PROOFS AND ARGUMENTS
THE SPECIAL CASE OF MATHEMATICS

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Abstract

Most philosophers still tend to believe that mathematics is basically about producing formal proofs. A consequence of this view is that some aspects of mathematical practice are lost out of view entirely. As I will defend, it is precisely in those aspects that similarities can be found between practices in the exact sciences and in mathematics. Hence, if we are looking for a (more) unified treatment of science and mathematics, it is necessary to incorporate these elements into our view of what mathematics is about. As a helpful tool I will introduce the notion of a mathematical argument as a more liberalized version of the notion of mathematical proof.

1. Introduction

In *Structures in Science*, chapter 13, entitled "Default-norms' in research ethics", Theo Kuipers defends the idea that the Merton norms for the ideal scientific community – summarized as the CUDOS-norms, viz. *C* for Communism, *U* for Universalism, *D* for Disinterestedness, and *OS* for Organized Skepticism – are best seen as standards to measure deviations in actual scientific practice, so-called defaults, rather than norms that are actually implemented. I feel quite sympathetic to this kind of approach as I have on another occasion[1] developed a similar view, the difference being that I made no reference to Merton's vocabulary – instead I wrote about an ideal community – and, perhaps more importantly, that my subject was mathematics.

The reason I consider the latter element as more important is that it opens up the possibility for a (more) unified treatment of the (exact) sciences as well as mathematics. Needless to say, the subject matters may be quite different – electrons, molecules, genes, species, … on the one hand, algebraic, topological, geometrical, … structures on the other – but that does not exclude that in other respects, e.g., as a (professional) activity, they are sufficiently similar to be treated in a uniform way. I would assume that Theo Kuipers would welcome such an approach since in chapter 6, "Empirical Progress and Pseudoscience" of *From Instrumentalism to Constructive Realism* he considers the extension of his approach to philosophy and theology.

1 See Van Bendegem (1993). There is a rather nice form of continuity present here. This paper was written at the occasion of the retirement of Else Barth, also from Groningen at the same department where Theo Kuipers is at present at work. Needless to say that the author of this paper has intentionally forced the continuity.

First I will say a few things about the ideal picture of mathematics and what is missing in it, then I will present my evidence that what is missing is essential for mathematical practice and close off with some observations of a more formal nature on the new “real” picture and how it relates to scientific practice.

2. Proof is a proof is a proof is a proof

Ask the question what it is that mathematicians do all day long and the answer will be: looking for proofs (dependent on your philosophical view “looking for” can be made more precise by expressions such as “discovering” or “constructing”). And what a proof is, is clear to all: a connected series of statements, the last one being the statement to be proven and every step in the proof is to be justified either because it is an axiom or the result of the application of one of the logical rules. Surely there is no room for experiments here – what could a mathematical experiment look like? –, surely there is no room for induction (no matter of what kind) – unless the author of this paper is suggesting that there is a similarity between scientific induction and mathematical induction, which is he definitely not –, surely there is no room for “competing” proofs, whatever that is supposed to be.

In addition, the idea that we have a clear idea of what proofs are and what they are about is supported by the fact that it is possible to have formal versions of the notion of proof. Let Proof(α,A) stand for the relation that expresses that α is a mathematical proof of A. Then all kinds of formal statements can be written down that reflect properties that “good” proofs have and should have, such as:

If Proof(α,A) then A

or If Proof(α,A) and Proof(β, A $\supset$ B) then Proof(γ,B)
(where γ is the result of joining together α and β)

and so forth. This is precisely what is being studied in the field of provability logics[2]. End of story.

As must be clear, this is not, as far as I am concerned, the end of the story. Two considerations will, I hope, make clear why one should have doubts. The first consideration is that most, if not nearly all *interesting* mathematical proofs do not satisfy the formal standards. It is sufficient to take any textbook on whatever mathematical topic and go the middle of the book, then it is immediately obvious that the “proofs” presented there are not proofs in the formal sense[3]. Of course, one might argue that these “proofs” can always be rewritten in the

[2] See, e.g., Boolos & Jeffrey (1989), chapter 27.

