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EVADING TRUTH COMMITMENTS: THE PROBLEM REANALYZED

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Abstract

While evaluating a version of the Quine-Putnam indispensability argument that's stronger than standard ones found in the literature, weak conditions for the dispensability of statements that quantify over mathematical entities — weaker than paraphrase — are argued for. These conditions are contoured to apply once a distinction between publicly held science and private belief is drawn. Dispensability projects face two problems: the representation problem and the deduction problem. The former is shown to be unsolvable. The deduction problem is no obstacle provided the representation problem can be solved. Because of the intractability of the latter problem, however, this is no comfort for nominalists committed to the dispensability of statements that quantify over mathematical entities. An important lesson is that "the Quine-Putnam indispensability argument" is concerned with *practical* dispensability not with *in-principle* dispensability. The assertoric use of a theory — by scientists — is a *practical matter*.

1. *The family of Quine-Putnam indispensability arguments*

Here is an enthymemic blueprint for a family of arguments usually described in the literature as "the Quine-Putnam indispensability argument" (hereafter "the QP"):

Premise: Certain statements that quantify over mathematical entities are indispensable to science.

Conclusion: Those statements are true.

This is an enthymemic blueprint of an argument, not merely because more is needed to justify the move from premise to conclusion, but also because philosophers have elaborated this purported argument in quite different and incompatible ways. One goal of this paper is to provide a reading of this blueprint on which the QP turns out to be very likely right. Along the way, I'll indicate some of the alternative readings that occur in the literature, and their drawbacks. I'll not be particularly concerned with the question of which versions of the QP were endorsed by Quine and Putnam, although I'll say *something* about this. We'll see that most philosophers interpret the QP in an unnecessarily weak fashion because they supplement the enthymemic blueprint with unnecessarily-strong assumptions. One virtue of my reading of the QP is its weak premises.

Readers familiar with the many previous discussions of the QP — including my own — will notice the absence of the ontological conclusion: *There <u>are</u> mathematical objects*. Many, if not most, philosophers care about the QP only because they worry whether the indispensability of mathematics to science forces a commitment to mathematical objects.[1] This isn't the topic of this paper. Thus, the opponent positions challenged by my reworking of the QP are various species of "fictionalist": philosophers who deny that the mathematical statements in question are true, or need be taken as true by those who take scientific practice seriously.[2]

2. *The assertoric use of sentences and its relation to truth-commitments*

The premise of the enthymemic blueprint of the QP mentions mathematical statements the *use* of which is presumed indispensable to science. The conclusion mentions the *truth* of such statements. What is the connection between indispensable use and truth? My reading of the QP relies on two empirical facts that describe this connection. These empirical facts will strike many philosophers as obvious truisms.

First, one way people *use* sentences is by asserting them. I may deduce that my dog is mortal by first saying, "All dogs are mortal," and then wistfully adding, "So, my dog is mortal."[3] Alternatively, I may describe a state of affairs, that my dog is chasing my cat, by asserting: "My dog is chasing my cat." Call this a *representational use* of a sentence; the first is a *deductive use*.

[1] See Yablo 1998, 2001, Melia 1995, 2000. It's also the concern of Quine, Resnik, Colyvan, Field, and others.

[2] With respect to ontology, I see two choices for those convinced by the arguments in this paper. Either (as I have) challenge the relevance of existential quantification to ontology, or acquiesce in an ontological commitment to mathematical objects. See Azzouni 1997a, 1998, 2004a.

[3] I do *two things* by saying "So, my dog is mortal." I announce my dog is mortal. I also acknowledge that "My dog is mortal" follows from what I uttered previously.

Both of these illustrate *assertoric uses* of sentences. The first empirical fact is this: people *use* such sentences, in the two ways mentioned, by *asserting* them. This empirical fact, notice, isn't that people *must use* sentences this way. If I utter a sentence in a play, quote someone's words, or work a sentence ornamentally into a painting, it can be said that I'm "using" such sentences; it can't be said (except in special circumstances) that I'm asserting them.

The second empirical fact is that, as we ordinarily understand the word "true," *assertoric uses* of sentences truth-commit their users to those sentences. This follows from the Tarski biconditional "'Snow is white' is true iff snow is white," and its numerous brethren. If I *assertorically use* a sentence, I recognize myself as bound by implication to the original sentence prefixed by "It's true that ...": the implication secures my assertoric use of the latter sentence, and thus my truth-commitment to the original sentence.

Two elucidations of this second empirical fact are needed so that it's not over-interpreted. First: although *assertoric* uses of statements truth-commit their users, Tarski biconditionals can be utilized where statements aren't assertorically used, but instead (as in plays) pretend-asserted. If I pretend that John is running, I (can) similarly pretend that "John is running," is true. Non-assertoric uses of statements can be accompanied by non-assertoric uses of the truth predicate.

Second: That Tarski biconditionals hold of "true" is what enables that word to play the essential role it plays in our deductive practices: to enable *blind truth-ascriptions*.[4] I'll now illustrate this point; in doing so, I'll also provide a second example of how a statement can be used, although not *assertorically* used.

Consider a theory TH that treats Jupiter as a point-mass. TH won't describe Jupiter's intrinsic properties correctly — that it's largely made up of gases, that its rotation distorts its shape, etc. — but if TH is properly constructed, it will successfully predict Jupiter's gravitational effects on the Sun. Label the true consequences of TH — those restricted to Jupiter's gravitational effects on the Sun — with the predicate P. Then we can truth-commit ourselves to the following statement instead of having to truth-commit ourselves to TH and to *all* of TH's consequences:

All consequences of TH that are P are true.

That the Tarski biconditionals are essential to blind truth-ascriptions, like the one just stated, is because the discovery of scientific truths — of all sorts

[4] A *blind* truth-ascription attributes truth to a set of sentences that aren't explicitly exhibited, e.g., "Everything Sarah said yesterday is true." "'John is running' is true" explicitly exhibits the sentence it attributes truth to.

— is motivated not by the (mere) need to be aware *that* such things are true, but by the need to assertorically use them. I can assert, one by one, the following three sentences:

All persons are mortal.
I'm a person.
I'm mortal.

In such cases, it's clear that the conclusion can be asserted (is being asserted) because it follows from the two previous assertions. In the same way, when a deduction is exhibited, one truth-commits oneself to *all* the premises, $\mathrm{Pr}_1, \ldots, \mathrm{Pr}_n$, that appear. One asserts $\mathrm{Pr}_1, \ldots,$ asserts Pr_n, and then relies on the truth-preserving property of deduction to (consequently) assert the conclusion C: C is "detached" from the premises that it's deduced from.

Researchers don't (generally) want to assert a false theory TH, although they do often want to detach true conclusions deducible from such theories and assert *them*. This is possible by utilizing (Quine 1953) the truth predicate in "semantic ascent": one notes *that* C follows from TH, *that* C is a P-statement, and therefore, because all the consequences of TH that are P are true, one concludes *that* C is true. This, although a use of TH *in* a deduction, isn't an *assertoric use*.

As just noted, the point of deducing a true sentence C from a false theory is rarely just to observe that the statement is true. Science is a practice of *assertorically using* truths in other deductions, and in evidential arguments, as well as assertorically representing aspects of the world. One needs (therefore) to assertorically use the deduced sentence C like so: C. Thus, one needs to deduce C from C *is true*. The reverse deduction, from C to C *is true*, is often needed as well. I can long-windedly recommend a book to a friend by uttering each sentence in it — by assertorically using *each one*. Or, I can say: *Everything in this book is true.*

So this has been established. The empirical facts are about *actual* usage: some sentences are assertorically used, and the Tarski biconditionals transform assertoric uses into truth-commitments. It has *not* been shown that *other* uses of sentences — even when coupled with Tarski biconditionals — force truth-commitments; as we've just seen, that's false: A false theory — like TH — may be used, but not *assertorically* used, to deduce true consequences. It can even happen that such a use of a false theory is *indispensable*: only by using *it* can certain truths be deduced.[5] Such indispensability

[5] This is far more common in science than most philosophers realize. Physical theories — for example — can differ greatly in their tractability: accurate true theories of a phenomena are often computationally intractable. But we may know that applying a tractable false theory will yield the needed truths. How do we know that the tractable theory in question is false?

of use isn't indispensability of *assertoric-use* because blind truth-ascription enables us to circumvent a truth-commitment to the indispensable theory.[6]

The previous description of nonassertoric (but sometimes indispensable) uses of sentences in blind truth-ascriptions makes natural this question: How widespread is the practice of *assertorically using* sentences? I've assumed that certain ordinary statements, "Some chairs are wood," "Neutrinos have mass," are representationally used to describe aspects of the world, and consequently are taken as true. It looks, in fact, like *many* statements of ordinary science, that describe various things we take ourselves (collectively) to have discovered about the world, are representationally used. (I'm also assuming, of course, that there are other statements that we don't take to be true and don't assertorically use.[7]) One issue to be explored in this paper is whether the assertoric use of many statements of ordinary science is compatible with one or another construal of the mathematical statements utilized in science as not assertorically used (and therefore, as either not true-apt[8] or as false). I'll show that a position that takes us as truth-committed to statements in any area where mathematics is applied, while assuming that we aren't simultaneously truth-committed to that mathematics, is unstable. It collapses into a position that denies the two empirical facts: no statements — nearly enough — are ones we assertorically use.

Fictionalism is one or another view that undercuts truth-commitments to *sentences*. It's one aim of this paper — as noted — to show that *localized* Fictionalism with respect to statements that quantify over mathematical entities isn't possible: if one is a fictionalist about mathematical doctrine, one must be a fictionalist about all empirical statements (nearly enough).

Because we know that not all of its consequences are true. See Azzouni 2004a, 2004b for further discussion of these kinds of common cases.

[6] Using blind truth-ascriptions to avoid truth-commitments to false indispensable theories was first explored in my 2004a, chapter 2. Despite Quine's (1970) stressing the importance of blind truth-ascriptions, he didn't anticipate using such ascriptions (in certain cases) to evade truth-commitments to otherwise indispensable theories. Overlooking this possibility may still be common among philosophers.

