Three Grades of Instrumentalism

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Abstract:

I defend a view, mathematical instrumentalism, on which quantification over mathematical objects in scientific theory does not entail commitment to their existence. I present a puzzle about the status of our beliefs in mathematical objects, and show how instrumentalism resolves it. MI undermines both the indispensability argument and the primary response to it which demands reformulations of scientific theory to avoid mathematical commitments.

§1. Posits and Homogeny

You look skyward on a clear night and say, "There are stars." We conclude that you believe that there are stars. You study atomic theory and say, "There are atoms." We conclude that you believe that atoms exist. You study quantum physics and say, "There are Hilbert spaces." We conclude that you believe that there are abstract structures called Hilbert spaces.[1] These three inferences are supported by Quine's dictum that to be is to be the value of a variable, which connects ontic commitment with quantification.

In contrast, I wish to urge that the third inference, unlike the first two, is invalid. I do not deny that there are Hilbert spaces. Nor do I deny that quantum mechanics refers to Hilbert spaces. My claim, in this paper, is merely that the inference from the sentences of quantum mechanics to the existence of mathematical objects is unjustified.

The third inference, unlike the first two, involves a special kind of leap. Rather than inferring stars from seeing the stars, or inferring atoms from a good theory of atoms, the third inference concludes the existence of mathematical objects from a theory designed to explain or describe non-mathematical objects. Such inferences are instances of Quine's indispensability argument. Quine's argument proceeds, I take it, as follows.[2]

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[1] Given common assumptions about mathematical ontology (i.e. that all mathematical objects are constructible out of, and thus reducible to, more fundamental objects) we might re-interpret your commitment to Hilbert spaces as a commitment to sets, or to categories. The point is that a commitment to mathematical objects may arise from considerations of physical theory.

[2] Quine nowhere presents a detailed indispensability argument, though he alludes to one in many places. Among them: Quines 1939, 1948, 1951, 1955, 1958, 1960, 1978, and 1986.

(QIA) QIA.1: We should believe the (single, holistic) theory which best accounts for our sense experience.
QIA.2: If we believe a theory, we must believe in its ontic commitments.
QIA.3: The ontic commitments of any theory are the objects over which that theory first-order quantifies.
QIA.4: The theory which best accounts for our sense experience first-order quantifies over mathematical objects.
QIA.C: We should believe that mathematical objects exist.

The theory at hand which purportedly yields sets is designed to study sub-atomic phenomena, the objects involved in which are taken to be constituents of ordinary objects, like trees. Proponents of QIA do not argue for a Pythagorean view, that trees are made out of sets.[3] Rather, their argument is that we need sets in order to regiment the best theory of trees.

The received opinion, I think, is that driving a wedge between the first two cases and the third is unsupportable. We might dismiss some of the more radical aspect of Quine's work, like ontological relativity, but no one could seriously question the link between quantification and existence.[4] Surely, Quine's decisive criticisms in 'On What There Is' on the Meinongian (or Russellian) Wyman settled the issue.

I have no intention of revisiting Wyman's attempt to distinguish various senses of 'existence'. Nor do I wish to introduce a new kind of existence. Rather, I wish only to distinguish between our ontic commitments and the claims of scientific theory. That is, I want to

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[3] Actually, Quine 1976 considers a Pythagorean physical theory, containing physical ideology, but using only space-time points, represented by sets (ordered 4-tuples), as objects. I take this amusing argument as a reductio of QIA.

[4] Proponents of the received view, in addition to straight Quineans, include those philosophers like James Higginbotham, Peter Ludlow, Jason Stanley, et al., who emphasize the logical form of sentences of natural language in order to make inroads into epistemological and metaphysical problems. See Preyer and Peter 2002 for a representative selection of papers.

deny the conjunction of QIA.2 and QIA.3. More than half a century of adapting to Quine's method for determining ontic commitment has not erased the oddity of the third inference, compared to the first two. The inference to the existence of mathematical objects on the basis of their use in physical science is a different kind of inference, one we should not countenance.

Applying QIA to discover our most sincere commitments, we find existence claims unified in the first-order quantifiers of a monolithic, regimented theory that best accounts for our sense experience. All our commitments arise together as the posits of our best theory, and all are to be taken equally seriously. Evidence for posits may differ. We have something like direct apprehension of trees. Sets are purely theoretical posits. But, according to confirmation holism, all evidence is evidence for the whole theory, and all the posits come out together. Call this aspect of Quine's method 'homogeny'.