[3] The reason that I mention the middle of the textbook is that in the introduction of a textbook, because the first few elements and concepts are introduced, “proofs” tend to be rather simple and so it is possible to spell them

formal style. Apart from the fact that the practical feasibility can be seriously doubted – how long would the formal counterpart of Andrew Wiles's proof of Fermat's Last Theorem be? I guess very, very long –, it is for the purpose of this paper sufficient to note that mathematicians themselves do not do it. They do not invest part of their time in rewriting existing "proofs" into formal correct ones. Why?

This brings me to my second consideration. A closer look at what mathematicians do reveals that they spend quite some time on activities that seem strange from the formal proof perspective. Why, to give but one example, waste time on proving special cases of a universal statement? Why, e.g., prove that $x^3 + y^3 \neq z^3$, for x, y and z integers, when the statement to be proven is $(\forall n>2)(x^n + y^n \neq z^n)$, for x, y and z integers?

The thesis of this paper can now be reformulated thus: it is precisely in those aspects of the activities of mathematicians that "disappear out of view" when seen from the formal proof perspective, that mathematics is quite similar to the sciences. Moreover formal proofs and not "proofs" are all things considered very few and far between, thus it seems more than appropriate to focus on these other aspects.

Yet another way of presenting what I have in mind is this: instead of talking about "proofs", let me introduce the notion of a *mathematical argument*. Just as in the case of formal proof, we could imagine a relation Arg(α,A) to indicate that α is a mathematical argument for or in support of the statement A. The Arg-relation can be seen as a weaker (and hence more general) notion than the Proof-relation. If α happens to be a formal proof for A, then it is obvious that we want that:

If Proof(α,A) then Arg(α,A),

But, if α is not a formal proof, then surely we do not want the inverse:

If Arg(α,A) then Proof(α,A).

This is all rather trivial – I will at the end of the paper return to the question of a formal analysis of the Arg-relation – but as a tool of thought I do believe that the Arg-relation is rather useful. First, the connection remains with the (formal) proof relation that now appears as an extreme case, one end of a continuum but, secondly, there is no reason anymore to expect certainty of mathematical statements, as an argument *supports* a statement but does not (necessarily) prove it. In short, it presents the mathematical activity as a fallible activity (though I am not claiming it is a form of fallibilism in the sense of Lakatos[4]), thereby reducing the philosophical importance attached to such questions as what can be the source of mathematical certainty. Along more modest lines, this approach at least helps to bridge the

out in full detail, hence they can be considered to be formal proofs. Often it is believed that the same holds throughout the book, but that is precisely the point I am denying.

[4] See, e.g., Koetsier (1991), who has further developed the Lakatosian fallibilist approach to mathematics.

unfortunately still existing gap between formal-mathematical reasoning on the one hand and informal-argumentative reasoning on the other hand.

All this being said and done, I assume that the reader is anxious to know what mathematical arguments could be. The next paragraph presents a summary of such possible candidates for α, some of which I will merely mention either because they are quite evident or because I have written about them in other places[5].

3. Presenting the evidence for mathematical arguments

(a) Obviously the first candidates for α are real proofs as they appear in the journals or as they are presented at conferences. It is a nice challenge for any student to rewrite a real proof in formally precise terms. Just one example: take the following problem[6]. When given 2n consecutive natural numbers and a random selection of n+1 numbers is made, then two of these are necessarily relatively prime. Argument: when n+1 numbers are selected from 2n consecutive numbers, necessarily two of them are neighbors, hence relatively prime. QED. Given the formal language of Peano Arithmetic (and perhaps a bit of set theory), it is not a trivial task to spell out this argument in detail. Do note however that as a mathematical problem, it is rather trivial.

(b) A second candidate for α are so-called “informal” proofs. These proofs are to be distinguished from real proofs where one believes that it is possible to rewrite the proof in all formal detail. In short, a real proof is an instance of correct reasoning. Informal proofs have the property that they are basically not (formally) correct, yet lead to a correct result. As one might wonder why mathematicians would waste their time doing such a thing, the answer is this: it gives the mathematician at least some idea of what the result could be.

