A Revealing Flaw in Colyvan's Indispensability Argument*

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Mark Colyvan uses applications of mathematics to argue that mathematical entities exist. I claim that his argument is invalid based on the assumption that a certain way of thinking about applications, called 'the mapping account,' is correct. My main contention is that successful applications depend only on there being appropriate structural relations between physical situations and the mathematical domain. As a variety of non-realist interpretations of mathematics deliver these structural relations, indispensability arguments are invalid.

1. Introduction. Mark Colyvan's *The Indispensability of Mathematics* (Colyvan 2001) presents the most vigorous and careful version of an indispensability argument for realism about mathematics since Putnam's original presentation of such an argument in his *Philosophy of Logic* (Putnam 1971).[1] The most compelling feature of Colyvan's argument is that it depends on only two premises:

1. We ought to have ontological commitment to all and only those entities that are indispensable to our best scientific theories;
2. Mathematical entities are indispensable to our best scientific theories.

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1. For two reviews see Cole and Shapiro 2003 and Peressini 2003.

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> Therefore:
> We ought to have ontological commitment to mathematical entities. (Colyvan 2001, 11, emphasis removed)

Premise 1 follows from Quinean naturalism, which is the doctrine that there is no non-scientific standard that we can use to assess our ontological commitments. Rather, we should look to our best scientific theories and accept the entities quantified over by these theories. Premise 1 is also supported by confirmational holism, although Colyvan maintains "that the argument can be made to stand without confirmational holism" (Colyvan 2001, 37). Holism of this type is useful because it blocks any attempt to allot confirmational support to the various parts of our best scientific theories in different ways. It is useful, for example, in thwarting Sober's objections to indispensability arguments based on the absence of contrasting mathematical and non-mathematical alternative theories. To the naturalist who is also a confirmational holist, the absence of alternatives is irrelevant to determining the ontological commitments of a theory.[2] In accepting the theory we commit ourselves to everything that it quantifies over.

As Colyvan recognizes, only one half of Premise 1 is needed for the conclusion to follow and in what follows I will consider the suitably weakened Premise 1′: "We ought to have ontological commitment to all those entities that are indispensable to our best scientific theories." 1′ can be accepted by many who would balk at the more controversial proposals that Quine and Putnam have made in the course of their respective discussions of applications of mathematics, e.g., Quine's inscrutability of reference. Premise 2 is also presented independently of any broader philosophical program. Colyvan supports it by arguing against Field's attempt to reformulate our best scientific theories in non-mathematical terms. As Field's program has many prima facie problems with carrying out such 'nominalizations' it seems that Premise 2 is on relatively solid ground.[3]

That said, I will argue in this essay that Colyvan's argument does not imply its conclusion, at least when the conclusion is interpreted in the realist way that Colyvan presents it. Briefly, while Colyvan claims to establish a metaphysical realism, or realism about mathematical entities, he is in fact only entitled to a semantic realism, or realism about the

2. See Sober 1993 and Colyvan 2001, §6.3 for Colyvan's reply. Essentially, Sober offers a competing theory of confirmation.

3. I will dispute this in Section 2. Colyvan also responds to Balaguer's brand of fictionalism which does not require that we actually formulate a nominalistic scientific theory. See Balaguer 1998 and Colyvan 2001, §3.6.1.

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truth values of mathematical statements. In fact, Colyvan himself appears to grant this point in the concluding chapter of the book, as we will see. There is a clear ambiguity in Colyvan's discussion of his argument's conclusion. I maintain that this ambiguity is no mere accident, but that it reveals the limitations inherent in any argument which attempts to draw conclusions about the correct interpretation of mathematical language based on how mathematics is applied in science. Thus my focus is much broader than Colyvan. If my objection to his argument is correct, then it should also undermine the other indispensability arguments for realism about mathematics, such as those attributed to Quine.

My diagnosis of the failure of this class of arguments focuses on their generality. Indispensability arguments attempt to draw conclusions from the applicability of mathematics without first considering the details of how these applications actually work. But once we introduce a workable account of how mathematical structures are applied to physical situations, it will become clear that such applications do not determine whether we should be realists or nominalists about the mathematical structures themselves. Both alternatives, I will argue, are consistent with all current applications of mathematics. At this stage, the defender of an indispensability argument for realism must either (i) reject the account of applications that I have offered in favor of another account or (ii) supply additional premises to rule out the non-realist interpretations of mathematics that are consistent with our current applications. Both of these options raise complications far beyond what Quine, Putnam or Colyvan have considered.