We do not make all our posits with equal enthusiasm, though. Surely we are, in some sense, more committed to trees than to, say, quarks, belief in which arose only tentatively from recent scientific theory. Our commitment to mathematical objects is the subject of serious debate, even within Quine's framework.[5]

The Quinean accounts for differences in enthusiasm casually, and meta-theoretically. We know, on reflection, that our best theory is ever-changing. Some elements are central to the web of belief and so our commitments to them are less liable to shift. Other elements are peripheral, and more apt to be abandoned. But if the metaphysics police knock at our door and demand our list of commitments, we hand them one list, with a single, homogeneous column.

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[5] I refer to Field 1980 and similar responses. See Burgess and Rosen 1997 for an elegant collection of such responses.

§2: A Puzzle About Mathematical Commitments

Quine's web of belief metaphor obscures the status of our beliefs in mathematical objects. On the one hand, sentences which refer to mathematical objects are ubiquitous in our best theory and our beliefs in them appear central. The notion that mathematical elements are core beliefs transcends Quine's position. Our commitments to broadly applied elements, like mathematical objects, should be most enthusiastic. The centrality of mathematical beliefs is buttressed by the difficulty of removing the posits from our best theory. Quine's confirmation holism, at least its logical point about our ability to retain any claim which conflicts with others we hold by re-organizing the web, and abandoning some of these other claims, is undeniable. Mathematical beliefs are central because excising them entails profound reshuffling of other claims.

In contrast, mathematical objects are among the most highly contested elements of our list of commitments, for even the most ardent Quinean. Whether one concludes that dispensabilist projects like Field 1980 are likely to be successful or not, one must admit that they call mathematical beliefs into question. Whether the Quinean is committed to mathematical objects depends on the availability of nominalistically acceptable versions of our most complete theory. Since it is an open question whether the dispensabilist can excise mathematics from empirical science, our mathematical beliefs should be peripheral to our web of belief.

So, the puzzle is whether our mathematical beliefs are central or peripheral. Taking them as peripheral, as I believe we should, better reflects our actual attitudes. Taking beliefs in mathematical objects as peripheral is also consistent with the oddity of the inference from quantum physics to the existence of sets. The arguments for centrality trace back to Quine's method for determining our ontic commitments. If we reject Quine's homogeny, we can differentiate among our posits, taking some as merely instrumental. I consider three grades of instrumentalism, rejecting the first, sensory instrumentalism, and accepting the second, mathematical instrumentalism. I also find a third version, reflective instrumentalism, defensible.

§3: Sensory Instrumentalism and the Double-Talk Criticism

A crude way to reject Quine's homogeny is to limit one's commitments to those objects we can directly perceive. Call this 'sensory instrumentalism', or SI. In the early twentieth century, proponents of versions of SI included Duhem, Vaihinger, and Mach. Mach, for example, denied the existence of atoms, at least for a while, despite affirming the utility of atomic theory. More recently, van Fraassen has denied that the utility of our scientific theories suffices for establishing knowledge about theoretical posits like atoms, and Cartwright is skeptical about the truth of sentences which refer to objects we do not perceive.[6]

The sensory instrumentalist owes us an acceptable notion of direct perception. I will not fuss about such an account. We can accept a rough distinction on the basis of paradigms. Dubious commitments are exemplified by electrons; legitimate commitments are exemplified by trees. SI affirms the existence of trees, and denies, or remains agnostic about, electrons.

Quine's method for determining ontic commitment was a response to SI. In the early twentieth century, the increasing role within scientific theory of objects which are not directly perceived had demanded an account, especially from empiricists, who had two options. First,

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[6] See van Fraassen 1980 and Cartwright 1983.

they could withhold commitments to such objects, maintaining belief in only objects which could be sensed. This is SI. Otherwise, they could account for our knowledge of these objects by showing how the objects we do perceive are constructed out of, or reducible to, the ones we do not perceive. All knowledge could still plausibly be grounded in perception, though some perceptions would be indirect, using microscopes, say.