The most often quoted example is the famous argument of Euler for the sum of the inverses of the squares, namely the argument that $\Sigma\ 1/n^2 = \pi^2/6$ (the summation taken over the natural numbers). I will not present the full argument but its general structure. Euler first reasons about polynomials of finite even degree 2n, of the following form:

$$b_o - b_1x^2 + b_2x^4 - \ldots + (-1)^n b_n x^{2n} = 0$$

with roots: $r_1, -r_1, r_2, -r_2, \ldots, r_n, -r_n$.

He shows that the following holds:

$$b_1 = b_o(1/r_1^2 + 1/r_2^2 + \ldots + 1/r_n^2). \qquad (*)$$

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[5] See, e.g., Van Bendegem (2000) and (to appear).

[6] See Aigner & Ziegler (1998), p. 123. This little problem has been suggested by Paul Erdös.

All of this is quite regular mathematics. He then assumes that the same line of reasoning applies to polynomials of infinite degree. It is at this point that the reasoning goes astray, for there is no reason to suppose that the same result will hold for the infinite case. Thus the polynomial:

$$1 - x^2/3! + x^4/5! - x^6/7! + ... = 0,$$

with roots: $\pi, -\pi, 2\pi, -2\pi, 3\pi, -3\pi, ...$,

(as it is the series expansion of sin(x)/x), will satisfy (*), thus

$$1/3! = 1/\pi^2 + 1/4\pi^2 + 1/9\pi^2 + ...,$$

or: $1 + 1/4 + 1/9 + ... = \pi^2/6$. QED (?)

This is not some kind of outlandish curiosity, for, as Dunham (1990), pp. 207-222 shows, the same line of reasoning can be used for other summations as well (as Euler actually did), such as :

$\Sigma\ 1/(2n)^2 = \pi^2/24$, i.e., the sum of the reciprocals of all even squares,

$\Sigma\ 1/(2n+1)^2 = \pi^2/8$, i.e., the sum of the reciprocals of all odd squares,

and $\Sigma\ 1/n^4 = \pi^4/90$, i.e., the sum of the reciprocals of all fourth powers.

It is worth emphasizing that all these results turned out to be correct, hence these arguments can be rightfully called arguments.

(c) A third candidate is so-called "career induction". Above I already mentioned Fermat's Last Theorem. Another famous example is Goldbach's Conjecture, i.e., the statement that every even natural number is the sum of two prime numbers. Career induction is the idea that, if you have to prove a universal statement of the form $(\forall n)A(n)$, then it is worthwhile to investigate A(1), A(2), up to some finite number k. Formally speaking there could only be one case where such an approach is interesting, namely, if it turns out that one of the special cases does not hold, i.e., one proves ~A(m), for some particular number m, thus refuting $(\forall n)A(n)$. But in cases such as Fermat and Goldbach, this has not happened. For sure, one could suggest that although one was looking for a counterexample, one ended up (almost by accident or unintentionally) proving the cases.

What does seem to be the case however is that by searching for proofs for special cases, the mathematician gets some insight into the kind of proof elements and proof concepts that will be needed if a proof of the universal statement is ever to be found. In the case of Fermat, this is clear: the method of infinite descent was used in the special cases and it turned out to be a powerful method for dealing with the general case[7]. As to Goldbach, here the problem is open

[7] Although I must add straight away that in the final proof by Andrew Wiles it is hard to see that this is a paper about number theory. Elliptic curves, group representations, Galois fields, …, those are the ingredients to prove the statement, hence there is no direct use for infinite descent here, as it is a typical number-theoretic idea: if there exists a solution in natural numbers, then there exists a solution that is strictly smaller; this is impossible,

as we do not have a proof at the present moment. However, as the excellent paper of Echeverria (1996) makes clear, even without a real proof of Goldbach, it is clear that the numerical evidence that has been gathered, together with other considerations, has convinced most if not all mathematicians that Goldbach's conjecture is true[8].

