Indexing and Mathematical Explanation

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Abstract: We discuss a recent attempt by Chris Daly and Simon Langford to do away with mathematical explanations of physical phenomena. Daly and Langford suggest that mathematics merely indexes parts of the physical world, and on this understanding of the role of mathematics in science, there is no need to countenance mathematical explanation of physical facts. We argue that their strategy is at best a sketch and only looks plausible in simple cases. We also draw attention to how frequently Daly and Langford find themselves in conflict with mathematical and scientific practice.

1. Introduction

Mathematical explanation has been attracting a great deal of attention lately. The interest initially arose out of debates about mathematical realism, where the existence of mathematical explanations of physical phenomena gave rise to a new form of the indispensability argument for mathematical realism. But the debate over mathematical explanations of physical phenomena has wider interest and has taken on something of a life of its own. The debate raises questions about the relationship between mathematical explanations in empirical science and intra-mathematical explanations of mathematical results (Baker forthcoming; Steiner 1978). It is also important for debates about scientific explanation more generally, because mathematical explanations of physical phenomena would appear to offer counter-examples to popular causal and interventionist accounts of scientific explanation (Colyvan 2001, pp. 45–53; Smart 1990).[1] A great deal is at stake here and it is thus no surprise that there are attempts to deny that there are mathematical explanations of physical phenomena. In this paper we look at the first detailed such attempt. We argue that the attempt in question ultimately fails, but it is interesting and instructive to see why it fails. This, in turn, sheds light on what is required from future attempts in this direction.

Chris Daly and Simon Langford (2009) have recently argued against mathematical explanations of physical phenomena. They suggest that the examples of such alleged explanations offered by the present authors (Baker, 2005, 2009, Colyvan 2001, 2002, 2007, 2010, and Lyon and Colyvan 2008) can all be accounted for by alternative means and thus there is no reason to countenance such mathematical explanations. Their

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[1] See Mancosu (2008, 2011) and Leng (2005) for more on the significance of the debate over mathematical explanation.

strategy is to follow Joseph Melia's (2000, 2002) lead and try to show that the only role mathematics plays in science is that of "indexing" physical facts.[2] According to Daly and Langford, this means that the real explanation lies in the physical facts and the causal relationships therein. We argue that their indexing account leaves a great deal to be desired, not least of which is considerable unclarity about the details of the proposal. Worse still, Daly and Langford frequently find themselves flying in the face of both mathematical and scientific practice, where mathematical explanations do appear to be important. The indexing view of mathematics in science is just too impoverished a philosophical account and does not do justice to the variety of roles mathematics plays in science.

2. Indexing

The indexing account of mathematics does have some initial plausibility. It takes it that mathematics in applied contexts is merely representing features of the physical world, and it is the latter that really matters. According to this account, mathematical modeling works in much the same way as map making or any other representational strategy. The basic idea is nicely illustrated in simple cases where mathematics is used to stand proxy for physical properties. The account works well in cases such as those Melia (2000) used to motivate it, several of which involve facts expressing distance relations, for example "*a* is 63 centimetres from *b*." The indexing strategy takes as its starting point the very natural thought that the above fact does not hold *in virtue* of the relation between *a*, *b* and the number 63; the fact in question is taken to hold by virtue of the spatial relationship between *a* and *b*, and this is all there is to it; this relationship is indexed by the number 63 but the number 63 does not enter into the relationship. The mathematics might look like it is a part of what is being described, but, according to the indexing account, the mathematics is there only to facilitate a smoother description of the physical situation.

So far so good. But what of more sophisticated applications of mathematics? Supporters of the indexing account have not yet developed the account beyond the simple numerical examples but it would be interesting to see how it might be extended to accommodate a broader range of scientific examples. Indeed, this has been the focus of much of the criticism of the account thus far. This problem is that the indexing account is, at best, not fully fleshed out. Were science to do nothing more than make descriptive claims about distances, temperatures and the like, the indexing account would be on firm ground. But once we consider more complex scientific examples we find only unsatisfying promissory notes from advocates of indexing.

