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MATHEMATICAL AIMS BEYOND JUSTIFICATION

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ABSTRACT

The past decades, there has been an increased philosophical interest in mathematical practice. These philosophers intend to answer questions related to the activities of working mathematicians. One of these questions is what aims direct mathematical research. After all, mathematicians often express the desire for good or beautiful mathematics. These types of reflections indicate the need for an understanding of aims that go beyond justification. In this paper, we explore potential ways in which philosophy can clarify what these aims are. Furthermore, we stress the importance of a multidisciplinary approach to this problem.

1. Introduction

The topic of this paper is mathematical aims, by which we mean directions that guide a mathematician's activities. Explicating such aims helps us to answer questions such as the following. Why do mathematicians prove? Why are certain mathematical arguments valued more than others? What is the role of non-traditional proofs in mathematics? As the title suggests, we are interested in aims other than justification. This choice is not inspired by a belief that justification is not an important mathematical aim, it certainly is. Rather, it is encouraged by the firm belief that the aim of justification is largely insufficient in order to account for much of mathematical activity. Our purpose is not to give definitive answers to the question what these other mathematical aims are, but to examine where this question might lead us.

In section two we introduce the topic of our paper by pointing to a growing trend in philosophy of mathematics to emphasize the importance of mathematical practice. In section three we discuss several possible roads to explore when talking about mathematical aims. Section four develops a critical note on methodology. In section five we conclude this paper.

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2. Philosophical Attention for Mathematical Practice

2.1. *A break in Tradition*

During the last decades, different currents in philosophy of mathematics have been recognized and distinguished; mainstream philosophy of mathematics, maverick philosophy of mathematics and philosophy of mathematical practice.

Two kinds of philosophical questions have mainly determined topics dealt with in mainstream philosophy of mathematics; ontological questions and epistemological questions. Ontological questions are concerned with the subject matter of mathematics, addressing what kind of objects mathematical entities actually are. Are they abstract or physical, are they objectively existing or not? Epistemological questions, on the other hand, concern mathematical knowledge. How do we acquire knowledge about mathematics? The answer to this question is of course not independent from, and is thus often addressed in connection with, a specific ontological position. Moreover, it leads to other epistemological questions on the nature of mathematical knowledge. In what respect, if any, is mathematical knowledge similar to or different from other kinds of knowledge? Does mathematical knowledge enjoy some kind of certain and infallible status, granting it a unique epistemological status? In order to answer these questions, the major philosophical schools of the past century have all associated themselves with a particular type of (either godlike or human) ideal mathematician. For (methodological) Platonists, an ideal mathematician is one with perfect intuition. For empiricists, (s)he is the ultimate empirical scientist. For logicists, a fully rational agent. For formalists, an entirely free one. From the traditional schools, only intuitionists (although generally as objectivist and perfectibilist as their rivals) come anywhere near giving a role to actual mathematicians: genuine mathematical theories, they hold, are not waiting to be discovered and then justified but are actively and permanently developed by creative subjects instead. What remains remarkably constant however throughout these (conservative to more liberal) accounts is that "ideal mathematicians are usually assumed to be infallible, eternal or atemporal, unlimited in memory or complexity, isolated from other mathematicians, mental beings without contexts. […] Insofar as actual mathematicians err, they fail to approximate ideal mathematicians and so are of no concern to philosophy" (Tymoczko 1986, p. 45).

Maverick philosophers, such as Thomas Tymoczko, question the approach of mainstream philosophy of mathematics, and suggest to break with this tradition epitomized by the foundationalist approaches. These have dominated the agenda of philosophy of mathematics for the first half of the 20th century, being mainly, if not exclusively, focused on the outcomes or ‘products’ of mathematical practice. It was only since the publication of *Proofs and refutations* by Imre Lakatos, one of the very few philosophers being occupied with this theme early on, that it gradually became clear to an increasing number of scholars that a full understanding of mathematics also involves a grip on mathematical activity itself, as a process (Lakatos 1976). This concern with what it *actually* is that mathematicians do when they do mathematics, only implicit in Lakatos’ work though, pointed to possible new ways of steering out of a lingering philosophical crisis (see 2.2). Clearly, this implies a reconsideration of what should be *philosophy* of mathematics in the first place. For the mavericks, in contrast with mainstream philosophy of mathematics, this much was or is clear: A meticulous analysis of mathematical practice is deemed indispensable to do proper philosophy of mathematics, and mainstream philosophy of mathematics, as e.g. conducted from within the foundationalist programs, is insufficiently equipped methodologically in order to properly deal with it.

