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Mathematics and the New Technologies Part III: The Cloud and the Web of Proofs

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"New technologies, new mathematics" might seem a defensible slogan, as we try to show in this threefold set of papers and likewise "New mathematics, new philosophy of mathematics" will hardly be doubted by mathematicians and philosophers alike, but the difficult question is whether transitivity is applicable here so that we can conclude that new technologies also produce new philosophical questions and problems. The previous papers (Löwe 2014, this volume) and (Koepke 2014, this volume) support to some extent the idea that transitivity is possible: the peer review process, automated theorem proving, rewriting procedures, formal proof checking and so forth are convincing examples. The same goes, I believe, for experimental mathematics[1] where number crunching can lead to unexpected results, that would not have been available without the sheer computational power required,[2] or where the visualization of geometrical shapes can inform us about particular properties of that shape.[3] In this paper another example will be presented that further supports the derived slogan. It will deal with networks and knowledge distributed over such networks. More precisely, the use of blogs, networks and discussion within an internet community or, as we refer to it today, "in the cloud" will be discussed. Do such structures alter mathematical practice—for that is what I will focus on rather than mathematical results on their own[4]—and thereby introduce new philosophical questions

[1] See, e.g., (Baker 2008) and (Borwein & Devlin 2009).

[2] A famous example is Goldbach's conjecture. This has been checked up into the billions but, apart from the fact that the conjecture has been verified for all these numbers, the graph of the function $G(2n)$ = the number of ways $2n$ can be written as the sum of two primes shows a clearly strictly increasing function. The shape of that graph could generate some hypothesis about the behaviour of G.

[3] The best known examples of course being all fractal structures.

[4] I will not go into details here but the philosophy of mathematical practice is a relatively new branch in the philosophy of mathematics that focuses on the whole mathe-

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and problems? Before addressing the larger question, it will be helpful to have a brief look at a particular example, namely the Polymath project.

1 Polymath: a short presentation[5]

In January 2009 mathematician Timothy Gowers, a Fields medalist, opened a website (http://polymathprojects.org/), accessible for everyone, mathematicians and non-mathematicians alike, announcing that he was searching for a proof of a particular mathematical statement. The "invitation" was to join him in that search. Anyone could post a message about almost everything on the condition that it was somehow related to the proof search. In fact, a set of ground rules was announced to avoid the whole enterprise becoming all too chaotic. The hope was to find a proof and, if that were to occur, to publish the proof through the usual existing channels, namely mathematical journals, using a pseudonym, itself not an uncommon practice. This description is not essentially different from normal mathematical collaboration, except for the large number of people, including laypeople, involved. It remains to be seen whether this means that this approach is substantially new or just a matter of scale. That being said, let us first have a look at the problem itself.

The problem Gowers launched on the website is known as the Density Hales-Jewett Theorem (DHJ) for $k = 3$ at first, but later generalized to arbitrary k. This problem is part of the field of Ramsey theory, involving the combinatorics of colouring problems. The typical format of such problems is that "Given a so-and-so structure of sufficiently large size, then there will always be substructures that have a particular property". We shall say that such a property is *unavoidable*.

More specifically, DHJ for $k = 3$ states the following. Let the following be given:

> a set $K = \{1, 2, 3\}$ (the parameter k is the size of K, $\#K = k$)
>
> a set $N = K^n$, i.e., the set of all words of length n, using K

Next we need four definitions:

> A *variable word* is an element i of N where some places are replaced by variables, thus $k_1k_2 \dots k_ixk_{i+2} \dots\ k_n$ is a (one-place) variable word
>
> A *filled-in word* is a variable word where all variables have been replaced by the same element of K

matical process and not merely the endresults. See for a first impression, (Mancosu 2008) and (Van Bendegem 2004).

[5]This paper is related to (Allo *et al.* 2013). The Polymath project is more fully discussed there and is presented in reduced form here.

A *combinatorial line* is a non-empty subset I of N that contains all filled-in words for all elements in K of a given variable word. *Example*: if we take $n = 6$, then:

the subset $\{122\mathbf{1}32, 122\mathbf{2}32, 122\mathbf{3}32\}$ is a combinatorial line, as is

the subset $\{\mathbf{1}12\mathbf{1}32, \mathbf{2}12\mathbf{2}32, \mathbf{3}12\mathbf{3}32\}$.

