MATHEMATICAL EXPLANATION: EXAMINING APPROACHES TO THE PROBLEM OF APPLIED MATHEMATICS

A Thesis

by

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ABSTRACT

The problem of applied mathematics is to account for the 'unreasonable effectiveness' of mathematics in empirical science. A related question is, are there mathematical explanations of scientific facts, in the same way there are empirical explanations of scientific facts? Philosophers are interested in the problem of applied mathematics for two main reasons. They are interested in whether the use of mathematics in empirical science is sufficient to motivate ontological conclusions. The indispensability argument suggests that the widespread application of mathematics obligates us to accept mathematical entities into our ontology. The second primary philosophical question concerns the details of the applications of mathematics. Philosophers are interested in what sort of relationship between mathematics and the physical world allows mathematics to play the role that it does.

In this thesis, I examine both areas of literature in detail. I begin by examining the details of the indispensability argument as well as some significant critiques of the argument and the methodological conclusions that it gives rise to. I then examine the work of those philosophers who debate whether the widespread application of mathematics in science motivates accepting mathematical entities into our ontology. This debate centers on whether there are mathematical explanations of scientific facts, which is to say, scientific explanations which have an essential mathematical component. Both sides agree that the existence of mathematical explanations would motivate realism, and they debate the acceptability of various examples to this end. I conclude that there is a strong case that there are mathematical explanations. Next I examine the work of the philosophers who focus on the formal relationship between mathematics and the physical world. Some philosophers argue that mathematical explanations obtain because of a structure preserving 'mapping' between mathematical structures and the physical world. Others argue that mathematics can play its role without such a relationship. I conclude that the mapping view is correct at its core, but needs to be expanded to account for some contravening examples. In the end, I conclude that this second area of literature represents a much more fruitful and interesting approach to the problem of applied mathematics.

DEDICATION

This thesis is dedicated to Amy.

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1. INTRODUCTION

The problem of applied mathematics is the philosophical quandary concerning the 'unreasonable effectiveness' of mathematics in empirical science. Many scholars have noticed that mathematics plays an important role in our best physical theories, and philosophical questions naturally arise about what exactly mathematics is doing when we use it in science, and what this tells us about mathematics. This issue is called the problem of applied mathematics. This general interest in the effectiveness of mathematics has led to more specific questions about the role of mathematics. In particular, there has been much recent interest in the topic of mathematical explanation. The literature surrounding mathematical explanation seeks to determine whether the role that mathematics plays in science, its unreasonably effective role, has to do with applied mathematics being explanatory.

There has been considerable attention paid recently to the problem of applied mathematics and the related question of mathematical explanation. The literature devoted to this problem divides rather neatly into two areas. For the purposes of this thesis, I refer to these two areas as the *ontological* literature and the *applications* literature. The ontological literature pursues the question of mathematical explanation with an eye towards the possible ontological consequences of accepting the existence of mathematical explanations of scientific facts. This literature centers around the indispensability argument for mathematical realism. The indispensability argument states that the presence of mathematics in our best scientific theories entails an ontological commitment to mathematical entities. The recent debate focuses on the indispensability of mathematical entities, but specifically their explanatory indispensability. Examples are brought forth from empirical science to support the contention that mathematics does feature in science in a genuinely explanatory way, while others argue that the putative examples of mathematical explanation actually indicate a less fundamental role for mathematics in science. The ultimate end of both sides of the debate is to reach a conclusion related to the ontological status of mathematical entities.

The applications literature takes a somewhat different focus. Whereas the ontological literature debates the role of applied mathematics as a means to an ontological end, the applications literature is more or less neutral on ontological issues, focusing instead on the details of what mathematics is adding to scientific theories. Advocates of the so-called *mapping* view argue that mathematics is useful in science because it enables us to construct structure-preserving, representational models that are more inferentially tractable than theories that do not use mathematics. Critics of this sort of view argue that mathematical practice does not bear out the claim that mathematics is useful because it faithfully represents the structure of the physical world. There are examples from science that seem to indicate that mathematics is deployed much more pragmatically than the mapping theorist might expect. Both sides of this debate approach the problem of applied mathematics from a broadly naturalistic perspective and seek to gain clarity regarding just what it is that our scientific theories are getting from being couched in terms of non-causal abstract entities.

The goals of this thesis are to examine the details of these two bodies of literature, comparing the approaches taken and conclusions reached by both. With this goal in mind, the structure of the thesis breaks into three main sections. In chapter 2, I will present an extended exposition and exploration of issues surrounding the indispensability argument, because it features very centrally in the ontological literature. In chapter 3, I will present the details of the main entries in the ontological literature. I will give the details of the primary arguments on both the Platonist and nominalist sides of the debate, with some critical remarks. In chapter 4, I will discuss the applications literature, giving the details of two significant formulations of the mapping view. To conclude chapter 4, I will present a significant challenge to the mapping view. Chapter 5 will recap and offer some tentative conclusions about approaching the problem of applied mathematics in light of the accounts that I examine.

2. THE INDISPENSABILITY ARGUMENT

2.1 Colyvan's Defense of Indispensability

Mark Colyvan has made many contributions to the literature on mathematical explanation, including a very good exposition of the indispensability argument and its role in this topic. The indispensability argument is derived, in its modern formulation, from the work of Quine and Putnam. Colyvan reconstructs the argument as follows:

> 1. We (ought to) have ontological commitment to all and only those entities that are indispensable to our best scientific theories (Quinean Ontic Thesis).[1]
>
> 2. Mathematical entities are indispensable to our best scientific theories.
>
> From these we conclude that we (ought to) have ontological commitment to mathematical entities. (Colyvan [1998], p. 40)

Premise two has been subjected to a well known critique by Hartry Field [1980], and other significant criticisms have been made by other authors, e.g., Penelope Maddy [1992]. The indispensability argument is, first and foremost, an argument for realism. The conclusion of the argument can be taken in a couple of different ways, according to Colyvan. One interpretation of the conclusion, which Colyvan draws from Dummett and refers to as *semantic realism*, is that mathematical realism commits us to the objective truth or falsity of mathematical statements. Whether or not mathematical statements are true is an objective matter that is independent of our ability to make this determination in any given case. The second way to

[1] I call this Quine's Criterion of Ontological Commitment (from Azzouni [2004]).

interpret the conclusion, called *metaphysical realism* by Colyvan (and often known as *Platonism*), is as a thesis about the existence of mathematical objects: from the indispensability of mathematics we are committed to the objective existence of the mathematical objects that are the subject matter of mathematical statements.

Intuitively, metaphysical realism entails semantic realism; the properties of, and relations among, objectively existing mathematical entities determine the truth value of mathematical statements. Semantic realism, by contrast, is only concerned with the objectivity of mathematical truths and stops short of affirming existence of mathematical entities which provide the objectivity. However, the converse does not hold, as Colyvan indicates by quoting Dummett and Putnam. It is possible to accept semantic realism without accepting metaphysical realism. For his part, Colyvan is primarily concerned with metaphysical realism.

In contrast with realism of either variety is *nominalism*, which Colyvan uses as a term for any view that denies the existence of mathematical entities. The challenge for the proponent of nominalism is to account for the "wide and varied" applications of mathematics.[2] The nominalist might respond to this challenge with a nominalist view called fictionalism. The fictionalist about mathematics holds that mathematical truths are true by virtue of being a part of the system of mathematics, but it does not follow from that fact that mathematical truths are truths *simpliciter*.

Colyvan's primary concern is to examine the potency of the indispensability argument as an argument for metaphysical realism and against nominalism. As a starting point for his discussion, Colyvan affirms that mathematics does inevitably feature in scientific theory (*contra* Field). Given the theoretical indispensability of mathematics, the primary concern is to explain the role that mathematics does play which makes it indispensable. This is the one of the most perplexing issues in the philoso-

[2] Colyvan [2001], p.4

phy of mathematics, Colyvan claims. I will refer to this problem as the problem of applied mathematics.

The general structure of the indispensability argument leaves some ambiguity that must be filled in for specific cases. Generally, an indispensability argument simply says that a certain belief is necessary relative to certain purposes. A specific formulation of the argument then spells out those purposes.[3] Colyvan contends that the indispensability argument has a particularly significant form relative to science, which asserts the "certain purposes" to be explanatory purposes. As the next section will make clear, this is the formulation that is up for debate in the recent literature on mathematical explanation that I will be examining. But Colyvan's exposition of the original formulation by Quine and Putnam shows that this is not the only way to take the argument with respect to mathematics.

Colyvan's interest in the indispensability argument is to convince scientific realists that to accept the indispensability argument and, hence, realism about mathematical entities, is merely to accept a new instance of a form of argument that they regularly deploy.[4] The indispensability argument as formulated for science generally takes an obvious form.

