spell out what the good physicalistic reasons are for committing oneself to (some of) the entities quantified over by a science plus an indispensable applied mathematics.

2.7 Do physicists distinguish between mathematical posits and non-mathematical posits?

A philosopher could try to defend Field this way: The opponent nominalists being imagined here are ones hoping for a distinction among terms that isn't manifested in science the way they hope it is. Field is making a distinction that physicists recognize, one between genuinely physical entities (for example, forces, fields, space-time, and so on), and genuinely mathematical entities (for example, numbers, functions, and so on). Physicists treat the properties of space-time—for example, that it's a manifold—as an empirical property of space-time, or as a property that it could be discovered space-time doesn't have. This isn't true of numbers and functions; physicists don't treat the properties of numbers (for example, that they are linearly ordered) as properties that could be empirically discovered to be false.

The counter-argument to this powerful defence of Field requires two points. The first is that the distinction between empirical and non-empirical properties being drawn by Field's defender actually collapses when the process of how scientific theories are replaced by successors is looked at closely. The second point is that, contrary to what's being claimed, physical practice distinguishes between those properties of entities that are imposed on those entities by the choice of mathematics being used, and those properties that are genuinely physical, that is, that are independent of the mathematics being used. We now make both these cases.

The first point: To merely notice that the properties of space-time are empirical—in the sense that empirical considerations might drive replacing this space-time with such-and-such properties with a different one that instead has these-and-those properties—misconstrues how to determine the empirical status of the posits involved in space-time. After all, it's undoubtedly an empirical question whether a successor physical theory uses the same mathematics that its predecessor used. And, in general, drastic changes in the look of a physical theory are often due to substantial changes in the mathematics that is being employed. Thus, without a closer examination, it simply isn't relevant that a set of posits and their properties are empirical in the mere sense that a successor scientific theory might dispense with them. It could mean that the posits in question are genuinely physical ones and that, subject to later empirical discoveries about them and their properties, the science may change. However, all it may mean is that the overall mathematics (with its imposition of structural postulations) is being switched out for another kind of mathematics because some successor physical theory (with a successor branch of mathematics) is empirically better; the shift in structural postulations is only a reflection of a change of mathematics.[11]

The second point: Fundamental to scientific instrumentation isn't the mere determination of observational effects of some sort—the way operationalists think of the movements of dials, the generation of pictures, and so on.

[11] We will eventually put this important point to use against Sober ([1993]). See Section 4 of this article.

Fundamental is the empirical examination of scientifically posited entities that we take to be real. Such items, instrumentally, are often tracked in space and time, and their other properties are often directly detected. This is simply not an exercise scientists engage in (or think is even sensible) when it comes to posits that are merely the quantificational yield of applied mathematics.[12]

There are two (sociological) indicators of this important distinction between posits that regularly occur in the sciences. The first is that instrumental access to entities—such as quarks, electrons, and so on—are among the explicit and advertised reasons for believing in those entities, as opposed to a complete absence of any attempts at instrumental access to mathematical entities—for example, space-time points and regions. The second sociological indicator is the kind of discussion that arises (both officially and unofficially, in print and orally) among professionals when a kind of posit undergoes a transition from being treated as purely mathematical to being treated as physically real. Illustrations of the first case are legion. Quarks, for example, are treated as genuine parts—as genuine structural postulations—of an important class of particles (hadrons). And, notably, various kinds of experiments have been undertaken that don't merely confirm the overall observational and theoretical virtues of those theories that posit quarks—detailed attempts to instrumentally access quarks are undertaken.[13] Related to this are the many attempts at instrumentally determining properties of contemporary (quantum-mechanical) fields. Contemporary fields—unlike classical

[12] For previous discussion of this important distinction, see (Azzouni [2004a], [2004b], [2012]; Bueno [2005]). In addition to the discussion of this in the next few paragraphs, we will bring this distinction to bear against van Fraassen ([2009]) in Section 4.

[13] Adair, in an accessible discussion, writes:

> Measurements of the scattering of very high-energy electrons, accelerated by the Stanford Linear Accelerator (SLAC), from neutrons and protons demonstrated that the nucleons contained, or were made of, point-like charged particles which scattered the electrons strongly. Through analyses of the magnitude of the scattering, the partons were identified as the fractionally charged quarks. ([1991], p. 1002)

Brown *et al.* write about the evidence for gluons that:

> The third jet in these 'three-jet events' is now recognized to be due to the emergence of an energetic gluon in a process analogous to *bremsstrablung* in QED. Although it took a few more years to make an absolutely convincing case, this *visual* evidence for gluons was perhaps the most influential factor in the acceptance of QCD as the correct theory of the strong interactions. ([1997], p. 21)

Note that our point isn't focused on the relatively superficial question of whether the word 'visual' is appropriate here; rather, we are highlighting the intense focus by physicists on attempting thick epistemic access to specific physical items in order to determine both that they exist and (some of) their properties. Azzouni thinks the science is as the scientists describe it—access to particle-like aspects of fields has been managed. Bueno has doubts about this. For current purposes this disagreement doesn't matter because our point is only to establish that access matters to the science. But in every case where we describe successful instrumental access to such sub-atomic structure, Bueno is registering a doubt.

fields—are complex items that manifest granular particle-like properties under certain circumstances. Determination of a field's properties involves instrumental access to its particle-like properties—something that scientists take to have been carried out with many kinds of fields (for example, electron fields) but not for other fields (for example, gravity).