2. The Ambiguity. Colyvan presents his indispensability argument about mathematics as an instance of a wider class of scientific indispensability arguments:

> If apparent reference to some entity (or class of entities) ξ is indispensable to our best scientific theories, then we ought to believe in the existence of ξ. (Colyvan 2001, 7, emphasis removed)

To obtain the argument given above, Colyvan claims that "we need merely replace 'ξ'. . . with 'mathematical entities'" (Colyvan 2001, 11) . Of course, much more has in fact been changed. In particular, all talk of 'apparent reference' has disappeared. How does Colyvan move from the apparent reference to mathematical entities to Premise $1'$ and $2'$s straightforward appeal to mathematical entities? His idea seems to be that anyone who is at all persuaded by scientific indispensability arguments generally will not make any significant distinction between terms that appear to refer to entities that we cannot dispense with and entities that we should believe in.

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Only anti-realists of some type about science would try to defend such a distinction and so scientific realists "are my main target in this book" (Colyvan 2001, 8).[4]

This all suggests that for Colyvan, scientific realism, naturalism, and confirmational holism form an integral package. If one accepts these theses and the empirical information summarized in Premise 2, one should not only be a scientific realist, but also a realist about mathematics as well. But, what, after all, does realism about these two disciplines amount to? We might adopt van Fraassen's minimal definition of scientific realism as a claim about the goals of science and about what we believe when we accept a scientific theory: "Science aims to give us, in its theories, a literally true story of what the world is like; and acceptance of a scientific theory involves the belief that it is true" (van Fraassen 1980). Typically, a scientific realist will also add some claim about the truth (or approximate truth) of the theories we currently accept. It follows, then, that upon accepting our current scientific theories, the scientific realist is committed to the existence of unobservable entities such as electrons.

Colyvan distinguishes between two types of realism about mathematics, metaphysical and semantic. The contrast is introduced merely to make clear that he is concerned with metaphysical realism: "Since, in this work, my interests are metaphysical, I will be concerned with metaphysical realism, not semantic realism. I will, however, speak only of *realism,* by which I will mean *metaphysical* realism" (Colyvan 2001, 3). In its most usual form metaphysical realism is just the belief that mathematical entities exist. Colyvan adds more qualifications, presumably to avoid confusion:

> [A]ccording to this conception of realism, mathematical entities such as functions, numbers, and sets have mind- and language-independent existence or, as it is also commonly expressed, we *discover* rather than invent mathematical theories (which are taken to be a body of facts about the relevant mathematical objects). This is usually called *metaphysical realism.* Anti-realism, then, is the position that mathematical entities do not enjoy mind-independent existence or, alternatively, we *invent* rather than discover mathematical theories. (Colyvan 2001, 2)

So, the conclusion of Colyvan's argument must be that mathematical entities exist, and that this existence is mind- and language-independent.

Right away one might worry about this talk of independence. A variety of philosophical proposals introduce mathematical entities but tie their

4. See 25 fn. 7. Colyvan also refers to his indispensability argument as an "explanatory" argument (Colyvan 2001, 11). He thinks his argument is of the same general type as inference to the best explanation arguments for scientific realism. See Psillos 1999.

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existence to the mental or linguistic activities of mathematicians or humans more generally. Here we might think of intuitionists, as well as more contemporary versions of constructivism and predicativism. I agree that we should not call these positions 'realist,' but at the same time Colyvan has not said anything to rule them out. What is needed is another kind of indispensability argument against non-classical mathematics which shows that the various kinds of mathematics associated with intuitionism, constructivism, and predicativism are insufficient to do science. None of the examples that Colyvan considers are presented in this way, although he does mention in a footnote a paper by Hellman that attempts to do just this.[5] Until these arguments have been worked out, everything that Colyvan says is consistent with a non-classical interpretation of mathematics. This would ensure that some mathematical objects exist, but not that we must be realists about mathematics. To be fair, Colyvan explicitly claims that this view "is of little interest" (2001, 4).