The third tradition mentioned above, philosophy of mathematical practice, is presented by Mancosu (2008*a*) as the middle ground between mainstream and maverick philosophy of mathematics. This position sees limitations in both previous approaches. Mainstream philosophy of mathematics neglects the mathematical practice, while maverick philosophy has not been able to get a substantial foothold in philosophy of mathematics. The methodology of this third, intermediate position generally speaking remains closer to the spirit of mainstream philosophy of mathematics, while keeping an emphasis on the importance of paying attention to mathematical practice.[1]

2.2. *Relation between Philosophy and Mathematical Practice*

Directing the attention of philosophers away from a perfectibilist view and towards the practice of mathematicians suggests previously unaddressed topics. Note that this does not entail demonstrating that mathematics is dishonorable or untrustworthy as an epistemic endeavour. Indeed, we are not at all in the business of bringing it down. However, its practices should be investigated dispassionately, and might even, as a result of that, be improved. That is, philosophy, with its particular competencies, can contribute to a useful reflection on mathematical practices, by shaping and facilitating these.

[1] For a more detailed discussions concerning the differences and relations between mainstream philosophy of mathematics, philosophy of mathematical practice and maverick philosophy of mathematics, See (Mancosu 2008*a*) and (Van Bendegem 2014).

In this very respect, it may address topics that are directly relevant for working mathematicians, breaking with a tradition of detachment so characteristic of mainstream philosophy of mathematics. Reuben Hersh, one of the early 'maverick' thinkers about mathematics, himself being a mathematician, noted that "mathematicians themselves seldom discuss the philosophical issues surrounding mathematics; they assume that someone else has taken care of this job. We leave it to the professionals" (Hersh 1998, p. 13), the latter of course supposing to be philosophers. Looking back on an entire career in mathematics, his occasional co-author Philip Davis confirms and specifies: "Most practicing mathematicians care very little about discussing the philosophy of their subject, but they work unconsciously with a philosophy of Platonism. […] If the shortcomings of Platonism are pointed out, mathematicians usually fall back on formalism" (Davis 2000, p. 50). With this he echoed the following classic lines from their joint, groundbreaking book *The Mathematical Experience*:

> Most writers on the subject seem to agree that the typical working mathematician is a Platonist on weekdays and a formalist on Sundays. That is, when he is doing mathematics he is convinced that he is dealing with an objective reality whose properties he is attempting to determine. But then, when challenged to give a philosophical account of this reality, he finds it easiest to pretend that he does not believe in it after all. (Davis & Hersh 1983, p. 321)

However, when it comes to challenging this rather careless and fairly superficial attitude, it seems "the professional philosopher, with hardly any exception, has little to say to the professional mathematician. Indeed, he has only a remote and inadequate notion of what the professional mathematician is doing" (Hersh 1998, p. 13). Again, this obvious neglect might be linked to the – philosophically unhealthy – preoccupation, throughout the twentieth century, with shaken foundations, i.e., the 'aftermath' of the famous crisis, which is a matter of hardly any immediate relevance to practicing mathematicians.

> People noticed that in their normal everyday work as mathematicians you don't really find results that state that they themselves are unprovable. And so mathematicians carried on their work as before, ignoring Gödel. The places where you get into trouble seemed too remote, too strange, too atypical to matter. (Chaitin 1999, p. 15)

Therefore, as Yehuda Rav has aptly noted, "it's important to remember that mathematics is not an edifice which risks collapse unless it is seated on solid and eternal foundations that are supplied by some logical, philosophical, or extra-mathematical construction" (Rav 1993, p. 80). This is indeed one of the central ideas to any alternative picture, as magnificently captured in the following legendary extract of contemporary philosophy of mathematics, by the hand of Morris Kline:

> The developments in this [twentieth] century bearing on the foundations of mathematics are best summarized in a story. On the banks of the Rhine, a beautiful castle has been standing for centuries. In the cellar of the castle, an intricate network of webbing had been constructed by industrious spiders who lived there. One day a strong wind sprang up and destroyed the web. Frantically the spiders worked to repair the damage. They thought it was their webbing that was holding up the castle. (Kline 1980, p. 277)

A question that arises when looking at the actual practice of mathematicians, leaving the cellar behind and entering the other floors of the castle so to say, is what aims of the mathematical endeavour can be identified. This requires a careful look into the reasons that support mathematician's actions and choices. For example, why do mathematicians prove? An answer to expect is that the method of rigorous proof gives mathematicians certainty about the truth of the theorems shown. This aim should of course not be neglected, as it is without any doubt one of the main driving forces of mathematical practice. In this paper we will however want to reflect on aims that go *beyond* this justificatory aim. Our reflection has two important reasons. Firstly, as indicated in the previous section, justification has received the most philosophical attention already. This does of course not mean that all interesting questions about it have been sufficiently answered, but we want to further open and facilitate the debate on other aims mathematicians might have. Secondly, a glance at mathematicians' own views on practice demonstrates that, *in effect*, other aims do enter the picture.