The positivist sense-data reductionism of Carnap's *Aufbau* takes the latter approach by attempting to show how all talk of physical objects, including those observed using microscopes, was translatable into a sensory language. Quine favored reductionism over SI, in principle. His method for determining ontic commitments was in part a reaction to the impossibility of a satisfactory reductionist account.

Quine's argument against SI is that affirming the existence of some elements of one's theory while denying others is double-talk. If our best theory requires electrons for its bound variables, then we are committed to electrons. Quine worries about such double-talk throughout his work. His response to Carnap's internal/ external distinction, for example, relies on the double-talk criticism; once one has accepted mathematical objects as an internal matter, one can not merely dismiss these commitments as the arbitrary, conventional adoption of mathematical language. For another example, Quine's response to the Meinongian Wyman, who presents two species of existence, is a variation on the double-talk criticism. We must distinguish between the meaningfulness of 'Pegasus' and its reference in order to avoid admitting that Pegasus subsists while at the same time denying that it exists.

Worries about double-talk bothered Quine's friends and foes, as well. Putnam, following

Quine, makes the double-talk criticism explicitly.[7] "It is silly to agree that a reason for believing that p warrants accepting p in all scientific circumstances, and then to add 'but even so it is not *good enough*'" (Putnam 1971: 356).

Field, accepting QIA.1-3, but rejecting QIA.4, applies the double-talk criticism directly to mathematics. "If one *just* advocates fictionalism about a portion of mathematics, without showing how that part of mathematics is dispensable in applications, then one is engaging in intellectual doublethink..." (Field 1980: 2).

There are various good reasons to reject SI which are independent of the double-talk criticism. We learn of scientific posits which are not directly perceived by using instruments as reliable as those of our senses, if not more so.[8] Also, atomic theory simplified ontology, since diverse physical objects could be all seen as constructed out of the same kinds of atoms. Further, it unified our explanations of sensory experience. Put these criticisms of SI aside.

The real problem for SI's refusal to accept particles we can not directly perceive is that its fundamental distinction between accepted objects and rejected ones is capricious. There is no reason to believe that the world is cut at human sensory joints. By relying on our arbitrary abilities to perceive objects as the source of the distinction between real commitments and merely instrumental posits, the sensory instrumentalist differentiates illegitimately. SI makes our our commitments relative to our sensory apparatus when they should be independent of us.

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[7] Putnam accepted QIA in his earlier work, but in Putnam 1971 he also produced a version of the indispensability argument which does not rely on Quine's holism or method for determining ontic commitments.

[8] Azzouni (1997a) and Azzouni (2004) defend this assertion.

This complaint suffices to reject SI, and demonstrates a lesson. The problem with SI was not its instrumentalism, but its arbitrary segregation of real commitments from instrumental ones. Suppose we had a principled distinction between those elements of a theory which we really think exist and those which we see as merely instrumental posits. In such a case, differentiating among the posits need not seem like double-talk.

§4: Mathematical Instrumentalism and the Eleatic Principle

Call the denial that we can infer commitments to mathematical objects from scientific theory, even if they are indispensable to that theory, mathematical instrumentalism, or MI. If we accept MI, we block the inference from quantum mechanics to sets.[9] Unfortunately for MI, the inference follows as a matter of logic. Quantum mechanics, and every other interesting physical theory, contains mathematical axioms. Thus, the proponent of MI must abandon some fundamental premises.

One route for the proponent of MI is to deny that we can read off our ontic commitments from the regimented sentences of scientific theory. '$\exists x \forall y \sim y \in x$' would still mean that there is an empty set and $\forall x \forall y \exists z \forall w[(w \in x \vee w \in y) \Leftrightarrow w \in z]$ would still mean that there is a union set for any two sets. But in using quantum mechanics, we would not commit ourselves to all of the commitments of the theory.

MI, like SI, opposes Quine's method for determining ontic commitments, and thus reduces the importance of projects which attempt to show how to reformulate scientific theories

[9] MI does not entail the falsity of mathematical existence claims. It merely blocks the inference to them from their appearance in empirical scientific theory.

in order to eliminate quantification over mathematical objects, like Field 1980.[10] It is generally accepted that the most promising reformulations are those which show how to construct a theory which does not quantify over mathematical objects, but which generates provably equivalent conclusions about the physical world. Burgess and Rosen call these projects Tarskian reductions.[11] There is disagreement about whether Tarskian reductions will be available for all current and future scientific theories, but it is likely that new nominalizing strategies can be found. As with SI, MI presents only a claim that we should distinguish quantification from commitment, rather than a reformulation which eliminates repugnant quantifications. Instead of removing mathematical objects, the instrumentalist denies that their presence entails that we should believe that they exist.