> Scientific Indispensability Argument: If apparent reference to some entity (or class of entities) [X] is indispensable to our best scientific theories, then we ought to believe in the existence of [X]. (*ibid.*, p. 7)

This pattern, Colyvan argues, is utilized frequently in the natural sciences, and it can be seen as nothing more involved than an application of inference to the best explanation. Furthermore this argument pattern is generally of the explanatory sort, asserting that the indispensable entities are indispensable because of the explanatory

[3] *ibid.*, p. 6

[4] *ibid.*, p. 8

benefit they offer. For example, dark matter is invoked in scientific theory (and its existence is accepted) on the grounds that it helps to explain otherwise unexplainable facts about the rotation curves of spiral galaxies.[5] Colyvan admits that those who are disposed to disapprove of inference to the best explanation may not find the comparison with the indispensability argument to be a compelling case for accepting the latter. Nevertheless, he notes that scientific realists are generally sympathetic to the use of inference to the best explanation, and inference to the best explanation is a kind of indispensability argument, so the comparison should have some traction with realists.

Colyvan's formulation of the indispensability argument is largely based on Quine's work, but he mentions historical precedent from Gödel and Frege that suggests that some form of the indispensability argument for mathematics is quite independent of the overall Quinean philosophical project.[6] As is the case with many of Quine's most famous doctrines, there is not just one canonical reference for the indispensability argument. As Colyvan documents, Quine's view is that mathematics and its ontology should be included in our overall theoretical apparatus for similar reasons as our other theoretical posits. In keeping with his confirmation holism, the mathematical posits that are included in the aforementioned apparatus are confirmed to the same degree as the apparatus as a whole. On the basis of this sort of argument, Colyvan contends that we can conclude that, whatever we take the purpose of science to be, mathematics is indispensable for that purpose.[7]

Colyvan argues that Quine's Criterion of Ontological Commitment — the crucial first premise of the argument — follows from the combination of naturalism and a

[5] *ibid.*, p. 8
[6] *ibid.*, p. 9
[7] *ibid.*, p. 11

kind of holism, specifically confirmation holism.[8] Quine's interpretation of naturalism, which is that there is no 'supra-scientific tribunal' to which scientific practice must answer, suggests that our ontological questions can only be answered by looking at science, which is of course a significant part of our overall theoretical apparatus. Colyvan thinks that this sort of naturalism makes it reasonable to accept the existence of the entities posited by our best theories, but he does not think that this alone is enough to compel belief in all of the posits of science. To make this leap, we need confirmation holism. Confirmation holism provides the blanket support for all of our posits that is needed to insulate individual posits against criticism. If we adopt the naturalist respect for the methodological practices of our best theories, along with confirmation holism,then it seems that all of our leading theories entail ontological commitments, including our mathematical theories.[9]

2.2 Challenges to Indispensability

2.2.1 Maddy's Objections to Indispensability

Penelope Maddy mounted a substantial challenge to the form of the indispensability argument advocated by Colyvan in *Indispensability and Practice* (Maddy [1992]). Her objection is multifarious, but the root of her objection to the argument is the rejection of confirmation holism and a disagreement about the proper form of naturalism with regard to mathematical practice. The basic form of the indispensability argument links the ontological status of mathematical entities to their roles in scientific theories and the solid confirmational foundation of those theories. Maddy's twofold objection to this indispensability argument is that it undermines the legitimacy of mathematical methods and that it is at odds with mathematical practice.[10]

[8] *ibid.*, p. 12
[9] *ibid.*, p. 13
[10] Maddy [1992]

Mathematical practice contains innumerable instances of entities that do not find application in science, and on a reading of the indispensability argument that does not include confirmation holism, these entities are not supported by the argument. Quine accounted for some of these sorts of cases by bringing them along with the applied portions of mathematics in the name of 'simplificatory rounding out,' but this only works up to a point, and anything beyond that point (for example, inaccessible cardinals) is deemed mere "mathematical recreation...without ontological rights."[11]

Maddy contends that Quine's solution disregards mathematical practice, which she argues is not sensitive to questions of applicability in the pursuit of new mathematical entities and truths. Set theorists who accept the existence of inaccessible cardinals do so on the basis of mathematical methods, since they can be seen to follow from certain non-canonical but nevertheless potentially attractive set-theoretic axioms. The simple indispensability argument grants ontological status only to entities that are made use of in science, and as few more as are needed to round things out. On the other hand, it seems that mathematicians have an entirely different mode of justification for their posits. This is important due to the sort of naturalism that Maddy is committed to, which extends to mathematics the same way that it does for science. She goes on to suggest that Quine is guilty of a sort of inconsistency inasmuch as his naturalism draws a line between mathematics and science that creates differing attitudes toward the two domains. A naturalism that excludes mathematics does not support the myriad of entities that mathematicians talk about, and it excludes purely mathematical methodologies for establishing our mathematical knowledge.[12]

Maddy calls the above objection to the indispensability argument the *scientific*

[11] *ibid.*, p. 278
[12] *ibid.*, p. 279

practice objection. She rejects the confirmation holism which underwrites the formulations of the indispensability argument advanced by Quine and Colyvan on the grounds that scientific practice shows anything but a uniform attitude towards theoretical posits in science. As she puts it: "...We find a wide range of attitudes toward the components of well-confirmed theories, from belief to grudging tolerance to outright rejection."[13] Experimental success of a theory does not seem to have a uniform effect on the attitudes toward the posits of the theory. Maddy raises the example of atomic theory, which, while indispensable to the best theories available as early as 1860, was nevertheless subject to significant skepticism until the early 20th century when more direct confirmation of the theory was available. This demonstrates that indispensability is not the criterion for ontological commitment that is actually used by scientists, at least in some cases.

Maddy rejects the response that philosophers may disregard the actual behavior of scientists on the grounds that experimental confirmation is the methodological principle on which we place significance. This response, she argues, is at odds with naturalistic principles, which compel us to accept a distinction between useful and true portions of theories. The naturalistic philosopher is not in a position to critique scientific practice, according to Maddy. There are countless examples in science where certain elements are understood not to be literally true, such as treatments of matter that assume it to be continuous. Nevertheless, these elements are often indispensable. Even if we assume such applications to be mere idealizations of a more fundamental theory that we find useful, the problem still arises that theories which are thought to be fundamental are often later revealed to contain false elements.[14] This indeterminacy of the truth of various scientific theories casts doubt on at least

[13] *ibid.*, p. 280
[14] *ibid.*, p. 282

some of the mathematical claims involved. We may not be able to determine which mathematical posits we should accept because it may not be clear which ones are involved in an idealization and which ones represent something that is literally true.

The second component of Maddy's case against the indispensability argument is what she calls the *mathematical practice objection*. According to this objection, the methodology of mathematics is irreconcilable with the indispensability argument and the ways that it would suggest that mathematics proceed. Taking set theory as a mathematical theory and quantum physics as a scientific theory, we can clearly see that mathematical practice diverges from indispensability. The continuum hypothesis is independent of the canonical set-theoretic axioms, but on the grounds that the continuum is a widely used structure in scientific theory, the indispensability theorist should be inclined to accept the legitimacy of the continuum hypothesis as a statement with a determinate truth value.[15] The indispensability theorist may even support the project of searching for new axioms that would settle the question, on the grounds that such purely mathematical methods of set theory have been fruitful in the past.

Suppose that there were a case in which previously endorsed mathematical entities have their applications revealed to be false. Maddy considers a possible example of this. The mathematics of quantum field theory sometimes generates invalid infinite values for physical quantities. It has been suggested by physicists that this infelicity is the result of using the continuum as a model for space-time.[16] This could mean that mathematics involving the continuum should be eliminated from our best theories. If this were to come about, Maddy contends that the indispensability theorist would have no choice but to recant her prior commitment to the determinate truth value

[15] *ibid.*, p. 284
[16] *ibid.*, p. 285

of the continuum hypothesis.

Maddy contends that if set theorists took seriously the methodology suggested by the indispensability argument, they should keep one eye on developments in science which could undermine their projects. However, the fact is that set theorists are not particularly interested in developments relating to the applications of the continuum in science, and they would most likely maintain their interest in the continuum hypothesis as a purely set theoretic matter, even in the face of disconfirming evidence of the scientific theories which implicitly utilize it. Maddy's strong contention is that the pursuit of truth differs from the pursuit of mathematical correctness. Independent mathematical questions are pursued that have no applied basis in empirical science and this suggests to Maddy that mathematical methodology is guided by internal rather than external concerns.

2.2.2 Colyvan's Response to Maddy

In *In Defence of Indispensability*, Coylvan devotes considerable effort to addressing Maddy's objections as they apply to his version of indispensability, which is in large part derived from that of Quine. The main thrust of his defense is that Maddy's version of Naturalism is at odds with the one that he and Quine endorse, and since that is the linchpin of her arguments, her objections are not strong. Furthermore, he argues that her version of naturalism is inconsistent with the sort of naturalism that she has argued for elsewhere. If we reject the methodological principle that philosophy must always defer to the decisions of science, then Maddy's arguments have less force.

Colyvan's position regarding Maddy's naturalism is not the sort needed to support the objections raised in *Indispensability and Practice.* The important distinction between the two forms of naturalism is that Quine's naturalism rejects the notion of a philosophy to which scientific practice must answer, whereas Maddy's formulation of naturalism suggests that philosophy must defer to science. Quine's formulation positions philosophy and science on an equal footing, which Colyvan contends surely does not entail Maddy's naturalism. The view that philosophy and science are continuous and that there is no 'high court of appeal' is surely a coherent one that does not by any means entail the sort of relationship that Maddy suggests philosophers must occupy with respect to science. Colyvan maintains that this is the form of naturalism that Quine intends.