[7] Perhaps among these are statements that we "pretend" to assertorically use, and correspondingly, only "pretend" to take as true, or that we take as true-in-a-pretence, or story. See, e.g., Yablo 1998, 2001, Walton 1990, among others. The crucial point (Yablo 2001, 77) is that "fictional" statements aren't genuinely either true or false. Also see Yablo (1998, 244), where the "external" truth-status of internal (in-a-pretence) truths is described as "quite irrelevant."

[8] Statements are truth-apt if they are susceptible to a truth value, in contrast (for example) to statements only pretended true or false.

My own view is that it's undesirable to fictionalize our discourse practices *across-the-board*, to adopt *global* fictionalism. One reason for this has already been indicated: Blind truth-ascriptions are used in the sciences to distinguish, with respect to false theories, their true implications from their false implications — talk of "truth" is crucial for this. Thus, talk of truth and falsity isn't merely laudatory remarks about statements; such talk is internal to our deductive practices. The global fictionalist must *recalibrate* this practice: there is (say) pretence-truth and pretence-falsity. Real truth and falsity become largely inexpressible within our discourse.

In this paper, therefore, I presume that there are many assertoric uses of empirical statements. I show the impossibility of treating prima facie assertoric use of mathematical statements as other than assertoric use because doing so (in one way or another) forces a similar construal of empirical discourse — a collapse, pretty much, into global fictionalism. In section 12, I revisit the untenability of the global fictionalist picture.

3. *The assertoric-use QP*

The scope of "often" in the following claim is the subject of this paper: We often *must* assertorically use (certain) sentences because otherwise we'll have nothing to say either to facilitate a deduction we need facilitated or to represent phenomena we need represented. Call such sentences "assertoric-use indispensable" (hereafter, "au-indispensable"). I thus read the premise of the ethymemic blueprint of the QP as stating that many sentences of mathematics are *au*-indispensable to science. Accompanying this premise is the assumption of the au-indispensability of the scientific sentences themselves (given a commitment to the scientific project). "Au-indispensable," here, means that such sentences must be assertorically used in deduction, for representations, or both.

The word "confirmation" doesn't appear anywhere in the enthymemic blueprint of the QP. Nevertheless it's true that, with respect to some of the sentences of any science, we'll describe ourselves as committed to their truth because they have been "confirmed" by evidence. Perhaps confirmation is sentence-specific: when a body of scientific doctrine enjoys confirmation, specific sentences are confirmed to specific degrees, and some aren't confirmed at all.[9] Many of us, furthermore, deny a truth-commitment to a statement unless it has been confirmed (to some degree).

If, however, in the context of one or another scientific study, a mathematical sentence is au-indispensable, then that alone suffices — because of the Tarski biconditionals — for a commitment to its truth, *even if it has enjoyed no confirmation from evidence whatsoever*. Thus, no claim about confirmation relations is required for this version of the QP; specifically, no claim of "confirmation holism," that confirmation from evidence accrues to whole theories (as opposed to individual sentences), is presumed. All that's needed to render a mathematical statement au-indispensable is its necessary assertoric use in an empirical context. And, in turn, all that's needed to render the necessary assertoric use of such mathematical statements into necessary assertions of their truth are the Tarski biconditionals.

A *different* version of the QP presupposes confirmation holism — that *all* the sentences of a theory are confirmed as a group by evidence for any of them. Sober (1993) notes against *this* version of the QP (and against confirmation holism itself) that mathematical doctrine never seems to be taken by scientific practitioners as either confirmed (or disconfirmed) by empirical evidence. Therefore (he concludes), mathematical statements haven't been established as items that we must take to be true. The empirical facts of section 2 seem to turn this point against him. Scientific practice opportunistically uses *any* mathematics that's invaluable for deduction and representation. All that's required of such applications is that the mathematics be used consistently — e.g., that intuitionistic or constructivist results not be simultaneously applied with incompatible classical results — and (of course) that such mathematical applications be successful. This routine assertoric use of (hitherto empirically idle) mathematical doctrine truth-commits scientific practitioners to that mathematics — even if it's only potentially applicable. Empirical confirmation isn't involved. And so, the falsification of a version of the QP that relies on a confirmation-holism premise doesn't damage the credentials of that version of the QP that doesn't so rely on an assumption of confirmation holism.

[9] There is no successfully worked out "theory" of confirmation. The difficulty is that confirmation relations — when they exist — are *content-specific*: they are sensitive to what the parts of a theory (its sentences) *say*. To recognize how the parts of a theory have been confirmed, one must look at the details of the theory and how it has been applied. Even professionals often presume a whole theory has been confirmed by a series of empirical results, although close inspection can show otherwise. For a detailed illustration of this with respect to the history of the confirmation of Newton's inverse square law and the third law of motion, see Smith forthcoming. It's Goodman's (1973) examples of "grue," etc., that first revealed the content-specificity of confirmation.

Maddy (1992, 2007) has a different argument against confirmation-holism indispensability. She notes that mathematics is often applied to scientific models known to be false (but predictively useful); and she writes (1992, 281) that

> we must allow a distinction to be drawn between the parts of a theory that are true and parts that are merely useful. We must even allow that the merely useful parts might in fact be indispensable, in the sense that no equally good theory of the same phenomena does without them. Granting all this, the indispensability of mathematics in well-confirmed scientific theories no longer serves to establish its truth.

Resnik (1997, 44, 46) counter-claims that such (false) theories are so utilized to force a truth-commitment to the accompanying mathematics. He writes:

> [Newton] calculated the shape of the orbit of a single planet, subject to no other gravitational forces, travelling about a fixed star. He knew that no such planets exist, but he also believed that there are mathematical facts concerning their orbit. In deducing the shape of such orbits, he presumably took for granted the mathematical principles he used. For the soundness of his deduction depended on their truth.

Resnik's point can be misunderstood.[10] The point is this. Never mind the (truth) status of the idealized empirical tools — models — that physicists apply a branch of mathematics to. Physicists use that mathematics to deduce results about the idealized model. Physicists presume (physicists have to presume) that *those deductions are valid*. But how can this be *if the mathematics employed in the deductions is false*?

A way to blunt Resnik's challenge is to deny the deductive role of applied mathematics — to treat the amalgam of mathematics plus (false) empirical doctrine as a (false) doctrine applied *all at once* to a subject area. But this ignores routine scientific practice. To reiterate the point raised against Sober, new (previously unapplied) mathematics is routinely brought to bear on both true and false scientific doctrine to derive new consequences *from that doctrine*. This requires taking the mathematics so applied to sustain

[10] In part because Resnik (1997, 44, 46) seems to imply it's part of his argument that the empirically false (idealized) science is true (or that what it describes exists in some sense). This is how Maddy (2007, 316, footnote 7) understands the argument, and responds to it.

valid deductions from scientific doctrine, a practice that looks incoherent if the mathematics is taken to be false.

Resnik (1997) has a "pragmatic" version of the QP that avoids the confirmation holism premise: the indispensability of mathematics *justifies* our taking it as true — regardless of whether it's otherwise confirmed or not. My version of the QP has a stronger conclusion: In the course of a deduction, we reason assertorically (because we intend to detach the conclusion). But it isn't that we're "justified" in describing an assertorically-used sentence as true; Tarski biconditionals make the use of the truth predicate *nonnegotiable*. For the same reason, it's misleading to describe the au-indispensability of mathematical doctrine to science as "evidence" of that doctrine's truth. Call my version *the assertoric-use QP*.[11]

The astute reader will recognize that blind truth-ascription, as discussed in section 2, is surely a tool that can (at least in principle) be used to circumvent the assertoric-use QP. How successful it is for this purpose will be explored in some detail later in the paper. Before getting to that, however, I characterize a second tool — proxying — that can also (at least in principle) be used to circumvent the assertoric-use QP. In order to properly prepare the ground for the discussion of proxying in sections 5 and 6, I first need to discuss the distinction between scientific doctrine as publicly held and as individually believed. This not only prefaces the discussion of proxying, but also contributes to the elucidation of "science" and "indispensable," both of which appear in the premise of the enthymemic blueprint of the QP.

4. *Public science vs. private belief*

There are *epistemic communities* — like-minded people engaged in group-projects of learning about the world. *We* belong to one such community. Some of our members are professional researchers, but most are intelligent

[11] The seasoned scholar of the QP will notice the many sorts of theses, that philosophers typically include, that are missing from the assumptions of the assertoric-use QP. Apart from the absence of confirmation holism, the absence of naturalism and the absence of inference to the best explanation should be mentioned. See, e.g., Colyvan 2001, Field 1989, or Maddy 1997 for (differing) versions of the QP presupposing one or more of these assumptions. In contrast, nothing about science being the sole or primary source of knowledge occurs among the premises of the assertoric-use QP, nor is any discussion of the nature of explanation or its role needed. Some might think that the latter must arise when the assertoric-use QP is extended beyond truth to the postulation of mathematical entities; but this is not so. All that's needed is Quine's criterion of ontological commitment.

It's worth adding that although I *am* committed to (a version of) naturalism and to (a version of) confirmation holism, I don't — in my 2004a discussion of the QP — presume *its* dependence on these assumptions.

and engaged fellow-travelers. There is a highly-developed division of labor among researchers: they are specialists with only a nodding acquaintance with other (even neighboring) specialties. All researchers — being persons — have beliefs; and although these beliefs differ greatly from person to person, a great deal of purported knowledge is "commonly held." But much — if not most — of this common knowledge *cannot* take the form of beliefs that are (even tacitly) held by (those) researchers because most researchers are necessarily ignorant of the details (and even broad aspects) of most of this common knowledge. Rather, the sense in which this knowledge is commonly held can only be sociological: as deference relations to the work of specialists in other fields, to the "experts" — indeed to communities of such experts — and derivatively, to the knowledge itself. Commonly held purported knowledge has been long described by Quine as a web of "beliefs"[12]; but we do better describing it as "a network of public knowledge," with the caveat that "knowledge" is defeasible: Anything we (currently) take as knowledge we may subsequently learn is wrong.

