Broadening questions in the philosophy of mathematics

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1. Introduction

The aim of this dissertation, as the title suggests, is to provide several suggestions to broaden several questions in the philosophy of mathematics. Philosophy of mathematics changed its focus. Instead of investigating the logical foundations and justifications of mathematics, topics have shifted to the practice of mathematics. Philosophers motivated the need for naturalism, that places this mathematical practice central. This have resulted in fruitful studies, and the questionless enrichment of mathematical philosophy.

This dissertation is the result of the suggestion of several questions, that can be posed in my opinion in this naturalist turn in philosophy of mathematics. I will discuss three questions that address the reliability of published proofs, the notion of pursuit in mathematics and mechanical explanation in mathematics. These questions have in common that I believe they address problems that are not sufficiently investigated in philosophy of mathematics. Furthermore, these questions are linked with discussions and problems in philosophy of science. I will not suggest that mathematics is a subfield of science, neither will I make a conclusive comparison between mathematics and science. It is my belief that mathematics can be approached from notions in philosophy of science, in order to find new interesting research questions for philosophy of mathematics.

In the second chapter I will discuss how, despite that mathematical knowledge is seen as unique in comparison with scientific knowledge in terms of reliability, the reliability published literature in mathematics faces problems. In the third chapter I address the notion of pursuit worthiness, which is a fruitful notion in philosophy of science. I will suggest that it can be fruitful for philosophy of mathematics as well, using an example from the history of imaginary numbers. The fourth chapter focusses on explanatory proofs in mathematics. I suggest that the notion of mechanistic explanation is interesting for mathematical proofs as well, based on an example from geometry.
2. Reliability of published mathematical proofs

2.1 Introduction

Mathematics has been characterized as an epistemic exception in comparison with other sciences, leading to more secure knowledge. Although this view is justifiable, it is hard to claim that this leads to secure knowledge in published mathematical articles. The aim of this chapter is to identify several phenomena that threaten the assurance that published mathematical proofs and mathematical truth (approximately) coincide. Naturally, no mathematician is ideal and no one will claim that the body of published articles is free of errors. In other sciences, where the degree of certainty is much lower, certain mechanisms and discussions exist in order to achieve a higher level of certainty. I will argue that such discussions are desirable in mathematical practice as well.

First of all, I will sketch what the epistemic uniqueness of mathematics implies. Further, the role of trust and testimony in mathematics will be discussed. In relation with this trust, the surveyability of mathematical proofs and mathematical peer review are possible problems. Finally, I will suggest several strategies in order to achieve a higher level of certainty in the correctness of mathematical proofs.

2.2 The epistemic uniqueness of mathematics

Several philosophers have claimed that mathematical knowledge is an exception compared to knowledge in other sciences. The argument states that mathematical knowledge is more secure. One of the defenders of this special epistemic status is Heintz (Heintz, 2000). She refers to two typical characteristics present in mathematics, namely coherence and consensus. Mathematics is strongly connected and coherent. Research by mathematicians is relatively isolated in small subfields of mathematics. Mathematical research is by no means centrally coordinated, but individual results still demonstrate a high level of coherence. Science on the contrary is also decomposed through specialization, but this results in theories that are often contradictory to each other in certain subparts of the theory.

The second specific characteristic is the high consensus between mathematicians. Heintz indicates that mathematics does not allow any flexibility for interpretation. Mathematics, again in contrast to other sciences, leaves no room for controversy in regard to its conclusions. Anyone who accepts the rules of the mathematical method will arrive at the same result. A similar conclusion can be found in an article by Azzouni (Azzouni, 2007), who states that mathematical proof is sociologically different when compared to other socially constrained practices. The main point of difference is that the standards in mathematical practice are robust. Mistakes in mathematical practice are possible. Such
mistakes can however be detected and eliminated, even if such a mistake eludes detection for a long time.

Prediger (Prediger, 2006) has rightfully raised questions to the claim that mathematics is an epistemic exception and it escapes sociological analysis, when it is only based on the status of proof in mathematics. She refers to Hersh, who makes a distinction between the front and the back of mathematics. The unique epistemic status of mathematics is limited to the front of mathematics:

“There’s amazing consensus in mathematics as to what’s correct or accepted. But just as important is what’s interesting, important, deep or elegant. Unlike correctness, these criteria vary from person to person, speciality to speciality, decade to decade. They’re no more objective than esthetic judgments in art or music” (Hersch, 1997: p. 39; quoted from Prediger, 2006: p.218).

I agree with Hersh and Prediger that a deeper investigation is needed in order to fully comprehend the differences between mathematical practice and other practices, and the epistemic exception will only be assumable to a restricted part of this practice. But even for correctness sociological analysis can be fruitful. Following the lead of naturalistic philosophy of mathematics, I will look at the mathematical practice rather than mathematical theories. On the level of the theory standards are indeed robust, and a high level of consensus grants mathematics a higher level of reliability than other scientist. But it would be wrong that a published proof has the same level of reliability. This will become clear in the following sections.

2.3 Testimony in mathematics

No mathematician is able, due to problems of time and expertise, to check every mathematical proof he uses for own research. Mathematicians will rely on testimony for certain proofs or parts of proofs. mathematicians refer to published literature without checking the proofs themselves. This phenomenon is described by the mathematician Auslander:

“We accept that a purported result is correct when we hear that it has been proved by a mathematician we trust and “validated” by experts in the author’s mathematical specialty. This is the case even if we haven’t read the proof, or more frequently when we don’t have the background to follow the proof” (Auslander, 2008: p. 64).

A similar phenomenon is described in an article of mathematician Thurston. He even suggests that some mathematical results do not have a written proof:
“Within any field, there are certain theorems and certain techniques that are generally known and generally accepted. When you write a paper, you refer to these without proof. You look at other papers in the field, and you see what facts they quote without proof, and what they cite in their bibliography. Many of the things that are generally known are things for which there may be no known written source. As long as people in the field are comfortable that the idea works, it doesn’t need to have a formal written source” (Thurston, 1994: p. 168).

Since mathematicians do not check the proofs, but use them for further research anyway, one can ask whether this trust in the correctness of the proof is warranted. Such questions arise, more generally, in the topic of testimony in epistemology. For an overview of both historical and contemporary positions in this debate, see (Origgi, 2004; Adler, 2012). The two major positions in epistemology are the reductionist and anti-reductionist. For reductionists, granting epistemic authority must be based on some independent reasons why the source of information is reliable. People rely on others because experience has shown that this trust is warranted. The anti-reductionist position rejects such reasons in granting epistemic authority, stating that evidence by testimony is not reducible to other forms of perceptual or inferential evidence.