Other work by Maddy seems to suggest that the 'philosophy must give' reading of naturalism is not consistently her viewpoint. Colyvan draws quotations from other works of Maddy's which suggest a view much more in line with Quine's naturalism. He quotes Maddy as follows[17] :

> How...does the philosophical methodologist differ from any other scientist? If she uses the same methods to speak to the same issues, what need is there for philosophers at all? The answer, I think, is that philosophical methodologists differ from ordinary scientists in training and perspective, not in the evidential standards at their disposal.

Contrary to Colyvan, I think that these remarks are reconcilable with the form of naturalism that drives Maddy's objection to indispensability. It seems that Maddy's view is simply that philosophers cannot criticize scientific practice on the grounds of methodological concerns grounded in philosophy, but that the philosopher is eligible, like anyone, to challenge science on the basis of scientific methodology. The quotation given by Colyvan seems to support this as she refers to philosophers who use the same methodology (scientific methodology) as scientists, and not philosophers who

[17](Colyvan [1998], p. 47)

try to use philosophical methodology. This seems perfectly compatible with the view that philosophical concerns cannot ground a criticism of scientific practice, because philosophers can certainly join in the scientific process, even if the particular methodologies of their discipline are impotent in this type of situation. Maddy's use of the term 'philosophical methodologists' may be misleading, because she seems to think that legitimate criticism of scientific practice cannot come from using the methodological tools of philosophy, but rather scientifically acceptable evidence.

Colyvan's response to the scientific practice objection is twofold. First, he argues that episodes of seemingly variable ontological commitment to scientific entities drawn from the history of science can be viewed in light of the notion that skepticism is a part of the scientific method and ontological commitment is not an all or nothing proposition. When a new contender for best theory comes on to the scene, it is perfectly reasonable that some may choose to suspend judgment on some or all of the new ontological commitments that it brings in tow.[18] Second, Colyvan emphasizes that Quine's proposal is (in part) a normative one, so it is not inconsistent with his notion of ontological commitment to observe historical episodes where scientists provisionally disavow entities of newly indispensable scientific theories. Quine's picture of science is certainly compatible with the possibility that science goes wrong, although cases of skepticism are certainly not always instances of this.

The contention that Quine's picture of scientific methodology is partially normative seems to be at odds with the form of naturalism that he espouses. If Quine's indispensability argument and the criterion of ontological commitment on which it is based are taken to have a normative component, then they seem to violate the naturalistic edict that there is no first philosophy that science must answer to. One may respond that this normativity comes from scientific practice itself, and that some

[18] *ibid.*, p. 49

such normativity is necessary for science. This is a perfectly good response, but it does seem to conflict with the use of this normativity to explain away instances of scientific practice which deviate from Quine's picture. If observations of scientific practice give us Quine's picture, and also cases which contradict it, neither set of observations has priority. This is not to say that there is not faulty science, but as I noted, Colyvan maintains that some instances of skepticism of novel entities are not faulty. Quine's picture should ideally jibe with every observation of non-faulty science that we have, if it is to be thoroughly naturalistic, but at the very least, we cannot use it to discredit those observations that it is inconsistent with.

Colyvan's response to the mathematical practice objection is that we should consider the speculative activities of mathematicians to be hypothetical.[19] This objection runs along the lines of what Maddy calls the modified indispensability argument, which separates the methodology of mathematics from its mode of ontological justification. Colyvan argues that when mathematicians do work in mathematical domains that outstrip applications, they are making hypothetical claims about what follows from given axioms, and what would therefore be true if those axioms were true. The attitude that the mathematicians take towards the mathematics thus derived is an agnostic one, because the ontological question, being wrapped up in applications as it is, is not answered until empirical science discovers an application.[20] Colyvan agrees that mathematicians should pursue their mathematical goals with little concern for ontological questions, but he maintains that this does not mean that they have violated Quine's criterion of ontological commitment, their area of specialization is simply not the one that is most concerned with those questions.

Maddy's concerns, as exemplified by the example of set theory and quantum grav-

[19] *ibid.*, p. 55
[20] *ibid.*, p. 54

ity, rely on her unique formulation of naturalism which differs from Quine's, Colyvan argues. Furthermore, he contends that Maddy's naturalism incorrectly distinguishes itself as being uniquely concerned with mathematical practice. Quinean naturalism respects mathematical practice and methodology, it simply requires a connection to empirical science in order to justify that respect.[21] Given that both approaches to naturalism undergird a respect for mathematical methodology, Maddy cannot cite this as a reason to prefer her formulation. The larger problem with Maddy's naturalism, according to Colyvan, is that it affords perhaps too much respect to mathematical methodology. Maddy's naturalism, in conjunction with the modified indispensability argument, seems to entail ontological commitment to mathematical entities by mere mathematical imagination. He finds this absurd, and he argues that such activity should be considered, to use a term from Quine, 'mathematical recreation.'[22]

This approach to speculative mathematics is the basis for Colyvan's response to the quantum gravity and set theory example. Colyvan contends that if the continuum were found to have no application in empirical science, then the set theorist who continued to work on questions related to the continuum would be engaging in mathematical recreation, which is to say mathematics which entails no ontological commitments.[23] Set theorists do not keep apprised of the developments in physics because this is not their area of specialization or focus, and there is no reason to imagine that they should. Colyvan suggests that this is perhaps a job for the philosopher of mathematics. The moral is, confirmation holism does not imply that all of the posits of a theory have the same priority.[24]

[21] *ibid.*, p. 55
[22] *ibid.*, p. 56
[23] *ibid.*, p. 58
[24] *ibid.*, p. 60

There are two main points of contention to raise with Colyvan's response to Maddy. The first is that his version of the indispensability argument relies to some degree on confirmation holism, but his relegation of some mathematics to the status of mathematical recreation seems at odds with this position. If we decouple ontological justification from proper methodology in the way that the modified indispensability argument suggests, and we divide mathematics into recreational and ontologically significant mathematics, there needs to be an account of how we can maintain this distinction in light of confirmation holism. The second, which I think Maddy would agree with, is that mathematicians do not observe the distinction between ontologically significant mathematics and mathematical recreation. I do not disagree with Colyvan's contention that it is undesirable to suppose that any act of mathematical imagination generates new ontological commitments, but if Colyvan is right that set theorists focus on set theory and do not generally concern themselves with matters external to their area of expertise, then there seems to be little reason to suppose that they would maintain different attitudes towards different sets of their posits. If mathematicians evince a uniform ontological attitude towards their posits, there needs to be more said about why the mathematical methods used sometimes produce truths and sometimes lead to mere recreation. It seems that the semantic realist about mathematics, of which the metaphysical realist is a variety, would have good reason to resist splitting mathematics into categories of different ontological significance.

3. THE ONTOLOGICAL LITERATURE

The indispensability argument plays a central role in one large portion of the literature on mathematical explanation. The literature discussed in this chapter deals with the ontological issue in mathematical explanation. To summarize, the ontological issue concerns connecting the indispensability argument to examples of applied mathematics taken from empirical science. The authors who debate this issue are mostly sympathetic to the simple indispensability argument and do not argue for the eliminability of mathematics from theories, but they nevertheless differ on what ontology they are willing to accept. The debate thereby shifts to the question of whether the mathematical entities that are indispensable to our best scientific theories indispensable in virtue of playing a genuinely explanatory role. The two opposing positions then become 1) those who think that mathematics does play a genuinely explanatory role in empirical science, and this fact mandates that we endorse the ontological commitment to those entities required of us by the indispensability argument and 2) those who think mathematics does not play a genuinely explanatory role in empirical science, because mathematics merely 'indexes' physical quantities, and therefore we do not need to accept ontological commitment to those entities despite their indispensability. Mere indispensability is no longer taken to be sufficient to entail ontological commitment to mathematical *abstracta*, and explanatory power takes center stage as the characteristic of applied mathematics that entails ontological commitments. Both sides of this debate generally agree that the use of the indispensability argument as a basis for Platonism will stand or fall on finding examples of genuinely explanatory applied mathematics.

3.1 Nominalist Indexing View

3.1.1 Melia's Views on Ontological Commitment

Joseph Melia is the primary author associated with the nominalistic view that mathematics simply serves to index physical quantities and that the quantification over mathematical entities does not entail ontological comittments. Although quantification over mathematical *abstracta* is necessary in our scientific theories, we do not need to accept any ontological commitments on this basis. Nevertheless, in *Weaseling Away the Indispensability Argument*, Melia contends that we do need theories quantifying over *abstracta* in order to say everything we wish to say about the world. He examines the possibility, but ends up rejecting the position, that there are nominalistically acceptable theories that generate all the same consequences as platonistic ones. He calls this position the trivial strategy. To examine the trivial strategy, he looks at nominalistic versus platonistic theories of mereology, eventually concluding that the platonistic content is not a conservative extension of the nominalistic content, because the platonistic content generates new conclusions about the nominalist components of the theory.