Even though this knowledge is officially held in common, it's not that all such is believed true (even by deference) by everyone in our epistemic community. Personal beliefs are often idiosyncratic. Some scientists, for example, regard the science they research in (and the science they rely on for their research) as not true. What's "true," they may privately confess, is either nothing at all (or nothing, anyway, that anyone can ever find out), or perhaps they may only commit themselves to the truth of a small group of commonsense personal beliefs, apart from the occasional spiritual item.

Indeed, so intricate are the evidential (confirmational) — and other supportive — relations that bodies of doctrine have to one another, and so buried from superficial inspection can be the applications of a theory to other areas, that it's no surprise researchers can be ignorant of those aspects of their work. A researcher can fail to see how her work depends on other theories (or how the evidence for these other theories is shared by her own); and so she can cleanly (but inconsistently) deny a truth-commitment to this other theoretical work, but not to her own.

The idiosyncrasy of personal belief — even when opposed by contradicting publicly held or "official" doctrines — is routine, and widely recognized. Even when individual tendencies to "deviant belief" can be accurately described as irrational, this won't justify the further step of denying that these

[12] Quine (1953, 42) both expresses his idea carefully, and carefully buries in metaphors the idealizations that I'm now trying to make explicit: "The totality of our so-called knowledge or beliefs, from the most casual matters of geography and history to the profoundest laws of atomic physics or even of pure mathematics and logic is a man-made fabric which impinges on experience only along the edges."

expressions of belief are correct descriptions of the holder's (personal) *beliefs*. We cannot say that such individuals don't *really* believe what they profess to believe, or that they are confused about what they believe. Furthermore, how we allow personal belief to be idiosyncratic (with respect to public knowledge) indicates that we take public knowledge to be distinct from the individual beliefs of the members of that public.

Notice what the foregoing shows: An analysis of common knowledge — science — cannot be captured in terms of the personal propositional attitudes of scientific practitioners.[13] No one individual knows enough for this to be even reasonable as an *idealization*. And, this is not only true of belief-attributions to scientific practitioners; it's also true if they simulate or feign belief. This makes pressing the appropriate sociological indicators of publicly-held common knowledge. After all — to repeat — there is no single *agent* responsible for the set of publicly-held beliefs whose pronouncements can be taken (when sincere) as expressions of those beliefs. Because of deference, a statistical notion of public belief defined by what *most* people believe won't do. Because of the idiosyncrasy of belief, a refined statistical notion (centered on "experts") won't work either.

A clue to the "location" of public knowledge is that, in scientific biographies, it's routine to distinguish what a scientist is taken to have discovered from what she makes of it. The nature of that discovery isn't ultimately to be characterized in terms of what the scientist thinks; instead, what's relevant are the actual causal relations she (or, more likely, her colleagues) have forged to the world — encapsulated in part (but not entirely) in instrumental interactions with things. Such relations, in turn, are characterized by the best scientific descriptions we can (now) muster.[14] Similarly, when contemporary research is utilized or reported, its surety (on the basis of evidence) may be reported along with it, and that evidence may be subsequently scrutinized to undercut the result. But the *beliefs* and *disbeliefs* that researchers express, although taken as interesting in the way that gossip is taken as interesting, are otherwise not reported.

The foregoing is to say that the various confirmation relations between theories and data — what amounts to observational and instrumental interactions between scientists and what they study — and the various inter-theoretical relations between statements and collections of such, are treated as part of public knowledge just like the statements (in the network of public

[13] The seasoned scholar of the QP will notice, by contrast, that most versions of the QP are couched in the idiom of personal belief, and involve additional assumptions about the consistency conditions on such. I say more about this towards the end of this section.

[14] See, e.g., Chang 2004, or the essays in Holmes and Levere 2000.

knowledge) themselves. Such relations supervene — not on the personal beliefs of researchers but — on their *research practices*: how statements and data are brought to bear on other statements. To put it another way, they supervene on the public record, and on the active technical research tradition that supports that record. The relationship of that public record and technical research tradition, in turn, to the personal beliefs of researchers is, as I've argued, not at all a matter of mirroring. Researchers must have some beliefs or other to do what they do, of course (since they are humans); but there need be no correspondence between their own beliefs and disbeliefs, and the statements that belong to the network of public knowledge.

There is one other important way emerging in which public belief doesn't supervene on personal belief. The presence of many mathematical and scientific results (in the network of public knowledge) is coming to reside not in human thought (as it were) but in artifacts like books, computers, and the fine-structure of scientific instruments. Such results can be brought to active psychological life when researchers need them; but otherwise they remain quiescent, insofar as no human is even tacitly aware of them; and this state of affairs can persist indefinitely. In these cases, public knowledge, that's nevertheless available, can fail to supervene on the personal psychology of *anyone*.[15]

I should also stress a little more explicitly the (recently emerging) encapsulation of (application-) knowledge in instrumentation and programs for such. A program (e.g., a medical diagnostics program) may be one that must be "trained up" to its expertise. That resulting expertise needn't be exemplified in the belief set of *any* human.

The absence of a responsible agent, who can state what's publicly known, means that only in the *use* of a statement by researchers, or more generally, by its uncontested sociological position to be used (by its manifestation in books, other language artifacts, or as a part of someone's or something's implicit expertise), can it indicate its presence in the network of public knowledge. This is a *manifestation condition* on a statement's public presence.[16] When *assertoric use* is involved, this is also the sociological equivalent to the *assertoric use* of a statement by an individual; thus I'll continue to use the language "assertoric use" when discussing the public setting. The metaphor

[15] Consequently, results are often rediscovered (sometimes repeatedly). But they can also be recovered during research forays into the language artifacts themselves. An illustration of the latter can be found in Naber (1992, 67), with respect to an "old and none-too-well-known, result in analytic geometry" that "is proved on pages 105–106 of [a reference published in 1882]."

[16] See Azzouni 2004a, chapter 2.

"public belief" can also apply to a statement that's in an uncontested sociological position to be assertorically used; in light of what's already been said in this section, it's clear that "public belief" — so described — needn't supervene on any individual's personal beliefs.

As I've already indicated, many philosophers read the QP as involving an assumption that one's (assertoric) use of a statement requires a belief in what that statement says. This is to avoid the possibility of an individual asserting statements, and even asserting the truth of such statements while at the same time saying in her heart: "I don't believe any of this," or "I'm only pretending that I believe these things." For Quine and Putnam, in particular, indispensability looks like a *normative* — not a descriptive — condition on belief. Both philosophers allow that one's beliefs can flout it, they clearly *deplore* such flouting, and they express their disapproval in ethical-sounding language.[17] The norm in question arises from a more basic one of consistency: one shouldn't knowingly contradict oneself.[18] More specifically, one shouldn't contradict oneself by — in effect — truth-committing oneself to a sentence S (by assertorically using the sentence), while simultaneously (in one's heart, or via an "off-stage" assertion) denying it. But publicly-held beliefs — as just shown — aren't directly amenable to the application of any such norm because there is no single agent holding those beliefs that a charge of flouting a norm can be directed against.

One might try to take seriously the suggestion that scientists have an obligation of some sort to believe the statements they assertorically use when they operate as "purveyors" of public knowledge (i.e., in their professional capacity as scientists). More strongly, one might claim that they are *irrational* if they deny statements they otherwise assertorically use in professional settings. Given the previous discussion in this section, these are substantial and hard-to-justify assumptions that are very likely wrong. It is better to keep the QP free of them.

[17] E.g., Quine 1960, 242, with the words "wishful thinking" and "philosophical double think"; Putnam 1975, 347: "This [indispensability argument] stems, of course, from Quine, who has for years stressed both the indispensability of quantification over mathematical entities and the intellectual dishonesty of denying the existence of what one daily presupposes."

[18] See Stokes 2007, 440–1.

5. *Characterizations of au-indispensability for public knowledge: The proxy norm*

How, then, are issues of "au-dispensability" and "au-indispensability" to arise for public knowledge? The Quine-Putnam rhetoric of indispensability obliges *individual believers* to shun (or to show that they *can* shun) the assertoric use of a sentence to legitimate their denial of a truth-commitment to it. But this rhetoric is irrelevant to public belief. The solution is to utilize the manifestation condition on the (sociological) presence of a statement. A statement is part of public knowledge (public belief) when it's incontestably available to be *assertorically used* in science: in deductions and in the representation of phenomena. What's needed, therefore, for the corresponding "au-dispensability" of a statement that's apparently part of public knowledge is a *responsible* notion of "laziness in practice." A *sufficient* condition of such — on showing that a statement or a theory, although part of the network of public knowledge is nevertheless *au-dispensable* — is showing that such is replaceable by something that *could be assertorically used instead*.

Strict abbreviation, as Quine (1960, § 39) describes it, is a stern — but illuminating — model. And it scales up nicely from the context of private belief to that of public belief. Suppose the truth-functional "&" and "¬" are in the public language. These, perhaps, are au-dispensable because in every statement in which they occur, they *could be* replaced by the neither/nor stroke ("|"). What's the content of the "could be" in the last sentence? It isn't the unbelievable sociological claim that researchers everywhere could *actually* replace their current uses of "&" and "¬" with "|." Even short and otherwise understandable statements become hopelessly unreadable.[19] In one respectable sense of "possible," it *isn't* possible for "|" to replace "&" and "¬" in public discourse.

Instead, an approach to au-dispensability can be described this way: A locution D is *proxy-dispensable* if there is an alternative locution D* such that:

(*The proxy norm*)

(i) The representation and deduction roles of D can be interpreted as ones in which D is proxying for D*,

(ii) Where disputes arise about whether discourse containing D has such and such implications, or applies in such and such circumstances, researchers can determine answers via access to D*.