Take for example the case of the Lorentz contraction. Colyvan (2001, pp. 50–51) argues

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[2] Mark Balaguer (1998) suggests a very similar account for the use of mathematics in statements such as "the physical system *S* is 40 degrees Celsius" (pp. 131–141). Balaguer argues that all that's required here is that "the physical world holds up *its end* of the 'empirical-science bargain'" (p. 134). According to Balaguer there is no need for the mathematics in question to be true or for there to be mathematical objects. The mathematics is just representing the physical relationships in question.

that the Minkowski metric helps explain the Lorentz contractions and so such physical events are at least in part explained by mathematics (or by geometry, if you prefer). Daly and Langford (2009, pp. 5–6) assert that the mathematics of special relativity is just indexing the physical realm. Maybe so, but let's see the details. Admittedly, the special relativistic space-time manifold is rather well behaved and the three spatial dimensions are modelled or indexed, if you prefer, by R^3, but what is crucial about the metric (and this was the point of the example) is that the time dimension has a minus sign (or alternatively, the metric is the standard four-dimensional Euclidean metric but with time measured by purely imaginary numbers). What, according to Daly and Langford, is being indexed by the minus sign in the metric? What is the property of time that corresponds to this piece of mathematics? Daly and Langford claim that the standard explanations of the Lorentz contractions can be accommodated by the indexing story but they do not produce the goods.

3. The Borsuk-Ulam Theorem

In another example of a mathematical explanation, Colyvan (2001, 49–50) argues that the Borsuk-Ulam theorem of topology can be used to explain surprising weather patterns: antipodal points on the Earth's surface which have the same temperature and pressure at a given time.[3] Before we go any further, let's distinguish two different senses of mathematical explanation: intra-mathematical explanation and extra-mathematical explanation. Intra-mathematical explanation is an explanation appearing within mathematics, when mathematicians speak of one piece of mathematics explaining why another piece of mathematics holds. Extra-mathematical explanation is the more controversial mathematical explanation of physical facts. The present authors have argued for the latter and it is these which are at issue in the present debate.[4] With this distinction in mind, Colyvan's claim is that the Borsuk-Ulam theorem offers an extra-mathematical explanation of the weather patterns in question and this explanation arises from the intra-mathematical explanation provided by the proof of the theorem.[5]

Daly and Langford rely on indexing for the straightforward parts of the story (temperature being modelled by a real-valued function mapping from the Earth's surface

[3] The result in question is the following corollary of the Borsuk-Ulam theorem: Let $f: S^2 \rightarrow R^2$ be a continuous map, then there exists an $x \in S^2$ such that $f(x) = f(-x)$ (Kosniowski, 1980, p. 159).

[4] We do not mean to suggest that these are two different kinds of explanation, requiring different philosophical treatments. All we are suggesting is that, for present purposes at least, it is useful to distinguish these two different senses of 'mathematical explanation'.

[5] With Colyvan (2001) we assume here that it is the explanatory proof of a theorem that offers the ultimate explanation of any empirical applications of the theorem. There is good reason to be suspicious of this claim though—at least as a general account of extra-mathematical explanation. Baker (forthcoming) defends the view that even mathematical theorems that lack explanatory proofs can feature in extra-mathematical explanations. Accepting this broader class of extra-mathematical explanations, however, just makes things worse for Daly and Langford. In any case, we persist with the shared assumption here of intra-mathematical explanations being the ultimate source of extra-mathematical explanation, at least in the cases in question.

and so on), but when we get to the crux of the story about the Borsuk-Ulam theorem explaining (in the extra-mathematical sense) the weather pattern in question, they deny that the proof of this theorem is explanatory (in the intra-mathematical sense). They assert that the proof of the theorem merely *justifies* the theorem but the proof is not *explanatory* (in the intra-mathematical sense) (Daly and Langford 2009, p. 648).[6] They provide no argument as to why they take this to be the case. In particular, they do not say whether they take all mathematical proofs to fail to be explanatory or whether it is just this one. In private communication, however, they inform us that they take proofs to merely justify theorems. However this flies in the face of mathematical practice and, as such, seems untenable as a response.

