

Assessing the “Empirical philosophy of mathematics”

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Abstract

In the new millennium, there have been important empirical developments in the philosophy of mathematics. One of these directions is the so-called “empirical philosophy of mathematics” (EPM) of Buldt, Löwe, Müller and Müller-Hill, which aims to complement the methodology in philosophy of mathematics with empirical work. Among other things, this includes surveys of mathematicians, which EPM believes to give philosophically important results. In this paper I take a critical look at the sociological part of EPM as a case study of sociological approaches to the philosophy of mathematics, focusing on the most concrete development of EPM so far: a questionnaire-based study by Müller-Hill. I will argue that the study has many problems and the EPM conclusion of context-dependency of mathematical knowledge is unwarranted by the evidence. In addition, I will consider the general justification and criteria for introducing sociological methods in the philosophy of mathematics. While surveys can give us important data about the philosophical views of mathematicians, there is no reason to believe that mathematicians have a privileged access to philosophical questions concerning mathematics. In order to be philosophically relevant in the way EPM claim, the philosophical views of mathematicians cannot be assessed without considering the argumentation behind them.

1 Introduction

In the philosophy of mathematics of the new millennium, empirical approaches to epistemological questions have become increasingly popular. These empirical approaches can take many forms. One research field takes the best empirical data from psychology, cognitive science and neurobiology to be philosophically relevant. Experiments, most often involving small children and animals, are used to explain the development of basic mathematical concepts like natural numbers. Empirical scientists like Dehaene (2011), Spelke (2011) and Nieder (2011) have presented theories

about the origins of arithmetical knowledge, and these have been developed philosophically by the likes of De Cruz, Neth & Schlimm (2010) and Pantsar (2014).

Another important development is the philosophy of mathematical practice, made known by the likes of Aspray and Kitcher (1988), Corfield (2003) and Mancosu (ed.) (2008). In this approach, the main objective is to move philosophical discourse away from idealized mathematical theories and methodology toward mathematics as it is actually practiced. The philosophy of mathematical practice is a wide research field with versatile methodology, ranging from historical analysis of mathematical texts to contemporary questions about mathematical practice including such questions as visualization, use of computers and the interaction of mathematics with other sciences - among many other issues (Mancosu (ed.) 2008).

As part of this rising popularity of the philosophy of mathematical practice, philosophers have become increasingly interested in the way mathematicians use and understand the key concepts in the philosophy of mathematics. Two such key questions are how the concepts of *proof* and *knowledge* are used among mathematicians. In the philosophy of mathematical practice, it is often noted that the robust conception of formal proof based on Frege's (1879) *Begriffsschrift* - and developed in great detail in *Principia Mathematica* by Whitehead and Russell - rarely corresponds to actual mathematics. But while it is indeed acknowledged that mathematicians mostly publish informal proofs instead of complete formal derivations, this has generally been seen merely as facilitating communication - not as weakening the formal criteria in mathematics. However, this has been challenged recently by Löwe & Müller (2008, p. 95):

...mathematicians publish informal proofs. However, there is more to informal proof than ease of communication. It just isn't the case that mathematicians have a derivation in mind and transform it into an informal proof for publication in order to reach a wider public—the entire procedure of doing research mathematics rests on doing informal proofs. [...] We need to take seriously the fact that derivations are hardly ever used. Subscribing to the tempting image of the derivations as the real objects of mathematical study to which informal proofs are imperfect approximations would be a violation of our maxim of taking mathematical practice seriously.

Rather than practical approximations, according to Löwe & Müller the informal proofs of mathematicians are in fact central to mathematical practice. If this is indeed the case, it would have

important consequences in the epistemology of mathematics. What would it mean, for example, to say that someone *knows* a theorem? In the traditional picture, in which informal proofs are mere short-hands for logical derivations, the idea is that to know a proof is to grasp its content and to be able, in principle, to work out the formal details of an informal proof (Steiner 1975, 100). But what if informal proofs do not work like that? What if they are, as Löwe and Müller argue, hardly ever transformed into formal derivations? Mathematical proof, and mathematical knowledge-ascriptions, would seem to be much more heterogeneous phenomena. This is an idea that Löwe & Müller, together with Buldt and Müller-Hill, want to take seriously in what they call the *Empirical philosophy of mathematics*.¹

The main purpose of Empirical philosophy of mathematics (from here on EPM) is “the development of a philosophical study of mathematics as a discipline based on empirical facts” (BLM, p. 323) and its “theoretical foundation should contain a sustained argument for the methodology of conceptual modeling, and should contain in particular an argument for the necessity to empirically check those philosophical theories that were established via this method” (ibid.).