[19] "Sally is jumping and it's not the case that Peter is jumping" becomes the horrible "Neither neither Sally is jumping nor neither Peter is jumping nor Peter is jumping nor neither Sally is jumping nor neither Peter is jumping nor Peter is jumping."

Conditions (i) and (ii) *don't* require that researchers actually be able to avoid D altogether, and move to a "parallel discourse" involving only D*. What they *do* require is that researchers can adjudicate disputes about their use of D by instead studying D*.[20] With abbreviation, therefore, au-dispensability takes this form: although researchers assertorically use D in representation and deduction, D "officially" stands for D*. "Paraphrase" is a broader notion than "abbreviation," but it has all the appropriate virtues to satisfy the proxy norm.[21] When we take D to be a "paraphrase" of D*, we take D* to be what we are *actually* asserting when we assertorically use D. Thus inconsistency (the assertoric use of D coupled with the assertion that D is false) is avoided because *all* assertoric uses of D — including those appearing within the scope of the truth predicate — are treated as standing for corresponding sentences in which D is everywhere replaced by D*. I'll describe D as *a proxy of* — or as *proxying for* — D*, and D* as *the target of* the proxy D.

Condition (ii) of the proxy norm has bite. Sufficient for the task is something like a decision procedure for transforming discourse containing D into discourse containing D*. I argue in the next section that condition (ii) can be satisfied with less.

But a preliminary weakening can be introduced now. I used the phrase "something like a decision procedure" because something less than a decision procedure works. On one view of informal mathematical proof, for example, such proofs are successful iff they correspond to one or another formalized proof. Transforming an informal mathematical proof into a corresponding formal proof, however, does *not* involve a decision procedure. Nevertheless, the task is (now) executable.[22] A formal proof not only indicates that the original informal proof is successful (that the premises imply the conclusion) but also "makes explicit" the tacit assumptions and concepts presupposed in the informal proof. In this case, it's official that a list of

[20] The intellectual division of labor can't be forgotten. The researchers who adjudicate disputes about D via D* needn't be the same researchers among whom the disputes arose to begin with.

[21] An "abbreviation" is usually understood to be a shorthand so that the transformation of D to D* changes nothing semantically. This is how transformations of statements containing "&" and "$\neg$" to corresponding statements containing "$|$" are understood. "Paraphrase" is broader: it allows a statement D to stand for a statement D*, where the implications of D — treated independently of any relationship to D* — can deviate from those of D*. Where D is understood to be a *paraphrase* of D*, we are to disallow the significance of those implications of D that aren't shared by D*. (I describe this condition more precisely later in this section.) In addition, we may recognize D* to have implications that cannot be recognized directly from D alone. This is why condition (ii) of the proxy norm demands access to D*.

[22] For further discussion, see my 2006. That informal mathematical proof is indeed subject to the norm of formal derivation is controversial. See Rav 2007 for disagreement.

sentences (an informal proof) proxies — as a whole — for one or another entirely different (formalized) set of sentences.

Another example is this. "Proofs" occur as routinely among physicists as they do among mathematicians. But it's widely recognized that physicists' proofs are rarely "rigorous" by mathematical standards. This isn't just because such proofs are missing steps. More usually, it's a matter of loosely-employed concepts — infinitesimals, the Dirac delta function, Feynman integrals — and reasoning with them by a combination of "physical intuition," empirically derived rules of thumb, and/or boldly-terminological slights of hand. The ultimate "mathematization" of such proofs may transform their character (as they appear in physics) dramatically.[23] The proofs — and this is widely acknowledged by physicists — aren't taken to be (fully) justified until appropriately "mathematized." Because a particular physical proof is itself neither taken to be asserted or true, but only the rigorous mathematical proof that it stands for is so taken, a contradiction between what one assertorically uses and what one takes to be true is avoided.[24]

Notice how paraphrase enables a case for the au-dispensability of some locution, D, to be made. D is taken as proxying for some other targeted exhibitable locution D^*. And D^* is available — not to actually replace D in our assertions but — as a visible authority for settling disputes. It's this exhibitability of D^* that's sufficient to motivate the description of D — as a proxy-tool of convenience for the target D^* — being a *responsible* description.

What does "responsible" mean? It means that researchers have a method — *a principled method* — that indicates what, by way of implications, the proxied statement commits us to. *Without such a principled method, the proxy functions in public independently of any constraints. Its literal implications, when recognized, are open to exploitation by scientific practitioners; and consequently there are no grounds for not treating it — and* all *its*

[23] E.g., the Dirac delta "function" becomes a distribution; epsilon/delta reasoning is the target of reasoning with "infinitesimals."

[24] Strictly speaking, it's only *after* the terminology in question is suitably mathematized that contradiction is definitively avoided. Before that, practitioners may actually try to deny their commitments to certain truths couched in such notation even though no way of replacing them is even in principle available. But that's hardly the worst problem facing researchers during times like this. Usually, the application of such terminology is *ad hoc* and opportunistic because anything systematic (that anyone can then think up) is inconsistent or useless. In such cases, especially if the terminology is au-indispensable, researchers are in a (hopefully temporary) pathological situation. They are forced to work within doctrine, and with rules governing the terminology of that doctrine, that are strictly inconsistent because they can — at the moment — see no way out.

implications — as literally part of public knowledge. In using the word "indicates," I am *not* claiming that the principled method provides a decision procedure for the recognition of implications. That's, in general, not to be had. Rather, what's in place is something much more "case-by-case": one has one's usual methods for generating the literal implications of the proxy, and one can determine (at least in principle) which of those are licensed by the target of the proxy. Abbreviation, for example, not only transforms proxies into their targets; it also transforms proofs of consequences from those proxies into proofs of consequences from their targets. One thing, I think, that should be *required* is this: If a literal consequence of a proxy — a consequence that follows from the proxy — is generated, one should be able to determine whether the target of that consequence is a consequence of the target of the proxy. One should also — more basically — be able to show whether or not a consequence of a proxy *has* a target. (I'm not claiming that these are *all* that should be required; for example, one should also be able to show that a proxy *itself* has a target.)

The motivation for condition (ii) of the proxy norm should be clear. Researchers assertorically use publicly-available statements for deductions and representations. One way to avoid a truth-commitment to particular publicly-available statements is by the proxy norm. Researchers can take those statements to stand for other things (that's condition (i) of the proxy norm). And, because (ii) is satisfied, their research aims aren't compromised by debates over which (apparent) implications of the proxy are genuine. If an issue arises about what implications a particular statement has, condition (ii) of the proxy norm officially guarantees an answer can be — in principle — adjudicated. This shows that the proxy norm is *sufficient* for circumventing the prima facie indispensability of a public statement. If we aren't to treat the statement assertorically used (the proxy) as a statement public discourse is truth-committed to, because it officially accepts some — but not all — of the implications of that statement, a methodology must be available for adjudicating which of that statement's implications should be accepted and which should be rejected. Paraphrase (and, more narrowly, abbreviation) satisfies the demand because it replaces the proxy outright with the exhibitability of another statement *all* of whose implications are acceptable. But, as I show in the next section, *exhibitability* of the target of the proxy statement isn't needed to satisfy (ii) of the proxy norm.[25]

[25] A caveat. To some extent, I've been treating "exhibitability" as idealized jargon. In practice, abbreviations are often introduced because the targeted sentences are too long to actually exhibit. Thus, the weakening of the demand of exhibitability — which I've glossed as a weakening of a requirement of paraphrase, and which is officially discussed in section 6 — already arises with narrowly-construed abbreviation. See, in particular, footnote 26. My thanks to the anonymous referee for urging me to sharpen the formulation of these points.

6. *Weakening the constraint on indispensability*

I have described the exhibitability of the target as *sufficient* for a "responsible" characterization of a locution as a proxy for that target. And I have *not* characterized the proxy norm directly in terms of paraphrase, abbreviation, or — more generally — in terms of exhibitability. This is because exhibitability isn't *necessary*: We can get by with less, and still satisfy the proxy norm. Suppose it's claimed that some purported implication S of some discourse in which D occurs isn't *actually* an implication of that discourse. And suppose the existence of D* is established by a method that doesn't enable — even in principle — the construction of D*. Can we still determine whether S is an implication of D? What the question comes down to is whether condition (ii) of the proxy norm is satisfiable. And this depends on *how informative the existence proof (of D* on the basis of D) is*. If the information the proof yields suffices — in principle — to adjudicate disputes about the (implicational) properties of D*, then a strict demand that D* be exhibitable is unjustified. D can still be treated as proxying for the — strictly inexpressible — D*. Call this requirement, on the relationship of proxies to their targets, the "informativeness condition on proxy/target relations."

It's important to realize that it isn't possible to precisify how informative an existence claim (more strictly, its proof) about a strictly inexpressible target D* should be to enable its relationship to a proxy D to be "responsible." The polemical points I make later against certain fictionalist programs turn on those programs generating relatively clear violations of the informativeness condition on proxies, as well as there being (apart from paraphrase) relatively clear cases where proxy claims are quite responsible. The details of the particular classes of proxy statements and the nature of the existence proofs of the targets — and what information can be extracted from such — are always crucial, however, to an evaluation of whether the informativeness condition is satisfied in those cases.

As I've just indicated, there are clear examples — in natural languages — of proxies and targets that satisfy the informativeness condition, but where exhibitability of the target isn't possible. Ellipses and the phrase, "etc.," can allow a finite locution to proxy a targeted *infinite sentence*. The finite expression: "John is running and John is running and . . . , etc.," is taken (in English) to stand for a particular *infinite* expression. This proxy satisfies the informativeness condition because it's easy to determine what the implications — and other semantic properties — of the infinite sentence (in question) are. Everything needed actually occurs in the finite proxy because of the massive redundancy in its target.

Only a relatively small number of infinite sentences, obviously, are amenable to this broadening of the proxy relationship between locutions used and locutions targeted.[26] It's notable, however, that many of the (standard) philosophical examples of au-indispensable locutions — that involve supplementary quantifications beyond those of the infinite sentences they are meant to replace — clearly bear a relationship to infinite sentences that satisfies the informativeness condition should the so-called indispensable locutions instead be called proxies of those infinite sentences.