Setting these worries aside, it is not even clear that Colyvan's premises imply that mathematical objects exist. His own discussion of the conclusion of the argument is quite confusing in light of his definition of metaphysical realism:

> As for the question of whether sets (and other mathematical objects) are sui generis or not, again the indispensability argument says nothing. They might be constituted by more mundane items such as universals and/or relations (as John Bigelow (1988), Peter Forrest and David Armstrong (1987) claim), patterns or structures (as Michael Resnik (1997) and Stewart Shapiro (1997) claim) or the part/whole relation (as David Lewis (1991) claims). Perhaps they are constituted by *more* exotic items such as possible structures (as Hilary Putnam (1967) and Geoffrey Hellman (1989) claim). In short, any (realist) account of mathematical objects is all right by the indispensability argument. (Colyvan 2001, 142)

What could Colyvan possibly understand by the term 'realist'? Consider the positions of Lewis and Hellman. According to Lewis' proposal, mathematics is the study of the mereological properties of some actual, concrete wholes and their parts.[6] If we make appropriate assumptions about the existence of such concrete wholes, and reinterpret mathematics accordingly,

5. Surveys of Hellman's worries are to be found in his 1992, 1993, 1997, 1998, and 1999. Vigorous criticism of Hellman's position can be found, e.g., in Bridges 1999 and Billinge 2000.

6. It is less clear whether Lewis himself thought of mathematics in this way. In what follows, I am assuming that Lewis 1993 can form the basis of a non-realist position that interprets mathematics in mereological terms.

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then the right mathematical claims come out true and false. This is not a realist philosophy of mathematics according to either the letter or the spirit of Colyvan's definition, for there are no mathematical entities, such as the natural number 2 or the real number structure, on the mereological interpretation of mathematics. This is because the subject matter of mathematics has been absorbed into Lewis' general mereology. Similar consequences follow from considering Hellman's modal-structural interpretation of mathematics. Hellman interprets mathematics in terms of the possibility of objects exemplifying various structural relationships, so he does not countenance any *actual* mathematical entities.[7] It is misleading for Colyvan to claim that in either case "exotic items" are involved.[8]

What Colyvan introduces in this passage is a distinction between autonomous or "sui generis" entities and entities that are "constituted" by objects of another type. Such talk seems perfectly appropriate when discussing the *ante rem* or realist versions of structuralism advocated by Resnik and Shapiro. Shapiro (1997) posits a natural number structure which has places for what the traditional realist would call genuine objects: the natural number 1, the natural number 2, etc. So, even though there are no 'freestanding' or distinct entities called "the natural numbers," we still have genuine mathematical entities. For the structuralist, mathematical entities are just the mathematical structures themselves that are used to interpret mathematical language.[9] But when we turn to Lewis' mereology or Hellman's modal-structuralism there are no genuine mathematical entities. It is not as if Lewis identifies some part of his actual, concrete wholes with the natural number 2, and so we could say that this part was the natural number 2 or that it was responsible for the constitution of 2. This is even clearer on Hellman's proposal. He invokes the possibility of natural number structures to interpret claims about the natural number 2. No entity, after such an interpretation of mathematics, is identical with the natural number 2 or the series of natural numbers.

The reference to Putnam's 1967 paper "Mathematics without Foundations" is particularly unfortunate because it is in this very paper that Putnam begins to argue that metaphysical realism about mathematics is untenable and that it should be replaced by semantic realism. Semantic realism about mathematics is just the belief that mathematical statements have objective truth-values, i.e., truth-values independent of our activities or abilities. After defining 'semantic realism,' Colyvan himself notes that Putnam's modal interpretation of mathematics is not metaphysically realist:

7. Cole and Shapiro (2003) also note Colyvan's puzzling claims about Hellman; see 333.

8. I thank one of the referees for emphasizing this point.

9. The same goes for Bigelow's proposal (1988), as, for example, natural numbers are identified with specific properties.

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> Hilary Putnam . . . points out that, according to this view [semantic realism], it is possible to be a mathematical realist without being committed to mathematical objects—realism is about objectivity not objects (Putnam 1979a, pp . 69–70).[10] For example, suppose mathematical discourse is reinterpreted so that it does not refer to mathematical objects and this reinterpreted discourse is true or false independently of our knowledge of it. Such a position satisfies Dummett's characterisation of [semantic] realism, and yet there are no entities peculiar to mathematics. Indeed, Putnam's own modal-structuralist account of mathematics is a nice example of a realist view of mathematics that has no mathematical objects. (Colyvan 2001, 2–3)

This is the view Putnam considered in "Mathematics without Foundations," which inspired Hellman's modal-structuralism.