Define the density d of a subset M of N by $d = \#M/\#N$

The DHJ for $k = 3$ says this: For every $d > 0$, there exists an n such that every subset M of N with density at least d contains a combinatorial line.

The "unavoidable" property here is the presence of a combinatorial line. So the theorem says that no matter how low the density of a particular subset, if the words made on the basis of the alphabet can be sufficiently long, there will always appear a combinatorial line.

In addition, one very special feature needs to be mentioned, namely that a proof already existed.[6] However, this proof relied on methods and techniques from domains far away from combinatorics, among other things, ergodic theory. So, as often happens in mathematical research, although one has a proof of the theorem, nevertheless this does not prevent mathematicians from searching an alternative[7] and, more importantly, an *elementary* proof, i.e., a proof using the concepts, proof methods and techniques of the domain itself.

What happened after the opening of the website? First, apart from Gowers, mathematician Terence Tao (UCLA), also a Fields medalist, joined the enterprise. After 6 weeks, 39 contributors had contributed 1228 comments (after every 100 comments, summaries were made by Gowers to keep an overview), not only a proof was found, but it became immediately clear how it could be generalized for arbitrary k. The proof has been published under the pseudonym: *D. H. J. Polymath* (which makes one think of course of other fictitious names in the history of mathematics, the most famous one no doubt being Nicolas Bourbaki).[8] Surely the most striking feature of the whole process is that "amateurs", both inside and outside of the mathematical community (so, e.g., high school teachers are here considered to be amateurs) could and did participate. Whether we should be as enthusiastic as Jacob Aron—see (Aron 2011)—in *New Scientist* and claim that this will "democratise the process of mathematical discovery" or as Michael

[6] See (Furstenberg & Katznelson 1991).

[7] An extreme example is Pythagoras' theorem for which at present some four hundred proofs exist. The website `www.cut-the-knot.org/pythagoras/index.shtml` lists nearly hundred basic variations.

[8] See (Polymath 2010).

Nielsen (2012), who states that "The Polymath Project is a small part of a much bigger story, a story about how online tools are transforming the way scientists make discoveries" (Nielsen 2012, 3), is of course another matter. The question to be dealt with here is whether philosophers of mathematics should be as enthusiastic as Aron and Nielsen about this phenomenon.

2 Yes, but is it philosophically relevant?

The answer that will be given to the question in the title above is basically one argument (scheme) that will be developed stepwise. Let us start with some simple premises that no one will doubt (although at this stage no statement has to be made about their philosophical relevance):

> (P1) Resources required for problem-solving available to mathematicians are finite.

In most cases the major resource will be time but not exclusively so. It must also involve, e.g., the (creative) capacities of the mathematicians involved and the externally available computing power (think, e.g., of the already mentioned rich area of experimental mathematics). All of these elements are clearly finite. What I am appealing to here, is nothing but the economical properties and aspects of problem-solving, that economists are perfectly aware of, as they are aware of the finiteness of resources or, to use their preferred term, the scarcity of goods.[9]

> (P2) There exist (many) mathematical problems that are beyond the resources of an individual mathematician or even a fixed group of mathematicians.

This premise can be supported in different ways. The first one is quite simply of an evidential nature. We have faced and are still facing with mathematical problems that either involve the use of computer programs, such as the four-colour theorem or the sphere packing problem, and pose a problem as to their correctness (see (Koepke 2014, 409–426) of this set of papers), or are amazingly long such as the well-known classification theorem for finite simple groups, estimated at fifteen thousand pages (although since then serious attempts are being done to reduce that number, down to some five thousand pages). Of course, one might argue that no matter how we

[9] Economical features are to be found everywhere in mathematics if one cares to look for it. Even a simple formula such as

$$\sum_{i=1}^{n} i = \frac{n(n+1)}{2}$$

reduces the computational power required to add the first n numbers.

got there, we do in fact have a classification theorem so it must have been in the range of what mathematicians can achieve after all. True, but it does indicate that we did at least move beyond the individual mathematician's resources and had to move to the community or group level.