Consider, for example,

(1) The number of electrons is the same as the number of protons.
(2) The average star has 2.4 planets.

As many have pointed out, there are infinite sentences corresponding to (1) and (2) with first-order quantifiers not ranging over (respectively) either numbers or average stars.[27] Despite the existence of these alternatives, one's best theory (so it's said) must include (1) and (2) (and so must quantify over numbers and average stars), since these infinite alternatives can't be written down. We *don't* have to write them down, however, to include them in our best theory instead of (1) and (2). Although we can continue to assertorically use (1) and (2), we can treat the latter as responsibly proxying for their infinite targets: the implications of the proxy-targets are appropriately visible given the construction-instructions in footnote 27.[28]

I now contrast the foregoing discussion of the proxy norm and the required informativeness condition with the "weaseling away" approach of Joseph Melia. I claim that Melia's approach — despite his (2000, 469) claims to the contrary — officially embraces contradictions, in contrast to the proxy-norm

[26] As mentioned, it's hard to state precise requirements. A decision procedure for determining, given any ordinal location in an infinite string, the alphabetic item in it, is too uninformative. But even a decision procedure that *exhibits* the target — given the proxy — is uninformative if the procedure takes too long to implement. Further, what's suitable as a target is relative to technical resources. Arguably, formal derivations are only now (i.e., in the last ten years or so) suitable targets for informal mathematical proofs to proxy because of our greatly increased computation abilities.

[27] Replace (1) with the sentence: "There are zero electrons and zero protons, or there is one electron and one proton, or ..." Next, paraphrase the cardinality quantifiers with first-order "there are at least," and "there are at most" quantifiers. (2) is similarly expandable. Replace it with "Either there are five stars and twelve planets, or there are ten stars and twenty four planets, ...," and then, as before, replace the cardinality quantifiers.

[28] It's also worth noting that we can blind truth-ascriptively handle (1) and (2). Call the infinite analogues of (1) and (2) (1*) and (2*), respectively. Even though we can't write (1*) down, we can say "All the implications of (1*) that we can write down are true." Arguably (although I won't pause to do so now) this *does* include (1*) in our best theory by name and by virtue of including all of its (finite) implications.

approach. Melia tells us that it's legitimate to assert a story (a series of statements), and then take back some of its details: deny the truth of one or more sentences that one has just assertorically used. For example, suppose we are told (Melia 2000, 470) that,

> In charge of each star is an angel, no two angels are in charge of the same star, and at the precise moment that each star is created the corresponding angel is also created. Moreover, the angels in charge of stars a and b were created at the very same time.

And then,

> Now I must modify something I said earlier — as a matter of fact, there are no angels, but apart from that, my story is correct.

Because something consistent could have been said *to begin with*, the example is intuitively cogent: we can *see* what the consistent statement is that the speaker's actual remarks proxied for. (Indeed, in this case, we can write it down explicitly.) Melia takes that aspect of the example to be irrelevant, however, claiming that "by taking back some of the consequences of [earlier sentences asserted, one can nevertheless succeed] perfectly well in communicating [something]" (Melia 2000, 471). This — nakedly asserted — is wrong: without informative access to the supposed targets that one's statements are proxies for, one *hasn't* avoided inconsistency. Melia-weaseling, given Melia's justification of it, legitimates "Calvino-weaseling":[29]

> [Once], as I went past, I drew a sign at a point in space, just so I could find it again two hundred million years later, when we went by the next time around. What sort of sign? It's hard to explain because if I say sign to you, you immediately think of a something that can be distinguished from a something else, but nothing could be distinguished from anything there; you immediately think of a sign made with some implement or with your hands, and then when you take the implement or your hands away, the sign remains, but in those days there were no implements or even hands, or teeth, or noses, all things that came along afterwards, a long time afterwards. As to the form a sign should have, you say it's no problem because, whatever form it may be given, a sign only has to serve as a sign, that is, be different or else the same as other signs: here again it's

[29] Calvino 1968, 31.

> easy for you young ones to talk, but in that period I didn't have any examples to follow, I couldn't say I'll make it the same or I'll make it different, there were no things to copy, nobody knew what a line was, straight or curved, or even a dot, or a protuberance or a cavity. I conceived the idea of making a sign, that's true enough, or rather, I conceived the idea of considering a sign a something that I felt like making, so when, at that point in space and not in another, I made something, meaning to make a sign, it turned out that I really had made a sign, after all.

Notice. Melia-weaseling is irresponsible. We are told that we can accept some implications of our statements and not others, and that the method to do so is to contradict ourselves. But that's exactly how Calvino-weaseling works. Unfortunately, by the time the narrator ends his (contradictory) qualifications, we have no idea what in that story can possibly be true. If we don't have in-principle ways of distinguishing what we can take as true and what not among the proxied statements and their implications, via informative access to the targets of those proxies, *ordinary* contradiction threatens. It's *this* the proxy norm is designed to stave off.

It's striking that the informativeness condition is satisfied by *all* of Melia's illustrations of ordinary "weaseling" — although *not* by his description and justification of Melia-weaseling. His examples of uttered contradictions (e.g., "Every F also Gs, but not Harry") can all be consistently paraphrased. In particular, as noted already, his angel-story admits of a consistent paraphrase without quantification over angels.[30] That is, we can tell the difference between the genuine communication of something consistent by something sounding contradictory, and actual contradiction when and only when the proxy norm is satisfied (along with the informativeness condition). But if these conditions are satisfied, "weaseling" is an inaccurate description.

7. *Intermission*

Here is the point we have reached. The assertoric-use QP attempts to oblige us to a truth-commitment to whatever mathematics we indispensably *assertorically use*. But even if the application of a branch of mathematics to a scientific subject-area is indispensable, we still have two possible escape routes. We can either blind-truth ascribe our truth-commitments to be not to the mathematics itself but only to certain (empirical) consequences of it

[30] Similarly paraphrasable is his (2000, 468) two-dimensional world picked out in terms of a sphere the existence of which is denied.

and/or we can assertorically use that mathematics but treat it as responsibly proxying for certain non-mathematical statements.[31] We turn to an exploration of these possibilities of escape from the assertoric-use QP in sections 9 and 10. Section 8 is dedicated to a preliminary matter: characterizing those statements that we should understand as referring to mathematical entities.

8. *Quantification over mathematical entities in empirical theories*

As noted earlier, most philosophers aren't interested in au-indispensable mathematical statements but rather in the ontological commitments that supposedly accompany that au-indispensability. Therefore it's an important (preliminary) question exactly what makes a statement "mathematical" — what makes the *relata* of certain terms mathematical entities. This concern gains poignancy when it's realized that mathematical concepts are richer and more wide-ranging than the short list (numbers, geometrical shapes) most think of. Three examples of such concepts are drawn from knot-theory, Turing-machine formalism, and rigid-body mechanics. All are subject-matters studied within (pure) mathematics. What makes such studies *mathematical*, and not empirical, is solely the attitude taken by professionals towards the theorems shown. Mathematical entities are *not* treated as items open to empirical test. In addition, it's precisely the tractability requirement of the theorem-proving process that compels the attribution of certain "idealized" properties to those mathematical entities (e.g., that points are dimensionless, rigid bodies are *rigid*, Turing machines don't break, etc.).

This, however, provides a symptom of reference to mathematical entities in otherwise empirically applied and tested theories: No attempt to empirically test such an item's properties by focused instrumentation (directed at *it*) is undertaken or contemplated. For a striking illustration, consider the differing professional (epistemic) attitudes towards spacetime points and towards quarks. Evidence was perceived as needed (and scattering experiments were

[31] Contrast the constraints characterized here with Field's self-imposed (1980, 2) requirement: "[The] (Quinean) objection to fictionalism about mathematics can only be undercut by showing that there is an alternative formulation of science that does not require the use of any part of mathematics that refers to or quantifies over abstract entities." Merely showing that *there is* an alternative formulation of science (all by itself) doesn't so suffice because showing such needn't either show (i) that the alternative formulation of science is one scientists can assertorically use the sentences of, nor even (ii) that scientists can responsibly take their currently non-nominalistic formulations to proxy for sentences in the alternative formulation of science (because showing that there is an alternative formulation of science needn't provide methods for distinguishing between the implications of the proxies that have true targets, and the ones that don't).

designed to procure it) before an ontological commitment to quarks was regarded as acceptable.[32] But nothing comparable is ever offered to empirically verify — for example — that spacetime points have *no* dimensions, or to empirically verify that spacetime is itself genuinely continuum-dense. (No studies have been undertaken to empirically verify the cardinal *number* of spacetime points that huddle together in a region of space.)[33]

The purely mathematical nature of these posits of (and structural assumptions of) spacetime can be obscured by noticing that physicists have considered rejecting the "continuity" of space. What's being contemplated, however, is — in part — the substitution of one background geometry *for another*. In string theories, for example, even though the continuum properties of four-dimensional space are rejected, another background geometry (involving many additional dimensions, but ones still possessing continuum properties) replaces it.[34] This should be no surprise: the background geometry has continuum properties (after all) for purely mathematical purposes: so that various functions and operators are well-defined. These mathematical purposes don't vanish because the empirical character of spacetime has shifted.

Mathematical entities, however, can also occur as the *relata* of terms in physical theory (as part of physical theory) because they are the mathematical appearance of physical *noumena*; space and time play this role in their Newtonian incarnations, for space/time is the backdrop against which accelerations are defined. This requires relations to (and so quantification over) absolute space and time, or relations to (and so quantification over) a successor notion (against which absolute velocities don't exist);[35] but doing so doesn't require attributing any other (physical) properties to space or to time. The spacetime of general relativity is different because its metric carries energy. Nevertheless, the terms "mathematical appearance" and "physical *noumena*" are barely exaggerations even in this case. The continuum properties of Einsteinian spacetime (like its Newtonian counterparts) are induced by mathematical presuppositions, and none of the resulting "ontological commitments" of spacetime points, nor the other mathematical (e.g.,

[32] See Azzouni 1997b, 478, for discussion. I should add that experiments are currently under way to "probe deep inside the proton." See Bass 2007.