Emphasizing the connections to empirical facts and mathematical practice is of course nothing new in philosophy. From Quine (1966, 1975) to Maddy (1997, 2007), the importance of doing philosophy in close connection to empirical results and scientific practice is a well-established field of study. BLM see themselves as a part of his development, and they want to conduct:

philosophical study of mathematics as a discipline based on empirical facts [...]. Such an approach could be called “naturalistic”, as in Maddy (1997), or it could be called a “Second Philosophy of Mathematics”, as in Maddy (2007). We shall use the label “*Empirical Philosophy of Mathematics*” in order to stress the fact that there is actual empirical work to be done. (BLM, p. 323)

¹ The account here is based on five articles: Buldt, Löwe & Müller 2008 (BLM from now on); Löwe 2007; Löwe & Müller 2008 (LM); Löwe, Müller & Müller-Hill 2010 (LMM); Müller-Hill 2009. There is also a book expanding the EPM case (Löwe & Müller (eds.) 2010), as well as presenting other angles to the empirical study of mathematics. The project is also sometimes called “new epistemology of mathematics” in the literature (e.g. BLM).

The connection between EPM and Maddy, however, is not necessarily as close as BLM claim above. Maddy's naturalism and second philosophy refer to a "mathematics-first" type of approach in which mathematics should not be held to philosophical standards. In such an approach, it is not obvious that empirical research should play a major role in philosophy.

Indeed, rather than Maddy, EPM has clearer connections to experimental philosophy as presented in e.g. Knobe & Nichols (ed.) (2008, 2013), in which traditional philosophical methodology is augmented by empirical studies conducted for philosophical purposes. In EPM, this empirical work could potentially include a wide array of possible subjects and methods, as long as they are committed to empirical facts and the empirical checking of theories. Indeed, the existing body of work in the EPM program already employs a diverse methodology, including analysis of historical texts and contemporary mathematical journals, as well as more traditional philosophical considerations.

In this paper, I will focus on the currently furthest-developed empirical study carried out in the name of the EPM program, the questionnaire-based study of knowledge-ascriptions of mathematicians by Müller-Hill (2009). As well as being the most pronounced case of empirical work in EPM, it is also a paradigmatic case of deriving traditionally philosophical conclusions concerning mathematics from sociological data. In this paper I will thus use Müller-Hill's study as a case study of the EPM program, but also as a case study of the methodology and applicability of sociological approaches to the philosophy of mathematics.

While increasingly many philosophers are ready to welcome a closer connection to mathematical practice – and perhaps even the introduction of empirical results to the philosophy of mathematics – in this respect the methodology of EPM is much more controversial. They argue that philosophical questions can - indeed, should - be tackled by surveys and interviews of mathematicians, which "can yield genuine philosophical conclusions" (BLM, p. 325). In this, EPM clearly takes the philosophy of mathematics to the realm of experimental philosophy. As described by Knobe & Nichols (2008, p. 3), experimental philosophers:

think that a critical method for figuring out how human beings think is to go out and actually run systematic empirical studies. Hence, experimental philosophers proceed by conducting experimental investigations of the psychological processes underlying people's intuitions about central philosophical issues. Again and again, these investigations have

challenged familiar assumptions, showing that people do not actually think about these issues in anything like the way philosophers had assumed.

As we will see in the next section, the study of mathematical knowledge by Müller-Hill falls right into this category. It is carried out by conducting experimental investigation of the intuitions of mathematicians. Interestingly, it challenges a familiar assumption often accepted in the philosophy of mathematics: that mathematical knowledge is objective and independent of context.

2 The data

The main focus of EPM so far has been on the concepts of mathematical knowledge and proof. In the most concrete empirical development, Müller-Hill (2009) studied the way in which mathematicians use those concepts by the method of an Internet questionnaire. To start off the survey, Müller-Hill presented the questions whether mathematical knowledge is objective and whether mathematical proof can be defined, getting positive answers 82.4% and 89.2% of the time, respectively.

At the next stage of the survey, Müller-Hill presented a scenario in which a graduate student John works his way to prove his supervisor Jones' conjecture (JC). John's proof is accepted in a distinguished mathematical journal and the subjects were asked whether John knows that JC is true. As expected, 84.9% answered positively, 7.5% negatively and another 7.6% "can't tell".²

However, in the scenario it turns out that everything was not in order in John's proof. In fact, John discovers that there is in fact a counterexample to JC. Now the mathematicians were asked whether John knows that JC is false. 61.3% answered positively, 14.6% negatively and 24.2% could not tell. The considerable move from "yes" to "can't tell" is curious, but the real surprise came when the subjects were asked whether John knew that JC was true the morning *before* he learned of the counterexample. 71.0% answered positively, 19.3% negatively and 9.7% could not tell.

