Application of Mathematics and Underdetermination[1]

Otávio Bueno
Department of Philosophy
California State University, Fresno
Fresno, CA 93740−8024
e−mail: otavio_bueno@csufresno.edu

ABSTRACT

Recent work in the philosophy of mathematics has focused on the indispensability argument. According to this argument, we should believe in the existence of mathematical entities, since they are indispensable to our best scientific theories. Empiricists have often tried to resist the commitment to unobservable entities in science by using the underdetermination argument. Given that scientific theories are typically underdetermined by the data, we are not forced to believe in the unobservable entities that such theories posit. In this paper, I extend the underdetermination argument to the application of mathematics, arguing that, similarly to its counterpart in science, the argument provides an empiricist alternative to avoid ontological commitment to mathematical objects. In this way, a strategy is presented to resist the conclusion advanced by the indispensability theorist.

1. INTRODUCTION

In recent philosophy of mathematics, a great deal of attention has been given to the application of mathematics. And a considerable part of this has focused on the indispensability argument. The argument, which was first suggested by Quine [1960] and, more thoroughly, Putnam [1971], can be put in the following explicit form: (P1) We ought to be ontologically committed to all and only the entities that are indispensable to our best scientific theories. (P2) Mathematical entities are indispensable to our best scientific theories. Therefore, (C) we ought to be ontologically committed to mathematical entities.[2]

In this form, the argument has two crucial assumptions: naturalism and the doctrine of confirmational holism. According to confirmational holism, scientific theories are confirmed or disconfirmed not as independent, separate hypotheses, but as wholes (Quine [1961]). In other words, the empirical data that confirm a theory confirm it as a whole, including any mathematics that is used in the formulation of the theory. After all, the evidence used to support our belief in the empirical part of the theory is the same as that used to support its mathematical part (Putnam [1971]).[3]

The second assumption of the argument steps in at this point. According to naturalism, in order to determine what exists, we should look to our best scientific theories. Scientific methodology is taken to provide a reliable strategy to generate theories about the world. In this

1 My thanks go to Steven French, Sarah Kattau, Bas van Fraassen, Terry Winant and Ed Zalta for helpful comments and discussions.

2 The argument is presented in this way in Colyvan [1998]. For further details, see e.g. Colyvan's paper, Maddy [1997], and Peressini [1997].

3 We speak here of the empirical part and the mathematical part of a scientific theory. But for the indispensability theorist that is only a way of speaking. It is typically extremely hard to separate one part from the other. And the fact that we can't make this separation (if it is a fact) is further grist to the indispensability theorist's mill.

way, naturalism gives us some ground to believe in the entities postulated by our best scientific theories (and only in such entities).

The two assumptions of the indispensability argument (confirmational holism and naturalism) are typically tied to another famous argument: the argument of underdetermination (in Quine's version of the latter, naturalism becomes crucial). Because we cannot separate and evaluate isolated hypotheses, we have to consider them as "whole blocks". But in this case, as Poincaré and Quine have pointed out a long time ago, provided that we are willing to make changes elsewhere in the overall system, we can always reconcile different (and even incompatible) hypotheses about unobservable objects with the data. As a result, the data don't uniquely determine a theory, and the possibility emerges of elaborating empirically equivalent, but theoretically distinct theories.

It comes then as no surprise that confirmational holism and the use of the underdetermination argument are two common features to most empiricist views. We certainly find them in Quine [1961], and to some extent, in van Fraassen [1980]. But together with the indispensability argument, these features put the empiricist in a curious situation.

As van Fraassen points out, "to be an empiricist is to withhold belief in anything that goes beyond the actual, observable phenomena" (van Fraassen [1980], pp. 202−203). This requires that one take seriously the underdetermination argument. In fact, the empiricist uses this argument as a powerful tool to raise doubts about the existence of unobservable entities that realists typically want to endorse. But given confirmational holism and the indispensability argument, the empiricist ends up believing in the existence of abstract entities (such as sets, functions and numbers). Since the latter are typically unobservable, the empiricist's set of beliefs turns out to be incoherent. Of course, unless the empiricist shows that mathematics is actually dispensable (no easy task!), or denies the doctrine of ontological commitment (but what should one put in its place?), he or she won't be in position to reject the indispensability argument. The result is an incoherent belief set.