Even more striking is that, in *Philosophy of Logic,* Putnam explicitly notes the limitations of indispensability arguments for realism about mathematics. When Putnam presents his argument for realism there, he has in mind what Colyvan calls 'semantic realism.' Here is how Putnam concludes his *Philosophy of Logic:*

> In this chapter, I have chosen to go into detail on one group of questions—those having to do with the indispensability of quantification over abstract entities such as sets—at the cost of having to neglect many others. One group of questions which I might have considered has to do with the existence of what I might call 'equivalent constructions' in mathematics. For example, numbers can be constructed from *sets* in more than one way. Moreover, the notion of *set* is not the *only* notion which can be taken as basic. . . . My own view is that none of these approaches should be regarded as 'more true' than any other; the realm of mathematical fact admits of many 'equivalent descriptions': but clearly a whole essay could have been devoted to *this*. (Putnam 1971, 356–357)

In fact a whole essay was focused on this issue and it was "Mathematics without Foundations." There Putnam includes his modal interpretation of mathematics in the list of acceptable 'equivalent descriptions' of mathematics: "Of the many possible 'equivalent descriptions' of the realm of mathematical facts, there are two which seem to me to have especial importance. I shall refer to these, somewhat misleadingly, I admit, by the titles 'Mathematics as Modal Logic' and 'Mathematics as Set Theory'" (1979, 46–47).

10. The reference is to "What Is Mathematical Truth?" in Putnam 1979.

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I suspect that in retrospect Colyvan would withdraw the claim that Lewis' and Hellman's interpretations of mathematics are consistent with the conclusion of his indispensability argument. The best alternative that I see is that he could revise the conclusion so that it expresses what Putnam had in mind, i.e., semantic (and not metaphysical) realism about mathematics. Barring this, Colyvan might motivate his rejection of Lewis and Hellman by maintaining that they violate Premise 2 of his argument: "Mathematical entities are indispensable to our best scientific theories." I would agree. Nonetheless, it is clear that the reasons Colyvan gives in favor of Premise 2 support only the more modest claim, which I will call Premise 2′: "Apparent reference to mathematical entities is indispensable to our best scientific theories." For recall how Colyvan supported Premise 2. He appealed to the failure of Field's program of giving nominalistic alternatives to our fully mathematical scientific theories. These alternatives either cannot be formulated, or when they are given, suffer from a variety of defects that render them inferior to our standard theories. This supports only Premise 2′. It is not clear how Premise 2 could be obtained without begging the question of metaphysical realism.[11] My Premise 2′ is supported by the problems with Field's program and it is also a premise that a wide variety of approaches to mathematics can accept.

In support of replacing 2 with 2′ one could also appeal to a footnote that Colyvan appends to his discussion right before he summarizes his indispensability argument: "I often speak of certain entities being dispensable or indispensable to a given theory. Strictly speaking it's not the entities themselves that are dispensable or indispensable, but rather it's the *postulation of* or *reference to* the entities in question that may be so described" (Colyvan 2001, 10 fn. 18). Here Colyvan claims that he has "stylistic reasons" for preferring to ascribe indispensability to the entities themselves. By reverting to talk of "reference" or, more properly, "apparent reference," though, I believe that the argument is rendered considerably clearer.

We are left, then, with this argument:

1′. We ought to have ontological commitment to all those entities that are indispensable to our best scientific theories.
2′. Apparent reference to mathematical entities is indispensable to our best scientific theories.

Therefore:
We ought to have ontological commitment to mathematical entities (metaphysical realism).

11. I return to this issue in Section 6.

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I maintain that Lewis and Hellman show this argument to be invalid, since they can accept both premises and deny the conclusion. Furthermore, they also accept much of Colyvan's evidence for his premises: scientific realism, Quinean naturalism, confirmational holism, and the failure of Field's program. What has gone wrong?

3. Mappings. The correct diagnosis of the failure of Colyvan's argument requires understanding how applications of mathematics work. The account of applied mathematics that I will assume is the 'mapping' account. According to the mapping account of applications, the truth of a statement of applied mathematics (or 'applied statement') depends on the existence of a mapping of a certain kind from a physical situation to a mathematical domain. Counting, for example, involves isomorphisms from the objects counted to an initial segment of the natural numbers. More sophisticated applications will involve other kinds of mappings, such as homomorphisms that respect certain features of the physical situation, e.g., the mass of physical objects. While this approach to applied mathematics is not accepted by everyone, it does fit with how many people naturally think of applications. For example, Baker thinks of applications this way, and even calls it a "familiar enough point": "it is solely the structural features of mathematical theories that are relevant to their use in science" (Baker 2003, 54). Baker uses this approach to applications to undermine indispensability arguments, but in a somewhat different way from what I am attempting. Baker appeals to the variety of different foundational frameworks that have been given for defining mathematical objects. The two best known are Zermelo-Frankel set theory and category theory. As both frameworks are adequate to define all the mathematics used in science, these applications cannot single out either framework, and so there are no particular mathematical objects that applications commit us to. This leads Baker to grant that indispensability arguments refute the claim "There do not exist any abstract (mathematical) objects" but that they fail to establish "There exist (at least) enough abstract objects to make literally true the bulk of the mathematical statements we accept" (Baker 2003, 49). This is because "no particular collection of mathematical objects is indispensable" (64).