It's important that instrumental access to these physical posits isn't a simple matter of imitating visual perception of something like a chair. Rather, it involves subtle statistical analysis of complex events, and it employs plenty of substantial background theory. It nevertheless involves instrumental determination of specific properties of specific physical entities. This is important because it shows that the nominalist who accepts this sort of requirement on the determination of something real is hardly manifesting operationalist or finitist tendencies.

It's also important that attempts to instrumentally determine some of the properties of fields don't extend to the entire description of a field within a scientific theory. This is because—as our continua mechanics examples already indicate, and like the cases of most objects when characterized within the context of a physical science—only some of the properties of a field are physically real, and only those properties are ones that instrumental interaction with the fields attempts to determine. A field (such as the way a piece of metal is treated in continua mechanics) is mathematically characterized in terms of point-like parts. But determination of a field's particle-like properties isn't a determination of the purported properties of their point-sized parts. In general, structural postulations are routinely distinguished in physics in just this way: some are impositions of applied mathematics onto the items they are the attributed parts of, others are physically real.

Things couldn't be more different for space-time points—or space-time regions, for that matter. No attempts of any sort are made to instrumentally determine the structural postulations of the parts of space-time (neither to the points posited in continuum-rich structures, or to regions composed of these). It's relatively easy to establish that this distinction between purely mathematical structural postulations and the ones taken to be real is manifested in any science to which mathematics is crucial, not just physics.[14]

Cases where a posit changes status from purely mathematical (albeit suitably interpreted) to genuinely real (physical) are also numerous. One nicely publicized case is the emergence of the molecular theory of matter around the turn of the twentieth century, where an important aspect of the case for the

[14] The distinction, for example, occurs in contemporary computer science. Characterizations of programmes at a design-level are often recognized to be unreal (idealized) in comparison to lower-level descriptions; the case is identical to that of continuum mechanics and fluid dynamics discussed earlier.

existence of molecules was instrumental access to molecules.[15] Another case is the postulation of the positron on the basis of a characterization that had been previously taken to generate a purely mathematical posit with no physical significance whatsoever.[16]

It's important that sometimes a set of mathematical posits are discarded entirely—replaced with a (at least partial) physical description—and not that posits are kept, but changed in their ontological status. This is how attempts to replace current space-time with something like quantum foam should be seen. If a successor theory of this sort were successfully implemented, the result would be the replacement of a purely mathematical characterization of something—actual space-time, as it is currently characterized in general relativity—with a partially physical description of that something. ('Partially', of course, because no construal of anything in contemporary physics is free of purely mathematical structural postulations.) We'll return to this example shortly.

2.8 Mathematical coding versus genuine metaphysics. Case study: Fields

We have now given a number of examples that illustrate our distinction between terms in a theory standing in for a physical reality as opposed to those terms actually referring to entities the science takes to be real.

We now apply this distinction to the case of classical fields, and then we extend it to the case of general relativity. We'll end with a brief recapitulation of our points about contemporary (quantum-mechanical) fields. One role of this discussion is to redeem our promissory note regarding Field's claims about the most natural way of construing fields by attributing causal properties directly to space-time points (and regions).

Imagine a case, first, where a classical field characterizes the movement of particles through space. In this simplest case, it's true—as Field suggests it is with every field—that it is most natural to attribute causal properties to space-time points and regions. But we should avoid thinking that this attribution suffices for space-time points and regions being real. Causal attributions to something (as the many cases from continua mechanics and elsewhere in physics illustrate) aren't sufficient to show that the something in question is functioning as real in the physical theory, as opposed to only possessing a coding role. And indeed, in the classical case, there was no attempt to determine that points and regions of space do indeed have the various topological/metrical/geometrical properties attributed to them. It's clear that

[15] See, for example, (Perrin [1990]), which includes and discusses the important evidence provided by C. T. R. Wilson's cloud chamber experiments. Also see our discussion of this very case, and philosophical debate about it, in Section 4.

[16] See (Bueno [2005]) for discussion.

space-time points (and, derivatively, regions) were playing purely coordinate roles in such field theories.[17] This is true even in general relativity where space-time is a repository of energy. Its being so doesn't involve any (empirical and thus testable) descriptions of the mechanisms by which space-time so functions. It's a mere mathematical device.

This isn't the case with quantum-mechanical fields, where the field-role is different; fields are (at least partially) here functioning as the basis of a genuine physical description of something. As we have noted, attempts to instrumentally determine some properties of the field (for example, the nature of its particle-like properties) are routine—and it's considered significant when these properties of a field haven't been instrumentally confirmed in this way.

Note that coordinate roles (of one sort or another) for one or another set of mathematical posits never vanish as physical theories succeed one another. As we noted a little earlier, a common way of understanding contemporary string-theory options is that they are meant to replace the current view of space-time (which characterizes space-time as smooth) with a granular string alternative. This common view is mistaken in at least two ways. First, what's actually happening—especially since strings are supposed to be items we are to take as physically real—is that a branch of mathematics (manifold mathematics) with a coding role is being replaced with a physics-and-mathematics package that's doing more than coding physical phenomena: it's actually trying to provide physical mechanisms. The second point is easily masked by the developing state of string theory. The successor physical characterization of physical items isn't purely physical. Rather, it's physics as usual: a physical theory that's deeply embedded in a background mathematics that enables (generalizations of) all the standard analysis operations on various mathematical objects. In particular, coordinate background mathematical posits (for example, ones that are used to describe the spaces that strings are in) are still in place.