Actually, the instrumentalist can easily eliminate quantification over mathematical objects. Consider all the conclusions of standard science. Replace each of these sentences with nominalistically acceptable formulas, taking each new formula as an axiom, and omit the mathematical axioms. This process will produce an ugly and unwieldy but adequate and purely nominalistic theory. One can clean up the theory a bit with a Craigian reaxiomatization.[12] Craig's Theorem insures that though the new theory has infinitely many primitives and axioms,

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[10] Field attempts to eliminate mathematical axioms, replacing them with ideology governing substantivalist space-time. Other dispensabilists, like Geoffrey Hellman and Charles Chihara, re-interpret mathematical claims as modal claims.

[11] See Burgess and Rosen 1997: §1.B.2.b.

[12] The Craigian method originally interested philosophers looking to eliminate theoretical terms from a theory and leave only observational vocabulary. The theoretical/observational distinction is just another way to voice the capricious SI. The use of the method I describe here should not be confused with its use to support SI.

and so the set of axioms may not be recursive, there is another axiomatization which yields the same theorems and is effectively decidable. We can determine, for every formula of the language of the theory, whether it is an axiom.

A Tarskian reduction eliminates all mathematical formulas and primitives by translating them into acceptable vocabulary. A Craigian elimination provides all the consequences of the original theory without quantification over mathematical entities, but it does not reduce diverse experiences to a few, simple axioms. Still, the resulting theory yields all nominalist consequences of the original theory. The most powerful criticism against it is its unattractiveness. "If no attractiveness requirement is imposed, nominalization is trivial... Obviously, such ways of obtaining nominalistic theories are of no interest." (Field 1980: 41)[13]

Attractiveness will not help us choose between the Tarskian and the Craigian theories. The Tarskian reformulation is unattractive, too. The only really attractive version of standard scientific theory is the standard version itself. Even the most attractive nominalist theory is practically useless and not perspicuous, especially when regimented into canonical language in order to reveal its commitments, as Quine demanded.

I leave the option to provide a Craigian axiomatization to the instrumentalist. My central point is that the instrumentalist does not need it, that reformulations are not necessary to avoid mathematical commitments.

While SI arbitrarily segregated legitimate and merely instrumental posits, MI has a

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[13] Similarly on p 8. Field discusses the Craigian theory specifically, at Field 1985: 129. Burgess and Rosen agree that Craigian eliminations do not meet community standards, at Burgess and Rosen 1997: 94. But, they do not defend those standards.

principled way of drawing the line, deriving directly from the abstractness of mathematical objects. We are causally isolated from them. Mark Balaguer calls the fact that we are unable to interact with mathematical objects the "principle of causal isolation," or PCI. He uses PCI to reject the indispensability argument, in essence defending MI.

For the Quinean, since our commitments to mathematical objects are revealed through the same monolithic theory as our commitments to ordinary objects, we can not appropriately distinguish mathematical objects from empirical ones. Since all posits of our best theory arise homogeneously, and our epistemology for them is the same, we have no basis for claiming that mathematical objects are any different from physical objects. Consequently, the indispensabilist has no foundation for an abstract/concrete distinction. Indeed, the terms 'abstract' and 'concrete' become rather meaningless, vulgar terms in which the learned may only lightly indulge. "In the case of abstract entities, certain protests against Platonism become irrelevant. There is no mysterious 'realm' of, say, sets in the sense that they need to have anything akin to location, and our knowledge of them is not based on any mysterious kind of 'seeing' into such a realm. This 'demythologizing' of the existence of abstract entities is one of Quine's important contributions to philosophy..." (Parsons 1986: 377-78)

The indispensabilist is thus forced to reject the commonsensical PCI. "The Quine-Putnam argument should be construed as an argument not for platonism or the truth of mathematics but, rather, for the *falsity of PCI*." (Balaguer 1998: 110)

PCI is an eleatic principle. The eleatic argues that there can be no empirical evidence for the existence of mathematical objects. Besides Balaguer, eleatics like Jody Azzouni and David

Armstrong reject the existence of mathematical objects despite their presence in scientific theory. Armstrong asserts that science can accept objects that help to explain the behavior of ordinary objects, but then denies that mathematical objects can do this, since they are merely heuristic devices which lack causal efficacy. "If any entities outside the [spatio-temporal] system are postulated, but have no effect on the system, there is no compelling reason to postulate them" (Armstrong 1980: 154).