While Baker's worries are in some ways similar to my own, it should be clear that I want to go further than he has in criticizing indispensability arguments. Unlike Baker, I do not think that indispensability arguments refute the views of Lewis and Hellman. Still, given that both of us are critical of indispensability arguments, it might seem unfair to assume that something like the mapping account of applications is correct. Fortunately, Baker and I here agree with Putnam's brief discussion of the details of applications in his *Philosophy of Logic.* Claims such as "The distance from x to y is r" mean there is a function with certain structural properties

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that takes spatial point x and spatial point y to real number r (Putnam 1971, 340–341). It is only the physical relations between the points and the mathematical relations between real numbers that enter into the definition of this function or mapping.

What are the implications of the mapping account for indispensability arguments? The mapping account implies that the truth of the statements of applied mathematics requires only the relations between mathematical objects as opposed to their non-relational properties. To see this, consider the statement "This book has a mass of 2 kg." This claim will be true just in case there is a mapping ϕ from the book to the real number 2 that satisfies a list of requirements. These requirements include that it take all 'standard grams' to .001, all objects with the same mass as the book to 2 and all objects with a greater mass to real numbers greater than 2. The only mathematical properties of the real numbers that are relevant are their ordering under the $\leq$ relation and their additive relations. If "This book has a mass of 2 kg" is true, we can conclude that these relations obtain between the real numbers. This is all that the existence of the appropriate mapping requires. More generally, for each mapping involved in an application, the existence of the mapping will depend only on a series of mathematical relations obtaining in the relevant mathematical domain (and, of course, on various physical relations obtaining in the physical situation).

Thus, the truth of statements of applied mathematics involves only the relational properties of mathematical objects. If an interpretation of pure mathematics can deliver these relational properties, then it can deliver the truth of statements of applied mathematics. I contend that both realist and many non-realist accounts of pure mathematics can deliver the relational properties of mathematical objects. If *even one* nominalist account of pure mathematics can recover the relations between mathematical objects, then indispensability arguments are invalid or unsound, in that the second premise (my $2'$) is undermined. The statements of applied mathematics will come out true, which is what the premises of indispensability arguments require, but their realist conclusion will be false.

We have already seen two proposals that fall short of metaphysical realism. In the next section I will argue that they can adequately interpret claims about the relationships between mathematical entities. Briefly, though, I want to summarize my general objection to indispensability arguments. If the mapping account is correct, then applications depend only on the relations between mathematical objects; but, the realist-nominalist debate concerns the non-relational or 'intrinsic' properties of mathematical objects. So, it is not surprising that looking at applications of mathematics is not going to resolve the realist-nominalist debate. To settle that debate we need to turn to pure mathematics and its practice.

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4. Options. I will now set out four accounts of pure mathematics that are consistent with the mapping approach to applied mathematics: set-theoretic realism of the kind attributed to Gödel, Shapiro's *ante rem* structuralism, Hellman's modal-structuralism, and Lewis' mereological account of mathematics. Two of these accounts are realist and two are non-realist, at least in terms of our definition of metaphysical realism adopted earlier,[12] and each offers a different account of the subject matter of pure mathematics. For Gödel mathematics is the study of sets (Gödel [1963] 1983) and according to Shapiro mathematics investigates abstract structures (Shapiro 1997). These approaches, then, are realist. According to Hellman mathematics studies possible structures and so Hellman is not committed to the existence of any mathematical objects (Hellman 1989, 1993). In this limited sense, Hellman is not a realist. Furthermore, for Lewis mathematics concerns the part-whole relations between certain concrete physical objects (Lewis 1993). Lewis also, then, denies that there are any distinct mathematical entities, and so is a not a realist either.

Despite their stark differences, these four accounts of pure mathematics can each accept the mapping approach to applied mathematics. Each account assigns the right relational properties to their respective mathematical domain, and so each can interpret applications in terms of mappings between physical situations and mathematical domains. For Gödel, Shapiro and Lewis this point should be obvious. They disagree on what claims about 2 and 3 are really about, for example, but each gives an analysis according to which 3 is the successor of 2. Hellman's position is more complicated since he wishes to avoid mappings from actual physical situations to structures in merely possible worlds. This pushes Hellman into requiring that applications involve mappings in a world where both the actual physical situation and the possible mathematical structure exist, and where the structure does not interfere with the physical situation. Once these additional modal assumptions are made, Hellman is able to offer a mapping-based account of applied mathematics.[13] Gödel, Shapiro, Hellman, and Lewis, then, can agree on which applied statements are true despite their widely divergent interpretations of pure mathematics.

