

Naturalizing Indispensability A Rejoinder to 'The Varieties of Indispensability Arguments'

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Naturalizing Indispensability

A Rejoinder to 'The Varieties of Indispensability Arguments'

Abstract

In 'The Varieties of Indispensability Arguments' the authors argue that, for any clear notion of indispensability, either there are no conclusive argument for the thesis that mathematics is indispensable to science, or the notion of indispensability at hand does not support mathematical realism. In this paper, I shall not object to this main thesis directly. I shall instead try to assess in a naturalistic spirit a family of objections the authors make along the way against the use of *indispensability* premises in indispensability arguments.

Indispensability arguments (IAs) have not convinced every philosophers to become mathematical realists. According to the authors of 'The varieties of indispensability arguments', it is also to be expected that in the future, improved IAs will not prove any more convincing. The situation, as the authors see it, is that either indispensability claims are unwarranted or, if they are warranted, they do not support realistic conclusions -the type of failure depending upon exactly how one conceives of the relation of indispensability.

'The varieties of indispensability arguments' offers a large variety of arguments against the soundness of present and future IAs that I will not be able to discuss. However, I believe that the authors' overall assessment and discussion of the role of mathematics in scientific endeavor may not fully do justice to the attitude of deference towards scientific practice that was congenial to the original spirit of IA.[1] In the following, I shall not discuss the issue of the special normative import that the application of mathematical theories to natural science supposedly has in the debate over mathematical realism, as opposed to the purely mathematical norms that those theories abide by.[2] My aim is more modest as I shall instead focus on indispensability itself and, trying to hold true to naturalistic values, emphasize tensions that arise between dispensability strategies considered by the authors on the one hand, and conditions of scientific practice -*a priori* as well as *a posteriori*- on the other hand.

In the first section, I shall discuss the authors' suggestion regarding dispensability strategies that hinge on special features of what I shall call here 'the architecture of mathematics'. I argue that these strategies are subject to two epistemic

[1] As expounded by Quine, and more recently defended in Colyvan [2001].

[2] Thus I shall not discuss the claim that we ought to have ontological commitment to all (and perhaps only) those entities that are indispensable to our best scientific theories, nor any other version of the crucial premise *iii* in the authors' reconstruction of IAs.

obstacles: the massive use of impure methods in mathematics, and the epistemic import of intrepretation functions. In the second section, I discuss a simple but fundamental example of another epistemic tension that may arise between elimination strategies and *practical* properties of their outcome. In the third section, I argue briefly that the authors' argument for the dispensability of real analysis to descriptive and predictive scientific tasks is based on a controversial picture of scientific knowledge. In the last section, I finally turn to the indispensability of mathematics to explanation and tentatively sketch a naturalist approach to address the problems allegedly surrounding the corresponding indispensability claims.

The architecture of mathematics and scientific dispensability. In their paper, the authors suggest several dispensability claims based upon a certain notion of the architecture of mathematics and more specifically upon the foundational position that set theory occupies.[3] I wish to nuance their claim by first drawing attention to the importance of impure methods in mathematics and secondly, by drawing attention to the variance in the epistemic properties of a statement under its re-interpretation.

The authors' definition of indispensability relation excludes the possibility of a transitive indispensability relation -that is, if a mathematical theory T_1 is indispensable to a scientific theory S and a mathematical theory T_2 is indispensable to T_1 but not used in S, then on the proposed definition T_2 is not indispensable to S.[4] My first remark is that this picture of indispensability conflicts with the fact that, as a matter of epistemic necessity, impure methods[5] are massively used in mathematics.

Assume that we know a certain arithmetical statement A to be true or, to avoid begging the question, to be a consequence of second-order arithmetical axioms, but that our knowledge of this fact depends on our having a non-arithmetical argument for it (be it set-theoretical or otherwise). Then assume that a given scientific theory S makes non-dispensable use of arithmetic, and of the statement A in particular, for a descriptive or predictive task.[6] Then it seems to me that the indispensability of arithmetic extends to this other mathematical theory upon which our acceptance of A relies. If set-theory, or the theory of elliptic curves, is indispensable to arithmetic in order for arithmetic to play its role in accomplishing of the descriptive/predictive

[3] Those claims are made in various places in section 4.2.1 of 'The Varieties of Indispensability Arguments'. Those claims bear some similarity to the anti-IA strategies analyzed in Baker [2003], as the authors notice (e.g. in f.n. 47), but they are different and do not especially hinge on the multiplicity of foundational frameworks for mathematics.