Armstrong, Azzouni, and Balaguer agree that we can distinguish between mathematical and non-mathematical content. Otherwise, we would not know which references in scientific theory are to be taken literally, and which are instrumental. In explaining physical phenomena, they say, we only commit to the non-mathematical, physical content of the explanation, even if it refers to mathematical objects along the way. We know going into our theoretic construction the kinds of things to which we are committed. Our explanation of why my hand does not pass through a wall may refer to mathematical objects, but the subjects of the explanation are hands and walls, and not mathematical objects.

SI, I argued, failed because of its arbitrary segregation of real and instrumental commitments. MI avoids the double-talk criticism because it relies on an eleatic principle. Still, that principle has its detractors. In the next section, I show how the challenge presented by one recent critic of the eleatic fails.

§5: The Eleatic and the Indispensabilist

Mark Colyvan, in Colyvan 2001, argues that attempts to refine eleatic principles suffer serious difficulties. While Colyvan may be right that these principles are difficult to specify precisely, important distinctions often elude specification. Still, Colyvan defends the indispensability argument, and Quine's method, against the eleatic instrumentalist, by arguing that we are committed by physical theory to non-mathematical, non-causal entities. He argues that non-causal, non-mathematical entities play indispensable explanatory roles.[14] If we admit non-causal, non-mathematical objects, then the eleatic principle fails, independently of what we think about mathematical objects. The door is wide open to admit mathematical objects, as well. And, Colyvan argues, there are good reasons to admit non-causal, non-mathematical objects. Thus, according to Colyvan, the principled distinction which supports MI is wrong and MI is rendered untenable. I show that Colyvan's allegation is false.

Colyvan presents three examples. The first concerns the bending of light. Colyvan argues that the best explanation of light bending around large objects is mathematical. Light moves along space-time geodesics. The large mass covaries with the curvature in space-time, but it is not clear, on a causal picture, which causes which. "Simple covariance doesn't guarantee that one of the factors causes the other" (Colyvan 2001: 48). Furthermore, according to the non-Minkowski vacuum solutions to the Einstein equation, there are empty, yet curved space-times. On the causal picture, these curvatures are uncaused, and thus unexplained.

[14] Indispensability arguments must present some goal for which commitment is indispensable. For Quine, this goal was the construction of scientific theory. Colyvan focuses on scientific explanation, as Armstrong did. But, the examples play the same role in both domains.

Colyvan's second example concerns the existence of two antipodes in the Earth's atmosphere with exactly the same pressure and temperature at the same time. The causal explanation, which refers to atmospheric conditions, does not suffice. The existence of antipodes is guaranteed by a topological theorem. The proof of this theorem provides the remainder of the explanation.

Lastly, Colyvan asks us to consider the Fitzgerald-Lorentz contraction. A body in motion contracts, relative to an inertial reference frame, in the direction of motion. Minkowski's explanation of this contraction relies on equations in four dimensions, representing the space-time manifold. Colyvan calls this, "A purely geometric explanation of the contraction, featuring such non-causal entities as the Minkowski metric and other geometric properties of Minkowski space" (Colyvan 2001: 51).

To evaluate Colyvan's examples, recall that he must show that non-causal entities other than mathematical objects play an explanatory role. For, his argument was that since we need non-causal non-mathematical elements, the eleatic principle which supported MI is shown false independently of the contentious mathematical case. The geodesics example, though, either begs the question, or is insufficient. If we take the geodesics as pure mathematical objects, Colyvan begs the question by presenting a geometric object as explanatory. If we take geodesics to be physical entities, then we should see them as properties of space-time, and we naturally see masses as causing curvatures.