5. An Example. To see this point in slightly more detail I want to review the example of the use of complex numbers that Colyvan presents in his book. His goal is to show how invoking complex numbers can deliver a more unified scientific theory. As unificatory power is one factor typically used to

12. All are semantically realist.

13. Of course, one could also be a modal-structuralist and accept mappings across worlds.

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compare competing theories, Colyvan takes this to show that ordinary scientific reasoning should convince us to prefer a fully mathematized scientific theory over a 'nominalistic' version.[14] My goal in discussing this example is somewhat different. Looking at the details of how complex numbers help in this case will show how the mapping account conceives of applications in particular cases and also how non-realist approaches can interpret these applications.

Colyvan considers the ordinary differential equation

$$y'' + y = 0 \tag{1}$$

where y is an unknown real-valued function of the real variable x.[15] Based on methods to be considered shortly, we can conclude that the solution to this equation is

$$y = c_1 \cos x + c_2 \sin x \tag{2}$$

where the c_i are constants given by the constraints placed on the system. Colyvan notes that, in the absence of complex numbers, the solution to the similar equation

$$y'' - y = 0 \tag{3}$$

which is

$$y = c_1 e^x + c_2 e^{-x} \tag{4}$$

would seem quite different. When complex numbers and functions of complex numbers are introduced, we get a unified method for solving (1) and (3) and we can understand how (2) and (4) are related. This can be important for our scientific understanding of the two physical systems, and might even help us to see how the two systems are similar. Significantly, when Colyvan discusses these insights he focuses on the structures of the physical systems:

> I take it that the algorithmic unity [i.e., the single method for solving these equations] arises out of deep structural similarities between the systems portrayed by these equations. For example, if two different physical systems are governed by the same differential equation,

14. This example is also used in Colyvan's objection to Melia's 2000. See Colyvan 2002 and Melia 2002 for Melia's response.

15. My discussion of this case closely follows Colyvan's at 2001, 81–83. He cites Boyce and DiPrima 1986. See especially §3.5.1.

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> it's clear that there is some similarity between these systems, no matter how disparate the systems may seem (just as a red planet and a red pepper have something in common). It seems plausible, at least, that this similarity is structural and is captured by the relevant differential equation. Even when the systems are governed by different differential equations, structural similarities may still be revealed in the mathematics. In the case of the two equations in the previous paragraph [(1) and (3)], the structural similarity is revealed by the connection between the equations' respective solutions. (Colyvan 2001, 83)

Of course, this talk of 'structural similarity' fits perfectly with the mapping account. When physical relations of a similar type obtain in the two physical systems, there will be mappings from these systems to similar mathematical structures, here real-valued functions. This similarity is consistent with large differences in the materials that make up the systems. Perhaps (1) describes fluid flow, whereas (3) concerns population growth.

Let's turn now to the details of how these simple differential equations are solved and the role of complex numbers in obtaining (2). First, we derive what are called the characteristic equations of (1) and (3) by replacing y and its derivatives with terms of the form r^n, where n is the order of the derivative of y that the term replaces. For example, (1) and (3) have characteristic equations

$$r^2 + 1 = 0 \tag{5}$$

$$r^2 - 1 = 0 \tag{6}$$

respectively. Second, we obtain the roots r_1 and r_2 of these characteristic equations and insert them into the general solution

$$y = c_1 e^{r_1 x} + c_2 e^{r_2 x} \tag{7}$$

As the roots of (6) are just 1 and -1, we get (4) from (7) by simple substitution.

Unfortunately, there are no real roots for (5). The solutions to (5) are i and $-i$, where $i = \sqrt{-1}$. Thus, appealing to the roots of the characteristic equation will work in these cases only if we can formulate a complex analogue of (7) and use it to extract the real-valued function y which is the solution to (1). To begin with, define the complex numbers C to be objects of the form $z = a + bi$, where a and b are real numbers and $i = \sqrt{-1}$ so that, for example, $i^2 = -1$. We can represent a complex number as a point on a two-dimensional real plane where a gives the value for the

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x-axis and b gives the value for the y-axis. Then functions of complex variables can be thought of as mappings from one such two dimensional plane to another. For example, $w = z^2$ takes each point on the original z plane to a point on the w plane. $1 + i$ goes to $(1 + i)(1 + i) = 1 + 2i - 1 = 2$i.