² Here I follow Müller-Hill's example and lump together the answers "yes" and "almost surely yes" into one positive category of answers, and likewise for the negative answers.

It should be noted that the last two questions were presented on the same web page, so the subjects could all the time see their answers to both of them. The majority of the test subjects did not seem to mind the contradictory position that mathematical knowledge is objective, but it is still possible that John knew JC to be true the day before, yet now knew it to be false.

Before we try to make sense of these seemingly contradicting data, I must present some methodological criticism of Müller-Hill's study. First, the target group was said to consist of "international research or teaching mathematicians from all branches of mathematics", led to the study by a link posted in Internet newsgroups. But since important claims are made based on the assumption that the subjects are *mathematicians*, can we be sure that the survey reached the correct target group? Indeed, what do we want to include in the definition of mathematician? Top researchers and high-school teachers are both mathematicians in the sense of the survey, yet in their occupations very different standards are required of mathematical knowledge. Since the understanding of mathematical knowledge is crucial to the answers, can we know that the heterogeneity of the subject group does not affect the data?³

Second, while the questions about the scenario of John and JC included the option "can't tell", the preliminary questions about objectivity and definability of mathematical proof did not. The objectivity of mathematical knowledge, in particular, is a deep philosophical question that many mathematicians would presumably not be ready to answer conclusively. To force them to do just that can affect the data dangerously.⁴

³ Further problems include the fact that only 108 of the 250 received responses were valid (this with extremely soft criteria: submitting personal data and one question answered was enough), of which 76 were from the target group. In addition, 21.0% percent of the target group answered that they do not possess a degree in mathematics.

⁴ Another important detail is that in the questionnaire, the two preliminary questions were given different alternatives. For the question about mathematical knowledge, there were only two options: yes and no. For the one about definability of proof, there were four options: agree, disagree and "strong" versions of both. It may not be insignificant that 60.8% of the subjects agreed (but not strongly so) with the definability of proof. It is worth asking how committed these subjects in fact were to the objectivity of mathematical knowledge.

Third, the amount of valid replies received in the study was ultimately quite low, only 53 in some questions and at most 74. In a sample that small, just a handful of “can’t tells” changed into conclusive answers can make an important difference. At best, a full 24.2% answered “can’t tell” to a question. One must wonder how many would have taken the same option in the question about the objectivity of mathematical knowledge, had it been available.

Fourth, the most important finding - that even after learning that JC was false, 71.0% still thought John knew JC to be true before the counter-example - seems to be largely unsupported by other data in the survey.⁵

With these and some minor issues, it seems that further study is necessary before making any conclusive claims about mathematicians’ beliefs - not to mention their possible philosophical conclusions. Nevertheless, the discrepancy in the main finding is an interesting one, and quite unlikely to be totally due to the methodological flaws in the study. The subjects were ready to claim that mathematical knowledge is objective, but they also answered that we can know a sentence and its negation to be true at different times. In LMM (2007, p. 12) it is revealed that of the 38 participants who thought that John knew JC to be false after the counterexample, a full 27 (71.1%) still answered that John knew JC to be true on the morning before the counterexample.

While eliminating the weaknesses from the survey (the lack of non-committal option in the question about the objectivity of mathematical knowledge is particularly pertinent here) could change the data considerably, it is still fair to believe that there is a large group of working mathematicians who hold both the position that mathematical knowledge is objective and that one can know a sentence to be true at time t and to be false at another time t' . This is certainly an interesting piece of data and it demands a deeper analysis.

3 Conclusions from the data

⁵ It should be noted, however, that there is also a second scenario described in Müller-Hill 2009 (pp. 14-16) which does give some added support for the main conclusions of the study.

The most surprising finding of Müller-Hill's study was that mathematicians were ready to accept that John knew JC to be true even after they have learned that there was a mistake in the proof. Based on the survey, mathematicians believe mathematical knowledge to be objective, yet they accept that John knows a sentence φ at time t and the sentence $\neg\varphi$ at time t' . Based on this, in Müller-Hill (2009) and especially in LMM (2010), together with other arguments, it is concluded that mathematical proof and mathematical knowledge are context-dependent for the subject group.⁶ Our old standards for mathematical proof and knowledge that take the two concepts to be objective, LMM (p. 16) argue, must be revised and in the philosophy of mathematics proof and knowledge should be seen as context-sensitive, i.e., the concepts of mathematical proof and mathematical knowledge should be seen to have different meanings in different settings.