Despite the fact that the trivial strategy is not a promising one, Melia believes that there is another way to avoid the ontological commitments that the indispensability argument suggests that we must accept. His strategy is based on his rejection of the Quinean Ontic Thesis as a methodological principle. He thinks that there are theories that imply the existence of certain abstract entities, but he thinks that disavowing ontological commitment to some of those entities is possible. Melia finds it perfectly reasonable to accept a theory which seems to entail ontological commitments, but then explicitly deny those commitments while keeping commitment to the non-ontological consequences of the theory. Using the mereology example, he argues that one can accept the platonistic (non-conservative) extension of the theory in order to derive all of the nominalistic consequences that one wants, but simply add an addendum that there are no such things as sets.

Melia contends that such an approach to unwanted ontological commitments is not only non-contradictory, but common practice among scientists.[1] Melia's naturalistic position is that ontological disavowal is analogous to the practice of writing in exceptions to universal claims. Sometimes we cannot express just what we mean without resorting to such techniques, with the mereology example being one such case. Some nominalistic facts about the spaces that exist only fall out as consequences from a non-nominalistic theory. Scientists often engage in just this sort of disavowal with regard to the mathematical entities quantified over in their theories. But if we accept the Quinean Ontic Thesis, it would be inconsistent for scientists to express the sorts of mathematics-laden theories that they do, while at the same time denying that there are actually such things as mathematical objects. Thus, to reconcile these two contradictory aspects of scientific practice, it seems like we need to allow something along the lines of disavowing unwanted ontological commitments.

3.1.2 Daly and Langford: Amplification of Melia

Chris Daly and Simon Langford argue that mathematics does not play a genuinely explanatory role when used in empirical science.[2] Daly and Langford maintain, along with Melia, that the role of mathematics in science is merely to index physical quantities and is not an explanatory role.[3] Given this, there is no reason to accept the existence of mathematical entities. The main thrust of this position is that every scientific fact which is expressible via mathematics supervenes on a more fundamental

[1] Melia [2000], p. 469
[2] Daly and Langford [2009]
[3] *ibid.,* p. 645

physical fact which does not involve mathematical entities. Whether we can purge our actual scientific theories of reference mathematical entities is immaterial, because Melia holds that mere reference does not entail ontological commitment.[4] Daly and Langford focus more on the question of whether the uses to which mathematics is put can be called genuinely explanatory. They argue that a mere indexing role is not a fundamental role, which they claim is necessary for the mathematics to be considered explanatory.

Mark Colyvan is one of the two primary philosophers representing the opposing side of this debate. In *The Indispensability of Mathematics*, he argues that mathematical entities do sometimes play an indispensable and genuinely explanatory role in scientific explanations, and he brings forth examples from science to build this argument. Daly and Langford directly address three examples given by Colyvan.[5] The three examples that Colyvan mentions are, the bending of light by massive bodies, antipodal weather patterns and Lorentz contraction.[6] Daly and Langford fairly quickly write off the first and third examples as cases of indexing, claiming that the mathematics involved serves only to pick out certain portions and features of space-time, the features themselves doing the actual explanatory work.[7]

Daly and Langford contend that unless there is a case made for an instance of applied mathematics being explanatory rather than indexing, the default position should be that mathematics is indexing. The Platonist and the Nominalist both agree that mathematics plays an indexing role, but only the Platonist thinks that mathematics also plays an explanatory role, so it is the task for the Platonist to build a case for this by producing examples of explanatory mathematics. Daly and

[4] Melia [1995], pp. 228-9
[5] *The Indispensability of Mathematics*
[6] Colyvan [2001], p. 47-51
[7] Daly and Langford [2009], p. 645

Langford maintain that it does not simply follow from the fact that mathematics can be used to pick out explanatory entities that the mathematical entities are themselves explanatory.[8]

Colyvan maintains that the mathematics used in science contributes to the explanatory power of theories by virtue of capturing the structure of the physical system. Mathematics is particularly useful for science because the same mathematical machinery can be used to represent the structure of multiple structurally similar systems. This portends the position taken by Pincock and others that will be discussed in the next section. Nevertheless, Daly and Langford reject this account on the grounds that the notion of a structural similarity is ill-defined and potentially as problematically abstract as mathematical entities themselves. Structural similarity could mean either that there is an isomorphism between the entities of two different domains, or it could mean that two different domains have all the same relational properties between entities. Daly and Langford find either definition of structural similarity insufficient on the grounds of the distinction between something being a heuristic device for identifying a feature, and being responsible for the possession of the feature. Similarly, we can distinguish between something being a heuristic for identifying unification and something being responsible for the unification. They hold that structural similarities between systems do not license the move whereby we add the mathematical structure to our ontology and thereby increase the ontological parsimony and unification of the theory. The fact that two systems might be describable with the same mathematical structure does not indicate that there is any ontology described in identifying a structural similarity. The mathematics points to the similarity, but it is not responsible for it.[9]

[8] *ibid.,* p. 648

[9] *ibid.,* p. 647

Colyvan's second example is particularly interesting because the fact in question, that there are antipodal points on the earth's surface at any given time that have the same temperature and pressure, is derived from a theorem in the mathematical domain of algebraic topology. This is also the most relevant of the three because it most closely accords with the mapping account of applied mathematics that I will discuss in the next section. In this example, the proof of the mathematical theorem becomes a proof of the physical fact by modeling the distribution of temperatures at various points on the earth using algebraic topology. This seems to lend credence to the notion that the mathematics involved is as explanatory as anything, given that the *explanandum* cannot be explained without the reference to the mathematical proof.

As I have indicated, the antipodal weather example is a bit trickier, and the fact that Daly and Langford pay particular attention to this example supports this contention. Daly and Langford attempt to categorize this example along with the others as another case of mere indexing. The earth's surface has spatial regions and features which can be picked out and tracked by the mathematical entities of algebraic topology. They argue that the mathematical theorem again supervenes on a more fundamental fact about the world, and that the features of the world (the actual physical regions of the earth's surface) are the genuinely explanatory aspects of this physical fact.[10] The role of the mathematical proof is to justify acceptance of the mathematical theorem, which in turn can be applied to the physical world. Perhaps, they claim, the proof can be thought of as justifying our acceptance of the physical fact, but the theorem itself is merely a fact about a model of the physical world, not a fact about the physical world itself.[11] Just like in the other two examples,

[10] *ibid.,* p. 648
[11] *ibid.,* pp. 648-9

they contend that the role of the mathematics is to index or model the physical world, and the physical world is what contains the genuinely explanatory things. This shows merely that mathematics is very useful as a tool, but it does not show that mathematics is explanatory or that we should accept that these entities exist.[12]

This divide between mathematical models and more fundamental physical facts is of key importance to this debate. Daly and Langford's objection to the antipodal weather example seems to rely on the premise that only physical entities which can rightly be considered causal, if only in the sense that a region of space can be causal, can be considered explanatory. But as I've noted, it is clear to all parties to this debate that explanation must be non-causal to accommodate mathematics. This causal account of explanation is question-begging for evaluating potential examples of mathematical explanations, and I think that one can still reject the explanatory indispensability of mathematics without doing so on the mere basis that mathematical entities are not causal. Daly and Langford want to reject the explanatory value of mathematics on the grounds that its role is to model the physical world. It seems plausible that this is indeed the role of mathematics in science, but it also seems inappropriate to reject mathematical explanations on this basis, because there are no explanations or facts without modeling. This is the case whether or not mathematics involved. Any explanation involves representation, whether it be in terms of mathematics or some other portion of our language. If some of our explanations incorporated modeling whereas others did not, it might make sense to look askance at model based explanations. But since all of our explanations inherently involve representation of some sort, to reject mathematics from explanations on this basis seems misguided.

[12] *ibid.*,

3.2 Baker on Indispensability and Mathematical Explanation

Alan Baker is the second key author who enters this debate on the side of those who contend that applied mathematics can be genuinely explanatory, but he takes issues with the case that Colyvan makes for this position. In particular, he finds Colyvan's three examples unsatisfactory, and he also finds Colyvan's formulation of the indispensability argument, especially its concomitant commitment to Quinean holism, to be unnecessary. Baker thinks that the indispensability argument which is based on confirmation holism lacks the resources to deal with instances of applied mathematics which involve idealizations and other falsehoods. Using whole theories as the unit of confirmation, the indispensability motivated Platonist is unable to account for the variety of roles that mathematics plays in science, some of which do not seem to involve the literal description of reality using objects that we can therefore take to exist.[13] This position anticipates some of the concerns that will become central in the next section of this thesis. On the basis of these concerns, Baker opts for a formulation of the indispensability argument which explicitly incorporates the notion that the important theoretical virtue that indispensable instances of applied mathematics must contribute is explanatory power. By specifying something beyond mere indispensability of reference, it becomes possible to evaluate the posits of applied mathematics on an individual basis, and it enables an account of the various roles that mathematics might play in science in the way that Baker wants.