For the further elaboration of the paper, Hardwig (Hardwig, 1985;1991) is a good starting point. Hardwig focusses on the role of trust in testimony. Both in daily life and science, we gain knowledge by relying on reports by others. The main and obvious problem is that who believes a certain proposition by trust in another’s testimony does not possesses evidence for this proposition himself. Hardwig acknowledges this trust as an ultimate foundation for a serious part of our knowledge. To rely on experts, what Hardwig calls epistemic dependence (Hardwig, 1985), is eligible since “if I were to pursue epistemic autonomy across the board, I would succeed in holding relatively uninformed, unreliable, crude, untested and therefore irrational beliefs” (Hardwig, 1985: p.340).

Mathematical progress would indeed be hard without testimony. Checking whether every accepted proposition of a proof that a mathematician would publish has a correct proof, would take a serious amount of time and expertise. And naturally, the proofs that the mathematician would check, start from accepted propositions themselves as well.

Several philosophers, in the anti-reductionist tradition, hold that evidence for such testimonies is principally insufficient (for example: Coady, 1993; Foley, 1994). If this is the case, evidence in favour of testimony of experts is even more problematic. When a person relies on an expert, such reliance is necessary blind (Hardwig, 1991). Blind trust means that a non-expert cannot possess enough evidence to evaluate whether an expert’s testimony is reliable. Hardwig states, that despite of this blindness, trusting
Experts is an legitimate acquisition of knowledge, if certain conditions are satisfied. The central rule is: “A must TRUST B, or A will not believe that B’s testimony gives her good reasons to believe p. And B must be TRUSTWORTHY or B’s testimony will not in fact give A good reasons to believe p, regardless of what she might believe about B” (Hardwig, 1991: p.700). The account of Hardwig states that trusting others is normatively accountable in terms of particular reasons that justify the acquisition of knowledge by testimony. The trustworthiness of B is thus based on reasons, for example his established competence, that makes that A can trust B and believe his testimony. Other philosophers, such as Goldman (Goldman, 1999) and Kitcher (Kitcher, 1993) develop similar accounts of testimony in which they considerate which factors contribute to the competence and trustworthiness of experts.

One can wonder whether the non-expert and expert relation is a genuine representation of the relation between mathematicians that adopt results without checking the proofs. Often, a mathematician will not check the proof because he lacks the expertise for particular parts of the proof. There are also cases where the mathematician does not check the proof due to time issues, but has the proper intellectual background. The main intention will however remain that such mathematical research is based on trust that the published proof is correct.

2.4 Peer review in mathematics

The ideal justification of the use in mathematical literature would be that the epistemic unique nature of mathematics allows referees to check the correctness of the proofs with an absolute certainty. Nathanson, in an opinion piece for a mathematical journal, outlines a more pessimistic, and presumably more realistic, picture of mathematical literature:

“How do we know that a proof is correct? By checking it, line by line. […] If a theorem has a short complete proof, we can check it. But if the proof is deep, difficult, and already fills 100 journal pages, if no one has the time and energy to fill in the details, if a “complete” proof would be 100,000 pages long, then we rely on the judgment of the bosses in the field” (Nathanson, 2008).

Nathanson addresses the problem between a published proof and a complete proof. I will come back to this point in the next section. Another problem Nathanson stresses is the role of the refereeing process in mathematical research:

“Many (I think most) papers in most refereed journals are not refereed. There is a presumptive referee who looks at the paper, reads the introduction and the statement of the results, glances at the proofs, and, if everything seems okay, recommends publication. Some referees check proofs line-by-line, but many do not. When I read a
journal article, I often find mistakes. Whether I can fix them is irrelevant. The literature is unreliable” (Nathanson, 2008).

This pessimistic picture of the reliability of mathematical literature contrasts with the picture of the high reliability due to the epistemic status of mathematics. Nathanson indicates several phenomena. First, he finds mistakes in the literature. Second, mathematicians refer to the literature without checking the proof. And third, there is a problem at the level of refereeing since they do not check the correctness of the proof.

Geist, Löwe and Van Kerkhove (Geist et al., 2010) point to exactly this problem of peer review in mathematics. They had send a questionnaire to editors of several mathematical journals. It is interesting to see whether the picture drawn by Nathanson is confirmed. One of the question was whether the referee should check the all the proofs in detail, some proofs in detail or none proofs in details. From the 27 editors that were addressed, 11 answered the above question. The first option was chosen five times, the second six times and the third option was not selected. Remarkably, one of the editors who choose the first option added: “but to be reasonable, I am happy when I find a referee doing (b)”(Geist et al., 2010: pp. 163-164) which is naturally the second option.

Not only practicing mathematicians trust the expertise of a mathematician and the reliability of the proof, but referees as well. For some proofs referees will check the complete proof, but others will rely on testimony for certain parts of the proof. Again, expertise and time seem to be the obvious reasons for this phenomenon. But the idea that every proof is sufficiently checked before publishing seems wrong. The fact that mathematicians publish new proofs, using assumptions of other articles without checking these articles themselves, becomes even more problematic: erroneous proofs can lead to new erroneous results.

2.5 Surveyability of proofs

The quote from Nathanson already mentioned the difference between published proofs and complete proofs. The high consensus described within the epistemic status of mathematics is based on the surveyability of mathematical proofs. If person A gives a proof to person B, person B only has to see whether person A has not made any inappropriate assumptions or steps. The question is whether published proofs are surveyable. The ideal of uncontroversial checkability of mathematical proofs only relates to formal derivations. In mathematical practice, proofs are however typically not published in a complete formal style. An argument between Fallis and Easwaran demonstrates the problem of surveyability of proofs.
Fallis (Fallis, 2003) describes the notion of gaps in mathematical proofs. A gap is any point where the written proof does not follow from the previous lines in the proof by applying formally valid rules of inference. Fallis agrees that most actual proof that are presented by the mathematical community contain gaps. He proposes the following categorization of proof gaps. Inferential gaps occur by leaving out a particular sequence of propositions that the mathematicians has in mind as being a proof, which not qualifies as proof. This sort of gap is problematic, since it undercuts the proof. Enthymematic gaps are gaps where the mathematician does not explicitly state the particular sequence of propositions which he has in mind. The mathematician has checked all the details, so it does not undercut the proof, but these details are left out for the style of length of the article. Untraversed gaps are gaps where the mathematician has not tried to verify directly that each proposition in the sequence of propositions that he has in mind follow from a subset of previous propositions in the sequence by application of a mathematical inference. Fallis notes that in some cases it is “considered acceptable for a mathematician to leave an untraversed gap” (Fallis, 2003: p. 59). If none of the members of the mathematical community has bridged the gap of the last category, Fallis speaks of an universally untraversed gap. He claims that universally untraversed gaps are not unusual in mathematical practice, and that proofs containing such gaps can still be accepted and justified by mathematicians.