We will return to that wider question in the next section, but for now let us focus on the survey data. That so many mathematicians were ready to make two conflicting statements about mathematical knowledge is a strong result, but what makes the result even stronger is the conclusion that, according to the survey, this contradicts with their *own* understanding of mathematical truth. In the survey, the vast majority agreed that mathematical truth is objective, yet they still accepted that we can know the same sentence to be both true and false at different times. Certainly there seems to be something peculiar in such conception of knowledge, and the claim of context-dependency is not unreasonable.

However, is the problem really with their conception of objective mathematical knowledge? The main difficulty with the conclusion of context-dependency that EPM makes is that we do not have a clear enough idea what the subjects understood the *relevant* notion of mathematical knowledge to be. It is true that the subjects clearly made contradictory knowledge-ascriptions when answering the survey. In this way, there is a sense in which mathematical knowledge was indeed context-sensitive for them. In the context of the initial question, they thought mathematical knowledge to be objective. In the context of the scenario involving John and JC, they appeared to contradict that.

In order to be philosophically relevant, however, we must be sure that the contradiction is due to something more substantial than simply a careless use of the word "know". If we want to make

⁶ The other arguments are developed furthest in LM 2008.

the kind of conclusions from Müller-Hill's data as EPM does, we would need to know what the test subjects understood by knowledge at each stage when answering the survey. The most explicit thing we know is that, when needing a definite answer, 82.4% of the subjects believed mathematical knowledge to be objective. But this is not quite the clarification we would need. First, we cannot tell whether they even meant to use the same concept of knowledge throughout the survey. Second, we don't know what the subjects interpret *objectivity* to mean.

Objectivity can be characterized both as "you can't make up your own rules" and "there exists a Platonist world of mathematical ideas". In order to make conclusions about context-dependency, we would need to clarify what objective mathematical knowledge is understood to be. In the survey of Müller-Hill, knowledge is only divided into objective and non-objective, and as such these are quite vague concepts. If, as the second scenario of Müller-Hill (pp. 14-16) suggests, knowledge is often ascribed even to a superficial memorizing of a proof, it is hardly surprising that mathematical knowledge ends up being context-dependent. However, this could equally well be interpreted as evidence that the subjects understood objectivity in a very weak way, thus explaining why they were prepared to accept both the objectivity of mathematical knowledge and the knowledge-ascriptions for both JC and its negation.

In any case, in a project of empirical epistemology, the result should not be interpreted automatically to concern mathematical knowledge in the *philosophical* sense – which is just what Müller-Hill and LMM seem to do. Without further questions, this is clearly something we cannot claim. What would be needed is a more thorough background questionnaire about the notion of mathematical knowledge that the subjects have, including questions differentiating between knowledge in the strict philosophical sense and knowledge sufficient to pass tests, and indeed, get articles published.

Without such information, all we know is that the subjects made inconsistent knowledge ascriptions when answering the questionnaire. But the philosophical relevance of that is questionable, since the subjects had no reason to be particularly careful in making knowledge ascriptions. They may well have thought, for example, that "to know" in the scenario involving John could mean something along the lines "had good reason to believe". That would not suggest context-dependency of mathematical knowledge. Rather, it could only be due to many uses of the verb "to know" being quite distant from any philosophical ideal of justified true belief, or similar.

In conclusion, there seem to be three possible interpretations of the data of Müller-Hill. The first one is that the survey gets the objectivity of mathematical knowledge correct, and the questions concerning the scenarios only worked to confuse the subjects. This interpretation can be rejected right away for two reasons. First, as stated above, there is too much that is not clear in the concept of objectivity to be settled this easily. And of course second, it would be highly questionable to outright reject survey data which clearly point out a difficulty in whatever concepts of objectivity and knowledge the subjects used.

The second interpretation is the one that LMM make: the survey gets the context-dependency correct, and when the subjects overwhelmingly professed to objectivity in the first question of the study, they did so under a mistaken conception. I see two problems also with this conclusion. First, we have seen that the concepts of knowledge and objectivity would have to be clarified in the survey. Second, this interpretation goes on to say that the majority of the 82.4% did not know what they were saying when they believed mathematical knowledge to be objective. The problems with the survey notwithstanding, this is a strong piece of data to just reject because it contradicts with other data in the study.