Field's argument for attributing causal properties to space-time points and regions in Newtonian physics is fairly weak (note the phrase 'most naturally'). But nevertheless, it's been worth going into some detail, first, because distinguishing cases turns directly on recognizing the very different roles

[17] Newton's famous bucket thought experiment illustrates this: that quantification over space-time (points or regions) is needed to anchor acceleration attributions. Contrastively, relationist views attempt to show that such quantification is dispensable. But nominalist needn't accept that mere quantification over anything (regardless of where that something is posited as 'located') suffices to make it nominalistically acceptable. The nominalist can thus accept that substantivalist construals of physical theories are superior to relationist ones—the nominalist can even accept that substantivalist construals of physical theories are indispensable. It doesn't follow (from this alone) that such posits are nominalistically acceptable. Thus the nominalist—at this stage in the debate—undermines the Quine-Putnam indispensability thesis that underpins this issue by rejecting Quine's criterion. For details, see (Azzouni [2004b], Chapter 6; Bueno [2005]) for example.

that mathematical posits play in physical theories and, second, because arguments of the same flavour are deployed by Arntzenius and Dorr ([2012]).

2.9 The upshot

We haven't denied the value of intrinsic over extrinsic explanations. We have, however, indicated how scientific practice employs a more fine-grained distinction between pure mathematical posits (in a physical theory) and those that have genuine physical content. The distinction turns on whether or not physical science is obliged to confirm instrumentally the properties attributed to a posit by methods that can be used to justify the claim that the yield of the instrumental access is itself directly due to the machinations of the posit. This point is amply illustrated in the physics literature where support for a theory because its posits (and some of the properties of those posits) have been detected in some instrumental way is distinguished from the very different confirmation of the global predictions of a theory. This distinction seems to have been largely overlooked in the philosophical literature we are specifically discussing here. A symptom of this is that there is no discussion of how specific physical theories are brought to transact with the world.

3 Troubles for Topologism

We now move to Arntzenius and Dorr's ([2012]) version of a Field-style nominalist programme. Their discussion of this programme (and this is true of (Arntzenius [2012a]), generally) is richly informed with a grasp of the role of mathematics in contemporary physics; it shows a sophistication in physics (and the mathematics used) that is unsurpassed by other philosophical literature in this area. Nevertheless, we will show that the philosophical underpinning for their characterization of their nominalist programme begs all the same questions against the genuine nominalist that Field's discussion did. Indeed, in some ways, Arntzenius and Dorr's ([2012]) discussion is strangely archaic. Their most important and overarching metaphysical principle—more strictly speaking, their most important and overarching principle for determining metaphysics on the basis of scientific theories—seems to be *a priori*.

Preliminarily, Arntzenius and Dorr ([2012]) replace Field's 'intrinsic explanation' with the broader-sounding 'metaphysical intrinsicality'. They write, after observing the ubiquitous role in standard physical science of relations between physical entities and mathematical entities, for example, that 'the mass in grams of body *b* is real number *r*':

> We would like to think that the physical world has a rich *intrinsic* structure that has nothing to do with its relations to the mathematical realm, and that facts about this intrinsic structure explain the holding

> of the mixed relations between concrete and mathematical entities. The point of talking about real numbers and so forth is surely to be able to *represent* the facts about the intrinsic structure of the concrete world in a tractable form. But physics books say hardly anything about what the relevant intrinsic structure is, and how it determines the mixed relations that figure in the theories. So there is a job here that philosophers need to tackle, if they want to sustain the idea that the truth about the physical world is determined by its intrinsic structure. (Arntzenius and Dorr [2012], p. 214)

Although the flavour of this credo seems metaphysically focused, distinct from Field's 'intrinsic explanation', this is an illusion. Field's focus is always on ontology: it's that the entities that appear in intrinsic explanations must be located so that these explanations are intrinsic to the phenomenon being explained. Thus Field's focus on intrinsic explanation is as metaphysical in effect as the programme offered by Arntzenius and Dorr ([2012]).

So far, of course, the nominalist needn't disagree, except to add that whether the physical world does have such a rich intrinsic structure is an empirical question. And, as long as the intrinsic structure posited (as long as the posits involved in the characterization of intrinsic structure) is nominalistically acceptable, there will also be no disagreement. But there is a major disagreement with nominalists, for Arntzenius and Dorr ([2012], p. 229) routinely posit all sorts of additional spaces with varyingly rich structures—a mass space, for example, and even more, scalar-value lines that are presumed to be located at each space-time point—in order to provide nominalistically acceptable replacements for functions from space-time points to real numbers.[18]

As with Field's approach, a powerful principle is needed to justify that these intrinsic structures (entire spaces, in this case) are nominalistically acceptable. Not much time is spent on this. Arntzenius, in passing, writes:

> The best (simplest, most natural) theory of the phenomena is our best evidence for what the fundamental objects and quantities are [...]. ([2012b], p. 158)

This credo (call it 'simplicity') isn't transparent. But what it means to Arntzenius (and Dorr) is clearly exemplified throughout (Arntzenius [2012a]), and throughout (Arntzenius and Dorr [2012]). The nominalistic acceptability of the posited structural additions to space-time is justified by impressionistic observations about the superior simplicity and naturalness of

[18] We won't dwell on this, but, as far as the resulting physical theory is concerned, that the scalar-line spaces are located at or in each point of space-time is an easily negotiated assumption; nothing is affected if a function is introduced that swaps them systematically to different points (for example). No doubt Arntzenius and Dorr would protest that the result is a 'less natural' theory. Still, all this smells strongly of mere stipulation.