In what follows, I shall examine some alternatives to overcome this difficulty, and argue how we can still be empiricists about science even if there is no way of avoiding the use of mathematics in theory construction, and without having to assume a particular nominalization program for mathematics.[4] An account of the application of mathematics will be indicated that can be endorsed by an empiricist without having to postulate the existence of mathematical objects. The crucial idea is to look to the way in which mathematics is actually applied (both within mathematics and in science) and to provide a version of the underdetermination argument in mathematics.

As an illustration of this idea, I shall discuss how Abraham Robinson applied model theory to construct nonstandard analysis (internal application of mathematics) and then how he applied nonstandard analysis to solve problems in the foundations of quantum mechanics (application of mathematics to science). In both cases, there is a crucial use of models (that provide structure in terms of which the application is achieved) and the underdetermination argument (to avoid ontological commitment to certain abstract entities). Given that the account captures important aspects of mathematical practice, it also provides a naturalist view of the application of mathematics (see Maddy [1997]).

2. THE UNDERDETERMINATION ARGUMENT REVISITED

4 Most nominalization programs, such as the one developed by Field [1980], assume realism about science (in Field's case, realism about space−time), and thus are incompatible with an empiricist view.

The discussion of the underdetermination argument above was too quick. This argument becomes a powerful tool if we consider *particular* cases of scientific theories, instead of making general claims about science. Beautiful examples of underdetermination come, for example, from space−time theories; in particular, from the work of David Malament and others in which observationally indistinguishable space−times, that are not invariant under diffeomorphism (and may not have the same causal structure), are constructed. There are also well known cases from quantum mechanics. Consider, for example, the quantum mechanical formalism plus different interpretations (say, the usual Copenhagen interpretation and Bohm's interpretation). The resulting systems (of theory plus interpretation) are radically different from the ontological point of view: there is no "quantum potential" in the Copenhagen interpretation, but such a potential is an important feature of Bohm's interpretation; Copenhagen particles can't have their position and momentum simultaneously measured with certainty, Bohmian particles can. So the two theories posit different objects, and where they seem to agree about the objects they introduce (e.g. particles), such objects turn out to have different properties. But despite these ontological differences, the two systems are empirically equivalent. Following van Fraassen's [1980] account, what this means is that the empirical substructures of the models of the two systems are isomorphic (i.e. they are mathematically the same).

In a recent discussion of underdetermination, Michael Dickson [1999] raised two problems: (i) How can two theories be empirically equivalent if all observation is theory−laden? After all, if what is observed depends on a theory, each theory should have its own set of relevant observations (the actual phenomena that are observed); but in this case, how can two theories be empirically equivalent? In indicating a solution to this problem, Dickson distinguished different senses of observation ([1999], pp. S49−S51), and pointed out that the relevant distinction here is not between theory and observation, but between theory and interpretation. (The example above of Bohm's interpretation and the Copenhagen view, indicates that what is at stake in the underdetermination argument is the *interpretation* of quantum mechanics.) But this immediately raises another problem ([1999], p. S57): (ii) If we want to make good sense of the distinction between the formalism of a theory and its interpretation, we need first to be able to distinguish the *mathematical* part of a physical theory from its *physical* part. How can this be done?[5] It is now clear that by considering the problem of underdetermination, we end up discussing issues about ontological commitment to mathematical entities. And of course an empiricist needs to provide an account of this situation; that is, he or she needs to make sense of the relevant distinctions without being committed to unobservable objects.

3. THE PUZZLE NOT SOLVED

Here is an attempt to overcome the difficulty. The distinction between the physical part and the mathematical part of a theory can be made in purely "syntactic" terms (see Peressini [1999]). Consider group theory, for example. It is part of the theory that given an element in a group, there is always another element in the group such that the group product of the two elements is the identity element. In symbols, we have:

(GT) $\forall x\, \exists y\, (x \in G \wedge y \in G \rightarrow x \cdot y = e)$

where "G" stands for group and "e" for the identity element of G. As Peressini ([1999], p. S3)

5 Moreover, is it true that for two theories to be empirically equivalent they have to share the same formalism?

points out, this proposition has consequences for a number of pure groups, such as the permutation group for three elements, P_3, or the group of 2 x 2 unitary matrices with determinant equal to 1, SU(2). For example, with the further assumption that

(AP) SU(2) is a group

we obtain a particular case of (GT) restricted to SU(2). Given that the relationship between (GT) and (AP) is purely mathematical, since (AP) is purely mathematical, we have here a case of what Peressini calls a *pure application* of mathematics ([1999], p. S3).