That brings us to the third interpretation: the answers reveal a possible confusion or equivocation which prevents us from making any strong conclusions. Based on the arguments above, I claim that this is the case. We simply cannot know that the concepts of knowledge, objectivity and proof were meant to be used in congruent enough ways to warrant strong conclusions about them. Subjects may well have taken the first question about objectivity to be a philosophical one, thus employing a strong conception of objectivity and mathematical knowledge. But when they approached the scenario about John and the JC, they may have had a different mindset: an everyday one in which we talk about knowing quite haphazardly.

Had it been clear that we are dealing with what most skilled mathematicians believe to be the nature of mathematical knowledge in the deep down philosophical sense, and would *still* be ready to ascribe this concept of knowledge both to the case of John knowing the falsehood of JC after the counterexample and him knowing its truth before the counterexample, *then* we could perhaps be warranted in making the kind of conclusions LMM do.

Even with these adjustments, however, there would remain some important questions to ask about the sociological approach. Most importantly, how much weight should we give the survey method

when it comes to philosophical questions? Certainly there can be value in such surveys, but are we warranted in making direct conclusions about philosophical concepts like knowledge in the manner of LMM - in particular when we have no idea of the argumentation and thought process behind the survey answers? We will deal with these questions in Section 5.

4 Context-dependency of mathematical knowledge

My criticism above concerns the Müller-Hill study and the interpretation of it in LMM (2010), but in LM (2008) there are also other arguments for the context-dependency of mathematical knowledge. What they oppose to is the view that there exists a uniform standard for accepted mathematical proofs. In particular, mathematical proofs are very rarely complete derivations of theorems from axioms, which is often given as the standard. As Fallis (2003, 55) writes:

The point of publishing a proof [...] is to communicate that proof to other mathematicians. In other words, the mathematician wants to get the particular sequence of propositions that he has in his mind into the minds of other mathematicians. Somewhat surprisingly, the most efficient way for the mathematician to do this is not by laying out the entire sequence of propositions in excruciating detail.

There should be very little to contest in Fallis' assessment: it is quite clear that the level of detail varies. Skilled mathematicians require less detail in order to understand the sequence of propositions that constitutes a proof, while a novice would no doubt benefit from more detailed derivations. In this fashion, LM (2008) arrive from the supposed invariant starting point (p. 92):

S knows that P iff S has available proof of P

at the context-dependent result (p. 104):

S knows that P iff S's current mathematical skills are sufficient to produce the form of proof or justification for P required by the actual context.

What is meant by "actual context" here are simply the requirements that proofs (or other justifications) have in that particular area of mathematical practice. So basically, mathematical knowledge according to LM is a widely varied concept determined by the subject and the surroundings. In this picture, formal derivation retains its power solely by being "important for the

foundations of mathematics, but [...] it hardly plays any role in determining the truth of 'S knows that P'" (LM, p. 105).

While the strict formalist picture of mathematics as complete derivations indeed is a bad fit with actual human mathematicians, to say that formal derivation "hardly plays any role" is an exaggeration. It is clear that mathematical communication is done largely informally, but it is commonly thought (e.g. Azzouni 2004) that the informal proofs come with the "promise" that there is an underlying formal structure that can be checked with algorithmic procedures. Indeed, with the development of modern computer tools for proof checking, this is not a mere philosophical idealization. Strictly formalized mathematics is now a well-established field and its growing importance goes directly against the claim of Löwe & Müller.⁷

Yet EPM takes the context-dependency even further: Buldt (BLM 2008, p. 314) states that "the conceptual framework of mathematics has changed so dramatically that, say, identifying Greek numbers with modern axiomatic characterizations just seems outrageous." I argue that also this is an exaggeration. Take the example of Euclid's (ca. 300 B.C.) proof that there are infinitely many prime numbers (*Elements*, Book IX, Proposition 20).

Let A, B, and C be the assigned prime numbers.

I say that there are more prime numbers than A, B, and C.

Take the least number DE measured by A, B, and C. Add the unit DF to DE.

Then EF is either prime or not.

First, let it be prime. Then the prime numbers A, B, C, and EF have been found which are more than A, B, and C.

Next, let EF not be prime. Therefore it is measured by some prime number (by proposition 31, book VII, *author's note*). Let it be measured by the prime number G.

I say that G is not the same with any of the numbers A, B, and C.