The second way—personally my favourite—is an "absolute" argument, relying on a Gödelian argument. Take any mathematical theory M and its language L in an axiomatic formulation. Look at all the statements whose length is n, i.e., the statements that consist of n symbols. If there were a computable function $K(n)$ bounding those proofs for various n, then one would indeed have a positive solution to the *Entscheidungsproblem*: determine the length n of the statement, compute $K(n)$, and then try all proofs of length $K(n)$. But since the *Entscheidungsproblem* is not solvable in that way, the function $K(n)$ cannot be bounded by any computable function. Therefore $K(n)$ must have some enormous growth, when n becomes very large. This can be interpreted as an indication, that already $K(10)$ $K(20), \ldots$ will be enormous. But this is only a heuristical argument, like saying that certain computations take a long time because an algorithm is (in the limit) exponentially complex. Such proofs, if encountered, will pose a challenge to any group of mathematicians. The argument can be easily extended to mathematical communities in the sense that, if any mathematical problem can be settled by a group of mathematicians with a size bounded by some finite number and with finite resources, likewise bounded, then the argument can be repeated. Such a group would become (in a sense) a decision instrument. We repeat however that the argument can only be seen as an additional argument for the first way to support the premise because it could very well be that, for "modest" n, the problem does not really manifest itself and hence its impact would be very small if non-existent.[10]

To the extent that these two premises are indeed acceptable and defensible, the following intermediate conclusion is then rather straightforward:

> (IC1) Given the finite resources available, some mathematical problems will either not get solved or not get solved easily and/or quickly.

This by itself is not sufficient to conclude that the resource boundedness makes (parts of) mathematical practice philosophically relevant. After all, we never get all mathematical problems solved anyway as there are an infinite number of them.[11] The mere fact that at any specific time we have only solved a finite number of mathematical problems cannot be a conclusive argument at all. More is needed and that is the role the next three

[10] With thanks to Peter Koepke for having pointed out this possibility.

[11] To which should be added that most problems do not get solved for being not interesting and not worth the waste of the mathematicians' time.

premises are supposed to play. All three of them are, I assume, simple and straightforward and they too find their basis in the study of mathematical practice.

> (P3) In many cases, a solution to a mathematical problem introduces new concepts.

A general argument in support of this premise is that any question or problem relies on some presuppositions some of which have to do with the mathematical structure the question or problem is about. Once one has the natural numbers, the prime number concept follows easily, whereas, e.g., the concept of all natural numbers that in a decimal representation have seven sevens in them seems not interesting at all.[12] Or, to put it in different terms, any mathematician when asked about a particular concept in his or her field of expertise, will be able to answer the question what the relevance of that concept is. Very often the answer will be that it allows you to formulate this or that problem in a convenient, perhaps even explanatory[13] way. This characteristic of concepts can be extended to proofs as well.

> (P4) In many cases, a solution to a mathematical problem introduces new proof methods.

Mathematicians have a range of proof methods at their disposal that are easily recognizable as they often have a specific name: proof by mathematical induction, proof by cases, proof by reductio (ad absurdum), proof by infinite descent, [...] In many cases these proof methods were developed because of a particular problem and later on it turned out that the same proof method could be applied to other problems. In that sense, an uninteresting problem can nevertheless possess a quite interesting proof.

> (P5) The development, relevance and use of concepts and proof methods is one of the core themes in the philosophy of mathematical practice and of mathematics.

This is, of course, the crucial premise to reach the conclusion. Apart from the obvious empirical fact that the above statement is true—it is sufficient to look at the literature in the philosophy of mathematics, both "purely" philosophical and foundational, to see how much attention is given to these

[12] Which is not to say that all such questions and problems are irrelevant. Whether or not the number π is a normal number, in the sense that all digits have the same frequency of occurrence, is considered to be an interesting problem but is clearly connected to a particular representation.

[13] See (Mancosu 2008) for a nice discussion about explanation in mathematics, an important and difficult topic.

topics, see (Rav 1999) for an excellent analysis—there is the negative argument: what else would philosophers of mathematics talk about? Both elements, concepts and proof methods, belong to the essence of what it is what mathematicians do and hence should be a topic of reflection for philosophers.

If these three statements seem acceptable, then stringing them together, we arrive at a second intermediate conclusion, namely:

> (IC2) The fact that problems get solved, implies that their solutions have a (potential) impact on the philosophy of mathematics.

Finally, if we put the two intermediate conclusions together, we arrive at the final conclusion, which states that:

> (C) The fact that we have to deal with finite resources for our problem-solving capacities has a direct (potential) impact on and is (potentially) relevant for the philosophy of mathematics.