Daly and Langford are simply denying that there is intra-mathematical explanation, but mathematicians routinely distinguish explanatory and non-explanatory proofs. For instance, Fields medalist Timothy Gowers and his coauthor Michael Neilson (2009, p. 879) claim that "for mathematicians, proofs are more than guarantees of truth: they are valued for their explanatory power, and a new proof of a theorem can provide crucial insights".[7] The only plausible option for Daly and Langford is to deny that the proof of this theorem *in particular* is explanatory, but this requires argument.[8] Moreover, even if they can mount a case for this proof being unexplanatory, we just need to change the example. Pick one of the many mathematical theorems with an (intra-mathematical) explanatory proof and find an application for the theorem in empirical science. From this application, construct an (extra-mathematical) explanation of the corresponding physical fact. Daly and Langford, cannot get off the hook here by simply denying that which is inconvenient for their philosophical account.

Another option for Daly and Langford, would be to weaken their stance on intra-mathematical explanation and allow that some proofs are explanatory but that mathematics does not explain in empirical science. But this too is problematic. The difficulty here is that intra-mathematical explanations may spill over into the empirical realm. The idea is that if, say, the Borsuk-Ulam theorem is explained by its proof and the antipodal weather patterns are explained by the Borsuk-Ulam theorem, it would seem that the proof of the theorem is at least part of the explanation of the antipodal weather patters. Our claim here is not that explanation is transitive, but rather that in some contexts the mathematical details of the proof will be required for a full explanation of

[6] Their text is ambiguous on this issue. Indeed, they assert that mathematical proofs are merely justificatory and then immediately contradict this when they claim that proofs also show why the theorem in question is true (Daly and Langford 2009, p. 648). Personal communication (27/4/10) with the authors, however, has clarified that they do indeed hold the position that proofs are merely justificatory and do not explain mathematical theorems.

[7] See also, Robert Minio (1984) for more on mathematicians discussing mathematical explanation.

[8] We're not suggesting that there is no such argument to be made. However, there is prima facie evidence against the proof in question being unexplanatory: namely, the connection between (the proofs of) the Borsuk-Ulam theorem and other fixed point theorems (most notably, Brouwer's celebrated fixed-point theorem). These connections seem to give some plausibility to the proof in question being explanatory.

the empirical phenomenon in question.[9]

Maintaining that there are intra-mathematical explanations but that these explanations never permeate beyond the boundaries of mathematics is prima facie implausible. It also does violence to our intuitions in specific cases. Imagine that we have a square wooden board that is divided into an 8×8 grid of smaller squares. We now cut off two of the opposite corner squares from this board, and try to cover the remaining 62 squares with 2×1 dominoes. After hours of fruitless effort we abandon this task. The best explanation for our failure is provided by the following geometrical argument: imagine coloring our original board with a checkerboard pattern of alternating black and white. When two opposing corner squares are removed, this then removes two squares of the same color. So there are now 30 squares of one color and 32 squares of the other color. Each domino covers exactly one square of each color. Hence a complete tiling of the mutilated board is impossible. Both this proof and the resulting impossibility theorem can be formalized, and generalized to boards of size $n \times n$. Yet it seems clear that citing the bare theorem, without the accompanying argument, fails to explain why we failed to tile our wooden board.

Another problem for Daly and Langford is that they do not tell us how the indexing story will work in more complicated cases. Even if they are right about the success of their account thus far (and they are not!), more serious problems abound in the recent literature.[10] In Lyon and Colyvan (2008), the main example involves explanations of galactic stability utilising Hamiltonian formulation of the classical theory. The point in a nutshell is that if the mathematics is indexing anything here it is indexing points in phase spaces. And while Daly and Langford may be happy to help themselves to many nominalistically-questionable entities, we suggest that they should draw the line at possibilia. Or consider even more problematic cases where the mathematics being used is inconsistent (Colyvan 2009). Are Daly and Langford committed to inconsistent mathematics indexing as well? If so, what does it index?