Colyvan rejects the causal interpretation. "[A]ny account that permits mass to *cause* the curvature of space-time is unintuitive to say the least" (Colyvan 2001: 48). The unintuitiveness, for Colyvan, may arise from thinking of space-time as abstract. If we think of it substantively, the causal explanation is not problematic. The case of an empty, yet curved space-time only reinforces that we do not need a non-causal explanation. The curvature of space-time is not an event which can be explained in terms of antecedent conditions, say, but a property of an object (or collection of objects).

In the case of the antipodes, we must again make a pure/applied distinction regarding the topological theorem. The pure mathematical theorem does not guarantee that these antipodes have the same temperature and pressure. We need bridge principles which apply this theorem to the Earth and its weather patterns. Once we add these bridge principles, the proof which guarantees the antipodes should be regarded as a causal explanation. For, the bridge principles will refer to causal structures within the Earth's atmosphere, and it is these which explain the existence of the antipodes. This explanation will, as Colyvan notes, refer to non-causal entities such as continuous functions and spheres, but these are mathematical objects. We are looking for non-mathematical, yet non-causal, elements.

A similar response applies to the contraction example. The equations which explain the contraction are supposed to make indispensable reference to non-causal entities. But the equations apply to the physical world, and thus explain the contraction of a physical body in motion, only if coupled with bridge principles which explain their applicability. The physical objects provide the explanation.

In no case has Colyvan shown that a non-causal entity, other than a mathematical object, plays an essential role in scientific explanation. The eleatic, ex hypothesi, need not show that mathematical entities can be removed from explanations in the physical world. Thus, Colyvan provides no reason to abandon the eleatic principle, or PCI, or to undermine MI.

MI renders dispensabilist reformulations moot. We need not see ourselves as committed to mathematical objects on the basis of scientific theory. Still, whether we are able to construct empirical theory without referring to mathematical objects or not, it remains an open question whether we should accept mathematical axioms as well as those of empirical science. To put the matter crudely, the proponent of MI may be either a realist or a nominalist about mathematical objects. Denying the existence of mathematical objects on the basis of MI begs the question of the legitimacy of mathematics in its own right.

§6: Reflective Instrumentalism

The double-talk criticism fails against an instrumentalism, like MI, which relies on a principled distinction between real and instrumental posits. MI thus provides a reason to reject Quine's method for determining ontic commitment, and his indispensability argument. Other versions of instrumentalism distinguish between legitimate and instrumental physical posits. There are various ways one might make such a distinction. A 'reflective instrumentalism', or RI, may distinguish among physical posits on the basis of activity within a causal nexus, or spatio-temporal location. If any RI is acceptable, MI is likely to be acceptable as well, since RI also entails the failure of Quine's method for determining ontic commitment.

Azzouni defends RI by arguing that Quine's method commits us to objects we do not really believe exist. He describes instances in which existential quantifications within science proper should be seen as merely instrumental. The users of scientific theories are not committed to centers of mass, quasi-particles, and mathematical objects.

Azzouni 1997b considers a system of two masses connected by a spring, moving in a gravitational field. The separate motions of the masses are too complicated to calculate, but if we consider the system in terms of its center of mass, which is not located on the springs, and its reduced mass, we can describe the system.

Quasi-particles are posits used to replace one intractable many-body problem in condensed matter physics with many one-body problems, using Fermi Liquid theory. Scientists introduce quasi-particles aware that a fictionalization is involved. "[I]t's not that physicists are failing to ask whether or not they're committed to the entities introduced in this way. *They already take themselves* not to be so committed. That's why, for example, such 'particles' are called quasi-particles" (Azzouni 1997b: 195).

On RI, we cleave ontic commitment from the existential quantifier, but we can maintain the quantifier's inferential role. If we want to clarify our commitments within formal scientific theory, Azzouni suggests minting a predicate to be read as 'is physically real'.[15] The principle underlying ascriptions of the predicate is that we have thick epistemic access to anything physically real.

Penelope Maddy cites, to a similar end, skepticism surrounding atoms in the early stages of atomic theory.[16] Before the experiments which yielded much more direct evidence of the

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[15] See Azzouni 2004: 383.

[16] Maddy's concern is to withhold truth to sentences of the theory, while Azzouni's concern is to avoid commitments to entities. For the purposes of this paper, the distinction is

existence of atoms, scientists hedged their bets about these elements, even when they accepted chemical theory. Atomic theory was accepted, it expressed ontic commitment to atoms, but scientists did not really believe that the atoms existed.