[4] See 'The Varieties of Indispensability Argument', section 3.5. Roughly, the proposed definition of indispensability says that a mathematical theory T is indispensable to a scientific theory S in order to accomplish a given task in an appropriate way if and only if all instances of S that accomplish the task in the appropriate way make use of some instance of T.

[5] A method of resolution is pure if it avoids the use of concepts that are alien to the proper domain of the problem to solve, and it is impure otherwise. For more on purity of methods, see Detlefsen and Arana [2011].

[6] Say, some use of Fermat's last theorem to predict that a certain planet will not hit the Earth.

task of S, it follows that set-theory, or the theory of elliptic curves, *is* indispensable to S to accomplish its descriptive/predictive task. If this is so, then I submit that given the pervasiveness of impure methods in mathematics, it would be safe to say that cases of transitivity of the indispensability relation considerably extend the range of mathematical theories which can at first plausibly be said to be indispensable to scientific theories in order to accomplish all sorts of tasks. This more tolerant picture of indispensability facts, where one insists on solidarities between theories, seems to me to be more in line with the Quinean view of science as a large web of dependent beliefs than the authors' stricter logic of indispensability allows.

Coupled with considerations about the epistemic import of interpretation functions, the foregoing remarks seem to cause trouble to the authors' set theoretical strategy against indispensability claims. Remember the authors argument. They argue that the interpretability of any mathematical theory in a fragment of set theory shows that no applied mathematical theory can be indispensable to a scientific theory in accomplishing its descriptive or predictive task, the substitution of one to the other being made without scientific costs insofar as descriptive and predictive tasks are concerned. They simultaneously suggest that set theory itself cannot be indispensable either, since whatever the descriptive or predictive role some of its fragments play in some scientific theory, it is quite plausible that this role would be played equally well by a classical theory.[7] It follows that neither the classical theories, nor set theory, can be descriptively or predictively indispensable to a scientific theory. Thus the expansion of the society of mathematical theories would be somehow detrimental to the empowerment of its citizens.

The difficulty with this view stems from the fact that, as a special case of the need for impure methods, a mathematical theory T_1 might interpret a theory T_2 and T_2 still be indispensable to T_1 in order for it to play its descriptive or predictive role within a scientific theory S.

To see this, notice that, as was implicit in the discussion above about transitivity, a mathematical statement, or theory, has to have prior 'good' epistemic properties in order to be used within a scientific theory to accomplish a predictive or descriptive task. What are those 'good' epistemic properties? Modest candidates that immediately come to mind are *being justified*, or *having known proof from accepted axioms*, or *to abide by the standard of mathematical methodology*, or perhaps even *being known* in a sense in which a fictionalist would be allowed to talk of mathematical knowledge.[8] The problem is that a statement A can be

[7] By the end of section 4.2.1, the authors write:

> It can be argued that [set theory's] role amounts, globally, to the role that could be played, piecemeal and conjunctively, by other mathematical theories, also conceived as autonomous.('The Varieties of Indispensability Arguments', section 4.2.1)

[8] I take the claim that mathematical statements need to receive some form of (mathematical) justification prior to their use in application as unproblematic and acceptable even to a die-hard instrumentalist. The assumption is very similar to a suggestion the authors make within the context of the discussion of explanatory indispensability in section 4.2 of 'The varieties of Indispensability

such a well justified arithmetical statement and the statement B have no known justification even though there exists, unbeknown to us, an interpretation function f such that B is a set-theoretical statement representing A under the interpretation f of Peano arithmetic in ZFC. Some theorems of ZFC may come to be formulated and mathematically justified only in virtue of the fact that they represent some arithmetical statements under a given known interpretation of PA into ZFC that, so to speak, guide our feet into the path to it. Consequently, we might not be in a position, epistemically speaking, to do the job needed to allow ourselves the use of B just by staying within the confines of set theory. This is meant to bear on the issue of dispensability both in contexts of discovery and in contexts of justification. In contexts of discovery because by limiting ourselves to set-theoretical concepts and methods, B might not have been formulated nor applied, nor any proof of B be found. In context of justification, because even given A and the interpretation function, we may be unable to provide adequate conceptual or formal justification for B in purely set theoretical terms,[9], or B might be too complex to be intelligible or to be tractable in scientific context. Set theory sometimes makes mathematics too difficult, and far from being able to play A's role in descriptive and predictive scientific applications, B may be too intractable to be used at all.