Baker's rejection of Colyvan's examples, the antipodal weather example in particular, differs from that proffered by Daly and Langford. According to Colyvan, the use of the mathematical theorem to explain the antipodal weather behavior is essential and explanatory because only the mathematical theorem can explain the

[13] Baker [2005], p. 224

coincidence of the antipodal points with the same temperature and pressure. If we were to attempt the sort of causal description of this phenomenon that Daly and Langford's response to Colyvan suggests, we could explain why the individual points have the temperature and pressure that they do, but we could not explain the fact that there are these two points which mirror each other the way the mathematical theorem predicts. The causal history cannot explain the coincidence, but the mathematics can. Colyvan says that to be an explanation, the *explanans* must make the *explanandum* less mysterious, which is what he argues that only the mathematics can do in this case.

Baker thinks that this example does not manage to be a genuine explanation, because the existence of antipodal points with matching temperature and pressure is not something that is of scientific interest apart from the fact that the mathematical theorem was discovered that predicts the behavior. Meteorologists would not search for this phenomenon, and they presumably did not know about it before the mathematical result was discovered, and if they had, they likely would not have ranked it as the sort of phenomenon that demands an explanation, argues Baker.[14] Since the *explanandum* in this case is not the sort of thing that would occur to us in the absence of the *explanans*, Baker does not consider this a case of mathematical explanation, but rather a prediction.[15]

Melia's indexing argument is also unacceptable to Baker on the grounds that it seems question begging, at least *prima facie*. The possibility of genuine mathematical explanation of empirical phenomena is dependent on the possibility of non-causal explanation. Baker thinks that Melia's position appears to rely on the claim that non-causal entities cannot be genuinely explanatory. In the end, he notes that Melia

[14] *ibid.,* p. 226
[15] *ibid.,*

hedges on this point a bit, claiming that empirical investigation will reveal the existence of non-causal explanations if there are such things. However, as Baker notes, Melia does not give us any non-question begging reason to suppose that it is unlikely that an empirical investigation would discover such explanations. Colyvan's examples do not demonstrate mathematical explanation, says Baker, but we have no reason to think that there are not such examples, and Baker attempts to provide one for the remainder of his paper.

Baker gives a particularly unique and compelling example from evolutionary biology which he claims is genuinely explanatory.[16] The so-called periodical cicadas are a type of insect that are dormant for long stretches of time at one stage of their life cycle, before emerging in their adult form. There are two subspecies of this sort of cicada, with a dormant period of 13 or 17 years. The explanatory question for evolutionary biologists concerning periodical cicadas is why they have the particular life cycle lengths that they do.[17] Scientists are interested about the lengths of the life cycle because they are unusually long and because they are very particular in length for the different subspecies. In looking at the two distinct subspecies, one particular question emerges from these general curiosities. Why do the periodical cicadas have life cycles that are always a prime number of years long?

There are two main possible explanations for the the periodical cicada lifespans. The first explanation is that, in its evolutionary past, the periodical cicada had to contend with periodic predators, and it would be advantageous to minimize coexisting with these predators, so a cycle that intersected with that of predators as seldom as possible would be very beneficial. A second explanation suggests that the subspecies benefit from not hybridizing from one another, so intersecting with other

[16] *ibid.,*
[17] *ibid.,* pp. 229-31

subspecies would be detrimental. In both of these possible explanations, the utility of a prime length life cycle comes from a simple number theoretic fact, namely, that prime numbers maximize their lowest common multiple with other numbers.[18] In practical terms, it simply means that cicadas which develop a prime length life cycle will run into predators and other subspecies less often that cicadas with a non-prime life cycle. Baker's contention is that the primeness of the life cycle explains the length of the life cycle, because natural selection favored primeness for one of the reasons given above.

The cicada example is a genuine case of mathematical explanation because, unlike the antipodal weather example, the *explanandum* is considered independently interesting and was discovered independently of the *explanans*. For these reasons Baker argues that this is a genuine explanation, but someone like Melia might still question whether the mathematics involved is essential to the explanation. To test this, Baker evaluates the example in light of the major accounts of explanation to evaluate whether the mathematics is serving in an explanatory capacity. He looks at three prominent accounts of explanation: the causal account, the deductive-nomological account, and the pragmatic account. The causal account automatically rules out mathematical explanation because abstracta are non-causal, so Baker disregards this account because to accept it is to beg the question against the Platonist. The deductive-nomological account can incorporate the cicada example if we consider mathematical theorems to be laws of nature, and given their universality and necessity it seems plausible to do so. The pragmatic account can also incorporate the cicada example because, in keeping with the pragmatic account, the involvement of prime numbers makes the *explanandum* more likely than any alternatives.[19] Fur-

[18] *ibid.,* p. 231
[19] *ibid.,* p. 235

thermore, Baker contends that this example seems to mesh with what the biologists who are interested in the cicadas think.

3.2.1 Daly and Langford's Response to Baker

Daly and Langford argue that the periodical cicada example is not a case of mathematics genuinely explaining a physical phenomenon. They acknowledge that it would be question begging to reject the example on the grounds that numbers are non-causal, and they grant that the cicada example is a genuine explanation and that it involves reference to numbers, but they disagree that the reference to numbers is genuinely explanatory.[20] Instead, they choose to pursue the argument that the cicada example is not a case of mathematical explanation because the involvement of the mathematical entities is arbitrary and it has no essential significance to the explanation. Daly and Langford contend that because the primeness of the life cycle depends on choosing years as the unit of measurement and Baker offers us no plausible reason to choose years as the unit of measurement over something like seasons, the supposedly explanatory mathematical entities (the prime numbers) are not essentially involved in the explanation. They argue that the life cycle of the periodical cicadas could just as well be measured in seasons or in months and that there is, at least *prima facie*, no reason to suppose that years have a special significance.[21]

I find this response to Baker inadequate for the following reasons. First, years are significant in the life cycle of the cicada because the cycle is always the same whole number of years. The emergence of the cicada is triggered by spring temperature changes (a yearly event) in the year they emerge. Both of these facts seem to indicate that years are particularly significant to the periodical cicada. Second, as Baker

[20] Daly and Langford [2009], p. 651

[21] *ibid.,* p. 652

argues elsewhere,[22] years are the chosen unit of measure by the scientists who study periodical cicadas, so clearly they believe that the 17 year length of the cycle has more significance than the 204 month length. Third, the fact that a unit of measure has equivalences (which of course they all do) has no bearing on the relevance of a unit of measure in a particular application, especially when it comes to natural cycles. Furthermore, the mathematical component of the explanations cannot be ignored because without it the explanation is incomplete. There is no natural selection explanation of the length of the life cycle of the periodical cicada that does not take the *explanandum* to be the primeness of the length of the cycle in years.

Daly and Langford anticipate this response and reply that the fact that biologists use the year as the unit of measurement when studying this phenomenon does not entail that the year is explanatorily privileged.[23] Other units of measurement are applicable to describe this phenomenon. They contend that this phenomenon can be explained nominalistically, making reference only to the concrete phenomena, namely, the durations of the life cycles. The duration of the cycle can be described numerically using any one of a number of different units of duration, but it is the duration itself, which is not essentially prime in length, and its property of minimizing intersection with predators, that explains the duration of the life cycle that we observe.[24] This is merely another case where mathematics plays an indexing role, picking out the concrete entities which do the real explanatory work. Daly and Langford conclude that nothing Baker has said rules out a nominalistic reading and account of the cicada example.

The literature just examined is a debate of the legitimacy of mathematics in scientific explanations. As I have indicated, I agree with Baker that mathematics does

[22] Baker [2009], p. 617
[23] Daly and Langford [2009], p. 653
[24] *ibid.,* p. 657

play an ineliminable and genuinely explanatory role in scientific explanations, as his example makes clear. In another body of literature, a number of philosophers take for granted the affirmative answer to the question of the ineliminability and explanatory necessity of applied mathematics, and debate some of the nuances concerning exactly how mathematics plays the role that it does in scientific theory. Some philosophers, such as Christopher Pincock, argue that mathematics figures in science by way of a 'mapping' between some mathematical structure, and empirical reality. Otavio Bueno and Mark Colyvan expand upon Pincock's account and try to define more precisely what exactly is required in order to use mappings. By contrast, philosophers like Robert Batterman use examples from scientific practice to raise questions about the cogency of a mapping view.

4. THE APPLICATIONS LITERATURE

4.1 Mapping Accounts of Applications

4.1.1 Pincock's Mapping Account

The literature examined in the previous section concerns the ontological and explanatory legitimacy of mathematics in scientific explanations. In another body of literature, a number of philosophers take for granted the ineliminability and explanatory necessity of applied mathematics, and instead debate some of the nuances concerning exactly how mathematics plays the role that it does in scientific theory. Some philosophers, such as Christopher Pincock, argue that mathematics figures in science by virtue of a 'mapping' relationship between mathematical structures and empirical reality. Otavio Bueno and Mark Colyvan expand upon Pincock's account and try to define more precisely what exactly is required in order to use mappings.[1] By contrast, philosophers like Robert Batterman use examples from scientific practice to raise questions about the cogency of a mapping view.