(for example) managing differentiable structure when positing the additional spaces.[19]

To show how the debate with opposing nominalists isn't moved beyond Field's work by Arntzenius and Dorr, consider (again) the disagreement between substantivalists and relationists (both the classical version and the more up-to-date versions of the disagreement subsequent to general relativity). Arntzenius ([2012b]) presumes the layout in logical space *vis-à-vis* substantivalism and relationism (and its relationship to nominalism) exactly as Field ([1989c]) does. Suppose substantivalism 'wins' because it's the best ('simplest', 'most natural') way to construe the relevant physical theories.[20] (Just like Field ([1989c]), Arntzenius finds the considerations for and against to turn on subtle technical details that are varyingly strong in how they affect the outcome of the debate.) Substantivalism winning means that space-time posits (in some intricate form or other) are nominalistically acceptable—so Arntzenius presumes. And that means, in turn, that all the important philosophical work is being done by simplicity.

Unfortunately, no argument is offered for simplicity; the principle is simply presupposed throughout (Arntzenius and Dorr [2012]), and throughout (Arntzenius [2012a]) (except for occasional proclamations, as above). It's true that the use of one or another simplicity-style principle as a metaphysical stop-gap (to blunt challenges to one's ontological proclivities) dates far back in the history of philosophy. Nevertheless, powerful principles need justification. We indicate what sort of justification is needed for this one, and we sketch why it's unlikely to get it.

The justification demand on simplicity is simple: Indications are needed for why the criteria that 'simplicity' singles out and that adjudicate between scientific theories make those theories sensitive to the world—specifically, make an indicated class of terms (of those theories) pick out real things in the world, and not simply codify phenomena. Our earlier discussion of how physical science itself marks out a distinction between referring terms and ones that codify phenomena—and the corresponding requirement of instrumental access to things when terms are taken to refer to those things—illustrates that science is sensitive to this issue (as it should be).

At this stage in the debate, what the philosopher needs for the case that some more coarse-grained list of virtues suffices is to show that the language of scientific theories, coupled with how this list of global virtues eliminates al-

[19] We're not objecting to the strategy by calling it 'impressionistic'; in lieu of a genuine theory of simplicity, Arntzenius and Dorr have no choice. Our interest is in their justification that positing such additional rich intrinsic structure is nominalistically acceptable to begin with.

[20] Arntzenius ([2012a], p. 182) writes, charmingly: 'Spaces rule!'.

ternatives, is enough to force a class of terms in those scientific theories to refer.[21]

We submit that the history of science shows this is hopeless. There are too many discarded scientific theories that fit the global criteria philosophers have offered that are not only false, but contain terms subsequently recognized not to refer. Worse, the same view is taken of our current theories; the same thing could happen to their posits.[22]

That this is true of simplicity, a virtue that invites invocations like 'most natural' and 'simplest', is perhaps easy to see. Consider, however, an early formulation of a global virtue: the success criterion. Here is how Putnam—who still endorses it—puts the point:

> The no-miracles argument brings out just how strange it is to suppose that a bunch of equations involving various parameters should give us successful predictions if not a single one of those parameters corresponds to anything real. ([2012], p. 93)

Notice the method: a global property, in this case predictive success, supposedly forces terms in the scientific theory to refer.

This criterion fails for reasons pretty close to the reasons simplicity fails: not only are there numerous counter-examples from the history of science, there are plenty of contemporary examples as well, some of which we presented earlier in this article, in particular continua mechanics and fluid dynamics.

4 Contrasts: Kitcher, Maddy, Sober, and van Fraassen

We have, in the foregoing, stressed a distinction between posits taken by scientists to exist and ones that they don't think exist. Maddy ([1992], [1995]) and Sober ([1993]) may seem to some to recognize exactly this distinction.[23] We turn, therefore, to indicating some important differences between our approach and theirs. We also take this opportunity to discuss Kitcher ([2001]) because his approach exemplifies a similar strategy for distinguishing between posits—one that contrasts with our approach. This will also give us an opportunity to highlight several important points that we've made in earlier sections.

To begin with, considerations of the truth of statements (or scientific theories) need to be separated from considerations of what the quantifiers of those statements (or scientific theories) range over. That these need to be separated has been an implicit assumption of our foregoing discussion, but

[21] These virtues apply to scientific theories globally and don't evaluate scientific posits on an individual case-by-case basis.

[22] With only one class of telling exceptions: successful instrumental access to a kind of scientific object enables its survival and transplantation to subsequent theories.

[23] Our thanks to anonymous referees for inviting us to compare our approach to the early 1990s work of Maddy and Sober. To do justice to the issues that their competing approach raises, we've had to somewhat broaden the discussion to include Kitcher and van Fraassen.

it needs a loud announcement now. The reason is that it is relatively easy to see how scientific attitudes about posits can be distinguished: There are distinct terms referring to distinct posits—space-time point, electron, and so on—that scientists have different attitudes about. As a result, on our approach, a scientist can be described as possessing an ontologically committing attitude to one sort of thing (physical items, say) and an ontologically denying attitude to another sort of thing (mathematical items, say). This difference in ontological attitude, however, can't be easily extended to a commitment to (or denial of) classes of scientific or mathematical statements, hypotheses, or theories unless those scientific statements, hypotheses, or theories can be neatly segregated into those with quantifiers that only range over purported mathematical entities and those with quantifiers that only range over purported nominalistically acceptable entities. The issue is complicated and perhaps controversial, but in our view it has been shown that there is a burden of proof on those who claim that scientific theories are such that mathematical content—at the sentential level—can be separated from nominalistic content.[24]

An immediate corollary is that any approach that distinguishes posits in scientific theories that scientists are (at least provisionally) ontologically committed to from those they aren't, in terms of statements or theories that are to be so distinguished, will fail. Mathematical posits will be divided from non-mathematical posits in ways that fail to track how it's done in science.