At the end of the day, even if we can in principle represent any classical theory in ZFC[10], we still need the classical theories in order to meet the epistemic requirements that allow us to accomplish some descriptive or predictive task within the context of a scientific theory. All mathematical theories might be representable in set theory. There is no easy road, though, from representability to dispensability.[11]

Feasibility. Let me push the foregoing line of thought further and introduce a clear illustration of the importance of 'in practice' considerations by pointing to the shortcomings of another dispensability strategy that the authors have called our attention to. What I have in mind is the suggestion to dispense with references to numbers by rephrasing the sentences containing such references as sentences

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Arguments'.

[9]Think of the case of someone who is not confident in the consistency of ZFC: then he cannot justify the use of B to make a scientific prediction, while he may have been able to do so with A given his confidence in arithmetic.

[10]This formulation would need serious refinements, among other reasons because many notions of interpretations might be used. For more on these notions of interpretability and useful insights about the relation between interpretation function and knowledge transfer, see Walsh [forthcoming], which discusses related issue in depth within the context of a critical assessment of the neo-logicist program.

[11] Regarding the authors' other suggestion that the mathematical role of set theory reduces to the role of an unifying framework, so that set theory's role itself could be played just as well by classical theories as far as applications are concerned, let me mention that not everyone agrees with this conception of set theory. In particular, Dehornoy [1996] has argued that set theory's role cannot be reduced to its foundational role. On a par with classical theories, Dehornoy argues, set theory has proper methods, with fruitful applications in topology -which in turn plays an important role in modern physics.

containing only numerical quantifiers.[12] I am willing to assume here that implementing this strategy for any given theory would result in a theory that dispense with references to natural numbers without change in empirical meaning. However, even granting this point, remains objectionable to conclude to the predictive dispensability of reference to natural numbers. For as soon as one pays attention to practical considerations in science, one might object to the claim by pointing to ways in which such a replacement would be highly ineffective for the purpose of providing natural science with the tools needed to accomplish its predictive task. Indeed, as shown by Ketland [2005] in the spirit of Boolos [1987], some basic inferences would be simply *unfeasible* if one had recourse only to numerical quantifiers instead of reference to numbers. To take one of Ketland's examples, consider the following inference, which can be readily seen as a prediction (the conclusion) drawn from observations (the premises):

1. The number of people in the room is 100.
2. The number of houses in the street is 99.
3. Each person in the room lives in exactly one house in the street. Hence,
4. At least two people in the room share the same house.

Ketland argues that a proof of the conclusion from the premise that avoids reference to numbers by use of numerical quantifiers would exceed 10^8 steps. It is thus plainly unfeasible, whereas an ordinary proof using references to numbers, functions and the Pigeonhole principle is quite direct. It seems then that rewording statements by using only numerical quantifiers is not *really* a feasible choice. The example above is not unique in mathematics and other speed-up results can be used to argue for the practical indispensability of some parts of higher mathematics to help lower parts to accomplish their share of the descriptive or predictive task in scientific theories.[13]

One strong motivation for pursuing nominalist programs lies in the hope that, if successful, they will be helpful in plausibly explaining how knowledge, especially knowledge involving apparent reference to mathematical entities, is possible. It is thus somehow ironic that the dispensability strategies we have examined result in making scientific knowledge (descriptive and predictive) impossible. On the other hand, once we take seriously the need for impure methods to achieve results

[12] The authors write:

> For (finitary) arithmetic things look even simpler. Indeed, to show that there cannot be any sound genuine IA's involving arithmetic in which 'Q's' is replaced by 'theories', and $\mathfrak{L}$ is replaced by $\mathfrak{L}_\beta$ or $\mathfrak{P}_\beta$, *it is enough to notice that natural numbers can be replaced with numerical quantifiers in any scientific statement where they occur, without missing or diminishing the descriptive and predictive power of this statement* ('The Varieties of Indispensability Arguments, section 4.1.1. p.?, my emphasis).

[13] See, e.g., Buss [1994] for relevant technical results.

that are relevant to some descriptive and predictive tasks, as well as the practical relevance that a mathematical theory may remotely have for another one held to be indispensable to science, who knows what one will end up taking on board?

Models, reality and scientific knowledge. In section 4.2.1 of 'The Varieties of Indispensability Arguments', the authors have a specific argument to offer for the descriptive and predictive dispensability of real analysis. Without going into detail, the argument given by the authors for this conclusion relies on the idea that any use of real analysis in a scientific theory can in principle be replaced by a theory appealing only to rational numbers, without any possibility of actually discerning any descriptive or predictive loss.[14] Presumably, the point behind this claim is that any actual measures that could be adduced to assess the descriptive or predictive accuracy of the theory would in any case be bound to use rational numbers only. I do not disagree with the fact that any empirical observations and measures will appeal to rational numbers (only). But is that sufficient to make real numbers and real analysis dispensable for descriptive or predictive tasks? It seems to me that it is not.