However, Peressini continues, in the case of a *physical application* of mathematics, the situation is different. Consider the application of SU(2) in quantum mechanics. In this case,

> the members of SU(2) are further interpreted as (components of) the physical property of spin, and the group operation is taken to correspond to the composition of spin states, thus allowing the mathematical terminology to fall under the physical principles that relate the property of spin to the rest of the physical theory. (Peressini [1999], p. S4)

In other words, the application of mathematics to physics is here conceived of as a two−stage process. Firstly, general mathematical statements are instantiated to particular cases; secondly, mathematical operations (such as the group operation) are interpreted by convenient physical processes (such as the composition of spin states). The process is "syntactic" in that instantiation is a purely logical relation, and the "interpretation" involved in the second stage aims at allowing "the mathematical *terminology* to fall under the physical principles" (*ibid.*, my italics).

It is tempting to use this account of the application of mathematics to provide a distinction between the physical part and the mathematical part of a scientific theory. The mathematical part involves Peressini's pure applications (those achieved simply by instantiation), and the physical part concerns physical applications (those requiring a "physical interpretation" of the mathematical formalism).

But clearly this won't do. Peressini's account *assumes* that the mathematical and the physical theories are already distinguished. On this account, we apply group theory to quantum mechanics by assigning group−theoretic notions to quantum mechanical principles. The two domains (of group theory and quantum mechanics) have already to be clearly separated for this proposal to get off the ground. Furthermore, especially in the case of quantum mechanics, no formulation of the theory is available in which this separation can be made. There is no way of even *expressing* the notion of a quantum particle, for example, without talking about convenient sets of invariants (Weyl [1931] and French [1999]).

Finally, it is difficult to reconcile some important cases of the application of mathematics with a "syntactically oriented" account of the application process. As I will argue now, there are more things going on in this process than can be captured by a purely syntactic view.

4. APPLYING MATHEMATICS: A CASE STUDY

In order to illustrate some features of the application of mathematics, I shall consider the construction of nonstandard analysis by Abraham Robinson (Robinson [1974]). By considering the way in which Robinson applied mathematics, both to mathematics itself and to physics, we will be able to examine a sophisticated approach to the use of mathematics, which will eventually provide a way out of the empiricist's predicament discussed above.

Application of Mathematics and Underdetermination

Let's start with some historical remarks. It is well known that the early formulation of the calculus, due to Leibniz and Newton, was heavily dependent on infinitesimals, which were employed, for instance, in the derivation of the rules of Newton's method of fluxions (Lavine [1994], pp. 15−26). Intuitively, infinitesimals were taken to be indefinitely small quantities, smaller than any finite quantity. And the major difficulty here was to provide a mathematically acceptable formulation of this notion. Given the lack of a precise definition, it is no surprise that infinitesimals were received with harsh criticisms, particularly by Berkeley (for references, see Lavine [1994]).

When Leibniz introduced infinitesimals, he tried to devise a program of construction of numbers that would include infinitesimals in a suitable way. The idea was to introduce the latter, by appropriate arithmetic rules, as ideal numbers into the system of real numbers, in such a way that the resulting system would have the same properties as the real number system. However, given that neither Leibniz nor his followers managed to produce such a system, infinitesimals gradually fell into disrepute, and were eventually eliminated in the classical theory of limits elaborated in the nineteenth century (Lavine [1994], pp. 26−41; Robinson [1974], pp. 260−282).