Easwaran (Easwaran, 2009) has introduced the notion of transferability. The discussion between Fallis and Easwaran is whether probabilistic proofs are acceptable. Fallis argues since we do accept proofs that contain informal language and gaps, there is no epistemic reason to reject probabilistic proofs. Probabilistic proofs are proofs that rely on a randomized argument. Easwaran argues that there is an important social element to mathematical practice. By looking at this social element of mathematical practice, Easwaran makes a distinction between deductive proofs and probabilistic. The characteristic of deductive proofs is that they are transferable. Transferability means that if a mathematician gives the proof to another mathematician he can verify the correctness of the result without having to rely on the testimony of the first mathematician. A concept that has to be considered here is proof sketch. Such a proof sketch is a proof where certain steps have been removed. It is still possible to transfer such a proof sketch, since these gaps can be filled in by the reader or must be accepted relying on expertise with the domain. The boundaries of what is allowable depends on the particular community of mathematicians where the sketch is written. But every community recognizes that a certain level of gaps in a proof sketch is acceptable. This is close to the notion of enthymematic and untraversed gaps that Fallis had suggested.
This discussion shows that even when a mathematician wants to check a proof, inside or outside the refereeing process, he is still confronted with the fact that proofs are not always presented in a way that allows uncontroversial checkability. The problem of surveyability is recognized for certain proofs, such as probabilistic proofs, computer-assisted proofs and typically very long proofs (Coleman, 2009). There are in fact cases where the correctness of a proof is topic of great discussion due to the problem of surveyability, such as Wiles’s proof of Fermat’s last theorem (Faltings, 1995). An important question is whether proofs that are not subject of such discussions, mainly deductive but long proofs that are presented in a semi-formal and incomplete way, can still be efficiently checked.

2.6 Problem detecting

It would be wrong to paint a too dark picture of mathematical practice, and state that it completely lacks the responsibility of checking published proofs. Some published proofs will be sufficiently checked by referees. And even when the proof is not completely checked, problems can still be detected. Thurston suggests a more positive look on mathematical practice than his colleague Nathanson:

“[I]n any field, there is a strong social standard of validity and truth. Andrew Wiles’ proof of Fermat’s Last Theorem is a good illustration of this, in a field which is very algebraic. The experts quickly came to believe that his proof was basically correct on the basis of high-level ideas, long before details could be checked. […] People are usually not very good in checking the formal correctness of proofs, but they are quite good at detecting potential weaknesses or flaws in proofs” (Thurston, 1994: p. 169).

The mathematical literature is not free of fallacies, but even when the proof is not or not yet checked in a formal complete way, mathematicians detect possible problems. It is certainly the case that some problems are detected in published proofs. I will give two examples. Hsiang published his solution to the Kepler conjecture, also known as the sphere-packing problem, in 1993. However, other mathematicians raised objections to this proof. Several papers showed that the argument of Hsiang contains major gaps and errors. For example Muder concludes that: “the community has reached a consensus on it: no one buys it” (quoted from Hales, 2006: p. 12).

Another example can be found in convex geometry. The Busemann-Petty problem asks the following question: if the section function of a centered convex body in n-dimensional Euclidean space is smaller than that of another such body, is its volume also smaller? During the second half of the 20th century, mathematicians tried to give answers to this conjecture. By 1994, only the case of four dimensions was unanswered. Zhang published
in *Annals of Mathematics* that the answer to the Busemann-Petty problems is negative for four dimension. However, three years later it was shown that the main argument of that proof was wrong, and Zhang published in 1997, in the same journal, that the answer is affirmative in the four dimensional case.

I agree with Thurston that mathematicians do detect weaknesses or flaws in proof. Referees can detect such problems, and even when the paper is published other mathematicians can correct flaws in proofs. But that still leaves us with two problems. First of all, it is hard to tell whether all weaknesses and flaws are detected. This is certainly the case considering the use of testimony in the refereeing process and in referring to proofs. A second problem is that the formal correctness of a proof depends on all assumptions and all steps, and the detection of errors that threaten the correctness of the proof can go beyond the detection of potential weaknesses.

### 2.7 Strategies

Several problems have become apparent, namely the surveyability of the proof and a certain lack of control on the correctness of published proofs. The role of trust and testimony in mathematics is makes that proofs build upon unchecked proofs are vulnerable. Testimony, and the associated vulnerability, is indispensable. Hardwig rightfully noted that knowledge without trust is impossible. A mathematician cannot simply check and know all the knowledge himself on which he builds new results.

The role of trust and testimony in the refereeing process is more problematic. The start of this paper was to draw an epistemic difference between mathematics and other sciences, where mathematical knowledge was more secure due to the high consensus and coherence in mathematics. At the same time, other scientific fields have placed more questions on weaknesses of the refereeing process. There is no consensus on this topic, but questions on the need (White, 2003) and working (Turner, 2003; Weller, 2001) of scientific peer review. This opens the discussion on the biases of referees, the expertise of referees, detecting fraud, etcetera. These results are interesting, but not simply transferable to mathematical peer review. The main goal of scientific peer review is to detect fraud, plagiarism, lack of quality and errors. One can agree that that mathematical peer review has the same goals. The notion of correctness is however completely different in science and mathematics, and more research is needed in order to fully uncover problems and solutions on the proof checking in mathematical journals. The article of Geist, Löwe and Van Kerkhove is the only endeavor in looking into the particularity of mathematical peer review I know, any systematic investigation is missing.
Next to more systematic investigations on how mathematical peer review is organized, and where the weaknesses are more detailed, I will suggest already some preliminary strategies to attack the problem of the correctness of the proof. First of all the lack of expertise to check a complete proof can be countered by dividing the refereeing task to a number of mathematicians that have, combined, the sufficient expertise. The problem of the surveyability of the proof can be countered by demanding two versions of a proof. On the one hand the complete proof, and on the other hand the publishable version. Referees should check the correctness of the first version, which also can be placed online when the second version is published. As a consequence, gaps will not be mistaken to be sufficiently traversed by the author when this is not the case. This opens however a discussion on how far this is feasible. Certain gaps are still sufficiently trivial, and certain proof are that long that a complete proof is nearly insurveyable. But this vagueness shows so much the more further discussions are needed, in order to eliminate the situation that certain inappropriate gaps are considered reliable.