As is well acknowledged by the authors themselves elsewhere [reference to be added here] scientific practice in description and prediction often proceeds by using *models* of phenomena, models that typically involve geometrical representations like spheres, ellipses, or real functions etc. What is controversial is the conclusion we should draw from that. A first reaction would insist that models are idealizations, and thus are false. This is the authors' stance, who would then argue that the idea that the indispensable role of talk about reals in false discourse could hardly yield any semantic realism about real analysis. Even more directly, in this perspective, the use of real numbers in models provides no ground for supporting the claim of indispensability of real numbers in *describing what there is*.[15] Our use of mathematics in scientific models, according to this view, would thus be irrelevant to IAs as far as descriptive and predictive indispensability of mathematics is concerned.

This reaction, however, seems too hasty. It is of course true that the *use* of models in describing a phenomenon sometimes involves knowingly false idealiza-

[14] The authors write :

> Consider, for example, any version of RealAn of real analysis and suppose that S_i is an instance of a scientific theory S that has recourse to an instance of RealAn. It is highly plausible to admit that there is another instance S_j of S [...] that has not recourse to RealAn, insofar as it replaces it with an appropriate theory of rational numbers [...] which accomplishes the same descriptive or predictive task as S_i in such a way that no difference between the two, with respect to the accomplishment of this task, can be appreciated on the basis of our capacity of discerning their descriptive and/or predictive power (on the basis of which we assign to these theories the epistemic property P^E) ('The Varieties of Indispensability Arguments', 4.2.1)

[15] I am not sure how this line of argument extends to predictive indispensability.

tions, as when they are used to analyze water waves assuming water to be infinitely deep[16]. But there are equally good examples of cases where the natural and rational attitude seems to be to take models as literally describing what is the case. Thus to describe a smooth variation of the speed of a solid, a continuous model of the variation seem to force itself upon us as true, and the fact that any measurement of speed at any time would give a rational speed number has no force against this model as a true description of the phenomenon. In such a case, real numbers and real analysis will quite naturally appear to play an essential role in describing what is going on 'for real'.[17] Contrary to the authors' claim, it seems that a scientific theory where space-time is modeled as continuous can receive more rational support as a description than one where space-time is modeled as non-continuous, despite our limited discrimination powers. Actually, precisely because we think of ourselves as having limited discriminating powers, we find it rational not to take our abilities to be the measure of what there is. This example of a literal descriptive reading of a model is not unique.[18] If this is taken seriously then scientific description involving large parts of higher mathematics, including real numbers and real analysis, are integral to scientific descriptive knowledge, and the reals cannot be replaced by rational numbers without epistemic loss.[19]

Mathematics and explanation. Let me finally turn to the role of mathematics in scientific explanations and make a few remarks inspired by the authors' views on this topic. The authors remain unconvinced by the conclusions drawn in Baker [2005] from the example of a mathematical-evolutionary explanation of the life cycle of Cicadas and argue that mathematical discourse and entities play no essential role in the proposed explanation (or scientific explanations in general). This skepticism does not seem to stem from any form of 'eleatism'[20], but more simply to follow from the conviction that an appropriate, non-mathematical and illuminating rephrasing of an explanation involving mathematical discourse would always be possible in this and similar cases.[21] More radically, the authors argue that trying to

[16]The example is taken from Maddy [1992], p. 281-282.

[17]Accepting this much is not incompatible with accepting the possibility that further reasons could later convince us to finally take such a model of continuous space-time to be descriptively false, as Maddy [1992] contemplates (citing Feynman).

[18]See the discussion in Maddy [1992], p. 281-282 and Colyvan's reply in Colyvan [2001], p.100 *sqq.*

[19]They cannot be replaced, that is, until someone has shown how to dispense with them without epistemic loss.

[20]Eleatism is the doctrine according to which the 'eletatic principle' is true. I borrow the 'eleatic principle' from Colyvan [2001]. The principle reads:

> An entity is to be counted as real if and only if it is capable of participating in causal processes. (Colyvan [2001], p. 40)

If mathematical entities are acausal, as probably most philosophers believe, then obviously their role in explanation is not a causal role.

[21]This is how I read their analysis of the explanation of the life-cycle of Cicadas in the last section of their paper.

prove the existence of mathematical explanations in science is a non-starter for IAs. This is because they believe that any argument to the effect that mathematics contributes something essential to a scientific explanation would have to assume that mathematical discourse has some sort of property that the IA is precisely designed to prove it has.