⁷ Perhaps the most notable medium for strictly formalized mathematics is the journal *Formalized Mathematics*, which uses the *Mizar* system. The *Mizar* library has the largest collection of strictly formalized mathematical proofs there is. There are also many other proof assistant softwares available, such as *Coq* and *Isabelle*.

If possible, let it be so. Now A, B, and C measure DE, therefore G also measures DE. But it also measures EF. Therefore G, being a number, measures the remainder, the unit DF, which is absurd.

Therefore G is not the same with any one of the numbers A, B, and C. And by hypothesis it is prime. Therefore the prime numbers A, B, C, and G have been found which are more than the assigned multitude of A, B, and C.

Therefore, *prime numbers are more than any assigned multitude of prime numbers*. Q.E.D.

It would indeed seem that Euclid's numbers are radically different from our modern axiomatizations. Numbers are not understood to be objects, but rather multitudes of unit line segments. Instead of divisibility, Euclid talks of *measuring*. And instead of the infinity of the set of prime numbers, as we would now state the matter, Euclid is proving that given a list of primes, we can find a greater prime.

There is much else in Euclid that looks unfamiliar to a modern reader. We would not write, for example, "A, B, and C" to refer to any finite list of prime numbers. This was a common method in Ancient Greece. Instead of a general method, an example was given. At first glance, then, it might seem that Euclid's proof does not count as a proof at all in the modern sense. Yet, to anybody who has taken an introductory course in number theory, Euclid's proof looks extremely familiar. Indeed, nowadays the standard proof of the infinity of prime numbers goes something like this:

Let p_1, p_2, \dots, p_n be the finite list of all prime numbers. Let $M = p_1 \times p_2 \times \dots \times p_n + 1$. Either M is a prime or it is not. Let us first suppose it is. In that case we have found a prime that is not on the list p_1, p_2, \dots, p_n . Let us now suppose M is not a prime. Following the Fundamental theorem of arithmetic⁸, M is the product of primes, and hence it is divisible by some prime p_i . Now p_i cannot be any of p_1, p_2, \dots, p_n , because if it were, it would divide $p_1 \times p_2 \times \dots \times p_n$. But in that case it would also divide 1, which is impossible. So p_i cannot be on the list p_1, p_2, \dots, p_n . In either case, we have found a prime that is not a part of the original list, so there is no greatest prime.

⁸ Euclid's proposition 31 in the Book VII is essentially stating the Fundamental theorem of arithmetic.

Compare that proof with Euclid's and it is immediately obvious that the structure of the proof is essentially the same. He works under a different conception of number, but he reaches the same result, by the same structure of argumentation. This is of course only one example and the current knowledge of arithmetic goes way beyond Euclid's number theory. There are also obviously great formal differences. There, for example, is no axiom of induction or infinity in Euclid. But is this enough to say that the numbers Euclid was working with are essentially different from our numbers?

We need to acknowledge that while mathematicians of different times work with different formal standards, as well as different interpretations of the basic concepts, that does not necessarily make their mathematics essentially different. Euclid saw numbers as multiples of the unit line segment, modern mathematicians often see them as sets. On a first look, it might indeed seem suspicious to identify the two. However, if this is indeed problematic in philosophy, it is problematic for EPM rather than for the traditional accounts that the mathematical proof to be objective. If mathematics is not about formal derivations, as the proponents of EPM claim, the fact that we cannot formally identify Euclid's numbers with ours seems to be of secondary importance. According to EPM, mathematics is *not* essentially about such formal proofs. While there are important differences in the notations and concepts, the remarkable thing is that the *structure* of Euclid's proof is essentially the same as the one in our informal proof above, which we could easily formulate into a proof in formal axiomatic arithmetic.

Indeed, rather than being an argument for the context-dependency of mathematical knowledge, Euclid's numbers seem more like an example of how *little* context-dependency there is in mathematics. Even with superficial differences, the same inferences can be used thousands of years apart with the same level of certainty. When Buldt claims that “identifying Greek numbers with modern axiomatic characterizations just seems outrageous”, the context-dependent aspects of mathematics – which undoubtedly exist – are given too much philosophical weight.⁹

⁹ Of course strictly speaking we *cannot* formally identify Euclid's numbers with modern axiomatic characterizations, so taken literally, I agree with Buldt. However, in the context of EPM the strict sense is clearly not the relevant sense. Even with the differences, I believe the

5 The relevance of sociological methods for philosophy

Above I have criticized some of the work done in the EPM program on two counts: the empirical methods and the philosophical conclusions. The methods can definitely be fixed and the conclusions would obviously have to be reassessed based on further empirical evidence. But regardless of the particular research details, there is also a third question that is independent of those issues: the overall justification of projects like Müller-Hill's study. For me it is hard to share the optimism of Müller-Hill and LMM when it comes to surveys as the basis of philosophical conclusions. I see no pressing reason to disagree with the traditional view that philosophical questions about mathematics can have correct answers and they can be reached with philosophical argumentation. In particular, there is little reason to believe that mathematicians have privileged access to those answers.