The next step is to extend our standard real-valued functions *e,* sine and cosine to the complex numbers. One clear goal is that these functions should agree on their values for the real number a and the corresponding complex number $a + 0i$. The following complex versions of *e,* sine and cosine do the trick:[16]

$$e^{ib} = \cos b + i \sin b \tag{8}$$

$$\sin z = \frac{e^{iz} - e^{-iz}}{2i} \tag{9}$$

$$\cos z = \frac{e^{iz} + e^{-iz}}{2} \tag{10}$$

We are almost done.[17] For now, we can take the complex roots of (5) and insert them into (7), where the latter is now understood as involving functions of a complex variable. This gives us $y = c_1 e^{ix} + c_2 e^{-ix}$. This function is a function of a complex variable, though, and the y we want is a function of a real variable. Fortunately, some simple manipulations can give us the general solution to (1) for real-valued functions. The key thing to note is that, given that e^{ix} and e^{-ix} are solutions, any linear combination of these two functions will also be a solution. But by (9) and (10),

16. Requiring agreement on the real line uniquely picks out these equations based on the theorem that analytic continuations of complex functions are unique. See Brown and Churchill 1996, especially §§52, 59.

17. One way to see the connection between *e,* sine, and cosine is to note the power series of the three functions, with the real variable *x:*

$$e^x = \sum_{n=0}^{\infty} \frac{x^n}{n!} = 1 + \frac{x}{1!} + \frac{x^2}{2!} + \frac{x^3}{3!} + \cdots \tag{11}$$

$$\sin x = \sum_{n=0}^{\infty} (-1)^n \frac{x^{2n+1}}{(2n+1)!} = \frac{x}{1!} - \frac{x^3}{3!} + \frac{x^5}{5!} - \cdots \tag{12}$$

$$\cos x = \sum_{n=0}^{\infty} (-1)^n \frac{x^{2n}}{(2n)!} = 1 - \frac{x^2}{2!} + \frac{x^4}{4!} - \cdots \tag{13}$$

Substituting ib for x implies that $e^{ib} = \cos b + i \sin b$.

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$e^{ix} + e^{-ix} = 2\cos x$ and $e^{ix} - e^{-ix} = 2i \sin x$. Ignoring constants, that means that the general solution of (1) can be written as a linear combination of $\cos x$ and $\sin x$. This yields (2).[18] These sorts of manipulations will always work and so the general solution when the roots are $r_1, r_2 = a \pm bi$ is

$$y = c_1 e^{ax} \cos bx + c_2 e^{ax} \sin bx \tag{14}$$

I agree with Colyvan that appealing to complex numbers allows a unified approach to solving differential equations of this type. It also makes clearer the connection between (2) and (4). For once we accept (8), the similarity between the two is obvious. It remains an open question, though, whether or not this application of complex analysis licenses a realistic attitude towards the complex numbers. If we think of this application in terms of the mapping account, then it is clear that it does not. Lewis and Hellman both deliver the needed structural relations between the real number line and the complex plane. All of the equations that we used in our discussion come out true on their interpretations of mathematics because they both agree on the relational properties of the real and complex numbers. This is especially easy to see if we remember that the complex plane has a part, namely the x-axis, that is isomorphic to the real line. It was this isomorphism that we exploited in moving back and forth between claims about real-valued and complex-valued functions, and it was this isomorphism which motivated our decision to have the real and complex versions of *e,* sine and cosine agree on a and $a + 0i$. Nothing about the 'intrinsic' or non-relational properties of the complex numbers entered into our reasoning. The same sort of reasoning would work with all of the structurally equivalent ways of introducing the complex numbers familiar to mathematicians. I merely want to extend this point to the structurally equivalent, but non-realist, proposals of Lewis and Hellman.

If all applications of mathematics work this way, then if an interpretation of mathematics delivers the right relations between mathematical entities, it can handle applications as well. On the assumption that Lewis and Hellman offer adequate interpretations of mathematics, indispensability arguments are shown to be invalid.

6. Conclusion. I want to close this paper by considering the sorts of objections that could be made to my criticism of Colyvan's indispensability argument. Perhaps the most obvious objection is that Lewis' and Hellman's proposals are unacceptable because they involve a non-literal or non-standard interpretation of mathematical discourse. Recall van

18. Strictly speaking this only works because $\cos x(\sin x)' - (\cos x)' \sin x \neq 0$. See Theorem 3.4 at Boyce and DiPrima 1986, 116.