I shall soon say something relevant to the circularity concern. For now let me just add that, regarding the first concern, I am not as sure as the authors are that what they accept as a best explanation of the life cycle of Cicadas, that is,

> the circumstance that the life cycle of periodical cicada is such as to minimise the frequency of intersection with the life cycle of hypothetical periodic predators or of hypothetical similar subspecies with which an hybridisation would be possible,

is itself a mathematics-free circumstance.[22][23] Actually, it seems to me that this is precisely what is in question. However, this is a difficult matter and not one I want to pursue here. What I want to suggest is an entirely different way to approach the question of the indispensability of mathematics in scientific endeavor.

The question, remember, is whether mathematics plays an 'essential' role in explanations.[24] Part of what makes the question hard to answer is that we do not know exactly what an explanation is, nor do we know what playing an essential role in it amounts to. This is why in examples like Baker's of a seemingly mathematical explanation of scientific facts, there will likely always be room for the unconvinced to argue that mathematics are not really *relevantly* involved in explanatory work.[25] However, I think that we can find an entirely different sort of evidence for the fact that mathematics actually plays an essential role in some scientific explanations.

It seems to me that if one could show that mathematical endeavor is really taken by the scientific community as being a critical part in the achievement of some scientific explanations, explanations as desirable from the scientist's point of view as 'intrinsic explanations' are from Field's point of view, this would be

[22] The quote is found in section 4.2.2 and continues :

> which is perfectly independent of the existence and properties of any sort of number, too. ('The Varieties of Indispensability Arguments', 4.2.2)

[23] In particular, the use of mathematics in Baker [2005] does not reduce to reference to arbitrary numbers in a conventional unit of time measurement. The explanation using co-primeness tells us something more essential about certain abstract relations between time periods and seems prima facie to give these abstract relations some explanatory role. More generally, the fact that *dimensionless* quantities play a role in a variety of physical laws and scientific explanations is of course of more significance for the indispensability of numbers in explanations than the mere use of numbers.

[24] Again: not necessarily a 'causal' role, which would partially begs the question in favor of the dispensability thesis. Moreover, it seems to me that the strength of IA does not depend upon accepting the metaphysics of causality. For lack of space, I am assuming without argument that the thesis that whatever plays a crucial role in our best explanations cannot be treated by us as fictional does not depend on a specifically causal conception of explanation. Thus I am assuming that to put so-called 'enhanced' IAs to work any *bona fide* mathematical scientific explanation will do. On criteria to distinguish what counts as *bona fide* mathematical scientific explanations, see below.

[25] See Baker [2005] p. 234.

something that we, philosophers, would have to take seriously. For the most radical naturalists among us, this is so serious that it means that the *normative question* of the indispensability of mathematics in explanations that philosophers have dealt with[26], can and should simply be replaced by an *empirical question*: the question whether the notion of a mathematical scientific explanation receives substantial credit in the scientific community. For the less radical, who holds bravely to the idea that the philosopher can be right against the scientific community regarding what counts as a scientific explanation, and in particular whether there are such things as mathematical scientific explanations, the empirical question does not supersede the normative one. Still, even in that case, minimal naturalism commands that in addressing the normative question, the philosopher should grant that the charge is upon him to prove that the epistemological attitude empirically found to be that of the current scientific community, if different from the one he recommends, is wrong.[27] It seems to me that in the spirit of philosophical naturalism from which IAs gains its force, radical or not, what I propose to call for short the 'empirical question of indispensability', should be given *prima facie* epistemic priority over other philosophical modes of inquiry. I would now like to sketch *very* roughly what such an empirical inquiry could look like and say more about what its benefits can be in the debate over IAs.

It would probably be possible to find anecdotal evidence of scientists regarding mathematical knowledge (however they may conceive it) as essential to explanatory work in a non-trivial way[28], with some of them perhaps spontaneously talking of genuine 'mathematical explanations'. But this would probably not be convincing, precisely because it would remain anecdotal, scarce and shaky evidence. Could we not do better than driving a few scientists out of their ordinary way to make science, ask them epistemological questions framed in a prejudicial philosophical jargon and wait for their uncertain replies? We should instead be looking at science itself, or rather at the tangible products of scientific activity, papers, books and various publications.

[26]The *philosophical* problem of the indispensability of mathematics to science is normative in the sense that it is all a debate about epistemic norms, e.g. what *ought to* count or not as a *genuine* scientific explanation, what can dispensed with in science without *significantly impairing* it, etc.