In the phase space example, the mathematics would be indexing, amongst other things, non-actual states of the system in question—ways this particular part of the world might have been. Some might allow that such spaces are indexing possible worlds, or the like, but for nominalsist such as Melia, and Daly and Langford, this does not seem a plausible option.[11] After all mathematical nominalists eschew mathematical objects because of epistemic and other problems associated with admitting them into one's ontology. Avoiding commitment to mathematical objects at the expense of admitting possible worlds is simply not a good deal. The case with inconsistent mathematics is even worse. It seems that here defenders of indexing must hold that inconsistent mathematical theories, such as (arguably) the early calculus, when used in applications are indexing

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[9] Proofs of mathematical theorems, however, need not be the only source of explanations in mathematics. Nor does this prohibit theorems with only non-explanatory proofs from featuring in explanations elsewhere. See Baker (forthcoming), for more on this.

[10] See, for example, Batterman, (2010), and Lyon and Colyvan (2008).

[11] David Malament (1982) made a similar point in relation to Hartry Field's nominalisation program.

impossibilities, such as magnitudes that are both zero and not zero. Indexing only has plausibility when the mathematics can be taken to be representing something that is nominalsitically kosher. Physical lengths and the like are kosher but non-actual states of the world and impossible states are not. In any case, it is just not clear how the indexing story is supposed to work in many realistic scientific cases like these. We suggest that those who deny that there are mathematical explanations need to look beyond the simple cases where indexing looks plausible.[12]

4. Cicadas and Prime Numbers

Next consider a case study from biology, originally presented in this context by Baker (2005). This example concerns the life-cycles of periodical cicadas and has been the focus of considerable attention in the recent philosophical literature on mathematical explanation in science. A striking feature of the two north-American subspecies of periodical cicada, the 13-year cicada and the 17-year cicada, is that they both have periods that are prime numbers. Biologists have argued that prime periods are likely to have evolved because they minimize the frequency of intersection with periodical predators. This argument depends on some elementary results from number theory. Baker puts this forward as an example of a mathematical explanation of a physical phenomenon. One of Baker's points is that the indexing strategy looks less plausible in this case than it does in Melia's original examples involving measuring lengths, because length-in-metres is an arbitrary unit in a way that duration-in-years is not. This, in turn, means that the numbers 13 and 17 play non-arbitrary roles in both the description and explanation of the phenomenon in question.

Daly and Langford take issue with this non-arbitrariness claim, arguing that there are various alternative units of time measurement, such as seasons and days, which are also biologically significant. One obvious rejoinder here is to point out that biologists do not in fact use any of these other units in describing or discussing cicada life-cycles, which suggests that years are the most salient unit in this context. Daly and Langford (2009, p. 653) consider this response but remain unmoved by it. They argue that the widespread use of a standard unit does not force the conclusion that it is explanatorily privileged, and claim that this can be seen by looking at a "parallel case" involving the measurement of galactic distances using light years. Switching to some other unit of measurement, for example kilometres, would be cumbersome and impractical given our entrenched practice, but not because the unit of a light year is somehow explanatorily privileged.

Daly's and Langford's argument here is flawed in a number of ways, but the underlying problem is that it flies in the face of actual scientific practice. They write that "it cannot be that the properties of the primes are of crucial explanatory importance if we could just as well measure the cicada cycle in seasons" (Daly and Langford 2009, p. 653). Just as well by what lights? The fact is that biologists do measure the life-cycle of the cicada in years, indeed the taxonomic names given to the two cicada species (*Magicicada*

[12] See Bueno and Colyvan (2011) for a more sophisticated account of the relationship between mathematics and empirical science.

Tredecim, *Magicicada Septendecim*) reflect the importance of this feature. Not only this, but biologists take it that there is something to be explained—at least potentially—concerning the primeness of these period lengths (measured in years). For example in a paper in the journal *Ecology*, zoologist Anthony Ives and his co-authors write that "periodical cicadas present numerous puzzles for biologists. ... [T]he 13- or 17-year periods of cicadas suggest there is something important about prime numbers" (Lehman-Ziebarth et al. 2005, p. 3200).