4.1 Sober's approach

Sober ([1993], p. 35) presumes that the Quine–Putnam indispensability argument presupposes confirmation holism, that 'theories are confirmed only as totalities'.[25] His counter-argument, straightforwardly enough, is that hypotheses can't be confirmed (or disconfirmed, for that matter) unless they are contrastively tested by empirical data—that is to say, if when a hypothesis of a theory is tested by a set of observations, it's contrastively tested in relation to a competing theory containing the negation of that hypothesis. Applied mathematical doctrine, Sober suggests, is not contrastively confirmed or disconfirmed.

It's important to see that the plausibility of Sober's suggestion is driven by his example: he focuses solely on applied number theory. But this gives the

[24] The discussion of Section 2, specifically the second example of fluid dynamics and rational continuum mechanics—where there are structural postulations of abstracta in, as it were, otherwise ordinary objects—shows how heavy this burden is when close attention is paid to applied mathematics. See, for example, (Melia [2000]; Azzouni [2009]) for previous discussion of this in the literature. The issue, of course, is intimately related to the question of whether a form of Field's project can succeed. But note: if the separation of sentential kinds of content—for example, confirmed versus unconfirmed content, mathematical versus physicalistic (or nominalistic) content—can be as easily managed as we will see that both Sober and Maddy presume, then why is Field's programme widely seen to have failed?

[25] We leave aside that there are versions of the indispensability argument that don't assume either confirmation holism or naturalism.

game away: most applied mathematics is contrastively confirmed (or disconfirmed) in Sober's sense, contrary to what he assumes. In particular, applications of specific (and competing) geometric theories (mathematics, if anything is) are confirmed or disconfirmed along with the physics they are bundled in with. Recall that, as we noted in Section 2, the most dramatic changes in successor scientific theories are often due to the change in the mathematics that is involved. (Consider, for the most obvious example, the replacement of Newtonian space-time with relativistic space-time, and the contemplated replacement of the latter with, say, quantum foam.[26])

We might defend Sober by arguing that disconfirmed mathematics isn't genuinely disconfirmed; it is redescribed as pure mathematics, and it remains true. But that defence also doesn't divide the mathematical from non-mathematical posits in ways that respects scientific divisions of posits. An empirical theory (for example, rigid-body dynamics) can be treated as a branch of pure mathematics. There is nothing *per se* about a group of posits (and their governing axioms) that marks them out as suitable for pure mathematics. Sober's approach overlooks that scientific theories really are bundled together with mathematics, and confirmed or disconfirmed together.

Perhaps Sober will complain that his approach does not overlook the fact that scientific and mathematical theories are bundled together; rather, it denies that this is the case. Thus, he may claim that we are begging the question against his view. We don't think, however, that this response will do. First, the question for Sober is not whether mathematical and scientific theories are bundled together, but whether hypotheses are confirmed by being contrastively tested by empirical data. So the relevant question is not being begged here. Second, and more importantly, as we have noted earlier, if Sober is indeed assuming that the mathematical and the scientific content of the relevant theories can be distinguished, he owes us an account of how exactly such a distinction is supposed to be implemented. Surely one of the significant lessons learned from the difficulties that Field's programme faced is that mathematical and scientific content are much more tightly connected than had been anticipated. Hence, it is unclear how Sober's manoeuvre, to the extent that it relies on the possibility of distinguishing mathematical and scientific content, can be made to work in the end.

Another drawback of Sober's approach is that it implies that the status of a set of hypotheses as mathematical (as opposed to empirical) turns directly on whether there are competing scientific theories that deny those hypotheses.

[26] There are other examples, of course, that seem to fit Sober's approach more closely, cases where what are regarded as the same scientific theories are formulated with different mathematics—for example, Newtonian mechanics formulated with standard analysis and calculus versus Newtonian mechanics formulated using non-standard analysis (an anonymous referee's example). Yes, there are such, but it's the numerous counter-examples that matter.

But this doesn't capture a significant phenomenon: the posits in a physical theory can shift from purely mathematical to physically real not because there is a competing scientific theory in the neighbourhood that has emerged, but because instrumental access to those posits has been achieved, or at least it is thought that such has been achieved. This is what happened in the case of molecules: Perrin's evidence for the reality of molecules did not turn on the emergence of a competing theory, but instead on the basis of apparatus that gave scientists reasons to think that the events they saw had to be due to the machinations of molecules. We've said a bit about this particular example already; but we'll say more momentarily.

4.2 Maddy's approach

Maddy's ([1992], [1995], [2001]) papers are rich because she is not only concerned with evaluating the same version of the indispensability argument as Sober, but also with evaluating how pure mathematical practice on its own does or does not force ontological commitments. For our purposes, we need only focus on her objection to the indispensability argument. This objection is similar to Sober's, and is based on the same sentential-sized tools of confirmation. In both her papers (Maddy [1992], pp. 280–1; [1995], pp. 253–4), she briefly rehearses the history of the scientific commitment to the reality of atoms. Maddy ([1992], p. 280) claims that 'atomic theory was well-confirmed by almost any philosopher's standard as early as 1860', and she notes in passing that the still-active debate seemed to be about the observation of atoms (Maddy [1995], p. 253). Maddy then quickly moves to the epistemic conclusion (one she presses in other articles as well) that:

> The salient point is this: in practice, scientists themselves don't regard the empirical success of a body of theory as confirming all its parts; in some cases, the parts will continue to be regarded as 'useful hypotheses' until some further 'direct verification' is possible. ([1995], pp. 253–4)

Like Sober, Maddy assumes as obvious what isn't obvious, namely, that scientific debates about ontology can be recalibrated in the sentential language of specific hypotheses and, more importantly, that the applied mathematics (with posits that scientists don't take seriously) can be separated sententially from the empirical theories (with posits that scientists do take seriously).