Another consequence of scientific publishing, is that scientific research benefits from replication, reproduction and meta-analysis. Mathematicians also re-prove theorems (Dawson, 2006), and one can argue that this increases the trust of correctness of a proof. I agree that if a theorem has several proofs, the truth of that theorem is more reliable than that it has one proof. Simply, the change that all proofs are wrong is smaller. But this does not mean anything about the correctness of one proof. Even if it is the case other proofs are available, certain steps can be mistaken and these mistakes can be adopted by testimony. Certain scientific fields, typically statistical research, have scientific journals which goal is only meta-analysis. Again, I can suggest a strategy for mathematics, namely journals that focus on contributions of mathematicians that have detected flaws or weaknesses in proofs. As such, mathematicians would be more encouraged to check proofs. I will discuss such encouragement further in the next section.

2.8 Credit-driven mathematics

Kitcher (Kitcher, 1990) considers two models of scientific activity. In the first model scientists are only motivated by acquiring true knowledge. The second model consists of scientists who are only motivated by acquiring scientific status or reputation or money. This can be translated to credit. Suppose that we have several scientific programs. Scientists from the first model will only focus on the program with the highest potential, because it is believed this program can deliver true knowledge. This has a cumulative effect, since this program will only increase and other programs will decrease as more and more scientists chose for that program. Scientists from the second model will show different behavior. They will consider, next to the probability of the success of a research
program, the number of scientists with whom they would have to share the credit of the program if it is successful. The expected credit can thus be higher in research programs with lower potential at first sight. The result is that credit creates a good allocation of epistemic labor.

We can ask similarly which tasks should be performed by mathematicians. Just as in science, the model of credit-driven research is worth considering. Mathematicians are not only interested in acquiring true knowledge, they are also interested in their academic position and reputation. It is easy to see that for an innovative or original or highly difficult result in mathematical research the mathematician deserves credit. Mathematical journals, just as in other disciplines, look for articles that are novel, correct and interesting. A journal that focusses on contributions on the correctness or flaws of proof, would encourage mathematicians to check proofs instead of focusing merely on new publications.

Another strategy would be to grant credits to referees that check the correctness. This would mean that referees are no longer anonymous. But I do not see why this should be the case for the checking of the correctness of the proof. Judgements on originality and publication worthiness of a paper are another case, and perhaps these tasks should be separated in the refereeing process. If a mathematician checks the correctness of a complete proof before it is published, he should gain academic credit. This encourages mathematicians to invest time in such tasks. Now, credits push them away of tasks where no credit can be gained.

2.9 Conclusion

This chapter aimed at the comparison with published mathematical proofs and the assumption that mathematical knowledge has an epistemic unique position. I have not rejected this unique positions of mathematical proofs, which is grounded in the high level of consensus between mathematicians and the coherence of mathematics. But in turning to mathematical practice, it is clear the uncontroversial checking of the correctness is unattained. Mathematicians indicate that most mathematicians refer to proofs without checking them. This is a form of epistemology, where knowledge is based on trust. The mathematical peer review does not ensure correct proofs either, and published proofs themselves contain typically gaps. The justification of trust in mathematical research seems problematic. I have argued that other scientific fields, that do not have such an epistemic unique position, have focused on the role of peer review and other mechanisms such as reproduction and meta-analysis. Mathematical research will always be fallible and rely for a certain amount on trust and testimony, but mathematics should benefit from a deeper investigation of peer review as well. I suggested that the idea of credit-driven
mathematics, where the checking of proofs is a task where academic credit can be obtained could be a valuable strategy.
3. Pursuit in mathematics

3.1 Introduction

The aim of this chapter is to discuss whether the concept of pursuit, discussed in the philosophy of science, is interesting for the philosophy of mathematics as well. The notion of pursuit addresses preliminary judgments of the pursuit worthiness of scientific objects and theories. I will give an example from the history of imaginary numbers, and argue that the notion of pursuit can have a contributing value to the discussion on revolutions in mathematics.

3.2 Pursuit in science

Several philosophers of science have argued for the recognition of a context of pursuit in science. According to Laudan, “acceptance, rejection, pursuit and non-pursuit constitute the major cognitive stances which scientists can legitimately take towards research traditions” (Laudan, 1977: p. 119). The context of acceptance addresses the questions scientists ask in order to choose a theory among a group of rivaling theories and treat it as if it were true. The context of pursuit deals with other questions, namely the question whether a theory is worthy of further pursuit. Such questions are typically asked when young and undeveloped theories emerge.

Similarly, McMullin (McMullin, 1976) distinguishes between two types of theory-appraisal. The first addresses theory acceptability. In order to evaluate the acceptability of a theory, McMullin introduces the criterion of P-fertility. This criterion looks at the actual success of a theory, embodied by the past performance of a theory. A second criterion, U-fertility, addresses the research-potential of a theory: “How likely is it to give rise to interesting extensions? Does it show promise of being able to handle the outstanding problems (inconsistencies, anomalies, etc.) in the field?” McMullin, 1979: p. 423).

Whitt (Whitt, 1992) emphasizes the role of certain values as indices of theory promise that go beyond the historical track record of a theory. For instance, the comparison between the actual explanatory power of a theory and its rivals does not suffices as an indicator for future success. Explanatory anomalies, what a theory cannot explain, is such an indicator of the actual success of a theory. But for the evaluation of pursuit worthiness of a theory: “we are rather interested in its programmatic character which indicates that the investigation can proceed in spite of the current anomalies and towards their resolution. Hence, we are interested in the prospective values” (Seselja and Strasser, forthcoming).
This is certainly not an exhaustive list or discussion on the question of the context of pursuit and questions of pursuit worthiness in science. But it shows that within science we can identify preliminary judgments on the pursuit worthiness of a theory, before this theory is accepted. The question is when these judgments or rational or justified. The goal is to see what the notion of pursuit can contribute to the philosophy of mathematics.

3.3 A first case of pursuit worthiness in mathematics

It is clear that mathematics is not a purely deductive practice. Mathematicians go beyond merely deriving statements from other statements. Franklin demonstrates this idea:

"Mathematics cannot consist just of conjectures, refutations and proofs. Anyone can generate conjectures, but which ones are worth investigating? Which ones are relevant to the problem at hand? Which can be confirmed or refuted in some easy cases, so that there will be some indication of their truth in a reasonable time? Which might be capable of proof by a method in the mathematician’s repertoire? Which might follow from someone else’s theorem? Which are unlikely to yield an answer until after the next review of tenure? The mathematician must answer these questions to allocate his time and effort” (Franklin, 1987: p. 2).