It is here that the alleged parallel with the galactic distance case most obviously breaks down. It would be bizarre for physicists to pose the question of why galaxies *X* and *Y* are both a prime number of light years away. This suggests, *contra* Daly and Langford, that measurement-in-years is explanatory privileged in the cicada case in a way in which measurement-in-light-years in the galactic case is not. It is also possible, we think, to say something about why there is this contrast between the two cases. Both years as a measure of duration and light years as a measure of distance are in an important sense "parochial" in that they are based on the period of rotation of the Earth. The difference is that, in the cicada case, the phenomenon being measured and explained is itself parochial in just the same way. The period of rotation of the Earth makes a huge difference to the evolutionary history of terrestrial organisms,[13] hence measurements using years as units can sensibly feature in the descriptions and explanations of biological phenomena.

Aside from the issue of arbitrariness, Daly and Langford (2009, p. 657) also sketch what they claim to be a nominalistically acceptable alternative explanation of the cicada life-cycles. For a given subspecies of cicada, the nominalistic explanation of the duration of their life-cycle is that the particular duration in question minimized the cicadas' contact with periodical predators given the specifics of their particular environment and evolutionary history. Numbers—prime or otherwise, depending on the units chosen—serve only to index these durations, and it is the durations themselves which are doing all of the explanatory work. The underlying problem with this proposal, as with their earlier analysis of arbitrariness, is that it is completely disconnected from scientific practice. If their explanation is being offered as a serious *scientific* explanation of cicada life-cycles then it must be judged by the prevailing standards of science. It seems unlikely that biologists would be at all impressed with Daly's and Langford's proffered alternative.[14] Not only this, but we believe that the biologists would be right to reject the nominalistic alternative, because it can do much less in the way of explanatory work than the standard number-theoretic explanation. Couched as it is in the particularities of the given

[13] For example, once per year (in non-equatorial regions) there is a sustained period of cold weather, and this has an impact on breeding and survival of many terrestrial species.

[14] Perhaps Daly and Langford should accept the challenge posed by Burgess and Rosen (1997) and see if their alternate explanation is publishable in a leading biology journal. After all, what is at issue here is a question of whether Daly's and Langford's proposed explanation is superior to the standard one accepted by working biologists. For this task we can set aside issues concerning nominalism and Platonism, which biologists lack the relevant expertise to make the judgments in question. But biologists can, and should, be relied upon to judge what counts as the best explanation of biological facts (and whether there is anything in need of explanation).

ecosystem and its history, the nominalistic explanation is both less general and less robust. It is less general because once you have explained why one cicada subspecies in geographical area *A* has (say) a 13-year life-cycle, you have to start all over again to explain why a second cicada subspecies in area *B* has a 17-year life-cycle. By contrast, the number-theoretic explanation can be applied to a new ecological situation by simply plugging in the relevant constraints (for instance that prevailing temperatures restrict life-cycles to a range of 14 to 18 years) and deducing the most likely resulting life-cycle duration, in this case 17 years. The nominalistic explanation is also less robust. It misses the fact that the details of what predators are around, and their various life-cycles, are largely irrelevant to the advantageousness of prime periods. In other words, what needs to be explained is (in part) the *stability* of prime life-cycles. Thus there is a modal aspect, and this is much more problematic for Daly's and Langford's indexing strategy than they appreciate (since, once again, it would seem they need to index possibilia).

One further feature of the cicada story, that's worth mentioning, is that the length of the two cicada life-cycles in question are *consecutive primes*: 17 is the next prime after 13. What needs explaining is not merely the fact that the life-cycles in question are primes, but that there is no prime between them. This, though, is easy to make sense of on the standard account Baker advances. It is clear that small prime life-cycles are not efficient for purposes of predator avoidance. (Life-cycles of 3, for instance, intersect with predators with life-cycles of 6, 9, and so on.) But very long life-cycles, of any kind, are not biologically feasible for other reasons. The preferred cicada life-cycles are thus squeezed from above and below, but leaving more than one viable option. It's no accident that the prime life-cycles in question were not, say, 11 and 17 years. Life-cycles of 11 and 17 would still succeed in helping the cicada populations avoid the relevant predators, but an explanation would be required for why life-cycles of 13 years were not found. But with consecutive prime life-cycles of 13 and 17 years, there is no such gap in the explanation. Daly and Langford do not address this important feature of the cicada case.