[27]What I call 'radical naturalism' here is the view that philosophy must give way to science everywhere empirical science is possible. What I call 'minimal naturalism' is the less radical thesis that philosophy is in continuity with science, a science among other sciences with no special legislative power. Moreover if, following Huw Price, we define 'object naturalism' as the view that 'all genuine knowledge is scientific knowledge' whereas 'subject naturalism' is the apparently weaker view that 'philosophy needs to begin with what science tell us about ourselves' (Price [2004], p.73), let us remark that 'subject naturalism' is enough to motivate an empirical inquiry about what we -as a scientific community- take as counting as scientific explanations.

[28]Here is perhaps one such anecdotal evidence: very recently, the French neurobiologists Thomas Boraud and Francois Gonon have argued in public debate that the theoretical difficulties presently facing neurobiologists are caused by their lack of mathematical sophistication (See Boraud and Gonon [2013]). Their idea is that there are a number of explanations that could now be within our reach in neurobiology, but that neurobiologists fail to provide them because of their lack of mathematical knowledge, especially in handling multivariate statistical methods.

Taking a walk on this path, one is (or should be) almost immediately surprised and moved by the existence and relative abundance of scientific journals outside mathematics whose title contains 'Mathematical X'. Thus for instance the *Journal of Mathematical Biology*, edited by Springer, whose editorial presentation starts with the following words:

> *The Journal of Mathematical Biology* focuses on mathematical biology - work that uses mathematical approaches to gain biological understanding or explain biological phenomena.

The *Journal of Mathematical Biology* is just one of its kin, and we could also have cited the *Journal of Mathematical Physics*, the *Journal of Mathematical Neuroscience*[29], the *Journal of Mathematical Psychology*[30], the *Journal of Mathematical Sociology*[31], and so forth. I shall not go here into the detail of how exactly these journals conceive of the key role they attribute to mathematics in achieving their disciplinary scientific goals. But it seems to me that these and related facts provide the kind of evidence that can be put to work here, because such facts intuitively attest in a substantial way to the fact that the role of mathematics in explanation is not regarded only as auxiliary in achieving core scientific tasks, but that appeal to mathematical discourse really is part of some explanatory processes.[32]

Encouraged by this initial intuition, we may want to go further and bring our

[29] Edited by Springer, the editorial presentation reads:

> The *Journal of Mathematical Neuroscience* (JMN) publishes research articles on the mathematical modeling and analysis of all areas of neuroscience, i.e., the study of the nervous system and its dysfunctions. The focus is on using mathematics as the primary tool for elucidating the fundamental mechanisms responsible for experimentally observed behaviours in neuroscience at all relevant scales, from the molecular world to that of cognition. The aim is to publish work that uses advanced mathematical techniques to illuminate these questions.

[30] This journal is itself affiliated to the Society for Mathematical Psychology.

[31] Published by Taylor and Francis. In the presentation of the aim and scope of the journal one can read:

> Because *Journal of Mathematical Sociology* is addressed primarily to sociologists it is anticipated that most articles will be oriented toward a mathematical understanding of emergent complex social structures rather than to an analysis of individual behavior. These structures include, for example, informal groups, social networks, organizations, and global systems.

[32] It may be true that what scientists regard as an explanation might sometimes differ from the philosopher's view of the matter. It might also happen that what scientists regard as an explanation quickly evolves over time. I take it as an advantage of the present suggestion that it does not rely only on the philosopher's interested opinion about what count as a good explanation to assess the role of mathematics in scientific explanations, and I do not regard as a defect of the method that it will yield different results as science and scientists' conceptions evolve over time. Moreover, I think that, even if it is true that scientists might be wrong about the ontological or various metaphysical conceptions they carry along their scientific activities, there is a sense in which we cannot, or should not, take them to be in the same way collectively wrong about what they take the aims and success criteria of the scientific activity to be -unless we can oppose *strong* evidence. See the above remarks about minimal naturalism.

inquiry at a scientific level. One could then, for instance, want to take a systematic look at a very (*very*) large and representative corpus of scientific papers and books and try to test for the existence and diffusion of mathematical methodology in the explanatory frames used in the documents. In order to get our hands on that last idea, we already know that it would certainly be irrelevant to check for the use of mathematical language in those papers and books -obviously mathematics are widespread and doing so would not help to distinguish cases where mathematics is simply used to state results, report evidence and measures, from cases where the use of mathematics is regarded as somewhat more essential to the task at hand. But we can build on our previous insights in journal titles and go proxy by checking for papers and books that somehow acknowledge explicitly that mathematics are essential to what they are trying to achieve. So what suggests itself, at a first approximation at least, is to check for papers and books that contain the string 'mathematic' in their title, abstract, journal title or conference title. We could then find scientometric clues regarding which parts of science, if any, are seriously involved with mathematics in achieving their core task.