This quote supposes a first role for pursuit in mathematics. In this case, the question is which conjectures are worthy of pursuit, and which strategies to obtain proofs of these conjectures. Deciding which conjectures and strategies are worthy of pursuit are important for a mathematician, since it involves the distribution of work effort within a wade space of possibilities. The question on the pursuit worthiness of strategies to solve a mathematical process have been, without the terminology of pursuit, addressed by philosophers such as Lakatos (Lakatos, 1979) and Polya (Polya, 1945). They focus on heuristics: “some tell us what paths of research to avoid (negative heuristic), and others what paths to pursue (positive heuristic)” (Lakatos, 1978: p. 47).

Although these discussions can possibly benefit from a comparative study between mathematical and scientific heuristics in order to evaluate pursuit worthiness, I want to focus in this paper on another case. The following case will be interesting since it has an extra condition, namely the fact that mathematicians pursue certain developments that are rejected by others.

3.4 A second case of revolutions in mathematics

The case I will use in order argue is situated in the historical development of imaginary or complex numbers and argue that one phase can be interesting for the notion of pursuit worthiness in mathematics. There will be other cases within the history of
imaginary numbers that are interesting, but I will take the liberty of pinpointing certain persons and developments. This does not hold that this is the only case within the history of imaginary numbers that we can speak of the context of pursuit. For a complete overview of this history in detail see (Nahin, 1998), and a shorter overview in (Nagel, 1979).

Imaginary numbers showed up in algebra, but were first seen as a paradox (Kleiner and Movshovitz-Hadar, 1994). Algebra, the branch of mathematics which focuses on numbers and operators, was without any conflict a proper part of mathematics throughout its history. Within algebra, the solution by radicals of cubic equations was one of the great achievements in mathematics in the sixteenth century. The solution of the cubic $x^3 = ax + b$, developed by Cardan, was:

$$x = \sqrt[3]{\frac{-b}{2} + \sqrt{\left(\frac{b}{2}\right)^2 - \left(\frac{a}{3}\right)^3}} + \sqrt[3]{\frac{-b}{2} - \sqrt{\left(\frac{b}{2}\right)^2 - \left(\frac{a}{3}\right)^3}}$$

Bombelli applied this solution to the equation $x^3 = 15x + 4$, which gave him $x = 3/2 + \sqrt{-121} + 3/2 - \sqrt{-121}$. Cardan had denied that his formula was applicable to such equations, since it introduced square roots of negative numbers. Bombelli noted that $x = 4$ is in fact a solution of $x^3 = 15x + 4$. This lead to the paradox that the roots of the equation are real, but the formula yielding the roots involved numbers that were considered meaningless and were rejected. Bombelli stated that this matter does in fact seem closer to mystery than to truth, nevertheless it showed how an unaccepted result followed from accepted rules. Imaginary numbers were thus not proposed as a new theory, expanding or replacing an actual theory, rather they emerged as a paradox within an accepted theory.

The reason why imaginary numbers were considered meaningless and their place in mathematics was rejected was based upon the notion of numbers. Mathematics was in the beginning of the nineteenth century looked upon as the science of quantity or science of magnitudes. Numbers were conceived as being an answer to the question ‘How many?’ or ‘How much?’ This was the only acceptable notion of numbers. Mathematics founded upon this notion was considered useful, fertile and trustworthy. During history the range of arithmetical operations however extended, and its application revealed challenges to this notion. The real challenge appeared when negative and imaginary numbers showed up in mathematical practices. These numbers were not welcomed openly by the
mathematical community. Since these new concepts of numbers are now accepted as a justified part of mathematics, it is clear what Davis means:

“It is paradoxical that while mathematics has the reputation of being the one subject that brooks no contradictions, in reality it has a long history of successful living with contradictions. This is best seen in the extensions of the notion of number that have been made over a period of 2500 years. From limited sets of integers, to fractions, negative numbers, irrational numbers, complex numbers, transfinite numbers, each extension, in its way, overcame a contradictory set of demands” (Davis, 1965: p. 305).

Imaginary numbers show up in a theory and method that is widely accepted. But while they were paradoxical and rejected in the first place, they are accepted now. Nagel (Nagel, 1979) gives two main reasons why imaginary numbers were eventually accepted. First of all, there were attempts and achievements in finding a geometrical interpretation for imaginary numbers. A second important modification is the recognition that pure mathematics is not concerned with a special interpretation of its numbers. The image of the subject matter of mathematics evolved to a recognition of a structure of systems of uninterpreted symbols. Such a structure does not depend on the interpretation of it, and is thus capable of different interpretations. I will look into one of the attempts to give a geometrical interpretation.

An important person in the development of a geometrical interpretation is Wallis (for a detailed description, see: (Nagel, 1979) and (Scott, 1936)). Wallis, a mathematician in the seventeenth century, did not rejected the idea that a number is to be identified with quantity. And in this notion, imaginary numbers and negative numbers remained absolutely paradoxical. But at the same time he did recognize that they could be useful as a bare algebraic notation, and we could search for an adequate interpretation of these numbers. A similar interpretation for negative numbers was already somewhat satisfactory, namely the physical interpretation that a negative number is familiar to the length, but with an opposite direction than that of a length symbolized by a positive number. Wallis thought that imaginary numbers could be approached similarly. He proposed a geometrical interpretation of an imaginary number. Wallis’ strategy was to see an imaginary number as a segment whose length is the mean proportional between lengths having opposite directions, and whose direction lies between these directions, and at some angle to the straight line. By proposing this idea, Wallis reached out to an intelligible interpretation of numbers that are unintelligible regarded as quantities.

Playfair, a century later, was equally puzzled with the usefulness of the unintelligible operations with imaginary numbers. He did however not approve the goal of mathematicians as Wallis:
“There have been more than one attempt to treat imaginary expressions as denoting things really existing, or as certain geometrical magnitudes which it is impossible to assign. The paper before us is one of these attempts: and the author [the mathematician Buée], though an ingenious man, has been betrayed into this inconsistency by a kind of metaphysical reasoning” (Playfair, 1808: p. 306).