(d) A fourth candidate are so-called mathematical "experiments" including visualizations and computer graphics (see Hege & Polthier 1997 and 1998). Such "experiments" cannot be considered as formal proofs, because as we all know, the translation of a mathematical problem involving infinite domains (such as the real or complex numbers) to the computer screen consisting of a finite set of pixels must involve approximations. To be specific, suppose that a three-dimensional object whereof an algebraic description is given, is visualized on the computer screen and the visual object has certain properties, then it would not be correct to conclude that the object does actually have that property. In fact, as the literature shows, it is always necessary to establish estimations of the errors involved but that needs to be proven mathematically, so, therefore, the image cannot add anything new. Or can it?

It is undoubtedly the case that an image can "reveal" certain aspects of a mathematical object. Seeing a(n approximation of a) mathematical object does provide information in a different format. It is rather tempting to give a semiotical analysis at this point[9], but the fact that a formal text and a picture are not the same can hardly be a point of discussion. Even if it turns out that a property of the visualization is a computer artefact, this might still provide some insight.

Another type of mathematical "experiment" consists of number crunching, but that is nothing else but a technological version of the previous type of argument, namely career induction. It simply consists of checking a finite number of cases of a universal statement $(\forall n)A(n)$ by direct computation, the only difference being that more cases can be checked than can be done by hand.

There is however one other interesting case, different from the previous ones. Ivars Peterson (1988) discusses the problem of Plateau: given a boundary curve B, what is the minimal surface S having B as its boundary? Mathematically this is a deep and difficult problem. Often,

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because one would then have an infinite number of solutions, hence there is no solution. Infinite descent is very closely related (in some cases equivalent) to mathematical induction.

[8] Although not of essential importance for the thesis of this paper, it is worth mentioning that Georg Cantor, the mathematician responsible for transfinite set theory also spent some time on Goldbach's conjecture. The standard story is that a nervous breakdown made it impossible to work on serious matters, so Cantor "wasted" his time calculating decompositions in two primes for all even numbers up to 1000. However, as Echeverria shows, the contribution was very important. What Cantor studied was the function G(2n), i.e. the number of ways that an even number 2n can be written as the sum of two primes. The cases studies by Cantor showed that G(2n) is an increasing function, that is, if $n > m$, then $G(2n) > G(2m)$. If this could be proved in general, it proves Goldbach, because $G(2n) \geq 1$ is sufficient.

[9] I am thinking here of authors such as Michael Otte, see his (1997) or Brian Rotman, see his (2000).

analytical methods are insufficient. There is however a simple way to find solutions, though not necessarily the set of all solutions. Construct the boundary B in metal wire. Dip it in soapy water and a film will form having B as its boundary. Physics tells us that this film is a minimal surface. Hence, Peterson says: “They can explore shapes that are often too complicated to describe mathematically in a precise way. They can solve by experiment numerous mathematical problems associated with surfaces and contours.” (p. 48). The relations between such experiments and mathematics is actually a quite deep philosophical issue[10].

(e) A recent candidate are mathematical arguments that involve probabilistic considerations. It is important to be rather precise about what such a type of argument looks like. On the one hand, what one presents is a real proof in the sense of type (a), discussed above. On the other hand, however, what the proof says involves probabilities. Examples of such proofs are typically to be found in number theory. It concerns theorems such as: Given a number n, if a test T, involving a random choice of k numbers all smaller or equal to n, is performed on n, and the answer is yes to T, then the number n is prime with a probability of $1 - 1/4^k$.

If the problem we want to solve is to know whether or not the number n is a prime with certainty, then it is obvious that such an argument supports that idea. In that sense it is an argument for the statement that the number n is indeed prime. It is quite interesting to note that this means that some proof for some statement can be an argument for a closely related statement. There is an additional element that I will not elaborate further in this paper why mathematicians are interested in such probabilistic statements. If one wants certainty, then the computational costs of actually checking whether the number is prime or not is exponential (or it is not known whether a polynomial procedure exists) in time or space needed, whereas the probabilistic approach runs in polynomial time or space. See Ribenboim (1989), pp. 107-120 for further details.