[33] It's not clear, of course, what an empirical test of this could look like.

[34] Similar is the replacement of the surface geometry of a table (treated as Euclidean) by a more complex (but still continuum-rich) geometry to better account for frictional forces — to better create a mathematical background for the characterization of such forces.

[35] See Stein 1967.

manifold) constraints on spacetime are tested for — except with respect to the global success of the entire empirical theory.

The same point holds of the rich mathematical structure of (e.g., electromagnetic) fields. These too, even when recognized as spatially extended entities that propagate out from their sources at lightspeed, and even given that these velocity properties of theirs *have* been tested for, aren't otherwise empirically tested for the (purely) mathematical properties they are posited to have — e.g., that they are continuum phenomena. Note again the crucial point that these mathematical properties induce quantification over mathematical posits of particular sorts — but no empirical tests are engaged in (or even contemplated) to verify the existence of such.

In cases like these (fields and Einsteinian spacetime), mathematical entities function as the indispensable theoretical *garb* — the Kantian appearance — of something unknown that may be physically construed in some other way entirely (although perhaps quite far in the future). Such mathematical entities — as noted in the case of general relativity — and despite their explicitly nonempirical character, can even be stipulatively endowed with causal properties. Insofar as this occurs, we may take it that *noumena* — the actual physical reality — is peeking (partly) through its mathematical finery. But so long as so many of the mathematical entities in question remain purely mathematical (remain, that is, immune to empirical test), just so long should we take them as mere stand-ins for something physical that science can currently represent in no other way.[36]

[36] Successor physical theories only respect in an epistemic way the (mathematical) properties attributed to mathematical entities functioning as the "appearances" of physical *noumena*: as calculational approximations of something physical. Consider a previous example: the flat table's Euclidean surface replaced by a quite complex (physical and geometrical) description of its surface. Similarly, of course, for the subsequent replacement of the relativistic spacetime continuum by, say, "quantum foam." Thus the energy carried by the metric of spacetime is likely to be attributed to something else entirely, or continue (perhaps) to operate as a stipulated property of successor mathematical entities. Successor physical theories, of course, never *totally* eliminate mathematical entities: the latter just show up elsewhere. This is because mathematical entities have two (often blended) roles in empirical theory. One is as mathematized stand-ins for physical unknowns; the other is to facilitate deductions about relations and properties with quantifiers.

An attempted sorting of the posits of (current or future) science neatly into empirical entities and mathematical entities may fail because (as the spacetime of general relativity already intimates) posits of a theory can be rather complex constructions of both mathematical and empirical content. Whether a posit with mathematical content should be regarded as a mathematical entity or not turns on whether that content plays a role in the *individuation conditions* of the posit. If so, the object is a mathematical entity because any successor theory that eliminates mathematical aspects of that posit will change it so drastically as to make cogent the denial that the posit occurs at all in the successor theory. I can't get further into this now.

In any case, for the purposes of this paper, all that's needed is the evident fact that spacetime points (and the points of fields — if distinct from the former) are mathematical entities.

Many philosophers presume the distinction between mathematical entities and empirical entities (e.g., physical ones) is metaphysically marked by distinctive properties; the breadth in the properties of twentieth-century mathematical entities refutes this. The real difference remains: professionals attempt epistemic access to empirical entities and their properties, and they don't attempt this with mathematical entities. Although *this* distinction isn't sharp in all imaginable cases, it doesn't need to be.

9. *The representation problem*

With this characterization of "mathematical entity" in place, let's now explore the application of blind truth-ascription and proxy methods to dispense with sentences (in our sciences) that quantify over such. Consider our representational and deductive uses of ordinary scientific statements directed towards some scientific subject area. We need to reformulate our representations of that subject area so that we never representationally use sentences that quantify over mathematical entities: either because all sentences representationally used are proxies, or because no such are representationally used at all. Second, the remaining deductive uses of statements that quantify over mathematical entities must be ones that don't force truth-commitments. I'll call the second (easier) problem "the deduction problem," and address it in section 10. The worse one is "the representation problem," to be discussed now.

Quantification over mathematical entities is indispensably assertorically utilized to represent empirical phenomena. Given that geometrical entities are mathematical entities,[37] the representation problem is formidable even for ordinary macro-objects. We represent the movements of such objects by imposing a space-time matrix on our pre-scientific macro-object descriptions. This enables the representation of shapes, distances, velocities, mass distribution, etc., of macro-objects to any realizable accuracy.

Our ordinary talk of (macro-) objects being near or far from us, being in so-and-so places, or being-roughly-so far away from other objects, is egocentered, vague, and qualitative. Perhaps such ordinary talk even relies on a

[37] Field (1980, 3, 1989) denies this; Melia (1998) is similarly complacent. On the other hand, several philosophers have raised objections to the suggestion that geometrical entities are empirical entities, e.g., Malament 1982, 532, and some have, anyway, disputed the cogency of the distinction between abstracta and concreta altogether. My point is different. As already noted, classifying spatial/temporal points as empirical entities is misguided, if motivated by the view that such items are (even in principle) epistemically accessible. The issue isn't whether the philosopher (e.g., Field 1989, 68–69) can *conjure up* epistemological access to such items; it's whether that *conjured-up* access plays (or can play) any genuine role in scientific epistemology. It doesn't (and it can't).

qualitative notion of "place," where such are perceived as patches next to one another. Regardless, the various ordinary-language predicates (of such talk) are easily embedded within that of three-dimensional Euclidean geometry (or other similar structures), so that macro-objects can be "precisified" (or "reconstituted") as loci of points (and their properties and relations, respectively, reconstituted as point-localized properties and comparisons thereof). This reconstitution of macro-objects, their properties, and their relations, leaves intact our pre-mathematized representations of them; this — no doubt — explains the impression that spatial relations between macro-objects are "external" ones. For the same reason, such reconstituted macro-objects, despite being invested with mathematical properties, are still individuated as before; and so they are still reasonably treated as empirical entities.

It's worth seeing in more detail why mathematical entities are essential to the representation of the properties of macro-objects. Were macro-objects to only have a small number of shapes and sizes, were their movements pixilated so that movements from one "node" to another were via finitely many intermediate nodes, and were the universe itself finitely noded, then quantification over spatial entities would be unnecessary. A finite number of size and shape predicates, finitely many distance predicates, and axioms governing such, would suffice.[38] It's that objects are arbitrarily sized, and that their distances from one another are both (infinitely) divisible and (potentially) incommensurable, that induces quantification over points, and induces the other fine-structure properties of space (and time).[39] Quantification over points is required because there is no lower limit on the distances of objects from one another, or on the relationships of the sizes — and the other properties — of such objects to one another.[40] It's this, with respect to distance,

[38] Notice that in such a context, questions of incommensurate distances can't be raised.

[39] The precise mathematical entities needed, of course, depends on the background physics.

[40] Notice the representation problem induced because of shape *alone*. Meager possibilities among macro-objects may enable our representational needs to be satisfied by four predicates Cu, Sp, S, L, describing — respectively — cubes and spheres in the two sizes, small and large. When macro-objects vary arbitrarily in both shape and size, characterizing them as loci of points, e.g., $x^2+y^2+z^2 \leq 17$, becomes au-indispensable on pain of uncountably many such predicates. Furthermore, arbitrarily complex functional characterizations of such shapes (algebraic, transcendental, etc.) are required too. And all this is because of the arbitrary shapes of (solid) objects alone! Our representational needs with respect to macro-objects overdetermine the need to refer to geometrical entities, given the many different properties that objects have, continuously and discontinuously, over their parts, and consequently, the many different relations they have to one another. This forces representation via quantification over points — or (perhaps) over arbitrarily sized regions. (Substituting minimal regions of spacetime for points is not to substitute empirical entities for mathematical entities.)

that induces an *uncountable* number of distance relations between objects, something that can only be handled by the introduction of a metric and its accompanying quantification over (uncountably many) mathematical entities.

The foregoing may suggest that such quantification could be eliminated were we to discover size and shape limits on objects, and that their movements are quantized. Two points. First, such quantifications aren't — in any case — to be eliminated by *logicians*. They can only be eliminated by a successor *physical* theory. The second point is that total elimination of mathematical entities from physical theory is, in any case, unlikely. Advances in physical theory always introduce more extensive utilization of mathematical entities to represent physical phenomena, not less. (For illuminating examples of this, see Malament 1982, 532–34.)

There is a neatly statable need that quantification over mathematical entities satisfies. This is that adjectival constructions impose *tractable* deductive demands (managed by the representations of properties and relations via *predicates*) only so long as such constructions are finite (or at most countable) in number. But once there are uncountably many such constructions, quantification over entities (that stand in for those constructions) is needed to make the representation of deduction possible. This is one of the fundamental arguments of this paper.

It should be added that this logical need is behind Davidson's (1967, 1977) positing of events to handle adjectival constructions and it's the same as the one behind the positing of spacetime points to handle the uncountably many possible distance relations between objects. The same considerations motivate the replacement of infinite sentences by statements like (1) and (2) (in section 6) with their resulting quantifications. The difference is that with respect to distance relations between macro-objects, proxy-targetable (countably) infinite sentences aren't possible.

The representation problem, therefore, is a major obstacle for the project of dispensing of truths that quantify over mathematical entities. Recognition of this has been impeded by a failure to acknowledge the symptoms of being mathematical. This isn't the possession of abstracta-attributes, e.g., being "outside of space and time," but rather official immunity to epistemic access (e.g., by instrumental interactions).

It's easy to overlook the relevance of the representation problem altogether. Melia (2000, 458) describes "the trivial strategy."[41] Given any non-nominalistic theory U, he writes,

> simply partition the predicates into two classes: those that are nominalistically acceptable and those that are not. Let theory T be those

[41] Also see Field 1989, 129.