2.3. *Aiming for Good Mathematics*

In this subsection, we will start by taking a look at some of these mathematicians' reflections. When mathematicians think about their own practice, they often refer to distinctions such as those between good and bad mathematics, or that between beautiful and ugly mathematics. A famous example has been given by G.H. Hardy:

> The mathematician's patterns, like the painter's or the poet's, must be beautiful; the ideas, like the colors or the words must fit together in a harmonious way. […] [T]here is no permanent place in this world for ugly mathematics. (Hardy 1992, p. 85)

Another famous mathematician, Paul Erdös, was fond of referring to the *The Book*, where the most perfect proofs of all theorems were to be found. In this respect it was not an actual physical book, but a virtual way to pay tribute to the idea that some proofs triumph in elegancy and beauty. *Proofs from THE BOOK* by Aigner and Ziegler (1999) is an effort at a concrete approximation of The Book. The authors do not claim to have a definitive collection of good mathematics, and do not suggest there is an unambiguous distinction between beautiful and ugly mathematics. In their brief introduction, they state:

> We have no definition or characterization of what constitutes a proof from The Book: all we offer here is the examples that we have selected, hoping that our readers will share our enthusiasm about brilliant ideas, clever insights and wonderful observations. (Aigner & Ziegler 1999: preface)

Notice that not only proofs are labeled with some judgment here, but also brilliant *ideas*, clever *insights* and wonderful *observations*. Terence Tao (2007), like the above scholars, claims that mathematicians should strive to produce good mathematics. This however, he argues, cannot possibly have a singular and fixed meaning:

> Almost immediately one realises that there are many different types of mathematics which could be designated "good". […] [T]he concept of mathematical quality is a high-dimensional one and lacks an obvious canonical total ordering. (Tao 2007, pp. 623-624)

Tao offers an open-ended list of more than twenty possible readings, including: good mathematical application, good mathematical pedagogy, rigorous mathematics, beautiful mathematics and strong mathematics.

The fact that these judgments about mathematics can play an actual role in mathematical practice can be recognized in the following quote by mathematician Michael Atiyah, who describes his experience with proving a theorem. The fact that he wants the proof to play a certain role, namely explain why the theorem is true, leads to the search for an alternative proof:

> I remember one theorem that I proved and yet I really couldn't see why it was true. It worried me for years and years. […] In order for the proof to work, every single thing had to go just right – you had to be remarkably lucky, so to speak. I was staggered that it all worked and I kept thinking that if any one link of this chain were to snap, if there was some flaw in the argument, the whole thing would collapse. […] I kept worrying about it, and five or six years later I understand why it had to be true. Then I got an entirely different proof […] Using quite different techniques, it was quite clear why it had to be true. (Atiyah 1988: p. 305)

It is possible to further augment this list with other similar quotes and observations by mathematicians, but this is not the point of this paper. In essence, we argue that, if distinctions such as those between good and bad or beautiful and ugly mathematics exist, it seems fair to assume mathematicians attempt to produce good or beautiful mathematics. But just acknowledging this does not address the matter of what these distinctions actually mean and what their exact role is in mathematical practice. In order to do this we must answer *why* some piece of mathematics is valued and perhaps why another piece of mathematics receives less valuation. In the next section, we will discuss several possible answers to the question of what might fall under the scope of aiming for better mathematics, or put otherwise: what mathematicians value besides justification. As to be expected, given that many mathematicians themselves have already labeled good mathematics as a multidimensional concept, different perspectives are needed. Our ambition is not to give an exhaustive analysis of what mathematical aims are, but rather to suggest what might be different meaningful starting points for getting a better grasp on this notion.

We will start by looking at the notion of scientific aims, and see whether these might be extendable to mathematics. Next, we will address two values that have received the most, albeit limited, attention in philosophy: mathematical explanation and mathematical beauty. In the last two subsections we discuss different ways in which proofs and non-deductive or non-formal methods deployed in them can be valued.

3. Perspectives on Mathematical Aims

3.1. *Scientific Aims*

A first possibility is to look at the aims of other sciences. The essential aims of science are often said to be prediction, control and explanation. A scientific discipline has to decipher the hidden truths of reality so that we can predict what will happen to this reality, control reality in accordance with our own individual capacities, and answer explanation-seeking why-questions concerning this reality. This seems to work for most specific scientific disciplines. The concrete case will, without any doubt, be more complex as more nuanced aims can play indispensable roles as well. Moreover, getting a clear view on what prediction, control and explanation really means in the actual practice, is still an ongoing debate in philosophy.

Be that as it may, we can not display a similar general view about the aims of mathematics by referring to these widely accepted aims of science. It may seem intelligible that mathematicians want to decipher the hidden truths of, whatever concrete picture someone has of it, a certain mathematical reality. Nonetheless, other problems arise when extending these scientific aims into mathematical practice. It is hard to imagine how we could identify analogous accounts, if any, of prediction in mathematics. Mathematicians certainly make guesses about what they think a mathematical result will or should look like, or whether a statement will turn out true or false. If an aim is to be recognized here, it is not to find an adequate prediction of a result, but to find a proof of the theorem. The same problem is encountered when thinking about the notion of control, obviously ubiquitous in scientific practice but seemingly absent in mathematical practice. Explanation, on the other hand, might be a good candidate for a mathematical aim.

We will discuss the possibility that mathematicians look for an explanation why a certain mathematical statement is true further on. In general, however, it is clear we need a new perspective to truly capture the essence of mathematical aims.