But in 1960, with the work of Robinson, Leibniz's program was brought back. Robinson realized that the model−theoretic techniques developed in our century provided the adequate framework in which Leibniz's intuitions could be properly articulated and vindicated (Robinson [1974], p. xiii). Robinson showed that the ordered fields that are nonstandard models of the theory of real numbers could be thought of, in the metamathematical sense, as non−archimedean ordered field extensions of the reals,[6] and they included numbers behaving like infinitesimals with regard to the reals. Moreover, since these ordered fields are *models* of the reals, they have the same properties as the latter. As a result, Leibniz's problem was solved.

The crucial notion Robinson used to provide nonstandard models of analysis was that of *enlargement*. Given a structure **R** (say, the real number structure), an enlargement ***R** of **R** is an expansion of **R** (in technical parlance, **R** is a substructure of ***R**), such that a sentence α is true in **R** if and only if α is also true in ***R** (that is, **R** and ***R** are elementarily equivalent). In other words, an enlargement B of a given structure A is an extension of A which preserves the truth−values of the sentences which hold in A. The decisive result from model theory that Robinson employed in the development of nonstandard analysis was the compactness theorem, according to which if K is a set of sentences such that every finite subset of K is consistent, then K is also consistent. What the compactness theorem allowed him to prove is that for any structure A there is an enlargement B of A. However, as Robinson pointed out, the enlargement is by no means unique. The real number structure **R** has several enlargements ***R**, and any of them provides a nonstandard model of analysis. But once an enlargement has been chosen, "the totality of its internal entities is given with it" (Robinson [1967], p. 29). As a result:

> corresponding to the set of natural numbers **N** in **R**, there is an internal set ***N** in ***R** such that ***N** is a proper extension of **N**. And ***N** has "the same" properties as **N**, i.e. it satisfies the same sentences of L just as ***R** has "the same" properties as **R**. ***N** is said to be a *Non−standard model of Arithmetic* [just as ***R** is called a *Non−standard model of Analysis*]. From now on all elements (individuals) of ***R** will be regarded as "real numbers", while the particular elements of **R** will be said to be *standard*. (Robinson [1967], pp. 29−30).

Now, the crucial feature of ***R** is that it is a *non−archimedean* ordered field. Therefore, ***R**

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6 A field over the real numbers is *archimedean* if for any pair of real numbers a and b, $0 < a < b$, there exists a natural number n (in the *ordinary*, *standard* sense) such that $b < na$. This postulate is not true in Robinson's nonstandard models, and in this sense the latter are non−archimedean (see Robinson [1974], pp. 266−267).

contains infinitely small numbers (infinitesimals), that is, numbers $a \neq 0$ such that $|a| < r$ for all standard positive r (Robinson [1967], p. 30).

Since the structures **R** and ***R** satisfy the same set of sentences, the properties of relations and functions in one structure can be "transferred" back into the other, and vice−versa. This provides the main heuristic move used by Robinson, namely *transfer principles*. These principles are straightforward consequences of the model−theoretic framework in which Robinson worked, given the elementary equivalence of the structures under consideration. In other words, there are significant (model−theoretic) interconnections between **R** and ***R**, and the decisive trait of nonstandard analysis is to explore them. Although we may not know whether a given result holds in **R**, by embedding it into ***R**, we have "more structure" to work with, and in this way, we may be able to establish the result. Using a transfer principle, we then establish that this result also holds in **R**.

By systematically exploring this heuristic strategy, Robinson was not only able to simplify several proofs of established theorems, but also to prove new results in classical mathematics. In particular, Robinson showed how analysis could be reformulated with infinitesimals. For example, he established that the real−valued function f is continuous at x_0 in the real number structure **R** if and only if $f(x_0 + \eta)$ is infinitely close to $f(x_0)$ in the nonstandard model ***R**; that is, $f(x_0+\eta) - f(x_0)$ is infinitesimal, for all infinitesimal η (see Robinson [1967], pp. 30−31, and Robinson [1974], pp. 49−88).

What is important to note here is that the application of model theory to mathematical analysis devised by Robinson (a case of "pure application" in Peressini's terminology) *cannot* be captured in purely syntactic terms; it is *not* a matter of instantiation. On the contrary, Robinson provides an enlargement of the theory under consideration – a convenient model – and in terms of this model he draws consequences for the relevant application.