4.3 Kitcher's approach

Maddy and Sober both wave their hands at the important issue of separating the amalgam of applied mathematics and empirical theory into distinguishable parts (Maddy [1995], p. 254; Sober [1993], p. 56). Kitcher ([2001]) doesn't ignore the challenge this poses.[27] (Kitcher, we claim, is the hero of this particular bit of philosophy of science because he recognizes and attempts to respond to this issue.) Kitcher gives an example, Fresnel's version of the wave theory of light, and he writes:

> Fresnel's presentation of his theory reveals that his hypotheses about the ether are quite remote from the derivations in which he justifies novel predictions about the observable phenomena. Instead of regarding Fresnel's mistaken belief about the medium in which light waves are propagated as infecting all his discussions of light [...] we do better to recognize that he achieved approximately true descriptions of some of the features of light waves (the mathematical accounts) while being wrong about others. ([2001], pp. 169–70)

The suggestion is that the derivational structure of the theory shows which hypotheses of a theory are really needed to derive the predictions (which the confirmational success of the theory is based on). Unfortunately, one example doesn't make a case. There's little reason to think that the kind of theoretical intimacy that space-time bears (for example) to what is in it—according to physical theories—can be sententially teased apart in the easy way that Fresnel's theory allows its unconfirmed parts to be teased apart from its confirmed parts. If someone empirically applies number theory by counting giraffes, it's easy to separate the mathematical theory from the empirical theory (about giraffes) that it is being applied to. But what makes the giraffe example so misleading is precisely that the role of mathematics in sophisticated scientific (physical) theories doesn't invite any such straightforward sentential division of labour.

4.4 Where do we go from here?

We are at least modestly recommending that sentential confirmational models—that require a sentential division of labour—be set aside as a tool for evaluating how scientists decide what in their theories they are ontologically committed to.[28] That is, a second corollary we have drawn in the foregoing (from our claim that the appropriate focus on the division of the mathematical from the non-mathematical must be the term and not the sentence) is that what is needed is further analysis on the relata of scientific terms to determine the source of the distinction between what scientists are committed to and what they aren't committed to. In our view, what is required precisely at this point are studies of attempts to observationally or

[27] Here we will, to some extent, echo certain points raised against Kitcher ([2001]) by Azzouni ([2004a]).

[28] This isn't a recommendation that confirmational analyses be dropped altogether; ontological commitment issues are only a small part of the epistemology of scientific theories. Our suggestion is that issues about the truth of scientific theories be separated from issues about ontology. Confirmational analysis can still have a (major) place in the analysis of how scientific truth is established even if such analysis is relatively irrelevant to questions of ontology.

instrumentally access what is referred to (by scientific terms).[29] To illustrate this, we return in the remainder of this section to atoms. We argue that what is needed is a focus on the instrumental apparatus (described by Perrin [1990]) that scientists found so convincing.

4.5 Van Fraassen's objection to Maddy's approach to ontological commitment

Van Fraassen ([2009]) has recently responded to his realist critics—Maddy among them—who, he takes it, all accept a certain bit of historical lore that he urges everyone to reject: Perrin's work epistemically legitimated the reality of atoms and molecules (van Fraassen [2009], p. 6).

Like many philosophers, van Fraassen can be quite condescending towards non-philosophers when they utter straightforward realist pronouncements. Van Fraassen writes:

> But do scientists, in practice, make the distinctions so familiar to philosophers, between what is true and what is good for the future of their enterprise? Between, on the one hand, counsel to doubt that there are atoms and, on the other, counsel to doubt that the atomic hypothesis points to the good direction for the advance of physics? ([2009], p. 7)

On the contrary, even a brief perusal of Perrin's own writings on this makes it quite clear that he knows exactly what he is trying to claim about molecules—and his opponents do also. He thinks of himself as a realist, and he thinks the science has shown the superiority of his realist thesis about molecules over competing views that were held by Duhem, Mach, and the like.[30]

Unfortunately, philosophers of science (and mathematics) who characterize the evidence that Perrin is offering in terms of the confirmation of hypotheses

[29] See the first sentence of Footnote 12.

[30] See, for example, (Perrin [1990]). For more evidence about this, see (Nye [1972]), especially Chapter 4. Van Fraassen, appealing to a potential incommensurability between the discourse of philosophers and scientists, instead writes:

> The presentation speech by Professor Oseen of the Royal Swedish Academy of Sciences included the diagnosis 'The object of the researches of Professor Jean Perrin which have gained for him the Nobel Prize in Physics for 1926 was to put a definite end to the long struggle regarding the real existence of molecules'. Such pronouncements are important for the historian, to indicate the terms in which such episodes were discussed, but we must always keep in mind that these words do not come in the context of a philosophy seminar, where our distinctions are made, or the conceptual problems are disentangled in the way we do. ([2009], pp. 22–3, Footnote 20)

We reject this suggestion of a potential incommensurability between the discourse of scientists and that of philosophers. The issue is whether molecules exist. And so what is ultimately at issue here is the proper assessment of the sources of evidence for the contested claims about existence. But this can only be done, to begin with, by a careful examination of the ways in which the scientists in question tried to forge access to the relevant objects.