But the work of mathematicians in giving a complete geometrical meaning of imaginary numbers did not cease, and in the beginning of the nineteenth century Wessel and Argand did complete satisfactory interpretations. The following decades mathematicians continued examining representations in geometry of imaginary numbers, what lead to intelligible interpretations of those numbers and a useful calculus for the geometry of the plane. On the other hand, mathematics still remained silent on the nature of the object of imaginary numbers, whose existence was still highly questionable. Like mentioned earlier, it would take the recognition that mathematics concerns the structure of systems of uninterpreted signs to make imaginary numbers fully acceptable to mathematicians.

3.5 Wallis and Berzelius

There is an interesting parallel between the case of Wallis and certain cases of pursuit in science. Whitt describes the pursuit of several scientists of the Daltonian theory in the nineteenth century as an example of pursuit (Whitt, 1990). I will consider the work of Berzelius in this field similar to the work of Wallis. Berzelius work was essential for the elaboration of experimental techniques in order to determine an extensive set of atomic weights. The determination of atomic weights was a substantial object of the theory of Dalton. At the same time he worked on pointing out shortcomings of the Daltonian theory. The case of Berzelius shows how scientists do more than only accepting or rejecting theories. Consider following quotes out of a small article, addressed to chemists who wish to work in the field of chemical proportions:

“It has given me pain to think that the respectable Dalton has taken my ideas on the corpuscular theory as a criticism on his […] Mr. Dalton has chosen the method of an inventor, by setting out from a first principle, from which he endeavours to deduce the experimental results. For my own part, I have been obliged to take the road of an ordinary man, collecting together a number of experiments, from which I have endeavoured to draw conclusions more and more general” (Berzelius, 1815: p. 122).

“When I endeavoured to draw the attention of chemists to the difficulties in the atomic theory, it was not my attention to refute that hypothesis. I wanted to lay open all the difficulties of that hypothesis, that nothing might escape our attention calculated to throw light on the subject” (Berzelius, 1815: p. 126).
These quotes show how Berzelius chose to further investigate the Daltonian theory, without claiming it was true or false. Wallis, for his part, did not claim that the notion of a number as quantity was wrong and imaginary numbers exists. We could regard the notion of numbers as quantity as a guiding principle of mathematicians, analogous to the first principle of Dalton. Such beliefs guide research in a certain directive. That notion of numbers was certainly defendable, since it was widely accepted and the base where mathematics was founded upon. But Wallis, just as Berzelius, without leaving this notion, furthered work upon a more comprehensive interpretation. I believe this to be a clear case in mathematics where a mathematician placed themselves in another context than the context of rejection or acceptance.

The main reason for work upon imaginary numbers was given their place in a theory such as algebra. Algebra was widely accepted and useful, so it was a puzzle why imaginary numbers were fruitful in such a theory. It is this fruitfulness that can be given as an answer whether the search for a suitable interpretation of imaginary numbers was worthy of pursuit. That is clear when Wallis calls imaginary numbers a bare algebraic notation. The pursuit worthiness of Wallis’s work could also be defended by appeal to consistency. An adequate interpretation, in contrast with treating it as a paradox, would make the emergence of imaginary numbers in algebra consistent. But on the other hand, such an interpretation was also inconsistent with the notion of what numbers are.

3.6 Revolutions in mathematics

I believe the context of pursuit and questions of pursuit worthiness can yield a serious contribution to the question whether revolutions occur in the history of mathematics. The extreme positions within the debate are represented by Crowe (Crowe, 1992) and Dauben (Dauben, 1992).

Crowe’s article states ten laws concerning patterns of change in the history of mathematics (Crowe, 1992). The tenth and final law states that revolutions never occur in mathematics. The basis argument for this assertion is that a necessary characteristic of revolutions is not met in mathematical history. This characteristic is that some previously existing entity must be overthrown and irrevocably discarded.

The remark of Crowe matches with the idea of revolutions in science. Weber and Seselja (Weber and Seselja, 2010) present identification criteria for scientific revolution. They state that:

“A revolution occurs if and only if a substantial group of researchers within a scientific discipline (i) shifts to a new paradigm which is such that a large majority of the auxiliary hypotheses of the old paradigm becomes pointless for theory building, and (ii) this group
of scientists keeps on working with the new paradigm for a certain period of time” (Weber and Seselja, 2010: p. 255).

The conflict between this identification criteria for scientific revolutions and the Crowe’s view on the process of mathematical progress is clear. This process does not overthrow mathematical objects or theories in such a way that they become pointless. Take for example non-Euclidean geometries. These theories do not replace Euclidean geometry, but are rather studied besides Euclidean geometry. In this sense, transitions in mathematical development seem much more continuous on first sight.

Dauben (Dauben, 1992) on the other hand defends the existence of revolutions in mathematics. Revolution is however not approached in a Kuhnian sense. Dauben agrees that there is a difference between scientific and mathematical history in terms of continuity, but he suggests that some transitions in mathematical development are critical enough to be defined as revolution:

“Discovery of incommensurable magnitudes and the eventual creation of irrational numbers, the imaginary numbers, the calculus, non-Euclidean geometry, transfinite numbers, the paradoxes of set theory, even Gödel’s incompleteness proof, are all revolutionary – they have changed the content of mathematics and the ways in which mathematics is regarded. They have each done more than simply add to mathematics – they have each transformed it. In each case the old mathematics is no longer what it seemed to be, perhaps no longer even of much interest when compared with the new and revolutionary ideas that supplant it” (Dauben, 1992: p. 64).

This shows how the discussion is in fact for a large part a semantic discussion. The ascription of revolutions to the history of mathematics depends on the meaning of the notion revolution (Corry, 1993). Another point in the discussion on the nature of revolutions in mathematics is proposed by Dunmore (Dunmore, 1992). She argues that while mathematics is conservative on the object-level, it is revolutionary on the meta-level. This is actually accepted by Crowe, who notes that revolutions do not occur content-wise, but may occur in mathematical symbolism, methodology or meta-level. The question remains whether it is credible or doubtful that revolutions can occur in meta-mathematics, and at the same time do not transform mathematics revolutionary on the content-level.

A further investigation of the pursuit of certain mathematical developments can be a welcome complement of this discussion. Rather than looking at the large structure of how theories, objects and mathematical notions are shaped throughout history, elaborating further on the context of pursuit focusses on the choices mathematicians make in order
to follow a certain research directive. It would be interesting to see whether we can identify which goals and questions of pursuit worthiness are present in mathematics. This would give a more nuanced view on the evolutionary or revolutionary character of mathematics, and could perhaps expose why mathematics is this continuous on an object-level and discontinuous on other levels.