Robinson has also explored the application of nonstandard analysis to the foundations of quantum mechanics (a case of "physical application" in Peressini's terminology). In a joint work with Bernstein, he solved an invariant subspace problem, using nonstandard techniques – this was an open problem, which hasn't been solved yet with the resources of classical analysis (Bernstein and Robinson [1966]). Bernstein and Robinson proved the following theorem: if T is a bounded linear operator on an infinite−dimensional Hilbert space H over the complex numbers, and if $p(z) \neq 0$ is a polynomial with complex coefficients such that $p(T)$ is compact, then T leaves invariant at least one closed subspace of H other than H or $\{0\}$ (Bernstein and Robinson [1966], p. 421). The main idea is to associate with the Hilbert space H a larger space $^*H$ which, given the construction of the enlargement, has the same properties as H. The problem is then solved by considering the invariant subspaces in a subspace of $^*H$, whose number of dimensions is a nonstandard positive integer (*ibid*.; see Dauben [1995], pp. 327−329). Again, the crucial role here is played by the enlargement (the relevant model), rather than by a syntactic instantiation relation.

But what is the status of nonstandard analysis and, in particular, of infinitesimals? Robinson was always clear in *not* believing in the latter. What is striking is that his argument in support of a lack of belief in infinitesimals was an *underdetermination argument*: the system of real numbers underdetermines its possible nonstandard extensions, and therefore we are not entitled to claim that there are infinitesimals (Robinson [1973*b*], p. 130). It is worth noticing that this is exactly the same kind of argument that van Fraassen uses in support of his agnosticism with regard to unobservable entities in science (see van Fraassen [1980], pp. 41−69). In Robinson's own words:

> Let me emphasize that [*nonstandard analysis*] *does not present us with a single number system which extends the real numbers, but with many related systems*. Indeed, there seems to be *no natural way to give preference to just one among them*. This contrasts with the classical approach to the real numbers, which are supposed to constitute a unique or, more precisely, categorical totality. However, [...] I belong to those who consider that it is in the realm of possibility that at some stage even the established number systems will, perhaps under the influence of developments in set theory, *bifurcate* so that, for example, future generations will be faced with *several coequal systems of real numbers in place of just one*. (Robinson [1973*b*], p. 130; the italics are mine.)

This passage indicates Robinson's emphasis on the plurality of extensions of real numbers that nonstandard analysis provides. And since there is "no natural way to give preference to just one" among the various extensions, we cannot be committed to belief in infinitesimals, which constitute such extensions.

Moreover, the second part of this passage concerns the possibility of bifurcation of even real number systems, and the consequent coexistence of several systems of real numbers. This indicates Robinson's pluralism about the models of analysis. It is therefore most unlikely that he would see it as an advantage that nonstandard analysis replaces the standard one. Given his concern with heuristics, the plurality of different systems is certainly welcome, for it allows the exploration of different aspects of analysis.

Robinson's discussion of *bifurcation* in the context of set theory refers to Cohen's result of the independence of the continuum hypothesis from set theory. In a paper also published in 1973, Robinson discussed the case of set theory, and its implications for the ontology of mathematics:

> One cannot predict for certain that some argument in arithmetic will not one day split the classical mathematicians down the middle. [...]
>
> Where others are still trying to buttress the shaky edifice of set theory, the cracks that have opened up in it have strengthened my disbelief in the reality, categoricity or objectivity, not only of set theory but also of all other infinite mathematical structures, including arithmetic. I am thus taking sides in an ancient controversy that has appeared and reappeared in different forms over thousand of years. In our time no less a man than Paul J. Cohen has indicated his agreement with my point of view. (Robinson [1973*a*], p. 514; see Dauben [1995], p. 456)

Together these passages make clear Robinson's nominalist attitude towards the ontological status of mathematical structures in general and infinitesimals in particular.

But wait. There seems to be two apparently conflicting tendencies at play in the above quotations from Robinson. On the one hand, we find a strive for *abundance* when Robinson points out that there is a plurality of number−theoretic systems all of which are equally acceptable. And Robinson also indicates that, in his view, this is the tendency that mathematics is likely to follow. (Given Robinson's concern with applied mathematics, it makes sense for him to expect to have a huge variety of mathematical structures to explore.) On the other hand, given the underdetermination argument, Robinson doesn't need to be ontologically committed to certain abstract entities, such as infinitesimals. And this supports a *nominalist* understanding of mathematical practice.