One could still try to state that an aim of mathematics is to facilitate these scientific aims. Mathematics has repeatedly shown to be indispensable for science in order to achieve better predictions, improved control and more adequate explanations. It might be the case for some mathematical research that these aims have an impact on their research. Yet a closer look learns us that the mathematical success in science does not direct for what and how mathematicians look in their own research. Mathematicians do mathematics for the sake of mathematics itself, as was already clear from the reflections of mathematicians mentioned earlier. Mathematics is not but the handmaiden of science, if at all. A nice metaphor on the limits of this idea is given by Ian Stewart:

> It was like an expedition to cross an unscalable mountain range. At the outset, you can see the peak that must be conquered. But there's no way to climb it. And so the expedition heads off into the desert, trying to go round the mountain, and bypass the peak. Now, the techniques you need to survive in the desert are not those that help you climb mountains. So you end up with specialists on cacti and rattlesnakes and spiders, and the flow of sand-dunes in the wind, and the causes of flash-flooding, and nobody cares any more about snow, ropes, crampons or peg-hammers. So, when a mountaineer asks the sandunologist why he's studying sand-dunes, and is told to 'to get past that mountain', he doesn't believe a word of it. And it get worse when the answer is 'I don't give a hoot about mountains sand-dunes are much more fun.' But the mountain's still there, and the desert still goes round it. And if the desertologists do their stuff well enough – even if they've forgotten about the mountain – then one day the mountain will cease to be a barrier. (Stewart 1997, p. 65)

The reality that Stewart wants to describe here is that while Poincaré may have been originally driven into topology by a problem in physics, the discipline led him so far away from reality and into abstract mathematics. The result is a distance between problems in physical science and aims in mathematical research.

3.2. *Mathematical Explanation*

One of the potential aims mentioned previously is explanation. Mancosu (2008b) differentiates between two senses in which mathematical explanation is discussed in the philosophical literature: extra-mathematical explanation and intra-mathematical explanation. The former is concerned with the application of mathematics in the physical sciences, and whether the role mathematics plays here can be considered to be an explanatory one. An often debated example is the role of mathematical properties of prime numbers in the explanation of why species of cicadas have, depending on the geographical area, either 13 or 17 year life cycles (Baker 2005). This discussion is usually linked with ontological considerations about mathematics. Literature on intra-mathematical explanation, on the other hand, deals with the role of explanation within mathematics itself. An example is that we could, given multiple proofs of a particular theorem, make a distinction between more and less explanatory proofs.

In the investigation of aims that guide mathematical activity, explanation is a good candidate. We previously saw the case where Atiyah described how, in need for a better explanation of the theorem, a mathematician might look for another proof. The search for explanation might be characterized in various ways, including a search for the deep reasons, a better understanding or a satisfying reason why. Philosophical models strive to clarify the notion of mathematical explanation.

Looking at contributions to recent analytic philosophy, two kinds of approaches can be recognized. A first, starting with the model presented by Mark Steiner (1978), tries to explicate the notion of explanatory proofs. In order to do this, Steiner introduces the notion of a characterizing property, a property unique to a given entity or structure within a family or domain of such entities or structures. An explanatory proof is a proof that depends on a characterizing property of an entity or structure mentioned in the theorem. But merely pointing to this characterizing property is not enough. One must be able to generate new, related proofs by varying the property (substituting it with the characterizing property of a related entity) while holding the proof-idea constant. Other authors, such as Carlo Cellucci (2008) and Marc Lange (2014) have proposed alternative models, while sustaining a primary focus on the explanatory value of mathematical proofs.

The second kind looks at a broader picture. Kitcher (1989), one of the main defenders of the notion that scientific explanation is linked with theoretical unification, has argued that his unification approach covers mathematical explanation as well. We will not go into the technical details of his notions of explanation and unification, but it stands to its credit that the possibility of deriving as much as possible mathematical statements whilst keeping the basic assumptions limited has an intuitive appeal. Hafner & Mancosu (2008) present doubts whether Kitcher's model is able to actually account for explanation in mathematical practice, but the general idea that explanation and unification share a link still is sustained. Other models of unification might be more successful, as well as other explanatory values on a theoretical level.

The notion of explanation can be a worthwhile topic to gain access to mathematical aims. We should, notwithstanding, be aware that explanatory power is only one way mathematicians value their work. This pitfall, of placing a cluster of positive values under the umbrella of explanation, is also noted by Avigad:

> Such proofs are sometimes called explanatory, and there is a small but growing body of work on the notion of explanation in mathematics. will use the term here only gingerly, for two reasons: First, the term is not so very often used in ordinary mathematical discourse; and, second, it is certainly not the only term which is used to voice positive judgments about proofs. Here, I would prefer to remain agnostic as to whether there is a single overarching concept that accounts for all such positive judgments, or rather a constellation of related notions; and also as to whether the particular virtues considered here are best labeled "explanatory". (Avigad 2006: p. 106)