> sentences that are logically entailed by U, yet whose vocabulary contains only nominalistically acceptable predicates. Since every nominalistically acceptable sentence logically entailed by U is logically entailed by T, it would seem as if T has all the kosher consequences of U and none of the unkosher ones, and thus can serve as the nominalist's replacement theory.

As Melia notes, this very strategy was offered by instrumentalists averse to theoretical entities via the deduction of the "observational consequences" of theories. The strategy fails. Just as scientific theories generally don't have "observational consequences," so too the application of a mathematical theory to a nominalistic subject matter won't in general have "nominalistic consequences." In both cases, needed consequences are induced in a theory — not by deductions from it — but by "reconstituting" objects (that are described in some other way entirely) in terms of the theory, so that implications from it *now* apply to those objects.[42] The Trivial Strategy runs aground on the representation problem *to begin with*.

I'll conclude this section with a couple of words about the proxy strategy. The foregoing has been directed at showing that we can't paraphrase representational uses of statements that quantify over mathematical entities — in particular, ones that are used to describe distances, shapes, and other properties. Can we treat these statements — more weakly — as proxying for statements that don't so quantify? What's required are construction-instructions that indicate, for each such statement, its target. Furthermore, what's additionally required is that these construction-instructions satisfy the informativeness condition. I haven't *proven* this is impossible. But given what's involved (e.g., the massive number of predicates needed to replace quantifications over points, etc.) the burden is surely on that philosopher who thinks otherwise.

10. *The deduction problem*

Call a sentence "nominalistic" if it doesn't quantify over or name mathematical entities. Similarly so call a theory or a language if the sentences of such

[42] This "method" of application is — unsurprisingly — ubiquitous. One applies Newton's laws to the planets by redescribing them as point-masses; one can make one's applications much more sophisticated by replacing these idealizations with others so that the "idealized objects" are much closer to their empirical targets — but "idealizations" of a domain of application to prep it for an (applied) theory are *always* needed; and the result is always objects that aren't nominalistically acceptable. Such prepping of objects also occurs when macro-objects are set into the context of Euclidean geometry, as I've argued in this section.

similarly don't quantify over or name mathematical entities. To continue evaluating fictionalist approaches to nominalization that require dispensing with non-nominalistic sentences, I now assume the representation problem of section 9 *can* be solved for a scientific subject-area A and for a scientific theory E applied to it. This is to assume that there is some nominalized language N such that each representationally-used sentence (describing a fact in A) either itself belongs to N or proxies for a sentence from N. It's also to presume that the empirical content of E can be characterized by a nominalistic theory N* in N.

Mathematical content is presumed to only arise in deductions. That is, any proof $\mathrm{E}_1, \ldots, \mathrm{E}_n$, that involves descriptions of A, the scientific theory E, and applications of mathematics is presumed to be either replacable by (or to proxy for) a proof $\mathrm{NM}_1, \ldots, \mathrm{NM}_m$, where each sentence NM_i is either a sentence of N — a characterization of a fact in A or a statement from N* — or a sentence of M*, a branch of mathematics that's jointly applied to A with N*, and where NM_m is in N.

At this stage we (presumably) have reduced the dispensability project to that of handling the case where sentences of M* appear in deductions with sentences of N, what may be called "the deduction problem." One constraint to a solution to this problem is this (Field 1980): any proof, $\mathrm{NM}_1, \ldots, \mathrm{NM}_m$, as above, must correspond to a proof $\mathrm{N}_1, \ldots, \mathrm{N}_p$, where each N_i is in N, N_p is NM_m, and any sentence of N occurs in $\mathrm{N}_1, \ldots, \mathrm{N}_p$ (as an assumption) only if it occurs in $\mathrm{NM}_1, \ldots, \mathrm{NM}_m$ (as an assumption). That is, the application of the mathematical theory M* in proofs along with statements of N is *conservative*.

But why is this condition *necessary* once the representation problem has been solved? Consider the scientist who assertorically uses sentences of the current scientific language.[43] This scientist, let's say, engages in a deduction of the form $\mathrm{E}_1, \ldots, \mathrm{E}_n$. We presume the availability of a responsible relation to proxies, $\mathrm{NM}_1, \ldots, \mathrm{NM}_m$, as above. The scientist, however, need only truth-commit himself to sentences of N that follow from M*+N. That is, he can use the blind truth-ascription method to avoid a truth-commitment to any sentences of M* that are used in any deduction. More strictly, he can truth-commit himself to conclusions E_n that proxy for sentences in the language N. There is no need, that is, to require conservativeness of the application of the mathematical theory M* to bodies of nominalistic statements couched in the language N. Once the representation problem is solved, there is no deduction problem left.

[43] This perspective is never explicitly considered by Field. I speculate this is because he mistakenly perceives the response to the QP in terms of footnote 31. He's hardly alone, of course. See footnotes 53 and 55.

Consider Field's arguments for his conservativeness constraint. He (1980, 10–14, 1989, 56–60) argues both that applied mathematics *is* conservative, and that it *should* be. One argument (Field 1980, 12–13, 1989, 59) for the latter claim can be quickly dismissed. This is that the conservativeness of applied mathematics is a necessary condition for mathematical theories to be "true in all possible worlds." It's easy to show that conservativeness *isn't* a necessary condition for this. Let $N^\wedge$ be some nominalistic body of statements. Suppose that $M^*+N^\wedge$ implies N_1, and $N^\wedge$ doesn't imply N_1. In one world (we presume) N_1 is true; in the other it's false. Both M^* and $N^\wedge$, we can nevertheless say (*pace* Field), are true in both worlds. What isn't true, however, is $M^*+N^\wedge$: we can't, that is, *apply* M^* *to* $N^\wedge$ in both worlds. To apply a mathematical theory M^* to a body of nominalistic statements isn't to *merely pool them together* — a mistaken impression Field's argument may be presupposing. As stressed in section 9, to apply M^* to $N^\wedge$ is to reinterpret (reconstitute) the posits and language of $N^\wedge$ *within* M^*. In this sense, many mathematical theories (that we nevertheless regard as true) can't be applied arbitrarily to any set of nominalistic sentences one likes. *But we knew that already.* If one is summing numbers of apples, one can't reinterpret the cardinal quantifiers governing such in a finite group theory (although one *can* do so if one is summing the positions a spinning needle can take on a wheel).[44]

In any case, is there any reason to think that — in general — mathematics applied to nominalistic bodies of doctrine *is* conservative? No, because nominalistic bodies of doctrine can be Gödel incomplete. Nothing precludes the inclusion of concatenation theory within a nominalistic subject area (one thinks of chromosomes, for example, or neurologically-based syntax processing).[45] In general, there is *no reason* why (*tractable*) nominalistic bodies of doctrine describing a physical subject matter can't be incomplete; indeed, this is rather likely.[46] And so, there is no reason why supplementing

[44] And, of course, it's an entirely empirical question whether — in a world — bunches of apples act like spinning needles.

[45] Shapiro 1983 illustrates how to model numbers in Field's "empirical" spacetime.

[46] What's being explored in this section is the use of the proxy methods of sections 5 and 6 to respond to the assertoric-use QP. In particular, scientific deductions are to proxy for ones in nominalistic bodies of doctrine to which one or another mathematical theory is applied. Thus the nominalistic bodies of doctrine so targeted had better be axiomatizable, otherwise, we're not talking about (nominalistic) bodies of doctrine that scientists can *use*. I allow two kinds of use — strictly speaking, two kinds of access to nominalistic doctrine. The first is the assertoric use of statements of the nominalistic language. In that case, we strictly utilize the mathematics only to recognize implications between nominalistic statements. The second is where we assertorically assert at least some of the statements in the nominalistic doctrine via their mathematized proxies.

(I should also point out that I'm assuming the "proof-theoretic" notion of consequence in N *allows* incompleteness. If not, the conservativeness constraint becomes empty.)

such with a mathematical theory — *as such applications are ordinarily implemented* — has to be conservative.[47]

Finally, would the failure of mathematics to be conservative when applied to nominalistic bodies of doctrine be a bad thing? No. By augmenting a nominalistic body of doctrine in a nonconservative way one accesses additional testable empirical content. This is *good*. Should that additional empirical content be refuted empirically, there are *two* options. (i) Reject the truth of the nominalistic doctrine. (ii) Reject the application of *this* mathematics to *that* doctrine. There are *no* methodological drawbacks to this.

Field (1989, 57) writes that "if a mathematical theory entailed that there were exactly nine planets in our solar system, all but the most unregenerate rationalist would feel that this showed that that mathematical theory was unacceptable." Sheer rhetoric. First, the example required isn't one of *pure mathematics* entailing exactly nine planets but such mathematics *coupled with empirical astronomical theory (and perhaps other empirical facts)* so entailing. Second, most of the intuitive power of Field's thought experiment turns on our antecedent dislike of *any* theory — empirical or otherwise — entailing a *specific* number of planets. (If empirical doctrine plus mathematics did so entail, however, we would be surprised but would reluctantly accept it — *were the amalgam empirically confirmed*.) Third, if this were the only way for *tractable* empirical doctrine to gain purchase on additional (testable) empirical content, whatever could be the harm — especially if it turned out *correct*? Note the point: Just as the Fieldian fictionalist claims that mathematics isn't true but is good because it provides proof-theoretic economy, the variant fictionalist contemplated here claims that the mathematics isn't true but it's *good* because it provides an instrumental tool for extracting nominalistic content from nominalist doctrine — *content that's otherwise not available*.

[47] The unnatural constraint Field imposes on applications of mathematics to empirical statements shows up as presuppositions in his two proofs for the conservativeness of mathematics in the appendix to his 1980 (16–19): No non-set-theoretic vocabulary appears in the "comprehension axioms," i.e., replacement or separation. How devastating a restriction this is is masked somewhat by ordinary mathematical applications not being (directly) set theoretic. But careful consideration of how ordinary mathematics *is* applied makes clear that when translated into the language of set theory, non-set-theoretic vocabulary must appear within these axiom schemas.