To reconcile these two trends is, of course, part and parcel of the development of any nominalist view. And the crucial idea is to indicate why one is not ontologically committed to abstract entities despite the talk of a plurality of structures (appropriate enlargements). By using the underdetermination argument, we have a style of reasoning that allows us to see why no ontological commitment to at least infinitesimals is forthcoming: the extensions (enlargements) are elementarily equivalent, but they are ontologically different, and therefore infinitesimals (that are part of the enlargements) are ultimately dispensable. Why? The enlargements typically have different cardinalities. They are not isomorphic, and hence they don't have the same structure. Thus, the structures provided by the enlargements don't contain the same objects (each enlargement "adds" new objects). However, the enlargements are elementarily equivalent, and therefore the same sentences are true in all of them. So the sentences of the mathematical theory under consideration are true even if the extra objects in the enlargements don't exist. In other words, since the enlargements are different ontologically, but are elementarily equivalent, we don't have to believe in the extra structure they provide. Thus, we don't have to believe that there are infinitesimals.

We can now see why Robinson was warranted in not believing in the existence of infinitesimals, despite exploring the latter in the development of nonstandard analysis. Moreover, given Robinson's use of enlargements, his approach to mathematics also illustrates an important feature of van Fraassen's constructive empiricism: mathematical activity is seen as a process of *model construction*. As van Fraassen points out,

> I use the adjective "constructive" to indicate my view that scientific activity is one of construction rather than discovery: construction of models that must be adequate to the phenomena and not discovery of truth concerning the unobservable. (van Fraassen [1980], p. 5)

Now, the same feature is notable in mathematical practice, and in particular in Robinson's work. As we saw, the construction of models is the starting point of, and the basis for, nonstandard analysis, since one has to construct enlargements of a given algebraic field. Moreover, given the plurality of different enlargements of the same complete ordered field, we cannot identify one and only one enlargement as *the* nonstandard model – any of them will do. As a result, the aim of model construction is not to "discover the truth concerning the unobservable", but only to provide models which are "adequate to the phenomena". But since we are considering nonstandard models of analysis, what does it mean for a model to be "adequate to the phenomena"? What are the "phenomena" in this context? Of course, the "phenomena" are not empirical features of the world, but interrelationships between different structures, namely different models of analysis. Roughly speaking, a model will be adequate in this context if it provides an enlargement of the relevant model of classical analysis. But since there are a number of different enlargements that will be adequate, we are not warranted in claiming that one of them is true (in the sense of being *the* model that captures the nature of the "nonstandard world"). Of course, this is part and parcel of Robinson's underdetermination argument discussed above.

Furthermore, Robinson's attitude towards nonstandard analysis wasn't "eliminativist", in the sense of trying to eliminate classical analysis. In this respect, Robinson also shared the empiricist's recommendation of preserving and extending mathematics, rather than mutilating it. As van Fraassen points out:

> I do not really believe in abstract entities, which includes mathematical ones. Yet I do not for a moment think that science should eschew the use of mathematics, nor that logicians should, nor philosophers of science. I have not worked out a nominalist philosophy of mathematics – my trying has not yet carried me thus far. Yet I am clear that *it would have to be a fictionalist account, legitimizing the use of mathematics and all its intratheoretic distinctions in the course of that use, unaffected by disbelief in the entities mathematical statements purport to be about*" (van Fraassen [1985], p. 303; the italics are mine).

Robinson would agree.

5. AN ALTERNATIVE: UNDERDETERMINATION IN MATHEMATICS

As indicated above, the process of the application of mathematics suggested here is based on the construction of convenient models and the exploration of structural relationships between them. It is “semantic” rather than “syntactic”. It is a matter of bringing structure from one domain to another: from model theory to mathematical analysis (in the case of the construction of nonstandard analysis), and from nonstandard analysis to the foundations of quantum mechanics (in the case of the invariant subspace problem). In these two cases, the application of mathematics is not a matter of finding a bridge−principle that connects mathematical and physical terms. It is a matter of constructing models that represent certain possibilities, and obtaining consequences based on them.