3.3. *Mathematical Beauty*

Another concept that receives some philosophical interest capturing mathematical appreciation is that of mathematical beauty. The list of distinguished scholars having pointed to aspects of the fundamentally *aesthetic* nature of mathematics is quite impressive. Mathematics has been likened to painting, music, sculpture or poetry, the successful mathematician to an artist. Because it has so often been defended, philosophically, that mathematical patterns are *inherently* beautiful, by abduction, the aesthetic appeal of specific mathematical hypotheses and results has been frequently considered as an indication of their coherence or truth as well as (potential) relevance and importance, e.g., by Russell or Poincaré. Hardy even explicitly confirmed the Aristotelian identification of the Beautiful, the True, and the Good in the case of mathematics. This indicates that mathematical beauty has long exclusively been thought of as purely abstract in nature, only *likened* to that of concrete art products. However, in recent times it has also been given a more earthly (or less idealist) dimension. With reference to the surprising facets of 'emergent' computer generated patterns, e.g., it is not unheard-of nowadays to plainly speak of 'fractal art'. Formerly, considering these or any mathematical objects as beautiful 'accidents' would no doubt have been sacrilege. Also, the status of aesthetic appreciation in matters mathematical has been thoroughly questioned. That is, the idea that mathematical beauty is *not* anywhere near absolute, but rather 'in the eye of the beholder', either individually or collectively, has been well supported indeed. See, e.g., the famous survey conducted by David Wells for *Mathematical Intelligencer* in 1988 (Wells 1988 and Wells 1990). So all right if beauty is an essential criterion of what is considered as mathematically valuable. "Yet no one can say precisely of what beauty in mathematics consists, and professional mathematicians will not necessarily agree on their definitions of mathematical beauty, on their practical judgements of which theorems, proofs, concepts, or strategies are most beautiful, or on the role their personal feelings for mathematical beauty play in their own work". (Wells 1988, p. 30). The results of the inquiry confirmed this picture, and allowed for "the negative conclusion that the idea that mathematicians largely agree in their aesthetic judgments is at best grossly oversimplified" (Wells 1990, p. 40).

Two remarks are in order. First, even a subjective reading of mathematical beauty might play an imperative role in mathematical practice. Second, philosophers can still try to capture a more objective reading of mathematical beauty as well. Gian-Carlo Rota (1997) states that, given a historical period and a specific context, one can find reasonable agreement as to which mathematics is regarded as beautiful. Rota's explication of the concept of mathematical beauty is based on the enlightenment a certain piece of mathematics provides. Mathematical ugliness, Rota argues, encourages mathematicians to develop more aesthetically appealing arguments. Another approach to mathematical beauty, presented by James McAllister (2005), suggests conceptions of mathematical beauty evolve under the influence of that what can be grasped in a single act of mental apprehension. As an example he gives the gradual acceptance of new classes of numbers in mathematics. Initially a new class, such as negative or complex numbers, was regarded as ugly. As mathematical theorizing led to show the applicability and fruitfulness of a new class of numbers, aesthetic merit rose accordingly.

Parallel to the remark made with mathematical explanation, we should remain cautious with linking positive judgments on mathematical arguments a singular concept such as beauty. Matthew Inglis and Andrew Aberdein (2015) stated that while many scholars associate simplicity with beauty, results based on empirical methods do not support that claim. It is our view that a further analysis of terms such as mathematical explanation and mathematical beauty is warranted, but at least as important is the inquiry of mathematical simplicity, depth, elegance, insightful and other terms that have received far less or no philosophical interest.

3.4. *Mathematical Proofs*

When looking at proof in particular, and more precisely the reasons behind favoring certain proofs, values other than explanation, beauty or justification arise without doubt. Rav's seminal paper *Why do we prove theorems* (1999) serves as a good starting point. Rav argues that proofs do much more than verify mathematical claims, and that the proofs themselves can better be seen as important bearers of mathematical knowledge. In order to make his point, he sketches the following hypothetical situation. He considers a machine that is capable of answering instantaneously and infallibly whether a mathematical claim is true or not. As delightful as this may seem, Rav suggest that we would continue looking for alternative proofs that have other merits, such as the fact that they demonstrate new insights, mathematical methods or mathematical strategies. Hence, proofs have a value that goes beyond merely establishing the truth of the theorem. Euclid's proof of there being infinitely many primes is mentioned as an example. The result of this proof is not (only) what makes it important for mathematicians, but (also) the strategy of proving that a set is infinite and then using the elements of this supposedly finite set in order to bring about a new element of that set, is. In other words, the real value of the proof goes beyond knowing that the theorem established by it is in fact true:

> It is a purely creative, topic-specific move; this move, simple as it is, constitutes a contribution to mathematical knowledge which goes beyond the statement of the proposition. Indeed, by the same method as in forming the number *N* one proves that there are infinitely many primes of the form $4n + 3$. Furthermore, Euclid's idea of forming $p_1 p_2 \dots p_n + 1$ was used by Gödel in order to show that the function P (n) taking the n^{th} prime number as its value is [primitive] recursive. (Rav 1999: p. 21)