It should be added that ordinary mathematicians, like ordinary scientists (e.g., physicists), find the distinction between mathematics supplementing scientific (or mathematical) doctrine nonconservatively as opposed to conservatively (but aiding in proof-theoretic economy) an irrelevant distinction. The aim is practical: enabling the tractability of deductions (mathematical or physical), and enabling the representations of physical phenomena in terms that empirical theories are sensitive to (in whatever ways this can be done).

What’s been shown in this section is that the deduction problem is trivial once a solution to the representation problem is given. Utilizing the blind truth-ascription technique, one can truth-commit to all and only those implications of a scientific theory E, mathematics M*, and facts about a domain A that E and M* are jointly applied to, that are entirely in N. The representation problem, unfortunately, is intractable.

11. *Nominalistic-content views of Balaguer and Rosen*

Were it the case that mathematical doctrine is applied to empirical doctrine that’s (always) formulated independently of mathematical concepts — were there (in other words) no representation problem — and were the role of that mathematics only to facilitate proof-theoretic economy, then proxy-dispensability tools wouldn’t be needed (at least, they wouldn’t be needed for empirical applications of mathematics). *Regardless* of whether the mathematics so applied was conservative or not, Resnik’s challenge would be met by blind truth-endorsing only nominalistic consequences of mathematical+nominalistic doctrine. It’s, however, the representation problem that forces would-be nominalists (wedded to the fictionalist strategy) to turn to proxy-dispensability — precisely because the purely nominalistic consequences that can be expressed in contemporary scientific language are so meager in scope. This places severe constraints on how one *should* claim au-dispensability. These points have been stressed in sections 5 and 6. I briefly illustrate them again.

Consider a “model-theoretic” approach to fictionalism about scientific doctrine.[48] One sketches — with more or less rigor — how all “worlds” can be described as having “concrete cores,”[49] where, for example, the concrete core of a world is that part of the ontology of the world that’s acceptably non-Platonistic (e.g., people and cats and moons, but not numbers, etc.).[50] One then claims that statements like

(3) The number of Martian moons = 2,
(4) There are exactly two Martian moons,

[48] Balaguer 1998, Rosen 2001, van Fraassen 1980.

[49] Rosen 2001, 75. What follows replicates his examples and suggestions.

[50] “Observational cores” — the observable parts of worlds — may be defined similarly. One can be as austere or as profligate (ontologically speaking) as one wishes: There are only tiny particles with such and such properties in the concrete cores; or, there are only macro-objects in the cores; or, there is only “gunk” there; etc.

say "the same thing" about the concrete core of our world, and one calls this same said thing the shared "nominalistic content" of (3) and (4). One then presumes, perhaps on sheer metaphysical grounds, that *every* sentence of science similarly has its own "nominalistic content" — that it says something about the concrete core of our world, *regardless of whether or not one has any access to the properties of such content*.[51] One furthermore claims that one is *truth-committed* — not to the consequences of any scientific theory E — but only to the "nominalistic contents" of such consequences.[52]

Notice that on such views one does *not* responsibly proxy-dispense the sentences one continues to assertorically use. Instead, one *presumes on* the existence of something else ("nominalistic contents") that one has truth-committed oneself to: one hitches one's truth-commitments to a class of *somethings* one otherwise has no access to. Even the most rigorous ("model-theoretic") proof of the existence of such "content" needn't show — and often can't show — that such nominalistic content corresponds to the individual sentences that one must continue to assertorically use.

In any case, the sheer positing of nominalistic contents corresponding to the sentences of a scientific theory E that one must continue to assertorically use doesn't indicate how anyone can (even in principle) recognize which implications of E can be taken seriously and which can't. It's not enough to have a list of items one is supposedly ontologically free of (via this positing of nominalistic content). The nominalistic contents of the sentences of E must satisfy the informativeness condition: A sheer existence proof of a

[51] That every world has a "concrete core" needn't imply that *each* sentence has its *own* nominalistic content. Indeed, the fictionalist presumes not only on the metaphysical claim that the world has a "concrete core," but on there "being" a nominalistic language that describes such. There is, of course, no such language since no one has invented it or uses it. Indeed, there *couldn't be* such a language as the discussion of the representation problem in section 9 indicates: any such nominalistic language would be massively intractable because of the sheer number and nature of its *predicate-terms*. So although a fictionalist could feel fairly confident of the existence of "concrete cores" — because of his metaphysical assumptions, it's hard to see why he should be confident of the existence of a corresponding nominalist language. A second threat to this approach is that many scientific statements — depending in part on what's posited to be in the concrete core — may turn out to have no nominalistic content at all. That would be fatal to the approach because scientific statements that are representationally used *must have* nominalistic contents. Good candidates, by the way, for scientific statements that are likely to have no nominalistic content are scientific laws — especially in physics — that quantify over abstracta. Such are not merely milked by scientists for their valuable consequences *but are also representationally used by scientists to describe what the world is like*.

[52] Balaguer (1998, 131) asserts: "It is coherent and sensible to maintain that the nominalistic content of empirical science is true and the platonistic content of empirical science is fictional."

parallel discourse, or even worse, a proof of the existence of "content-like" items such as sets of possible models, doesn't satisfy this.

The root mistake of these fictional approaches (and the earlier ones discussed) is a systematic misreading of the QP. The focus should be on the *assertoric use* of scientific theories — that in so *assertorically using them* one commits oneself to their truth. This is an issue of "practicality."[53] What's often substituted (by fictionalists pursuing nominalism) for the scientist's assertoric use of a scientific theory, however, is the more detached (philosophical?) recognition *that* some theory or other is true. After all, if one mistakenly thinks that the contemplation of the truth of a scientific theory is *all* that the *scientific* use of a theory comes to, then what's wrong with substituting for the contemplation of the truth of one theory the contemplation of the truth of some other theory with the same virtues and none of the (ontological) vices of the original? But this, I have argued, misses the force of the strongest version of the QP: its concern with the indispensability of the *assertoric use* of theories. One should conclude, therefore, that these approaches have changed the meaning of "dispensable," and have shown only that mathematical content is "dispensable" in the *contemplative* sense, but not in the *assertoric-use* sense.[54]

12. *Concluding remarks*

If we start with the assumption that many of our empirical statements are meant to be taken as literally true or false, and we couple that with the assumption that validity-preserving deduction is to take true premises to true conclusions, there are obstacles to coupling these assumptions additionally to the truth-aptlessness of mathematical statements. There are no methods that successfully proxy-target a class of mathematical-entity-free statements that we can regard as truth-apt. The representation problem shows that very few of our empirical claims can be seen as free of reference to mathematical entities. Nothing, presumably, that alludes to the spatial or temporal properties of empirical objects, for example, can be seen as free of reference to mathematical entities. In fact, nothing that involves measurable properties *of*

[53] Burgess and Rosen 1997, 213, give the standard (mis)reading of the indispensability argument: "[The QP] makes the major concession to nominalism that it is only indispensability in principle (not in practice) . . . that counts."

[54] Rosen (2001, 76–77), by the way, invokes the reasonableness of a community's *belief* in the nominalistic content of their sentences. *Pace* this, recall the discussion of public knowledge in section 5. Publicly-held belief requires manifestation *apart from* the beliefs expressed by the individuals in that public.

any kind can be handled in a mathematical-entity-free language: only crude qualitative attributions survive.

The obstacles to fictionalist views of mathematics have been misdiagnosed. One reason is that the challenge to be faced is the *assertoric use* of statements in deductions and in representations.[55] This is a practical matter that requires a solution sensitive to practicalities. If we are to separate truths from falsehoods, we must have methods that indicate what truths we are to be taken as *really* asserting: the targets of our proxy-statements must be determinate enough for us to be able to tell what it is that we are saying that's true and what it is that we're saying that's false. But there are no methods (that work!) that can separate scientific statements that refer to mathematical entities from statements that don't: that's been the burden of this paper.

I return, therefore, to an option briefly raised at the end of section 2: *Why couldn't a responsible fictionalist accept that non-literal utterances pervade our linguistic practices? Why couldn't she claim that (most) empirical content can't be expressed without the help of metaphorical uses of mathematical vocabulary*?

The crucial fictionalist move is that non-literal utterances aren't to be regarded as truth-apt.[56] But as soon as the appearance of mathematical vocabulary in statements is seen as making those statements not truth-apt, because of the representation problem (and because of needed deductions from mathematics-rich statements), all empirical statements must (pretty much) be regarded as not truth-apt. It's hard to wrap one's mind around the suggestion that nearly all of one's discourse isn't truth-apt, but let's try. The main issue is that a sophisticated distinction still has to be drawn by the global fictionalist between pseudo-truth and pseudo-falsity because such a distinction, as we've seen, is operative in the sciences. There's all the difference, a scientist will point out, between a theory getting Jupiter's effects on the Sun right, and its getting the intrinsic properties of Jupiter wrong. More dramatically, as we've seen, the truth-predicate *itself* is an indispensable element of scientific discourse (because of its role in blind truth-ascription). So the global fictionalist must make a distinction between *external* "truth" and "falsity," which are literal but largely useless, and *scientific (internal)* "truth"

[55] It's striking that van Fraassen (1980, 10) when contrasting the scientific "realist" with the "anti-realist" writes: "According to the realist, when someone proposes a theory, he is asserting it to be true. But according to the anti-realist, the proposer does not assert the theory; *he displays it*, ..." (italics *his*). Neither description, of course, is accurate: the (real) scientist *assertorically uses* theories. Semantic descent, however, is available for the realist so that the scientist can be taken as assertorically using scientific theories that the realist regards as true; nothing equivalent is available for the anti-realist who only "displays" a false — but empirically adequate — scientific theory.

[56] See Yablo 1998, 2001.

and "falsity" which are indispensable, but metaphorical. This is surely undesirable.

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