5. Conclusion

We have focused on Daly's and Langford's proposal for developing the indexing approach to mathematical nominalism. Theirs is the most recent and most detailed attempt to address some problematic cases for the indexing account: cases of mathematical explanations of empirical facts. Mathematical explanations of empirical facts are problematic for the indexing account for two reasons. First, if mathematics can contribute to explanations in empirical science, the role of mathematics in science is more complicated and more central than is allowed for by the indexing account. The latter sees mathematics as merely standing proxy for physical details and the latter are what matters. But if mathematics is centrally involved in explanations, the mathematics is a large part of what matters. The second way in which mathematical explanations are problematic for the indexing account is that at least some of the cases in question are examples taken directly from science and the explanations in question are genuinely scientifically-acceptable explanations. The indexing account needs to deny that mathematics is explanatory in the cases at issue. We have argued that Daly and Langford have given us no reason to doubt that we have mathematical explanations in any of the key examples in the literature. Of course, if you're in the grip of a causal theory of explanation and an "indexing" theory of mathematical applications, mathematical explanations will strike you as odd and perhaps in need of being dispensed with. If so, dispense with them! Daly and Langford have not done enough in this regard. It is also worth noting how many times they find themselves at odds with scientific practice: they seem to be committed to no mathematical proof ever being explanatory (contra mathematical practice); they suggest that biologists are wrong in thinking that there is something significant (and non-conventional) about measuring the life cycle of cicadas in years; and they suggest that biologists are mistaken in thinking that the prime cycles are anything other than an artifact of an arbitrary measuring scale.

We are not suggesting that philosophers should never criticise science or that philosophers should not propose revisions to current science. We do think that before such criticisms and revisions are advanced, philosophers need to be up to speed on the relevant science and have good reasons for the revisions in question. Biologists have good *biological* reasons for measuring in years in the cicada case and they do think there is something to explain here. In the case of intra-mathematical explanation, mathematicians do claim that some proofs are explanatory and they do so for good *mathematical* reasons. We are reluctant to overturn such scientifically-based accounts. As for the reasons for Daly's and Langford's proposed revisions, they seem to be motivated at every turn by poorly-supported metaphysical theories. Commitments to philosophical theories such as nominalism, a causal theory of explanation, or the "indexing" view of mathematical applications are not good reasons for rejecting well-supported scientific and mathematical claims.

It might be argued that until we provide a full account of mathematical explanation and how it functions in the cases in question, we are in no position to be criticising the indexing account. No one, it seems, has a complete story of what is going on in the cases in question, so we are left with an unsatisfying standoff. We agree that further philosophical work is required to elucidate the notions of mathematical explanation under discussion, and that this work needs to be done independently of the present debate (Colyvan 2012; Mancosu 2008; Mancosu 2011). But it is a mistake to suggest that such work is required in order to adjudicate the matter at hand: whether the indexing strategy provides adequate alternative explanations in the cases in question. After all, in this paper, we are relying on the standard scientific explanations on offer and in so far as these accounts invoke mathematical explanations, we stand on firm ground. The burden of proof is clearly with defenders of the indexing strategy who deny the legitimacy of these mathematical explanations. This is not to say that those of us who countenance mathematical explanations in science can rest easy. As we say, a philosophical account of mathematical explanation is something sorely needed for both philosophy of mathematics and philosophy of science. But we do not need such an account in order to make the point that standard scientific and mathematical practice seems to invoke mathematical explanations. It is Daly and Langford who must reconcile their philosophical position with the apparent conflicts with the practices in question.

We have seen that the Daly and Langford attempt to do away with mathematical explanations of physical phenomena comes up short. It remains an open question whether the indexing strategy can be improved upon and made to work in anything other than toy cases. This is an interesting question, which we leave for future work. What is clear, however, is that such work will need to engage with actual scientific examples and provide a philosophically satisfying account that pays due respect to mathematical and scientific practice.[15]

References

Baker, Alan. 2005: 'Are there Genuine Mathematical Explanations of Physical Phenomena?'. *Mind*, 114, 223–238.

——2009: 'Mathematical Explanation in Science'. *British Journal for the Philosophy of Science*, 60, 611–633.