In this respect, EPM has clear parallels with experimental philosophy. As Williamson (2015) notes, in a broad sense experimental philosophy covers experimental inquiry with a philosophical purpose. Few philosophers are likely to find such empirical work unacceptable. The problem only comes with a narrower understanding of experimental philosophy, the so-called “negative program” which challenges many of the methods of analytic “armchair” philosophy.¹⁰

As presented in BLM, EPM does not aim to dismiss all traditional philosophical methodology, and as such should be included with the broader sense of experimental philosophy. However, in the conclusions of LMM (p. 16), the result of Müller-Hill is analyzed as follows:

a philosopher endorsing the traditional view of epistemology of mathematics is not able to accept the statement “John knows φ at time t and John knows $\neg\varphi$ at time t' .” However, a large number of our test subjects did exactly that, and so we either have to accept a notion

huge similarities in the structures of proofs etc. are more relevant when we consider claims of context-dependency of mathematical knowledge.

¹⁰ Weinberg, Nichols & Stich (2001) is perhaps the best-known paper advocating the negative program.

of mathematical knowledge that ignores the usage of this large portion of the community, or give up the traditional view. Based on our methodological position, we discard the former option and choose the latter.

Even forgetting the problems of Müller-Hill's study, this seems like a radical conclusion. Note that LMM are not merely saying that we should *re-evaluate* the former option: they claim that we should *discard* it. When traditional analytic philosophy is put against experimental philosophy, at least in this case EPM chooses the latter - thus suggesting parallels with the narrower negative reading of experimental philosophy.

This is quite problematic. It is easier to agree that empirical studies can be introduced to give a better overall understanding of philosophical issues. Understood in this way, we would not be advocating a radical revolution in the methodology of philosophy of mathematics. But the ease with which LMM dismiss traditional epistemology of mathematics in the above quotation suggests something else. While they may not explicitly reject traditional philosophical methods, they are ready to claim that a questionnaire-study that neglects all philosophical argumentation is enough to discard traditional epistemology of mathematics.¹¹

In EPM, the role of *mathematicians* in the philosophy of mathematics becomes considerably wider and stronger than it has traditionally been. In addition to providing the subject matter for philosophers of mathematics, even in the strong primary sense of Maddy, mathematicians are seen to have a privileged insight into philosophical problems concerning mathematics. In doing that, in addition to the negative program in experimental philosophy, the sociological angle of EPM has parallels with the sociology of science of Bloor (1976) and Latour (1987). Bloor (1976, p. 2) writes that:

The sociologist is concerned with knowledge, including scientific knowledge, purely as a natural phenomenon. His definition of knowledge will therefore be rather different from

¹¹ It should be noted that LMM do not suggest that we must *conclusively* discard the traditional view, only that we should now develop the context-dependent view. But this in itself is a strong result, as it clearly values the experimental methodology over the traditional one when the two are at odds.

that of either the layman or the philosopher. Instead of defining it as true belief, knowledge for the sociologist is whatever men take to be knowledge.

Based on the study of Müller-Hill and its interpretation in LMM, EPM seems to be open to such understanding of knowledge when it comes to mathematics. But while expanding the methodology of philosophy is easy to accept, weakening the role of argumentation in the philosophy of mathematics is much more controversial. Even if the subjects of Müller-Hill's study were undoubtedly the best experts on the question of mathematical knowledge, how much value do we want to give to philosophical opinions given completely without *arguments*? This is an extremely radical methodological revolution in the philosophy of mathematics. Taken to its extreme, it could replace all arguments from philosophy with the opinions of working mathematicians. While I am not claiming that EPM necessarily wants to go that far, it would be important to introduce philosophical criteria for evaluating the sociological explanations.