A direct corollary of this conclusion is that:

> (Cor) The ways in which finite resources are distributed over a problem-solving community (of mathematicians) is directly (potentially) relevant for the philosophy of mathematics.

All this being said, even if the reasoning presented in this section is acceptable, it still remains to be shown that the Polymath case is such a case that might change our views on certain philosophical questions. This raises another question that I will briefly address in the next section, namely, whether there are ways to investigate such a claim. In general: suppose you are confronted with a particular way mathematicians have tried, successfully or not, to solve a particular mathematical problem, should their strategy invite us to have a different look at certain philosophical questions? I think this question can be positively answered and, more specifically, what I have in mind are formal models of shared or distributed knowledge.

3 Formal modelling as an additional argument

The literature on the topic of shared or distributed knowledge is quite extensive and I will not try to present a survey here. I will briefly comment on some approaches that for different reasons are directly relevant, ranging from multi-agent systems for obvious reasons, including argument and dialogue structures to describe the interactions between the members of a community and a formal approach of Lakatos' method of mathematical discovery. Before doing that, let me sketch in a few words the informal idea.[14] A network of mathematicians can be described as a Kripke model. We have a set M of worlds, in this case the mathematicians and a relation R on $M \times M$ that tells us what the communication channels are between them.[15] One thing stands out as quite obvious: given what R is, the community M will be able to solve or not solve certain problems. Think of extreme cases: surely if everybody is in touch with everybody else much more information will flow between them, compared to a structure where one mathematician is addressed by all others who themselves have no contact with one another, corresponding to an inward-pointing star-like structure. Take a simple example: suppose that a mathematical problem P can be decomposed into two problems P_1 and P_2 such that solving both these subproblems solves the original problem. In the first case all mathematicians can have a go at the subproblems, whereas in the second case, if someone manages to solve P_1, someone else P_2, then only the mathematician in the center will know that the original problem has been solved, as the two mathematicians who have solved the subproblems cannot communicate with one another. In short, how the community is organized should make a difference as to their problem-solving capacities, as is stated in (Cor) above.

An illustration of the first approach is the recent presentation of multi-agent systems in Dunin-Keplicz and Verbrugge (2010). The reason for this choice is that they discuss the specific situation where the agents are searching for a proof (Dunin-Keplicz & Verbrugge 2010, 91–97). The language they develop involves such elements as GOAL(agent, action), in the case of theorem proving obviously GOAL*(i, prove(theorem(T)))*, the beliefs each agent has, expressed by a belief-operator BEL(agent, statement), involving in this case whether or not the agent believes he or she can contribute to the finding of the proof. On this basis the team leader can put together his or her team and develop a plan that involves, among other things, ways of dividing or splitting up the given problem. In their approach it basically comes down to the reduction of the search for the full proof to the search for proofs of a set of lemmas, the idea being that, once all lemmas have been proven, thereby the original theorem has been proved. The execution of this

[14] I have been playing around with this informal idea for some time as early as 1985, see (Van Bendegem 1985).

[15] There is an interesting link to be explored here, namely, the study of small worlds, see, e.g., (Watts 1999). Here the object of study is to describe networks and develop measures for the length of the chains that connect two members in the network. Small changes in such a network can have a tremendous effect on the efficiency of communication in terms of speed.

plan also involves a means-and-ends analysis, in this case the possibility to check a proof and establish its correctness. In their own words:

> There is a division of the theorem T into lemmas such that for each of them there exists a proof, constructed by the lemma prover and checked by the proof checker. Also, there is a proof of the theorem T from the lemmas, constructed by the theorem prover, which has been positively verified by the proof checker. (Dunin-Keplicz & Verbrugge 2010, 94)

Of special interest in their approach is that plans can always be reconfigured, dependent on the state of affairs. In the case of theorem proving, one of the obvious obstacles is that an agent who committed him- or herself to prove one of the lemmas does not succeed (because of shortage of time or, in my terms, because the economic resources have been exhausted). In that case the commitments and beliefs of the agents involved are checked again to see whether another agent can take over the task. Another obvious obstacle is that the proof checker finds a mistake in the proposed proof. All taken together, this model comes pretty close to real-life scenarios. This formal description could be—up to a number of special issues that I will discuss a bit further—easily applied to the Polymath Project, where we have clearly two team leaders—Timothy Gowers and Terrence Tao—and where the other participants believe they can contribute something to the overall problem. It also raises the interesting question whether the Polymath Project should maintain the social structure it has at present. The teamwork approach, sketched very roughly here, suggests a regular update to see whether a reorganisation at a certain point in time is needed or not.