(f) Another recent candidate are proofs involving the use of computers. Without any doubt, the famous example for this case is the four color theorem. The theorem states that four colors are sufficient to color any planar map in such a way that neighboring areas get different colors. The first published proof consisted of two parts. The first part was a “classical” mathematical proof, where it is shown that the set of all possible maps can be reduced to a finite set such that if all maps in that finite set can be colored, so can the full set. It is actually a beautiful piece of mathematics. But the second part consists of a computer listing, presenting the details of a computer program that has actually colored all the maps and said, “yes, I have colored them all” at the end of the day. Obviously, according to the definition of proof given at the beginning of this paper, this is not a proof, definitely not a formal proof. But it is obviously a mathematical argument because it shows that the theorem is very likely to be true. In fact, as a mathematical argument it is clear that this result is far better than if a human mathematician had actually colored all the maps as humans are more likely to commit

[10] I refer the reader to Van Bendegem (1998) for more details.

errors compared to computer programs. For a discussion of this type of argument, see Tymoczko (1986).

Computer programs also play a part in checking existing mathematical (real) proofs. Something quite curious is happening here. Suppose we have sufficiently complex computer programs – there are some good candidates around at the present moment – that can rewrite real proofs into formal proofs. One can imagine that a real proof of fifty lines in a rewritten form turns into a formal proof of a couple of thousand lines (probably presented in some type of clausal form[11]). The program tells us that the proof is formally correct. Since we have used a computer program, what we have here is a mathematical argument. Hence, a mathematical argument can help convince us that a real proof is correct because the formal counterpart has been declared all right by the program. It shows that real proofs and arguments can support one another in quite complex ways.

(g) Finally one should take into consideration arguments that are not "purely" mathematical, but involve non-mathematical elements. A very fine example in this connection is the use of foundational-philosophical arguments to arrive at the probable truth or falsity of a particular mathematical statement. These are most certainly not mathematical proofs, whether formal or real, but they do help to decide certain questions or, at least, to give an orientation to the proof search.

An example would be a situation whereby a statement is believed to be false because it has implications that, although strictly mathematically speaking are without fault, nevertheless are considered to be paradoxical on philosophical and/or non-mathematical grounds. Two specific examples are:

(i) The Banach-Tarski paradox that throws doubt (for some) on the axiom of choice as it used in set theory. The paradox states that it is always possible to decompose in three-dimensional space a ball of volume V into two balls of volume V, using only rigid motions (translations and rotations). There is no mathematical problem here, but the paradoxical character of the result is clear and serves as an argument against the axiom of choice[12].

(ii) The Continuum Hypothesis (CH) as it is discussed in the writings of Kurt Gödel[13]. On the basis of philosophical arguments, Gödel had the deep conviction (at least during a specific

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[11] This is the favorite way of representing statements in automated reasoning. It is based on the fact that every statement in a classical logical system, such as first-order predicate logic can be rewritten in a standard format whereby all quantifiers end up at the beginning of the statement and the quantifier-free part can be rewritten in terms of conjunctions, the members of which are disjunction of atomic formulas with or without a negation in front of them. Thus $(\forall x)(Px \supset (Qx \& Rx))$ is rewritten as $(\forall x)((\sim Px \vee Qx) \& (\sim Px \vee Rx))$. These disjunctions are called clauses.

[12] See Moore (1982) for details.