All this is not to deny the importance of sociological studies of mathematics. Geist, Löwe & Van Kerkhove (2010), for example, provide an important reminder that also the mathematical community functions in part on knowledge by testimony and a necessarily imperfect peer review system. There are many other areas where studying the mathematical community and the opinions of mathematicians can give us important insights in philosophy. But we should be careful not to overestimate the reliability of sociological methods. The strong sociological program took relativizing scientific knowledge to extreme distances:

[S]ince the settlement of a controversy is *the cause* of Nature's representation not the consequence, *we can never use the outcome – Nature – to explain how and why a controversy has been settled.* (Italics in the original) (Latour 1987, p. 99)

In Barnes, Bloor & Henry (1996, p. 168) it is claimed that:

Scientific boundaries are defined and maintained by social groups concerned to protect and promote their cognitive authority, intellectual hegemony, professional integrity, and whatever political and economic power they might be able to command by attaining these things.

This way, Latour aimed to reduce philosophy of science to the sociology of science and Barnes, Bloor & Henry presented science as a power game distinct from any considerations of finding out

truths. I fear that if left unchecked, the sociological aspects of EPM could lead to such dramatic interpretations and reduce the philosophy of mathematics to sociology of mathematics.¹²

In one way, however, the study of Müller-Hill is fundamentally different from the sociology of science. Whereas Latour and others emphasize that we should follow scientists “in action”, observing what they do, the study of Müller-Hill takes a fundamentally different approach: asking mathematicians what they *think* they do. Curiously, in BLM (2008, p. 320) there is a quote of Rota to the effect that the first line of study is the one we should prefer:

An honest conception [of mathematical activity] must emerge from a dispassionate examination of what mathematicians do, rather than from what mathematicians *say* they do, or from what philosophers think mathematicians *ought* to do. (Rota 1991, p. 108)
(Italics in the original)

This approach, which is at the heart of the study of mathematical practice, has many advantages. If we want to gain insight into mathematics that we can use in philosophy, the crucial question would seem to be what mathematicians *do*, not what they *say* they do. There is no real evidence that expertise in mathematics somehow makes one better positioned to have philosophical insights.

To present just one example, Kurt Gödel was perhaps the most important logician of the 20th century and a large part of his work is canonical in mathematical logic. Extremely few philosophers and mathematicians disagree with his logical and mathematical insights. Yet he was also a Platonist who held that mathematical objects such as numbers and sets exist independently of us in an abstract world of ideas that we access by a special mental faculty (Gödel 1964). Nowadays most philosophers do not give much weight to Gödel’s philosophical argumentation. His great ability as a mathematician is not seen to have given special insight to the philosophy of mathematics. But just as importantly, aside from the paper cited above, there is very little in Gödel’s logical writings to suggest his radical Platonism. In the systematic study of Gödel’s logical work, the philosophical angles seem largely irrelevant.

¹² This connection between EPM and experimental philosophy, as well as the strong sociological program, is not mere similarity in argumentation and methodology. In LMM (p. 8) connections to, e.g., Stich (2001) and Latour and Woolgar (1979) are explicitly stated.

This is the main methodological problem when sociological studies draw on the insights of mathematicians. Professional mathematicians may or may not have philosophical insights that correspond to their professional efforts. We must certainly appreciate their mathematical work, in which they have training, experience and high credibility. But when it comes to their philosophical opinions, the matter is quite different. When given completely without argumentation, there are two potential problems. First, we have no way of estimating the value and reliability of the philosophical view even for that particular mathematician. We do not know whether it is the result of systematic drawing of insights from the subject's mathematical work, or perhaps little more than a hunch. Basically, when applied as directly to philosophical questions as in the study of Müller-Hill, the methodology of surveys comes down to accepting the word of an authority as having philosophical weight. But if there is one great strength in the traditional methodology of philosophy of mathematics, it is the lack of appeal to authorities. This still seems like a good rule to follow.

Second, when accepting opinions without arguments as philosophically important, we are moving the focus on mathematicians from the area we know they have great expertise in, mathematical research and practice, to a much more dubious area. We simply have no reason to believe that mathematical expertise implies expertise in the philosophy of mathematics.

That is also why the methods of philosophy of mathematical practice provide at the moment a more interesting direction than the questionnaire-based studies. They are based on expanding the domain of philosophy of mathematics to include something potentially essential to mathematics as a science: how it is actually practiced. In such a manner, there is great potential for sociological approaches. Replacing an ideal picture of formal mathematical theories with real-world mathematics can give us important insights in philosophy. After all, it gives us information about the time-tested methods that have made mathematics successful.

Perhaps there are also philosophical beliefs inherent in them. It could well be the case that philosophical insights *do* emerge from practicing mathematics. However, determining this requires detailed philosophical study of mathematical practice, or a careful analysis of the argumentation behind those insights. It is not something we can simply assume.

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