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Fraassen's definition of scientific realism. It precludes interpreting claims like "There are electrons" to be either abbreviations for long-winded claims about possible sensory experiences or merely instruments with no semantic content. The same standards, it seems, should apply to statements such as "There are prime numbers," at least for those scientific realists who accept naturalism and confirmational holism. And, if we take "There are prime numbers" literally, then we cannot interpret mathematical claims in terms of mereological facts or possible structures.

I have two responses to this objection. First, the objection cannot be used to defend an indispensability argument because it makes no mention of applications of mathematics. The objection moves from scientific realism, naturalism, confirmational holism, and our acceptance of the sentence "There are prime numbers" to the realist conclusion that prime numbers exist. If this sort of argument is correct then we must be realists about all of mathematics, including mathematics that is not applied. However, this is not the kind of argument that I have criticized. I have been targeting the attempt to move from an examination of what parts of mathematics is applied to a realist conclusion about those parts of mathematics. Lewis' and Hellman's writings show that this is not possible.

Second, even if we ignore this first response, I worry that this objection assumes that it is obvious or uncontroversial what a literal reading of sentence is. Does a literal reading of "There are prime numbers" require that prime numbers be primitive entities, unreducible to other entities? I would hope not, or else set-theoretic realists read such claims non-literally. Perhaps we can all agree that the fictionalist or the instrumentalist about mathematics offers a non-literal reading, to the extent that she separates mathematical language off from non-mathematical language and adopts different rules for handling mathematical inferences. Still, such a dramatic division is not a consequence of Lewis' or Hellman's respective approaches. Both offer principled procedures for interpreting mathematical language that stop short of bringing in abstract entities. If we knew that mathematical language did involve abstract entities, then of course this would be a non-literal reinterpretation. But this cannot be assumed as it is precisely the issue between the realist and the non-realist.

A second objection focuses on the mapping account of applications that I have assumed. Do all applications work this way? I don't know, and can think of no philosophical argument to prove it. What is needed is a careful review of a representative sample of applications to see if they can be thought of in the structural way that we saw in the last section. To date, the only work on the details of applications (at least in the context of indispensability arguments) has been by Field and his followers. It does not seem that their fictionalist proposal can work, but it is at least a genuine alternative to the mapping account that should be further explored. The

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burden must be placed on the advocates of indispensability arguments to, first, formulate an alternative account that delivers a realist conclusion and, second, demonstrate that this account of applications is superior to the mapping account in some respects. This may be possible but, until something is offered the mapping account is the best account we have.

Thirdly, the realist might claim that the mapping account itself brings in mathematical entities, and so there is no need for the realist to formulate an alternative account of applications. Putnam, in his original argument, moved directly from his version of the mapping account to some kind of realism about functions: "we cannot account for the numericalization of distance without quantifying over functions" and functions are typically defined in mathematics as a certain kind of set (Putnam 1971, 341–342). I think that this is the most serious objection to my criticism of indispensability arguments, and it raises subtle issues about the role of functions or mappings in applications. I restrict myself here to noting that this objection implies a different kind of indispensability argument from the ones offered to date. For we are no longer looking at the mathematics that is applied in science to track our ontological commitments; rather, we are examining the functions used to connect physical situations to the mathematical domain. These functions have received detailed discussion by Lewis and Hellman, who argue that functions do not commit their respective positions to sets.[19] I believe that we should think of these functions as relations of a certain type, where these relations are non-mathematical entities akin to properties (Pincock forthcoming). These alternatives show that we are not forced to identify the functions involved in applications with sets. I am prepared to concede that it remains a live issue, though.

Finally, let me turn to the response that I imagine most realists would offer, even if they are not particularly sympathetic to indispensability arguments. Don't we know, they might say, that these non-realist proposals and many others involve improbable assumptions about modality or the physical world? Given that non-realists cannot really give an analysis of pure mathematics, we are not going to be able to come up with a genuine counterexample to indispensability arguments. So, the response concludes, we can defend the validity of these arguments by explicitly adding that no non-realist account of pure mathematics is possible.

I grant that adding this premise makes the argument valid, but the augmented argument is no longer informative. We already knew, presumably, that if no non-realist account of pure mathematics is possible, then a realist account of pure mathematics is correct. If even one non-realist

19. See the appendix to Lewis 1991 by Burgess, Hazen, and Lewis as well as Hellman 1999. Important work is also being done by the 'partial structures' program. See Bueno, French, and Ladyman 2002 and da Costa and French 2003, as well as the references given there.

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account consistent with the mapping account is possible, then the original argument does not work. The problem is that when we explicitly add the premise that no non-realist account is possible, then the augmented argument assumes what is at issue.

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