[13] CH is the statement that between the countable infinite, $\aleph_0$, and the infinity corresponding to the continuum or the set of reals, $2^{\aleph_0}$, there are no other infinities, hence, the "next" infinity $\aleph_1$ must be equal to $2^{\aleph_0}$. See Feferman et al. (1990) for the full details, in particular, the introductory note by Gregory Moore, pp.154-175.

period of his career) that CH cannot be the case, i.e., $2^{\aleph_0} \neq \aleph_1$. This conviction was, in his own words, based on the fact that CH had implausible consequences. This is particularly interesting because he himself had already produced a result that shows that there exists a model of the set-theoretical axioms wherein CH is actually true and, hence, not refutable! So this left only two possibilities: either CH is provable or it is undecidable (which turned out to be the case). As the former case was excluded for Gödel, one would expect that he concluded that CH is undecidable. Which indeed he did, but the undecidability for him meant that we had to look for additional axioms that would decide CH, in his case in the negative. To avoid any confusion, Gödel was not being incoherent here. The additional axioms would exclude the model that Gödel considered to be quite artificial and that was constructed with the sole purpose of showing CH to be true in it.

Although I do not claim any completeness for the above list of types of mathematical arguments, I do believe that it shows that, firstly, mathematical arguments are different from formal-mathematical proofs, secondly, that such arguments are abundant and, thirdly, that such arguments do allow mathematicians to convince themselves of the truth, falsity, provability of refutability of particular mathematical statements.

4. Mathematical arguments and empirical evidence

Let me return to the relation Arg(α,A) in a more general setting. At first sight, it seems that not much can be said about this relation in general. It is definitely not the case that

If Arg(α,A) then A

for obvious reasons, neither that

If Arg(α,A) and Arg(β,A ⊃ B) then there is a γ, such that Arg(γ,B)

for the simple reason that α and β can be of an entirely different nature. How to compare a philosophically motivated argument for A with a career induction argument?

There is, however, one important idea that is worth exploring. It seems quite reasonable to claim that if there is a mathematical argument α that supports A, then we are willing to express a commitment about A. It seems also reasonable that we could imagine a scale between 0 and 1 (these values of course being arbitrary), and a function P that assigns to a statement A a value P(A) between 0 and 1. P(A) expresses our degree of confidence in or our commitment to the fact that A is indeed correct or true. Three conditions seem extremely plausible:

If Proof(α,A) then P(A) = 1
If Proof(α,~A) then P(A) = 0
If Arg(α,A) and not Proof(α,A) then $0 < P(A) < 1$.

Furthermore, what a mathematical argument does is to increase the degree of our commitment, hence the following principle is defensible:

If $P_{before}(A)$ is given and $Arg(\alpha,A)$ then $P_{after}(A) \geq P_{before}(A)$

This strongly suggests that mathematical arguments behave, generally speaking, in the same way as empirical evidence for a scientific hypothesis. I am not claiming that the function P should be a probability function[14], but there are some properties of P that are common both to mathematical arguments and empirical evidence:

(a) A statement is supported more strongly if the number of mathematical arguments for it increases. Note that this also holds for real proofs. It is a common practice among mathematicians to find more than one real proof for a mathematical statement.
(b) Arguments that are independent of one another are more interesting than mutually dependent arguments. Note again that for real proofs if another proof is found it should be different (usually meaning either using a different proof method or using a different mathematical domain) to increase the support.
(c) Unexpected arguments have a greater impact than expected arguments. This feature too is typical for real proofs. When a proof is unexpected – an example would be the use of a proof technique from another mathematical domain that one did not expect – this counts as more important and/or convincing than a "regular" real proof.

No doubt this list could be further extended, but my aim here was only to show that the similarities are not superficial, justifying the conclusion that I do believe – and I assume that Theo Kuipers would join me in this – that at least in some aspects of mathematical practice the way we build up our support of mathematical statements is quite similar to the corresponding way it is done in the sciences. It also justifies the hope therefore that this paper might be a small contribution to a unified treatment of science, philosophy, theology *and* mathematics.

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[14] A good argument against a probability interpretation is that conditional probability is lacking in this presentation. In the best of cases we could talk about such expressions as $P(A,\alpha)$ – i.e., the degree of commitment to A given argument α - but this runs counter to a definition of $P(A,\alpha)$ in terms of $P(A\&\alpha)$, as $A\&\alpha$ is a "mixed" expression.

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