Actually, this is exactly what Kevin Boyack did.[33] The result is the map below where colored points are clusters of similar papers in which at least one of them matches the query we have just mentioned, and grey points are clusters who do not have papers matching the query.[34] What we observe is that the 'footprint' (Boyack's word) left by mathematics over the entire map of science is quite large. Remember that this does not simply mean the trivial fact that sciences use mathematical tools: the query goes proxy for a device selecting papers where mathematical achievements are somehow the object of the paper (though we know that the query does not do that in an exact way, of course). In light of this, the facts reported by the map about scientific activity seems to speak in favor of the thesis that mathematically-driven explanations actually plays some role in a large range of scientific fields, confirming to some extent our initial impressions based on anecdotal testimonies and the observation of a bunch of scientific journal titles.

It is obvious that in its present form the proposed method for assessing indispensability claims has fatal shortcomings.[35] Critically analyzing further these results would lead us too far afield, and this example was just meant to stimulate discussion about the possibility and import such an inquiry, not about the degree of achievement of the given example. The general idea is clear enough though:

[33] Personal communication. Thanks to Kevin Boyack for performing the relevant queries on his map of science and for allowing me to use his data and maps.

[34] See the pdf version of this paper to see the colored version of the map. I have provided a quick clarification of my use of the term 'similar' in the caption below figure 1.

[35] For one, it would of course be necessary to check the typical contexts of occurrences of the string 'mathematic' in the titles, abstract of papers and books, titles of journals, and see if we have even remotely succeeded to capture the core idea of a set of papers where mathematics are 'essentially involved' in explanation. After all, for all we have said, the results may well all be papers that explicitly devote to 'metaphorical explanations', or 'untrue but short explanations', etc. However, I see no *a priori* reasons why an empirical inquiry along these lines could not be conducted in a convincing way.