The initial impetus for Pincock's version of the mapping account comes from the so-called problem of 'mixed statements.' Mixed statements are those that contain both empirical and mathematical terms. Pincock begins with the simple example 'The satellite has a mass of 100 kg.' Statements of this sort, he argues, depend for their truth on a 'mapping' between a mathematical object and empirical reality. These mappings are intended to possess structural properties that correspond to properties of empirical reality, so Pincock calls his account structuralist.[2] For simple physical properties, the mappings are straightforward. Points in space are mapped

[1] Bueno and Colyvan [2011], p. 1
[2] Pincock [2004], pp. 145-6

on to a real number coordinate system, for example. The statement above involving mass would require for its truth a mapping between a standard kilogram, the satellite, and the natural number 100. For Pincock, mappings are relations, conceived as intensional entities distinct from the tuples of objects that stand in those relations.[3] For mappings more complicated than these, Pincock says that the structural relation is not quite an isomorphism, because there is undoubtedly structure which is either added or not preserved by the mathematics used.

In a later paper, Pincock expands on this rudimentary version of the mapping account. In "A Role for Mathematics in Empirical Science," Pincock gives a clearer account of the notion of a mapping in a way that speaks to some of the concerns raised by nominalists. This paper speaks to the ontological side of the debate more than any of the literature than I am examining in this section, and it does so in a way that anticipates my biggest conclusion, namely, the authors that focus on scientific practice and mostly disregard the ontological issues discussed in the previous two sections end up having the best line on the problem of applied mathematics.

In this paper Pincock takes it as his task to reconcile the *theoretical indispensability* of mathematics with its *metaphysical dispensability*. The case for the theoretical indispensability is just the various formulations of the indispensability argument, which I have already covered at length. The metaphysical dispensability of mathematics is associated with the mathematical anti-realist positions that I have looked at, but for Pincock, who is not a nominalist, the case for the metaphysical dispensability of mathematics is simply that it does not interact causally with other things in the world. This by itself does not entail nominalism, of course, since all of the authors I have examined so far would certainly agree that mathematical *abstracta* are non-causal. Pincock contends that advocates of nominalism and platonism in this

[3] *ibid.,* p. 151

debate differ according to which of these two principles they prefer to emphasize.[4]

Pincock's main conclusion about the role of mathematics in empirical science is that applied mathematics allows scientists to make claims about the large-scale features of physical systems while remaining neutral about the micro-scale features.[5] Furthermore, theories which don't use mathematics will not be as well confirmed as those that do. As previously indicated however, Pincock does not advocate this position in order to vindicate Platonism. The role that he specifies for mathematics is intended to be consistent with what he considers the guiding principles of nominalism and Platonism, namely, metaphysical dispensability and theoretical indispensability. Nevertheless, the account that Pincock gives presents problems for some of the major nominalist and Platonist accounts. Mathematics is indispensable for scientific theories, Pincock claims, but in order to play the role just described, it must be in large part confirmed prior to being applied. This view of course runs counter to that of the indispensability motivated Platonist.

The main addition to the mapping account that Pincock adds in this article is the notion of an 'abstract explanation.' An abstact explanation, as defined by Pincock, is an "...explanation that appeals primarily to the formal relational features of a physical system."[6] Pincock's purpose in introducing abstract explanations is to block the conclusions of those who deny theoretical indispensability. These sorts of explanations have very different features, Pincock argues, than the sorts of mathematical explantations involving mappings onto numerical coordinate systems which can be called into question on the basis of arbitrariness in the choice of units. The example he gives of an abstract explanation is the Euler example. The Euler example concerns the bridges in Königsberg. There are seven bridges connecting four pieces of

[4] Pincock [2007a], p. 254
[5] *ibid.,* p. 255
[6] *ibid.,* p. 257

land in Königsberg. The layout of the bridges makes it impossible to cross all of the bridges once and only once in a continuous path. The explanation for this fact can be given in terms of a mathematical representation of the bridges. The setup is as follows: we treat the pieces of land as vertices and the bridges as edges, considering the vertices in terms of their valence, or how many edges connect to them, understanding a path to be a series of edges that connect via common vertices, and finally, we define a graph as Eulerian if and only if there is a path that begins and ends at one vertex and contains each edge once and only once. Given this way of mapping the bridges with a graph structure, we can explain the fact that it is impossible to cross all of the bridges once and only once by virtue of the fact that the graph which maps the structural relations between the bridges is non-Eulerian.[7]

The Euler example is intended to give a clear cut case of a mathematical explanation of a physical fact which fits Pincocks picture of a mapping accounting for the large scale features while disregarding the micro level ones. Importantly, it also does not rely on a mapping of physical quantities onto a system involving arbitrarily chosen units, which some have questioned (as seen in the previous section). Pincock's contention is that the Euler example clearly relates directly to the actual structural properties of the bridges, and so truly gets at the fundamental features of the physical system. Pincock advances this concept for reasons that are tangential to this thesis, but it addresses the indexing account discussed in the previous section as well. Rather than merely indexing some physical quantities, as Daly and Langford would likely argue, Pincock's contention is that the graph structure used in this mapping represents the important and fundamental features of the system.

[7] *ibid.*, p. 258-9

4.1.2 Bueno and Colyvan's Inferential Account

Otavio Bueno and Mark Colyvan offer an account of applied mathematics that is intended to address some shortcomings of Pincock's account. They call this account the inferential conception of applied mathematics.[8] They share with Pincock the central intuition of the mapping account, that mathematics is a rich source of structures, and the utility of mathematics in empirical science comes from the mathematics capturing and representing structural features of reality. The account they offer differs from Pincock's account because Bueno and Colyvan consider mapping to be a part of the larger process of applying mathematics. The structural similarities that the mapping relation identifies are only a part of what scientists are concerned with when using mathematics, and Bueno and Colyvan's broader account brings in pragmatic and other considerations. This involves a less strict notion of a mapping, but this leeway allows the inferential conception to deal much better with the problems surrounding idealization, which I will discuss in more detail in the following section on Batterman's objections to mapping.

The main point of contention between Pincock's simple mapping account and the inferential conception is the notion of a mapping. Bueno and Colyvan argue that Pincock's account leaves this key notion critically underdefined. They illustrate the mapping intuition with the fitting example of a map. A map represents many structural and relational features of the area that it maps, but some structure is left out of even the best maps. The actual geography has more structure than the map does. In the case of the mathematical maps used in science, the reverse situation may obtain, where the mathematics may contain structure that is absent from the situation that is being mapped.[9] Given these different possibilities, Bueno

[8]Bueno & Colyvan [2011]
[9]*ibid.,* p. 3

and Colyvan raise the question of what sort of relationship a mapping should involve (e.g. isomorphism, homomorphism). They note that Pincock does not offer much in the way of an answer to this question. In some of the simpler examples discussed in the foregoing section, Pincock refers to isomorphisms, but he also says that this is far from necessary in many instances.[10]

One important feature of the inferential conception is that it takes a neutral response to the ontological question. This account of applications does not require commitment to mathematical entities, or the denial of those commitments, in order to go through. To anticipate the final section of this thesis, this fact, along with the strengths and richness of the account itself, suggests an approach to the problem of applied mathematics that mostly disregards the ontological question. In this respect the inferential account is similar to Pincock's account[11] as well as Batterman's account discussed below. For this reason, it seems that a focus on mathematics as a resource which enables modeling and inference, whatever conclusions we might reach on the details, offers a better approach than that taken by the indispensability focused authors examined in the previous chapter of this thesis.

The centerpiece of the inferential conception is the immersion/interpretation framework for applications. This framework breaks down the application process into three steps. The first step, *immersion*, sets up the mapping relationship which will be used for the relevant situation with a mathematical structure of some sort that suits the purpose. The second step, *derivation*, involves using the mathematical structure to draw inferences about the system as it is modeled. The final step,

[10] *ibid.,*

[11] Pincock's account does make use of potentially questionable intensional entities, as he himself acknowledges, but regarding the nominalism/Platonism debate in particular, his view does not seem to exclude either side. He is a mathematical realist in the semantic sense to be sure, but as we discussed in section 2, semantic realism does not entail metaphysical realism despite what other dubious entities he may invoke.

interpretation, consists of the opposite operation as the immersion step. In this step the consequences drawn within the mathematical formalism are interpreted as consequences about the physical situation.[12] A key distinction to understand between this schema and Pincock's account is that the immersion and interpretation steps need not be simply inverses of one another. This may be the case, but the process does not commit one to using the exact mapping relationship invoked in the immersion step when re-physicalizing the inferences that have been drawn. Scientists enjoy significantly more latitude in deducing what their mathematical inferences mean in empirical terms on this account, whereas on Pincock's account the mapping relation is considerably more fixed.