are not in a good position to defend Perrin's realist evidential strategies against van Fraassen: First, as van Fraassen ([2009], p. 8) observes (explicitly in opposition to Maddy's discussion of this), it is highly tendentious to describe molecular theory as already empirically adequate, 'given the severe problems of the atomic theory in the two decades preceding Perrin's work'.[31] Second, and more importantly, philosophers who explicitly and implicitly rely on confirmation relations among sentential-sized units to characterize the relationship of evidence to ontological claims have a great deal of trouble explaining why Perrin's evidence—in contrast to earlier evidence—does something more to establish the existence of atoms and molecules that goes beyond just supplying additional (and superior) empirical adequacy to what the molecular theory had before his work appeared.[32] Van Fraassen ([2009], p. 8) claims that, 'the bottom line in the empirical sciences is to meet the criteria of success that relate directly to test and experiment'. But he develops this into an empirical grounding criterion: a theory (such as the kinetic theory) needs additional specific hypotheses that imply stricter and stricter connections between the measurable parameters and the parameters pertaining directly to the posited theoretical entities. Van Fraassen writes:

> The result of these additions is that relative to the theory the empirical measurements take on a special significance: their outcomes place constraints on what the values of the molecular parameters can be. And when the process is completed, the constraint must be so strict as to determine those values uniquely, at least in principle. ([2009], p. 19)

He stresses: '*That is what empirical grounding of a theory* is'.

In contrast, what kind of evidence does Perrin think is now available that establishes the reality of molecules? Here is what he says in a section entitled 'The Path of Each Atomic Projectile Can Be Made Visible':

> Thanks to the scintillations produced, we are able to perceive the stoppage of each of the helium atoms that constitute the α rays. But the path followed by each atom is nevertheless invisible, and we only know that it is approximately rectilinear (since the α rays scarcely diffuse at all), and that it must be marked by a train of ions, liberated from the atoms passed through. Now, in an atmosphere saturated with water vapour, each ion can act as the nucleus of a visible drop [. . .] and C.T.R. Wilson, who discovered this phenomenon, has made use of it, in a most ingenious

[31] Van Fraassen ([2009], p. 8, Footnote 6) cites historical sources on this.

[32] This especially applies to Maddy, who says:

> In a case like the post-Einstein/Perrin atomic theorist, it seems incorrect to interpret the claim 'there are atoms' to mean that the assertion of the existence of atoms is empirically adequate: it was considered empirically adequate *before* Einstein and Perrin; afterwards it graduated to another status. ([2001], p. 59)

The issue is, of course, what that other status is, and how it 'graduated' to it.

> manner, to demonstrate the path as a visible streak. (Perrin [1990], p. 212, Section 119) [33]

His subsequent discussion is significant as well:

> A minute radioactive speck, placed at the end of a fine wire, is introduced into an enclosed space saturated with water vapour. A sudden expansion increases the volume and produces supersaturation by cooling. At very nearly the same instant a spark is produced and lights up the enclosure. In the form of white rectilinear streaks starting from the active granule rows of droplets can be seen (and photographed) along the paths followed by the few particles emitted after the expansion and before the illumination of the vessel.

In the next paragraph, he continues:

> Closer examination, however, shows that the trajectories are not rigorously straight, but bend noticeably during the last few millimetres of their path, and even show sharp angles (several are visible in the figure). Each time the atomic projectile passes through an atom it undergoes a deviation, very slight, but nevertheless not absolutely negligible; these deviations, which act cumulatively and in opposition to one another quite irregularly, explain the observed tendency to curve. Finally, in very exceptional cases (owing to the extreme smallness of the atomic nuclei) it happens that the nucleus into which all the mass of the projectile is condensed strikes the nucleus of another atom; a considerable deviation is then suddenly produced. At the same time, the nucleus that has been struck receives an impulse sufficiently intense to make it become, in its turn, an ionizing projectile, with a trajectory that, although very short, is nevertheless recorded quite clearly on the plate as a kind of spur.

The analysis that Perrin is engaging in here is the same kind of analysis that is carried out in contemporary particle physics *vis-à-vis* photographs. It's exactly the same kind of analysis that's carried out to determine the details of an explosive device based on the location and nature of the debris that's found around the bomb site.[34] There is a strict methodological continuity in the tools employed here across a wide spectrum of kinds of entities.

Our intention is not here to analyse whether in fact Perrin's discussion suffices to make the case of the reality of the atomic items he clearly thinks this evidence establishes. That, in fact, involves matters that philosophers are likely to disagree over.[35] But we do want to indicate, in light of the previous

[33] Van Fraassen ([2009], p. 17, Footnote 17) sets aside discussion of (Perrin [1990])—originally published in 1913—and restricts his attention solely to the earlier Perrin ([2005])—originally published in 1910—on the grounds that 'it is much closer to the actual work than his later book'. As a result, van Fraassen omits discussion of the material we subsequently quote.

[34] For that matter, the analysis isn't much different from the kind of analysis someone might engage in when looking at the tracks an animal has left in the snow.

[35] For example, Azzouni thinks that the reality of atoms really is established the way that Perrin thinks it's established; Bueno doesn't. But we agree that it's evidence of just this sort that is relevant to establishing realist claims, and we agree that what follows shows that van Fraassen's

discussion in this article (especially earlier in this section), why those who analyse confirmation in sentential terms (for example, Sober, Maddy, but also Achinstein ([2001])) are not in a good position to make sense of why Perrin thinks this is evidence for the reality of molecules.