Good or interesting mathematical proofs, in this sense, is mathematics that brings about concepts and methods that are fruitful for other mathematical research. David Corfield seems to support Rav's ideas, pointing to the:

> dual roles of mathematical proof: establishing the truth or correctness of propositions and contributing to the conceptual development of a field. What mathematicians are largely looking for from each other's proofs are new concepts, techniques, and interpretations. (Corfield 2003, p. 56)

Michael de Villiers (1990) claims, with a special interest in the role of the proof in the educational context, that it would be intellectually dishonest to maintain that verification is the only function a proof can play. For example, proof can play an important role in the systematisation of various known results into a deductive systems of axioms, definitions and theorems. Such a systematisation can have diverse merits; it is easier to identify inconsistencies, mathematical theories are unified by integrating formerly unrelated statements with one another or it provides a global perspective of a topic by exposing the underlying axiomatic structure. De Villiers discusses five such functions of proof in his paper, namely verification, explanation, systematisation, discovery and communication. What end a mathematician wants to achieve will have an impact on how and which proofs are valued.

Another perspective on this topic is provided by John Dawson (2006), discussing the phenomenon of re-proving theorems. If justification of the theorem is the only aim of mathematicians, why are multiple proofs of the same theorem omnipresent in mathematics? Dawson argues this has various reasons including the wish to discover new routes, remedy perceived gaps in earlier arguments, demonstrate the power of a different methodology or to employ reasoning that is simpler than that of earlier proofs.

In sum, how a proof is developed, read and valued may be influenced by one or more aims. An important factor seems to be that mathematical proofs are valued for the fact that they introduce a specific concept, technique or interpretation. The previously mentioned values of explanation and beauty can still be in play here, as well as others including fruitfulness, generality or applicability. Furthermore, as proving and reproving can be motivated by several distinct aims, proof valuation becomes highly contextual. One proof might be admired for its simplicity if the aim is communication, but disregarded when the aim is to demonstrate the power of a methodology. These observations make it more challenging to get a firm philosophical grip on the subject matter, but should do honour to the complexities of mathematical practices.

3.5. *Non-Formal and Non-Deductive Methods*

Shifting our attention from the aims concerning proofs to the actual arguments contained in them, leads to the straightforward observation that mathematical proofs do not at all consist of or develop via deductive procedures alone, and just as with proofs in general we can question the role or value of these other types of mathematical arguments. Indeed it can be quite easily seen that formal derivation does not exhaust the category of rigorous, that is mathematically accepted, argument. One can point to a number of specific *inductive* methods applied in the course of both the process of discovery and justification in mathematical practice such as visualization, number crunching, or probabilistic reasoning. But the repository of methodological resources of working mathematicians is surely bigger than even that, also including *other-than-inductive* non-deductive arguments getting into mathematical play. To be clear, not 'just' the preparative or informal stages on the road to formal proof are hereby envisaged, but also the ways these proofs get supplemented or framed, in order for the intended audiences to grasp their content and judge their quality.

The late American mathematician and Field Medalist 1982 William P. Thurston has with regard to this addressed the matter of proof *vs* progress in mathematics (Thurston 1994). What exactly are they, what is their role in mathematical practice and how do people relate to them? Like matters touched upon in previous subsections, also questions like these are bound to take the philosopher of mathematics way beyond the matter of whether a newly proposed piece of mathematics, that is a proof, adds to the collection of established truths or not. More particularly, it raises the issue of mathematical *understanding*, and that of the conceptual dynamics facilitating it. The rise of computer proof, for one, has brought this matter to the fore very strongly. If we really want to get a grip on this topic, Thurston complained, then the traditional DTP or Definition-Theorem-Proof-model of mathematical practice should be put to the test, and some of its 'other' dimensions become the object of consideration as well. Thurston in this respect distinguishes a number of alternative but non-exclusive modes of mathematical thought contributing to a grasp on mathematical units (propositions or proofs) in general, among which the linguistic, visual, logical, and dynamical ones, noting:

> People have amazing facilities for sensing something without knowing where it comes from (intuition); for sensing that some phenomenon or situation or object is like something else (association); and for building and testing connections and comparisons, holding two things in mind at the same time (metaphor). These facilities are quite important for mathematics. (Thurston 1994, p. 165)

Rigorous but non-formal arguments often come in the format of proof-outlines: sketches of the characteristic global shape of a proof, e.g., infinite descent, splitting in cases, or *reductio* (Van Bendegem 2000). Unlike many of their formal counterparts, these typically have the virtues of surveyability and robustness (Mackenzie 1998). The reason why is simple: they are concise but powerful ways of grasping the upshot or meaning of proofs, their overall structure being what counts, not so much meticulous detail. Because of this, they facilitate the transfer of particular proof techniques to other contexts, for example more general ones; the association facility referred to by Thurston above.