Finally, it should be noted that my approach to the case of Wallis only hints at a certain case of pursuit worthiness of mathematics. An unifying pattern of pursuit worthiness is proposed by Seselja, Kosolosky and Strasser (Seselja et al., forthcoming), and they show how different notions of pursuit worthiness are obtained by varying between the person or groups that pursue, what is pursued and the definition of the set of goals that a pursuit should be conductive of. A further elaboration of pursuit in mathematical history can investigate the pursuit of epistemic objects, such as imaginary numbers, but of theories as well. It would for example absolutely be interesting to see what made the calculus theory worthy of pursuit, before it was accepted. By further developing which cases can be interesting for pursuit in mathematics, a much clearer classification of goals will emerge. I looked at Wallis, concluding that the emergence of imaginary numbers as a useful notion in an accepted theory was crucial in the pursuit of a geometrical interpretation of these numbers. But certainly other goals and reasons can be found within a more systematic approach to several case studies.

3.7 Conclusion

The context of pursuit addresses the situation where scientists, rather than accepting or rejecting a theory, devote their work on the further development of a theory that is promising. Since this is a typical situation in the history of science, philosophers of science focused on this context of pursuit. I have questioned whether attention for the context of pursuit would be required for mathematics as well.

A first parallel can be seen with the question what mathematicians investigate, and which strategies they use. Mathematicians can use heuristics for this purpose, as discussed by Lakatos and Polya. I suggested another type of case, where a certain mathematical development is pursued while this is rejected by other mathematicians. Such a case can be found in the search for a geometrical interpretation of imaginary numbers. Imaginary numbers emerged in algebra, but they were treated as a paradox since the view was that numbers are representations of quantity or measure. Nevertheless, certain mathematicians, such as Wallis, tried to find an intelligible interpretation of those numbers by embedding them in geometry. Philosophy of mathematics could, similarly to philosophy of science, investigate what reasons there were to pursue these attempts. Such an approach can be interesting to investigate progress and discovery in
mathematics, without referring only to the semantically laden debate on evolutions and revolutions in mathematics.
4. Mechanical explanation in mathematics

4.1 Introduction

There has been an extensive philosophical analysis and discussion of scientific explanations in contemporary philosophy. The role of explanations in mathematics had less attention in these debates. The aim of this paper will be to contribute to the discussion of explanation within mathematics. The main topic will be the notion of an explanatory proof. It is a common perception that, like scientists, mathematicians search for more than a mere collection of truths. If we look at mathematical proofs, it is clear that the main aspect is to establish the truth of a mathematical claim. Another aspect of proof is however explanation. While all proofs of theorem \( p \) show that \( p \) is true, some proofs also reveal why \( p \) is true.

I will look into several accounts that have been proposed to characterize explanatory proofs. I will show how these accounts have difficulties facing mathematical proofs that use a bottom-up direction of reasoning. I will discuss, comparing it, with mechanical explanation in science, that such proofs can be explanatory. This leads to a suggestion that a plural and pragmatic notion of mathematical explanation is desirable.

4.2 Accounts of mathematical explanation

4.2.1 Steiner

Steiner states that “to explain the behavior of an entity, one deduces the behavior from the essence or nature of the entity” (Steiner, 1978: p. 143). He uses the concept of characterizing property in order to have a clear distinction between proofs that explain and those that do not. A characterizing property is a property unique to a given entity or structure within a family or domain of such entities or structures. A proof is explanatory when it satisfies following criteria. Firstly, the proof should make reference to a characterizing property of an entity or structure that is mentioned in the theorem. Secondly, the proof should depend on the property, meaning that the proof no longer works if other entities or structures from the same family with a different characterizing property are substituted in the proof. And thirdly, the proof can be deformed by varying with the characterizing properties so that related theorems are obtained.

4.2.2 Unification

The unification model for explanation has a prominent place in discussions on scientific explanation. This account, first proposed by Friedman (Friedman, 1974) and extended by Kitcher (Kitcher, 1981), represents the idea that a good scientific theory explains by providing a unified account of a large and diverse set of phenomena. An important
difference with the account of Steiner, is that Kitcher does not aim at criteria to make distinctions between explanatory and non-explanatory arguments in isolation. Kitcher rather aims at a global account of explanation. I will sketch the general idea of unification proposed by Kitcher (Kitcher, 1981), without entering into the technicalities of the model.

Kitcher sees science as providing us with an explanatory store. This is a set of arguments that can be used for the purposes of explanation. The idea is that, given a consistent and deductively closed set K of beliefs accepted by a scientific community, the explanatory store is the best systematization of K. Systematizations of K are sets of arguments which derive members of K from other members of K by applying valid rules of inference. While there are many possible systematizations of K, the explanatory store is the systematization is the set of derivations that best unifies K. In order to do so, Kitcher needs an evaluation of the degree of unification. This evaluation depends on the notion of argument pattern. An argument pattern is a triple consisting of a schematic argument, a set of filling instructions and a classification for the schematic argument.

As an example, Kitcher (Kitcher, 1981: pp. 515-519) represents the pattern of argument introduced by Newtonian mechanics:

1. The force on α is β
2. The acceleration of α is γ
3. Force = mass . acceleration
4. (Mass of α) . (γ) = β
5. δ = θ

The set of filling instructions are: ‘all occurrences of α are to be replaced by an expression referring to the body under investigation’; ‘occurrences of β are to be replaced by an algebraic expression referring to a function of the variable coordinates and of time’; ‘γ is to be replaced by an expression which gives the acceleration of the body as a function of its coordinates and their time-derivates’; ‘δ is to be replaced by an expression referring to the variable coordinates of the body, and θ is to be replaced by an explicit function of time’. While the filling instructions give directions for replacing the dummy letters, the classification gives us the inferential information about the schematic argument. Here (1) to (3) have the status of premises, (4) is obtained by substitution and (5) follows from (4) using techniques of the calculus. The pattern will instantiate arguments that will have a similar form, and make use of the same theoretical concepts.
Although the model originally addresses scientific explanation, several philosophers (Hafner and Mancosu, 2005; Tappenden, 2005) share the idea that unification can cover mathematical explanations as well. Kitcher himself defended this idea:

“For even in areas of investigation where causal concepts do not apply – such as mathematics – we can make sense of the view that there are patterns of derivation that can be applied again and again to generate a variety of conclusions” (Kitcher, 1981: p. 437).

4.2.3 Account of why-questions

Resnik and Kushner formulate a criticism to Steiner following the pragmatic theory of scientific explanation, proposed by van Fraassen (van Fraassen, 1980). This account involves thinking of explanations as answers to why questions. Given a question Q, there is a topic T, a contrast class C and a relevance relation R. This results in an answer of the form ‘T in contrast to C because X’. Crucial is that both A and T are true, there are no members of the contrast class besides T that are true and X bears the relation R to (T,C).