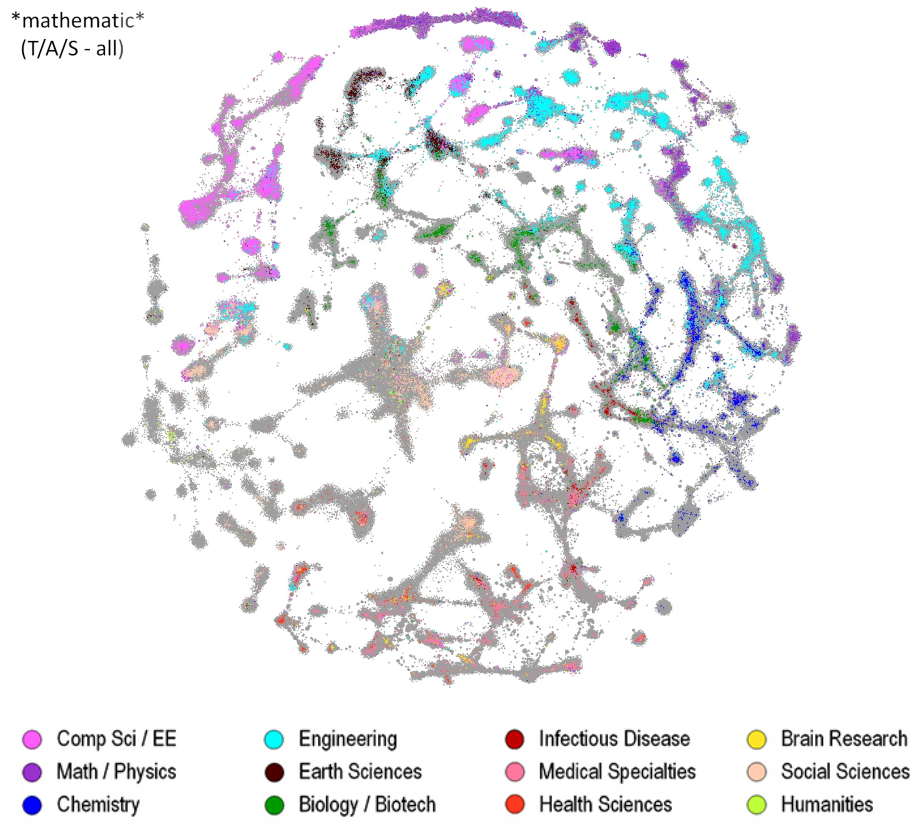


Figure 1: The result of the query 'mathematic' mapped over the map of science as described in Boyack and Klanvans [2014]. The map is created out of a 1996-2011 time slice of the Scopus database plus items cited more than once in this slice, with a total of 43,431,588 items. The 156,085 points of the maps are clusters of 'similar' papers, clusters which can roughly be described as the outcome of running a modularity algorithm for detecting communities of papers (of size at least 20) over the graph of direct citations between papers. Colored clusters are clusters containing at least one paper where the string 'mathematic' appears in the title or the abstract or the journal/conference title. The layout of the map minimizes distance between thematically closed clusters of papers. See Boyack and Klanvans [2014] for the methodological details concerning the creation of the underlying map of science. (The above map is published here for the first time, by courtesy of Kevin Boyack.)

if one is to be faithful to the idea of the primacy of scientific judgement over the prejudices of first philosophy in scientific matters, it is natural to take a naturalistic approach also towards the analysis of indispensability and explanatory role.

I see three advantages of the kind of approach very roughly illustrated here. The first is that to a certain extent it allows one to 'blackbox' the notion of explanation and thus to circumvent the difficulty raised by the fact that we do not understand what an explanation is. After all, in a Quinean spirit, what is important in the debate is whether mathematics is dispensable or not to achieve whatever is central to scientific activity -be it explanation in the sense of answering to 'why' questions, in the sense of producing understanding, in the sense of unifying, or providing causal explanation, or whatever construal the current state of science appears to favor. The second advantage is that taking this path would make for an argument in favor of the indispensability of mathematics in explanation that would not be assuming anything about the prior semantical or epistemological status attached to mathematical discourse. To put it otherwise, it would provide a non-circular argument for the indispensability of mathematics in explanation and would thus alleviate the concern expressed by the authors in the last section of their paper regarding the possibility of a non-question-begging argument for the truth of the indispensability premise of IA. Finally, it seems reasonable to submit that questions regarding the alleged indispensability or dispensability of (part of) mathematics with respect to (part of) scientific explanation be also investigated in such a way that the content and factuality of the claims can be assessed empirically. This would at least be thoroughly in keeping with Quine's naturalization *motto*.

Conclusion Where does the foregoing discussion leave us? In the first two sections of the paper, I have argued *a priori* that some of the main strategies presented in 'The varieties of dispensability arguments' in support of descriptive and predictive dispensability of mathematics really are checks that cannot be paid in scientific practice. This is because, I have argued, we are too limited in intellectual resources and time to afford to dispense with our preferred mathematical theories, or with reference to numbers. However, as is well known, it has been argued recently, following the discussions in e.g. Melia [2000] and Colyvan [2010], that indispensability for descriptive or predictive task is not clearly relevant to ontological issues. What is more relevant, according to the current wisdom, is whether mathematics are indispensable to scientific explanations in an essential way.

In the last section, following the authors, I have thus turned to the dispute over the indispensability of mathematics in scientific explanations or, to put it otherwise, the dispute over the existence of properly mathematical explanations in science.[36] The dispute appears to involve delicate normative considerations, with philosophers typically arguing about explanatory grades that various explanations -with or without appeal to mathematics- deserve. Whatever their philosophical interest, it might be, as some suspect, that these normative discussions will nonetheless not

[36] I conflate the two issues since they are basically equivalent. In one direction, the indispensability of mathematics in science is ordinarily understood as implying that mathematics do not merely play an auxiliary role in scientific explanation but that there are proper mathematical explanations in science. In the other direction, if there are properly mathematical explanations in science, then we cannot dispense with mathematics in some scientific explanations.

help to settle the ontological issues because the norms that recommend one kind of explanation over another might at bottom be the same as the norms that recommend one kind of ontological attitude over the other -nominalists tend to find 'intrinsic' explanations better and realist to find alleged mathematical explanations beyond reproach. If this is correct, then it is worthy to note that the naturalist philosopher can make some progress by taking an indirect route. Considering scientific practice *a posteriori*, as it can be observed from its products, I have tried to illustrate how one can argue indirectly for the explanatory indispensability of mathematics in science by relying on evidence taken from the observed scientists' attitude. Because this 'empirical approach to indispensability' avoids the trouble of a philosophical elucidation of the notion of explanation by deferring to the way the scientific community is working it out in practice, and because this is coherent with a naturalistic stance, it might end up proving to be an interesting move to make for the defender of IA.

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