> [T]hough atomic theory was well-confirmed by almost any philosopher's standard as early as 1860, some scientists remained skeptical until the turn of the century - when certain ingenious experiments provided so-called "direct verification" - and even the supporters of atoms felt this early skepticism to be scientifically justified. This is not to say that the skeptics necessarily recommended the removal of atoms from, say, chemical theory; they did however, hold that only the directly verifiable consequences of atomic theory should be believed, whatever the explanatory power or the fruitfulness or the systemic advantages of thinking in terms of atoms. In other words, the confirmation provided by experimental success extended only so far into the atomic-based chemical theory *T*, not to the point of confirming its statements about the existence of atoms. (Maddy 1992: 280-1)

Maddy cites other examples of false assumptions in science: taking water waves to be infinitely deep, and treating matter as continuous in fluid dynamics. According to Maddy, it is accepted scientific practice to separate our actual commitments from those made by our best theories. "If we remain true to our naturalistic principles, we must allow a distinction to be drawn between 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." (Maddy 1992: 281)

Other versions of RI are possible. One could make a principled distinction between real and instrumental commitments based on space-time properties, or on the commitments that scientists see their theories as making. In any of these cases, we have principles which may deflect the double-talk criticism.

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irrelevant.

§7: Resolving the Puzzle

The puzzle in §2 was whether we should take mathematical statements to be central to the web of belief, or peripheral. The puzzle is generated by Quine's method for determining ontic commitments, in contrast to instrumentalism, which can distinguish between the posits we really believe exist and those which we take to be mere heuristics. Quine's double-talk criticism of instrumentalism applies effectively to SI, since the difference between what we can and cannot sense is arbitrary. I argued that a principled distinction between the real commitments of a theory and the instrumental ones deflects the double-talk criticism. Since MI is a viable alternative to Quine's method, ubiquity and utility are no arguments for centrality. Even if we use mathematics always and everywhere, we can maintain a fictionalist attitude toward it, as far as scientific theory is concerned. We should take our beliefs in mathematical objects to be peripheral.

Taking our beliefs in mathematical objects to be peripheral not only better reflects our actual attitudes. It also accounts for the debates between nominalists and realists. Instrumentalism trumps dispensabilism; there is no need to reformulate science to deny that it commits us to mathematical objects. Still, the nominalist can appeal to the brute fact of such reformulations. The mathematical realist can appeal to the brute fact of the obviousness of mathematical truth, independently of science. These arguments are at the periphery of the web of belief, as instrumentalism predicts.

Here is one last consideration in favor of MI, on the basis of work which seems to deny it. Field argues that all we want out of mathematics is goodness, in the guise of conservativeness, and not truth. Mathematics is conservative if adding it to a nominalistic theory does not produce further nominalist consequences. Field wants to show that the indispensabilist is a nominalist who has his facts wrong about the requirements of scientific theory. But Field's arguments actually show that the nominalist is really an instrumentalist in disguise. For, if all we want is mathematical goodness, and we can get that on the basis of conservativeness, then not only do we not have an argument from applications of mathematics in science to mathematical truth, we do not have an argument from dispensability of mathematics to mathematical falsity. We have an argument for the independence of mathematical truth from considerations of science. And this is just MI.[17]

Beliefs in the causal isolation of mathematical objects and their independence from scientific theory underlie MI, and make the indispensability argument tempting for one who believes that mathematical objects exist, but who hesitates due to worries about their epistemology. It looks like we are getting something, abstract objects, for nothing, the strictly empirical theory to which we are independently committed. But caveat emptor. The indispensability argument does not really yield mathematical objects. MI makes the indispensability argument powerless to yield mathematical objects. Dispensabilist reformulations of mathematized science are moot. The technical work at the core of the reformulations is perhaps interesting, but not as the foundation for mathematical nominalism.[18]

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[17] Actually, Field considers a version of MI in case his dispensabilist reformulation were found untenable. See Field 1989: 20.

[18] I am indebted to Mark McEvoy, Jody Azzouni, Michael Levin, Bryan Pickel and the audience at the 2006 UT Austin graduate conference *Thoughts Words Objects*, for valuable comments.

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