An additional feature is that their framework also deals with dialogues and argumentations, next to and apart from proofs. The main object is to determine under what circumstances and conditions an agent i who believes A can persuade an agent j to accept A. One possibility is on the basis of trust. But the object of a dialogue can also be to seek information from an agent. What is worth mentioning is that the sources they refer to concerning dialogue and argumentation theory are such authors as Erik Krabbe and Douglas Walton.[16] This is to be sure a quite different approach than the recently developed one in terms of argumentation systems, see (Besnard & Hunter 2008) for an overview, where the focus is on attacks and counterattacks, on the weight of an argument and, especially, on conflicting arguments and how to resolve them. At present it seems less clear how this could be easily applied to answer the main question of our

[16] Both authors have an impressive publication record so I will only mention a joint work, namely (Walton & Krabbe 1995).

contribution, namely in what ways different social structures can lead to different mathematical developments because of different problem-solving capacities. I will not explore this road any further here.[17]

A few words should also be said about the "founding father" of the study of mathematical practice, Imre Lakatos, whose *Proofs and Refutations* (1976) marked the beginning of the study of mathematics in its actual historical development. Although his proposed method has been both criticized and extended in several ways, it is worth mentioning that a few authors have tried to formalize the Lakatosian method and to connect it with recent developments in theory change and development ((Pease 2007)[18] and (Başkent 2012)).

Nevertheless in order to come to a comprehensive theory of how problems are distributed and how they get solved in a group or community setting, some additional features will have to be dealt with. To round off this section, I just list three of them:

> It must be clear that more complex structures are needed than the lemma-theorem relationship. Especially the other direction, so to speak, should be dealt with. Think of the case where several theorems have been proved and a generalization is proposed that brings the theorems together in an overarching framework but that requires that several theorems have to be reformulated. This process of reformulation strikes us as an important element to understand how mathematical change comes about.

> What needs to be looked at as well are all possible relations between proofs. Sometimes analogies between different proof methods are important—this, incidentally, were comments often made in the Polymath project where suggestions were made to look at a particular proof method as source of inspiration for the proof searched for—or between the same proof method used in different mathematical contexts.

> Above all, any such model should include concept formation. How and why do certain concepts arise and others don't? Do concepts keep their relevance or do they in some cases "disappear"? Is it possible to define the fruitfulness of a concept? Typical examples are of

[17] Although it should be mentioned that Andrew Aberdein has been investigating for some years now the use of argumentation theory in mathematics, see (Pease & Aberdein 2011) and (Aberdein & Dove 2013), but this deserves a separate treatment in another paper.

[18] Of special interest is the fact that Pease has recently also contributed, together with Ursula Martin to the study of the Polymath project. See (Pease & Martin to appear).

course mathematical constants. To give but one specific example: all mathematicians share the feeling of puzzlement that the number π appears in the outcome of the summation of the inverse squares of the natural numbers, namely $\pi^2/6$.

4 Conclusion

What has been presented here is, first and foremost, a philosophical exercise. Starting from a specific real-life case study I have tried to formulate a general philosophical argument to show or at least support the hypothesis that social structures do matter to the development of mathematics and thereby also affect the problem agenda of the philosophers of mathematics. That being said, it should not be excluded that laboratory experiments can be done. Imagine two groups of students that have been evaluated beforehand in such a way that, as far as mathematical capabilities are concerned, they are sufficiently comparable, i.e., the individual characteristics do not differentiate between them. Organize the two groups in a different social structure, e.g., one group with a central authority to whom everybody has to report and who is the only one to have an overview and one group where everybody has access to everybody else. Although one might think that the second group could, maybe should be more successful, this is not necessarily so as they run the danger to get stuck in too many details that everybody is offering to the whole group. This thought in itself makes the experiment interesting and, as it happens, there are sources that can be used, namely the work being done in experimental economics, especially where game theory is concerned. This brings us back to cooperation, collaboration and competition, basic social relations in any social group, including that of the mathematicians.

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