In contrast with Pincock's account, the inferential conception is not purely structural insofar as pragmatic concerns relating to the process of applying mathematics are allowed to influence what mathematical model is chosen.[13] Bueno and Colyvan's approach is intended to cover the role of applied mathematics as it is used in the theorizing of working scientists. This role, they contend, is to enable inference about empirical phenomena via mathematical structures. Or to put it another way, mathematical modeling is used in science because it lets scientists draw inferences about the empirical world via mathematical machinery that would otherwise be "extraordinarily hard (if not impossible) to obtain."[14] Bueno and Colyvan acknowledge that there are other roles which applied mathematics does play, such as unification of theories, enabling prediction, and providing explanations. Nevertheless, the role of mathematics in empirical science is to enable inference, which is necessary for generating novel predictions and explanation as Bueno and Colyvan go on to argue.[15]

[12] *ibid.,* p. 9
[13] *ibid.,* p. 10
[14] *ibid.,* p. 8
[15] *ibid.,*

The differences between the inferential conception and Pincock's mapping account are subtle but significant. Bueno and Colyvan argue that the inferential conception is not subject to two significant problems that beset the Pincock's account. I'll call these two problems the problem of explanation and the problem of idealization. I have already alluded to the problem of idealization and as I have indicated, it will play a central role in the next section. Bueno and Colyvan characterize the problem as an incompleteness in the mapping account; if there are some applications which involve a mathematical structure that does not map on to the physical world, then the mapping account at least owes us something to explain these examples.[16] The problem of explanation is more novel at this point. If mathematics is to be thought of as a representation system for modeling the physical world, as the mapping account recommends, Bueno and Colyvan find it hard to reconcile this with mathematical explanations, for which there seems to be at least a prima facie case. Again, they contend that the mapping account needs to account for mathematical explanations if it is to be a complete account of applications.[17]

Bueno and Colyvan argue that the inferential conception is not subject to these two problems.[18] To solve the problem of idealization, they introduce the notion of a *partial mapping*. As one might suspect, a partial mapping is a mapping between a mathematical structure and *some* aspects of the physical situation. Even in cases of idealizations, where there is no complete mapping of the physical world onto a mathematical structure, it is still the case that some aspects of the physical situation map onto the mathematical structure. The existence of this partial mapping explains why the idealization works to the extent that it does.[19] Bueno and Colyvan

[16] *ibid.,* p. 6
[17] *ibid.,* p. 7
[18] *ibid.,* p. 12
[19] *ibid.,* p. 13

provide a formalism for partial relations, where the *partial structure* that accounts for an application contains the domain of relevant physical and mathematical objects, as well as a set of relations between some of the elements of the domain. The incompleteness of a partial structure can be read ontologically or epistemically, and Bueno and Colyvan take no stand on this. Given the notions partial stucture and partial mapping, they suggest that in these cases the mapping relation is a partial homomorphism or partial isomorphism.

The solution that Bueno and Colyvan offer to the problem of explanation is a bit more sparse. They claim that mathematical explanations require that one draw appropriate inferential relations between the mathematical structures used and the empirical set up. To enable mathematical explanations, the immersion step and the interpretation step must be mathematically sound and empirically significant.[20] The example they use is predator-prey pairs in cyclical populations. Briefly, populations of organisms that exhibit certain cyclical behavior cannot be explained without stipulating a predator-prey relationship. Bueno and Colyvan say that this fact is explained as a mathematical explanation; the differential equations used to model population do not admit of periodic solutions unless they are second order differential equations. These facts about the differential equations used to model the system become an explanation for the empirical fact that emerges when we interpret the need for a second order differential equation as a need for a predator-prey pair. This mathematical explanation can only go through if suitable inferential relations between the biological domain and the mathematical structure are drawn at the immersion and interpretation stages. As Bueno and Colyvan put it:

> In particular, one needs to establish a biologically significant interpretation of periodic solutions, and a mathematically sound reading of the

[20] *ibid.,* p. 20

> predator-prey pair. The latter is achieved in the immersion stage, and the former in the interpretation step. In other words, attention is needed both in the immersion and in the interpretation stages, so that the relevant mathematical facts...can be invoked to yield the appropriate derivations.[21]

This is certainly an illustrative example, but problems still arise for Bueno and Colyvan when it comes to accounting for explanations arising from idealizations, as I will discuss in the next section.

4.2 Batterman's Objections to Mapping Accounts

Robert Batterman has offered a thoroughgoing critique of the various mapping views based on the problem of idealization. The particular issue that drives this critique is that the mapping accounts fail to provide an adequate account of mathematical explanation. Batterman's main thesis is that the mapping accounts are unable to deal with a certain type of idealization that frequently occurs in science, and this failure prevents them from being a full account of mathematical explanation. Whereas the advocates of the mapping views attempt to mitigate the presence of idealizations with additional theoretical constructs, Batterman's contention is that idealizations are often necessary for the theory with the greatest explanatory power. Like the previously discussed authors in this section, Batterman largely leaves the ontological question to the side. He works from the assumption that there are indeed mathematical explanations, but he avoids ontological conclusions with the contention that many instances of mathematical explanation do not require an accurate mapping of reality onto mathematical entities, given a particular sort of idealization which he discusses at length.

[21] *ibid.*,

Batterman's objection to mapping accounts starts from the seeming fact that the presence of idealizations seems to indicate that the mathematics does not correspond with the physical world.[22] If a mathematical model contains structural features which are known not to be representative of reality, then it seems that mathematics can play the role it does without needing to map reality in a structure-preserving way. Batterman's critique of the accounts offered by Pincock and Bueno and Colyvan centers around the emphasis put on explanatory value. The mapping accounts focus their attention on representational accuracy, or how well the mapping preserves the structure of reality, while neglecting to account for what aspects of mathematical models (including idealizations) contribute to the explanatory power of theories.[23] To quote Batterman: "How does having a representation or a partial representation of a physical situation in mathematical terms provide an *explanation* of that physical situation."[24]

Batterman suggests that, in order to reconcile the mapping account with the prevalence of idealizations, one would need to utilize what he calls a 'de-idealizing story.' A de-idealizing story is an account of how a theory which contains an idealized use of mathematics can be corrected by removing the idealized portions.[25] The idea is that, despite the presence of known falsifications, there is no problem because they can, in principle, be removed. This sort of solution seems compatible with the sorts of idealizations that are the result of ignoring some structural elements of the physical world when choosing a mathematical model, which were alluded to by Bueno and Colyvan. While Batterman's ultimate claim is that a de-idealizing story will not work in come contexts, even this partial solution is at odds with the mapping

[22] Batterman [2010], p. 10
[23] *ibid.,* p. 16
[24] *ibid.,*
[25] *ibid.,*

accounts, because Batterman argues that the notion of a de-idealizing story requires that one accept a global ranking of idealizations, which neither Pincock nor Bueno and Colyvan seem to want to do.[26]

Pincock recognizes the problem posed for his account by idealizations and he offers a schema for understanding instances of applied mathematics that involve them.[27] Pincock maintains that different idealized models can be evaluated and ranked against one another, and more importantly, a fully accurate, perfectly exact mapping. To this end, he posits so called 'equation models' and 'matching models.' Equation models are those used by scientists, which may or may not contain idealizations. Matching models are the hypothetical models that perfectly characterize every aspect of the physical situation. Given this distinction, different equation models for a particular situation can be evaluated or ranked on the basis of their relationship with the matching model for the situation.[28] It is important to note, however, that Pincock's ranking project is not a global measure, but rather one that factors in context relevant thresholds of accuracy.[29] This relationship is evaluated in light of the goals of the scientists using the model: depending on what the model is being used for, different margins of error will be appropriate. Pincock says that if there is a mathematical transformation between the equation and matching models which falls within the appropriate margin of error for the parameters of interest, then the idealization is good or adequate. This sort of mathematical transformation is essentially what Batterman intends when he talks about a de-idealizing story, but since Pincock denies that this is a global ranking, it seems that his measure of the goodness of an idealization does not allow us to compare different idealized models,

[26] *ibid.,* p. 17
[27] Pincock [2007b]
[28] *ibid.,* p. 962
[29] Batterman [2010], p. 12

which Batterman sees as problematic.[30]

If Pincock's solution is something like a de-idealizing story, then it does meet the preliminary standard that Batterman gives for idealizations, at least in some cases. The problem that Batterman has with this solution is that it does not account for all of the idealizations in science. Mathematics gets used in science in ways where, not only is there no mapping between the mathematics and reality, but there is no way to connect the equation model to the matching model in the way that Pincock's solution suggests. Batterman calls such cases 'non-traditional idealizations' and he gives detailed treatment of a few different examples.[31] Non-traditional idealizations are characterized by what he calls 'asymptotic reasoning.' The first thing to note about asymptotic reasoning is that it is an *operation*, by means of which idealizations are introduced which increase the explanatory power of a model. These sorts of idealizations arise by taking a certain parameter of the system to a limit. This does not quite square with the mapping account, which Batterman characterizes as involving static mappings of empirical situations to mathematical structures.[32]

An example of a non-traditional idealization is the scientific description of rainbows. According to Batterman, the spacing of bows in rainbows is a structurally stable property across rainbows, which cannot be explained in terms of light wavelengths of any finite length. To explain the structure of the rainbow, we must take the limit of the wavelength as it approaches zero. In other words, we must leave a wave account of light in favor of a ray account in order to successfully account for the universal pattern found in rainbows.[33] The key thing to notice about this example is that the idealization is not the sort of simplification that can be de-idealized. There

[30] *ibid.,*
[31] *ibid.,* p. 17
[32] *ibid.,* p. 10
[33] *ibid.,* p. 21

is a discontinuity between the wave model of light, which we know to be more representative of reality, and the ray model, which we must use in order to explain the spacing of rainbows. This discontinuity is at odds with the matching model/equation model schema given by Pincock. The description of the rainbows as given by the ray model is incommensurate with the wave model, so it seems impossible to judge how far this model is from the matching model involving waves. Put simply, idealizations which are continuous with the matching model can potentially be evaluated and de-idealized in the way that Pincock suggests, but these sorts of non-traditional idealizations cannot be.