Moreover, Robinson’s case also illustrates that there is something in the way mathematics is done (something about the practice of mathematics) that allows one not to believe in the existence of certain mathematical entities. What I want to suggest is that the use of the underdetermination argument in both pure and applied mathematics provides a way out of the difficulty (discussed in section 1) which the empiricist faces. By considering the way in which Robinson used that argument, we saw how ontological commitment to infinitesimals can be avoided. In a nutshell, the possible nonstandard extensions of the system of real numbers are underdetermined by that system; thus we are not compelled to believe in infinitesimals.

But how general is this strategy (namely, of finding different enlargements of the theory under consideration and exploring their model−theoretic features)? Of course, similarly to the underdetermination argument in science, its use in mathematics generates at best a case−by−case approach. The strength of the strategy derives from the examples we consider. However, the use of model−theoretic resources provides grounds for claiming that it has some generality (at least as much generality as model theory provides).[7]

We can now return to the indispensability argument. As we saw, given the argument, the empiricist would be led to believe in mathematical entities. But why should he or she *accept* the argument in the first place? Without a nominalization strategy for mathematics that actually establishes that mathematical entities are dispensable, the empiricist cannot deny the second premise of the argument, (P2). And if we consider the works of Quine and van Fraassen, it is clear that the use of mathematical theories plays a decisive role. Quine, of course, explicitly acknowledges the point, emphasizing the importance that classes play in his account (see Quine [1960]). Similarly, although van Fraassen would rather be a nominalist, he thinks that neither science nor the philosophy of science should try to avoid the use of mathematics (van Fraassen [1985], p. 303). Constructive empiricism is a proposal to interpret and make sense of science rather than to change its mathematical and physical content. Since the use of mathematics is a fact about science, it is something that the empiricist has to accommodate rather than revise.

With regard to premise (P1) of the indispensability argument, Quine of course wholeheartedly accepts it. Van Fraassen himself is more cautious. For in his view, we may not have reason to *believe* in unobservable entities postulated by current scientific theories, despite the fact that we

7 But it also raises some doubts: if the empiricist uses model theory (and some set theory) to construct the relevant models and enlargements, isn’t he or she making unacceptable ontological commitments? On the face of it, yes. But this is not the whole story. To overcome the difficulty, the empiricist can always adopt a stronger background theory, which is still acceptable for the empiricist, such as second−order mereology plus plural quantification (see Boolos [1985] and Lewis [1991]). At least this provides enough background to express the relevant model−theoretic notions.

accept such theories (van Fraassen [1980] and [1985]). However, given van Fraassen's commitment to the semantic approach – according to which to present a theory is to specify a family of models (van Fraassen [1980], p. 64) – the constructive empiricist ends up committed to unobservable mathematical entities (see Rosen [1994]).

The strategy suggested here indicates one way in which mathematical entities can be dispensed with, and so (P2) can be resisted. As we saw illustrated in the case of Robinson, by generating conservative extensions of a given mathematical theory (such as real analysis), the commitment to the entities involved in the extension can be avoided. This justifies a nominalist attitude towards such entities, in the way recommended by Robinson and van Fraassen.[8]

A final point. Just as van Fraassen is agnostic, but not skeptic, about the existence of unobservable entities in physics (such as electrons), the best option for an empiricist in the case of mathematics is similarly to be agnostic, but not skeptic, about the existence of abstract entities (such as numbers). The strategy indicated here supports this agnosticism, since we have several different enlargements to play with, and given their plurality, we are not required to believe in the existence of the objects in their domains. After all, different objects are postulated in different enlargements, and given their elementary equivalence, all the enlargements do the same work. So, as expected, the empiricist can simply remain agnostic about them.

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8 But this also leaves the commitment to the initial theory (in the present case, real analysis) untouched. But one can use second−order logic, plural quantification and some mereology to avoid ontological commitment to the entities in question (see Lewis [1991]). A lot less is then demanded of Lewis's strategy, since one doesn't need to nominalize the whole of mathematics, but only its basic portions, such as real analysis.

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