——forthcoming: 'Science-Driven Mathematical Explanation'. *Mind*.

Balaguer, Mark. 1998: *Platonism and Anti-Platonism in Mathematics*. New York: Oxford University Press.

Batterman, Robert W. 2010: 'On the Explanatory Role of Mathematics in Empirical Science'. *British Journal for the Philosophy of Science*, 61, 1–25.

Bueno, Otávio and Colyvan, Mark. 2011: 'An Inferential Conception of the Application of Mathematics', *Noûs*, 45, 345–374.

Burgess, John P. and Rosen, Gideon. 1997: *A Subject with No Object: Strategies for Nominalistic Interpretation of Mathematics*. Oxford: Oxford University Press.

Colyvan, Mark. 2001: *The Indispensability of Mathematics*. New York: Oxford University Press.

——2002: 'Mathematics and Aesthetic Considerations in Science'. *Mind*, 111, 69–74.

——2007: 'Mathematical Recreation Versus Mathematical Knowledge'. In M. Leng, A. Paseau, and M. Potter (eds.), *Mathematical Knowledge*. Oxford: Oxford University Press, pp. 109–122.

——2009: 'Applying Inconsistent Mathematics'. In O. Bueno and Ø. Linnebo (eds.), *New Waves in Philosophy of Mathematics*. Basingstoke UK: Palgrave MacMillan, 2009, pp. 160–172.

——2010: 'There is No Easy Road to Nominalism'. *Mind*, 119, 285–306.

——2012: *An Introduction to the Philosophy of Mathematics*, Cambridge: Cambridge University Press.

Daly, Chris and Langford, Simon. 2009: 'Mathematical Explanation and Indispensability Arguments'. *Philosophical Quarterly*, 59, 641–658.

Gowers, Timothy and Neilson, Michael. 2009: 'Massively Collaborative Mathematics'. *Nature*, 461 (15 October 2009), 879–881.

Kosniowski, Czes. 1980: *A First Course in Algebraic Topology*. Cambridge: Cambridge University Press.

Lehman-Ziebarth, Nicolas, Heideman, Paul P., Shapiro, Rebecca A., Stoddart, Sonia L.,

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Hsiao, Chien Ching Lillian, Stephenson, Gordon R., Milewski, Paul A., and Ives, Anthony R. 2005: ‘Evolution of Periodicity in Periodical Cicadas’. *Ecology*, 86, 3200–3211.
Leng, Mary. 2005: ‘Mathematical Explanation’. In C. Cellucci and D. Gillies (eds.) *Mathematical Reasoning and Heuristics*. London: King’s College Publishing, pp. 167–189.
Lyon, Aidan and Colyvan, Mark. 2008: ‘The Explanatory Power of Phase Spaces’. *Philosophia Mathematica (3),* 16, 227–243.
Malament, David. 1982. ‘Review of Field’s Science Without Numbers’. *Journal of Philosophy,* 79: 523–534.
Mancosu, Paolo. 2008: ‘Mathematical Explanation: Why it matters’, in P. Mancosu, (ed.), *The Philosophy of Mathematical Practice*. Oxford: Oxford University Press, pp. 134–150.
——2011: ‘Explanation in Mathematics’, in E.N. Zalta (ed.), *The Stanford Encyclopedia of Philosophy* (Summer 2011 Edition), URL = <http://plato.stanford.edu/archives/fall2008/entries/mathematics-explanation/>.
Minio, Robert. 1984: ‘An Interview with Michael Atiyah’. *Mathematical Intelligencer*, 6, 9–19.
Melia, Joseph. 2000: ‘Weaseling Away the Indispensability Argument’. *Mind*, 109, 455–479.
——2002: ‘Reply to Colyvan’. *Mind*, 111, 75–79.
Smart, J.J.C. 1990: ‘Explanation—Opening Address’. In D. Knowles (ed.), *Explanation and Its Limits*. Cambridge: Cambridge University Press, pp. 1–19.
Steiner, Mark. 1978: ‘Mathematics, Explanation, and Scientific Knowledge’. *Noûs*, 12, 17–28.