Batterman's criticism of the inferential conception runs along similar lines to his criticism of Pincock's account. Despite the dissimilarities between the inferential conception and Pincock's account, Batterman finds their attempted solution to the problem of idealization, namely the notion of a partial mapping, insufficient to account for the explanatory value of applied mathematics in idealized contexts. Batterman finds partial mappings very similar to Pincock's equation models.[34] For similar reasons he asks why we should think that a partial representation contributes to the explanatory power of a theory. He looks at the account given by Bueno and Colyvan and finds hints towards an answer to this question. It seems that Bueno and Colyvan's partial mappings rely for their explanatory usefulness on the implicit assumption that the possibility exists of replacing them with less idealized models that account for more details than the more idealized model.[35] This type of solution is very similar to what Pincock offered with the notion of ranking idealizations, and Batterman finds it flawed for similar reasons. Before ending this section, I need to note that ultimately neither Pincock nor Bueno and Colyvan want to pursue a

[34] *ibid.*, p. 14
[35] *ibid.*, p. 15

solution to the problem of idealizations that implies that their explanatory value is derivative of the greater explanatory value of less idealized models. Nevertheless, Batterman finds their reluctance at odds with their desire to evaluate idealizations in terms of representational accuracy.[36]

4.2.1 Pincock's Response to Batterman

Pincock maintains that his mapping account can incorporate non-traditional idealizations with the addition of a bit more machinery. He argues that Batterman's non-traditional idealizations are a species of abstract explanation.[37] Non-traditional idealizations will simply require an additional component to account for the explanatory power of the (non-representative) model. In addition to the matching model (model A) and the idealized model (model B), non-traditional idealizations will also require a third model (model C) which is given in terms of both models A and B. Pincock calls this an intermediate model.[38] Pincock thinks that the mapping account is potent enough to construct models of the description of A, B, and C, and that this accounts for the gap between a matching model and the idealized model in cases of non-traditional idealizations.

I find this response flawed because the notion of an acceptable mathematical transformation seems exactly like what Batterman calls a de-idealizing story, and so the addition of an intermediate model remains subject to the same problems relating to non-traditional idealizations that Batterman raises. It is not clear what an intermediate model adds to Pincock's account. Presumably he thinks an intermediate model would bridge the gap between A and B, and allow us to account for the mathematical transformation between the two, but constructing model C suffers

[36] *ibid.,* p. 16
[37] Pincock [2011], p. 213
[38] *ibid.,* pp. 213-6

from the very difficulty that it is meant to overcome. If A and B are mathematically incommensurate, the problem of constructing an intermediate model between the two is essentially the same problem as that of giving a mathematical transformation between the two. If this is right, intermediate models do not relieve the difficulty of non-traditional idealizations. I agree with Batterman that any account of the role of applied mathematics must be an adequate account of the explanatory power of applied mathematics. The problem of applied mathematics is to give an account of the role of applied mathematics, given that mathematics does not interact causally with the non-mathematical portions of scientific theory. Applied mathematics seems to make a unique contribution to scientific theory, and it appears that in our attempts to elucidate just what this role is, we have to pay close attention to explanatory practice.

Batterman's proposal relies on the assumption that mathematical explanation involves abstraction and unification. In his earlier work, Batterman identified two types of explanatory questions that might be asked. One might ask why a particular event occurred, but one might also ask why events of that sort tend to occur in general. The latter question concerns the universality of phenomena, and this is the type of explanatory reasoning that Batterman focuses on. In his discussion of the critical behavior of fluids as an example of non-traditional idealization, Batterman notes that the explanatory value of taking the limit of the number of particles comes from the fact that by doing so, one can explain the critical behavior of a wide range of fluids which display qualitatively similar behavior. We can explain the universal behavior of a wide range of fluids via our limiting operation, but not without it. The mathematical operation gives us an equation model which explains the structurally stable properties of not only different instances of the same fluid, but many different fluids. This account of fluid behavior is even applicable to magnets if we draw an appropriate analogy between net magnetization and our description of the fluid systems. It seems implausible that the applicability of the mathematics to such a broad range of empirical situations is due to the fact that there is an appropriate mapping in each case.

Asymptotic reasoning plausibly demonstrates that the mapping accounts of Pincock and Bueno and Colyvan cannot explain all idealization, since Pincock's intermediate model solution does not seem adequate for the reasons given above. Furthermore, non-traditional idealizations often play a crucial role in unifying diverse phenomena with a common mathematical description, as the fluid example makes clear. Therefore, if we find Batterman's arguments convincing, the overall lesson that we should draw from his examples is that explanation is the primary goal of scientific theory, not representation. Of the things that we think applied mathematics might do in science, scientific practice shows us that scientists will purchase increased explanatory value (i.e. increased unification and abstraction which generates tractable mathematical descriptions) at the cost of representation. So, whatever we can say about the representational content of scientific theories, scientific practice seems to consider representation to be subservient to explanation.

4.3 Idealization: Epistemic Worries

In addition to the concerns raised above, Pincock's account of idealized scientific models is subject to a significant epistemic difficulty. His account of idealization relies heavily on the notion of an (acceptable) mathematical transformation between the matching model and the idealized equation model. In certain cases, we have some notion of the way in which our accounts are idealized, and so an evaluation of the equation model relative to the matching model seems at least plausible. But if it is the case that the equation model that we are examining represents our best scientific account of the situation we are investigating, what can scientists have to say about the distance of our equation model from the matching model? If this situation occurs in science, I think this undermines Pincock's attempt to reconcile idealizations with the mapping account.

If our theories contain idealizations that we are ignorant of, this severs the matching model-equation model connection which is key for Pincock's account. His account requires that the relevant scientists believe that there exists an acceptable (relative to the context) mathematical transformation from the equation model to the matching model. Pincock describes three possible epistemic situations the scientists might occupy concerning these transformations, one of which seems similar to this sort of ignorance. In this epistemic situation, Pincock argues that successful prediction is sufficient evidence to convince us that our model is a good representation. The possibility persists that we will discover the mathematical transformation between our model and the matching model, but it may well never happen, and the matching model may turn out to be very different than what we imagined. The plausibility of such a situation seems like a problem for Pincock's account. In such cases, what could ground the belief that an appropriate transformation exists? If we can say nothing about the matching model, then any predictively adequate model is equally acceptable. This is not to say that predictive adequacy is not a good reason to accept a scientific theory, but merely that predictive adequacy is insufficient to infer representational accuracy.

5. CONCLUSION

I will close by suggesting some possibilities for conclusions to be drawn from the literature that I have examined in the preceding chapters. I have examined a number of approaches to the problem of applied mathematics under two broad themes. As I have previously indicated, I think that my examination of the applications literature suggests that approaching the problem of applied mathematics from an ontologically neutral perspective is the more fruitful option. All three authors examined in chapter 4 approach applied mathematics without any substantial commitment to one side or the other of the ontological debate. Bueno and Colyvan explicitly add the proviso that their account is consistent with realist and anti-realist views. Pincock, in his more ontological moments, positions himself as the mediator between Platonist and nominalist concerns, and his view seems consistent with either ontological position if the inferential conception is. Indeed, the indexing position seems like a primitive precursor to a mapping type view in some respects, and a Platonist position could be used easily to augment a mapping account. Batterman's view seems likewise consistent with either Platonism or nominalism. Platonism is not inconsistent with his rejection of the mapping view because whether or not the mathematical models used in science serve to accurately represent the physical world does not seem to bear on the ontological status of the entities used in the modeling system.

In addition to being consistent with either ontological position, an applications-focused approach seems to be a much better option for getting at the details of applied mathematics. Whereas the debate in the ontological literature centered around (and one might argue never moves past) examples drawn from science thought to provide examples of indispensable mathematics (and rejections of these examples), by taking for granted the explanatory indispensability of mathematics, Pincock, Bueno & Colyvan and Batterman are able to shed light on details regarding mathematical models that the ontological literature never approached. Even if the accounts offered by all three of these authors end up being insufficient, they represent a better general approach, because the ontological question seems to impede progress of our understanding of the issues of modeling and representation in applied mathematics. None of this is to deny the significance of the ontological question, but by linking the answer to this question to applications the way the indispensability argument does, some interesting questions about the explanatory significance of mathematical models are clouded.

In the end, it seems like some amended version of the inferential conception is the most promising way forward. If the account can be modified to address examples of explanatory idealizations of the sort that Batterman discusses, the inferential conception seems like a promising direction for future work. The flexibility of the inferential account with regards to the mapping relationship makes it an ideal starting point for an account of applied mathematics that incorporates the core idea of mapping, that mathematics provides structures that are ideal for modeling, with a more nuanced understanding of the realities of scientific practice, which seems at times to prize mathematical models for purposes other than accurately representing reality.

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