The role of non-deductive methods might also lead to the consideration of the role of mathematical experimentation. While the term experiment may seem odd in mathematics, its use can be found in mathematical practice. Whether you take this term literally or not, the notion of experimental mathematics can be used to label activities in the process of mathematical creation. This can involve work carried out on computers, results of pencil-and-paper work or even experimental techniques involving physical models. These activities often remain hidden from public discussions, but when investigating what it means to aim for good mathematics this context should not be left aside. The editorial columns of the journal that carries the name *Experimental Mathematics*, founded in 1992 believe mathematical practice benefits from making this experimentation process accessible for the mathematical community:

> *Experimental Mathematics* was founded in the belief that theory and experiment feed on each other, and that the mathematical community stands to benefit from a more complete exposure to the experimental process. The early sharing of insights increases the possibility that they will lead to theorems: An interesting conjecture is often formulated by a researcher who lacks the techniques to formalize a proof, while those who have the techniques at their fingertips have been looking elsewhere. Even when the person who had the initial insight goes on to find a proof, a discussion of the heuristic process can be of help, or at least of interest, to other researchers. There is value not only in the discovery itself, but also in the road that leads to it. (From the

> "Statement of Philosophy and Publishing Criteria" of *Experimental Mathematics* by David Epstein, Silvio Levy, and Rafael de la Llave; retrieved from the journal's website http://www.emis.de/journals/EM/ on 3 December 2016)

When addressing the context of discovery, the work by George Pólya should not be underestimated. In *How to solve it* (1945), his most famous and accessible work (later expanded with more sophistication in two double volume books), Pólya set out a four-stage model of the mathematical problem solving process. According to it, one first tries to understand any problem at hand (1), on the basis of that devises a plan (2), then carries out that plan (3), and finally returns to the problem and verifies whether it has hereby been satisfactorily settled (4). Pólya himself got closest to being philosophical at the outset of *Induction and analogy in mathematics* (1973), which he himself even labels as 'in a sense' a philosophical essay, then making an instructive distinction between *demonstrative* and *plausible* reasoning. The former method, aimed at proving mathematical statements, "has rigid standards, codified and clarified by logic, [… it] is safe, beyond controversy, and final" (Pólya 1973, p. v). The latter's standards, on the contrary, are 'fluid', and directed towards generating *and* supporting conjectures. This type of reasoning, Pólya claims, is being unjustly neglected in education. "Certainly, let us learn proving, but also *let us learn guessing*" (Pólya 1973, p.vi). Demonstrative reasoning, as he has it, is but one side of mathematics, being the format of *finished* mathematics.

> Yet mathematics in the making resembles any other human knowledge in the making. You have to guess a mathematical theorem before you prove it; you have to guess the idea of the proof before you carry through the details. You have to combine observations and follow analogies; you have to try and try again. (Pólya 1973, p. vi)

The two kinds of reasoning distinguished by Pólya do however not contradict but complete each other. Both are indispensable for any student of mathematics worthy of that name. Plausible, heuristically driven reasoning essentially constitutes the way to establishing rigorous proof. Focusing on this hitherto neglected type, Pólya offers many examples, something he considers the most proper way of instructing interested readers, that is by offering them ample material for imitation and opportunity for practice. The reason is that there exists, in his view, no foolproof method to learn guessing. Explicit heuristics and search strategies may of course be offered, but these, in principle, will only approximately fit any of the concrete circumstances in which they will ever be applied.

In conclusion, reflections on mathematical aims can and should also address to what end non-deductive and non-formal methods find their place in mathematics. This involves further investigating several heuristic devices, both in the context of discovery and the context of justification.

Philosophical talk on mathematical aims should circumvent earlier mistakes of philosophy of mathematics, namely purely focussing on the outcomes or products of mathematical practice. All aspects of mathematical practice, in its complete richness, can be considered as a part of a route to a certain mathematical aim.

4. Reflections on Methodology and Disciplinarity

With all in mind that has been said in preceding sections with respect to the inherent pragmatic dimension of mathematical knowledge acquisition, it seems appropriate to raise the question of whether the task of properly assessing it can actually be realized by philosophy alone. The answer to which, in our view, should be clearly negative. If meaning is not independent from use, then aspects of said use are surely open to investigation from a variety of angles, philosophical and non-philosophical alike. If this is admitted, however, in its turn, a consideration of the relative importance of these different perspectives is in order.

Should one rest content, in epistemological matters, with traditional 'armchair' philosophical analysis of things? In our young field called 'the philosophy of mathematical practice', this has turned out to be an important point of reflection in its own right. If we truly want to shed light on mathematics as a contextualized human activity resulting in fallible intermediate product, that is, should not be more order than your typical rendering of an idealized picture of this intellectual craft? Following the lead of Lakatos, at the very least historical considerations are in want, complementing the kind of monolitic abstract models of mathematical knowledge traditionally arrived at by philosophy.