The problem is simply that the package of hypotheses that is confirmed includes too much. As the calculation of the kinetic energy of an α projectile that Perrin ([1990], p. 212, Section 118) carries out, the evidential basis of C. T. R. Wilson's cloud chamber confirms a great deal more than that '[t]here are atomic projectiles with such and such properties'. It also confirms a large chunk of physical theory and a large chunk of mathematics (including, of course, geometric presumptions). Maddy and other philosophers often invoke language like 'directly confirms' or 'directly verifies', but this language is never spelled out, and it implies a derivational structure among the hypotheses involved that no one has ever worked out either. The problem is that the implicit sentential confirmation theory presumed involves sentence-to-sentence relations and doesn't take sufficient account of the content of the sentences involved. To make sense of the explanation Perrin is offering, one instead has to analyse the situation in terms of the moving parts that play a role in what can be observed. Our job now is not to give details about how to carry this out; it is only to show the direction needed.[36]

We do need to note that the dialectic that's being run here has an argument-branch that's directed *ad hominen* against van Fraassen. For he allows that observed entities and their properties are to be treated differently—epistemically speaking—from entities like atoms and molecules that he instead treats as node-points in additional theory-structure meant to just tighten the relationship between a theory and its empirical evidence. The problem (for him) is that the reason Perrin places the discussion of Wilson's apparatus at the end of (Perrin [1990]—as the crown jewel of the analysis, as it were—is that it is precisely the observability of the movements of the atoms and molecules that Perrin takes Wilson's work to have revealed (although, of course, not the observability of the atoms and molecules themselves).

We leave van Fraassen with a challenge: how can his approach make sense of Perrin's views so that it is intelligible to us now why Perrin assigned to the instrumental apparatus such importance as a source of evidence for his claims? This is a significant omission from van Fraassen's discussion of this case.

opponents, and anyone else who attempts to analyse this evidence along sentential confirmation lines, isn't in a good position to respond to van Fraassen.

[36] Again, see the first sentence of Footnote 12 for references. Notice, however, a linguistic pitfall: It's natural for someone, convinced by Perrin's evidence, to describe the reality of atoms as confirmed. But this isn't the same notion of confirmation that the philosophers—Sober, Maddy, and others—are using.

5 Conclusion

It is a melancholy fact about the literature that we focused on in the earlier part of this article—(Field [1980], [1989a]), and the subsequent work in the tradition of his approach, specifically that of Arntzenius and Dorr ([2012])—that it fails to ever mention the nuts and bolts of how scientific theories are used by scientists to interact with the world. That is, there is no discussion—not even in passing—about the applications of physical theories or about the nature of the evidence for them.

By and large, what is missing from this philosophical literature is a discussion of the rich array of tools (both in theory and by means of accompanying instrumentation) used to actually forge the reference relations of what—after all—are entirely artificial terms. Numerous studies in the history of science—ones that nearly entirely concern how scientific theories are established—are of significant philosophical interest.[37] Only by actually studying case-by-case the different kinds of ontological statuses that scientists themselves attribute to the posits of their theories, and only by studying how those ontological statuses are (actually) justified, can we determine what mechanisms scientists use to make their otherwise unnatural scientific terms refer (when they succeed in this). For all the theoretical sophistication of philosophers like Field, Arntzenius, and Dorr, the major philosophical work in their positions requires philosophical principles—like the acceptable parts principle, or simplicity—that are either invoked in passing or employed tacitly, and for which (in any case) no arguments are offered.

Other philosophers, more sensitive to evidential questions about how ontological claims are established by scientists (for example, Maddy, Sober, and Kitcher), are instead hamstrung by presuppositions about the logical form that scientific evidence must take—that it must manifest confirmation relations among sentential vehicles of some sort. *Instead of the sentential vehicle (for example, a true sentence) being the appropriate target for ontological study, we are recommending the term, when utilized in an applied empirical theory*. For only in that context, as we have tried to illustrate in this article, can it be recognized whether or not scientific practitioners are taking a term (as used in that scientific application) to refer to something worldly. Only in that context can it be recognized whether genuine reference or instead what we've described as coding is taking place.

The continued focus on sentential vehicles by philosophers interested in evaluating the ontological commitments of scientific theories is (we speculate) ironically the result of tacitly held operationalist doctrines that treat the

[37] In addition to the material we've already cited in passing, a (very short) list of notable works: (Buchwald [1985]; Chang [2004]; Franklin [1986], [1993]; Smith [unpublished]). An important book on instrumentation: (Strobel and Heineman [1989]).

relationship of scientific theories to their evidence as mediated by the observation consequences of those theories. Such a holdover impression yields the result that evidence for a scientific theory can only be applied to—at best—sentence-sized chunks of that theory; and so any claims that evidence for a theory can bear differentially on the status of a class of posits—apart from whether those posits can be segregated in sentence-sized bits of theory that quantify over them—are ruled out of court. We invite a sweeping-out of the remaining operationalism still at work in contemporary philosophy of science, and a closer investigation of how scientific theories forge worldly relations. In particular, we invite a closer look at the observational and instrumental evidence. That such scientific evidence is understood by scientists to apply to term-sized units of scientific theories is the key to whether, and in what ways, the terms of theories actually refer or instead play mere coding roles.

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Jody Azzouni
Department of Philosophy
Tufts University
Medford, MA, USA
jody.azzouni@tufts.edu

Otávio Bueno
Department of Philosophy
University of Miami
Coral Gables, FL, USA
otaviobueno@mac.com

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