> A philosophy of mathematics needs to be adequate for actual mathematical practice and provide an explanation for the procedures of the mathematical community. Empirical and conceptual work needs to be brought together in order to reach this goal. […T]he puzzle of the objectivity of mathematical knowledge cannot be solved by philosophers alone. Involvement with mathematical practice means that other disciplines, such as the history of science, the fields of science education, sociology of science, cognitive science, and possibly psychology hold parts of the answer to our questions. (Löwe & Müller 2010, editorial preface)

The previous metaphilosophical claim entails a shift from mathematical epistemology as a rational, normative (a priori) to an at least partially descriptive (a posteriori) endeavour. Conceptual *analysis* of 'mathematical knowledge' then turns into conceptual *modeling* of it, whereby the limited intuitive (and often strictly individual/introspective) stock of data supporting the exercise is traded in for or expanded by empirical material of various sort, tuning the resulting conceptual (explicative) definitions into ‘reflective equilibria’ between these various sources. Note that this, importantly, entails the avoidance of any kind of reductionism. However, a dominant disapproval of any form of naturalism, whereby one arrives at prescriptions taking into account descriptions (*naturalistic fallacy!*), has kept epistemology away from any empirical considerations until recently. This is e.g. the case when it comes to the achievements of psychology and cognitive science.

> Philosophers of mathematics in general, and analytic philosophers in particular, have shown great reservations toward taking seriously the work of psychologists on mathematical reasoning. This is possibly due to the influence of Frege’s arguments against psychological accounts of mathematical objects, which he deemed either unsatisfactory or subjective. Since then anti-psychologistic tendencies have been popular in philosophy of mathematics, so that philosophers have shunned the idea that any psychological insights might be relevant to their enterprise. (Cruz et al. 2010, p. 94)

It is instructive to note that ‘even’ Imre Lakatos, despite his call for a legitimate place for the history of mathematics complementary to philosophy of mathematics, remained a rationalist about mathematical knowledge after all. Indeed, what he intended to offer where not actual but *rational* reconstructions of mathematical development, with the aim of uncovering a large scale logic of mathematical discovery the actual history should submit to (*footnote history*).

> Thus one cannot replace philosophy of science by sociology of science as the supreme watchdog. If both history and sociology of science are norm-impregnated, rational appraisal of scientific progress must precede, not follow, full scale empirical history. […] Psychologism and sociologism both seem to me to be open to the following fundamental objection. Everyone […] is bound to use normative third-world criteria, whether explicit or hidden, in establishing criteria for a scientific community. (Lakatos 1978, p. 115-116)

In a recent paper, Donald Gillies has denied that the so-called metasciences are necessarily in a worse position *vis-á-vis* demarcationist philosophy when it comes to pinpointing the nature of science. The latter *also*, he observes, start their conceptual work from a number of ‘intuitively’ accepted exemplar cases (e.g. proper mathematical proofs), before starting a critical analysis improving initial explicative definitions distilled from those. Indeed, that might be precisely what metascientists from a historical, sociological and/or cognitive perspective are doing when engaging in epistemic modelling practices: start from a number of rather intuitive conceptions, and then begin a back-and-forth between empirical and theoretical phases (during the latter of which philosophy might have a considerable impact), until their conception of the knowledge phenomena under investigation are reasonably stabilized.

> We begin, say, with some rather crude philosophy of science. This is used as a guide in the study of history of science. However, these historical studies suggest some improvements in our philosophy of science. This improved philosophy of science is used as a guide to further study of the history of science, and so on. What seems to be ruled out by Lakatos' saying is the possibility of first working out a philosophy of science and then applying this philosophy of science to the history of science. […] How then could rational appraisal of scientific progress precede full-scale empirical history? The two have to be developed together through continual interaction. […W]hy should there be a supreme watchdog? Why not two or more watchdogs of equal status? (Gillies 2014, p. 16-17)

5. Conclusion

In this paper we engaged in questions concerning mathematical aims. Formulating such questions is related to a fairly young philosophical trend of taking the actual practice of mathematicians seriously. A glance at mathematical practice, we argue, learns us that justificatory aims are not sufficient to account for much of mathematical activities. Mathematicians often express the desire for good mathematics and the aversion for ugly mathematics. Philosophy can investigate what this difference amounts to by clarifying what it exactly is what mathematicians praise or look for. We explored several perspectives on this subject matter.

A first possibility is to focus on what might count as a mathematical value, in order to see why mathematicians prefer some piece of mathematics over another. Mathematical explanation and mathematical beauty are, considering recent philosophical interest in these notions, promising candidates for grasping mathematical valuation. Nevertheless, more research is needed to fully understand these notions and other notions might capture mathematical appraisal as well.

A second possibility is to look at specific mathematical products and mathematical creative processes, in order to specify to what end these are and can be used in practice. We emphasized that this approach has to acknowledge all mathematical activities, and thus also address the role and significance of non-formal and non-deductive arguments.

The understanding of mathematical aims is far from complete. We addressed some philosophical insights and future questions for philosophy, but we also stated completing such a theory is not a task for philosophy alone. Other approaches, such as empirical methods, should be welcomed as well. Ideally, theoretical and empirical work stand in communication with each other. The former should not be blind for the results that are offered by empirical sciences, while the latter may benefit from philosophical work on concepts and definitions.

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