Resnik and Kushner apply these ideas to mathematics. Whether a proof is explanatory depends on the particular question on the table. The explanatory characteristic of a proof is not objective on this view of explanation, since the topic, contrast class and relevance relation define the question. There is a difference between the question why the Pythagorean theorem holds only for right triangles and the question why the Pythagorean theorem holds in Euclidean space. In that sense, there is no objective distinction between proofs that explain and proofs that don’t explain. A proof allows us to answer why-questions. The minimal question seems to be ‘why is the theorem true?’. However, a proof can be more illuminating if it answers more questions. Such proofs “present more information and do so more perspicuously than do ‘nonexplanatory’ proofs of the same results. Thus they provide the ingredients for answering more why-questions than other proofs of the same results. But they are not explanatory in and of themselves”(Resnik and Kushner, 1987: p. 154). The concept of characterizing property is thus typically useful for the question ‘Why do mathematical objects of class X have property Q, while other objects in the same family do not have this property?’ However, Steiner’s account may not be similarly useful for other why-questions.

Another discussion of Steiner’s account based on the theory of why-questions is provided by Weber and Verhoeven (Weber and Verhoeven, 2002). They agree with Resnik and Kushner that the account of Steiner is too narrow because it focuses on the particular question mentioned above. Weber and Verhoeven also refine the idea of Steiner. To have a successful explanation, and thus an answer of a why-question, they argue that one
should put a couple of proofs together. The question ‘Why do mathematical objects of
class X have property Q, while those of class Y have property Q’?, is explanatory
answered if you can produce a proof that mathematical objects of class X have property
Q and a proof that mathematical objects of class Y have property Q’. In accordance with
Steiner, both proofs should depend on the use of a characterizing property of the objects
of class X and Y respectively. Let us look at an example Weber and Verhoeven give:

Proof 1

(1) For every triangle ABC holds: $c^2 = a^2 + b^2 - 2ab \cos(a,b)$

(2) For every angle $\theta$ holds: $\cos(\theta) = 0$ if $\theta = 90^\circ$

(3) For every right-angled triangle ABC with hypotenuse c holds: $(a,b) = 90^\circ$

(4) For every right-angled triangle ABC with hypotenuse c holds: $c^2 = a^2 + b^2$

Proof 2

(1) For every triangle ABC holds: $c^2 = a^2 + b^2 - 2ab \cos(a,b)$

(2) For every angle $\theta$ holds: $-1 < \cos(\theta) < 0$ if $90^\circ < \theta < 180^\circ$

(3) For every right-angled triangle ABC with obtuse angle in C holds: $90^\circ < (a,b) < 180^\circ$

(4) For every right-angled triangle ABC with obtuse angle in C holds: $c^2 > a^2 + b^2$.

The example makes clear how the criteria of Steiner works. We have a general law in
(1),namely the law of cosines. The fact that in a right-angled triangle the cosine of the
right angle equals zero is the characterizing property. And finally a deformed proof can
be obtained by variation of this characterizing property. These conditions are sufficient
for Steiner to regard the proof as explanatory. The couple of both proofs do the
explanatory work in the account of Weber and Verhoeven.

4.3 Bottom-up and top-down explanation

Borrowing his terminology from Kitcher, Salmon (Salmon, 1989) distinguishes bottom-up
from top-down explanation. A bottom-up approach to explanation describes the causal
processes, interactions and mechanism responsible for the occurrence of the
explanandum. The top-down approach on the other hand subsumes the explanandum
under general principles. This second approach aims at unifying several phenomena
under the same general principles. Salmon suggests that both approaches give genuine
explanations: “It is my present conviction that both of these explanations are legitimate and each is illuminating in its own way” (Salmon, 1989: pp. 183-184).

The question is whether such a distinction is useful for mathematical explanation as well. The unification approach of Kitcher is clearly a case of top-down explanation. Steiner, in his own way, searches for unification as well. Steiner wants explanatory proofs to be deformable, so that related proofs by deformability subsume under the same set of principles. I will argue for a place for bottom-up approaches to mathematical explanation, starting from mechanical explanations.

Following the initial account of Salmon, a process theory of causation, explanation is the identification of the net of causal processes underlying the explanandum. Recent contributions to the bottom-up approach to scientific explanations are mechanical accounts. Several philosophers, such as Glennan (Glennan, 2002), Machamer, Darden and Craver (Machamer, et.al, 2000) and Bechtel and Richardson (Bechtel and Richardson, 1993) have proposed models of mechanical explanations. These models are usually linked with explanation in biological and cognitive science, where explanations take the form of describing and specifying mechanism. Bechtel and Abrahamsen give the following definition of a mechanism: “A mechanism is a structure performing a function in virtue of its component parts, component operations, and their organization. The orchestrated functioning of the mechanism is responsible for one or more phenomena” (Bechtel and Abrahamsen, 2005: p. 423).

There are differences between the several mechanical accounts, but it is not necessary to address them here (for an overview of the difference in how parts of the mechanisms are understood to behave, see: Tabery, 2004). The common ground between the approaches is that a mechanical explanation successfully identifies variables that are situated and make a difference within the mechanism. Using an example of a geometrical proof, I will argue that this is the case in mathematics as well. Bechtel and Richardson introduce two notions that will appear useful for this example, namely decomposition and localizations (Bechtel and Richardson, 1993). Roughly, decompositions means that a scientist can divide the system into separate sub-processes. The authors assume that an activity of the system is a product of a set of subordinate functions performed in parts of the system. Localization subsequently means that the scientist can indicate in which component parts these sub-processes occur.

4.4 A geometrical example

The butterfly theorem has gained a lot of interest in the mathematical community, mainly in the second half of the 20th century (for an overview, see Bankoff, 1987; Cerin,
2003). I will give two approaches to proof this theorem. I will argue that the first proof can be seen as an example of a mathematical bottom-up explanation. The second approach shows that the same theorem can be proved, and explained, top-down as well.

**Butterfly theorem:** Let C be the midpoint of a chord AB of a circle, through which two other chords FG and ED are drawn; FD cuts AB at M and EG cuts AB at N. Then C is the midpoint of MN.

![Figure 1: Butterfly theorem](image_url)

**Proof of the butterfly theorem**

1. $\angle FDC$ and $\angle EGC$ are equal (Property inscribed angles)
   $\angle DFG$ and $\angle CEG$ are equal (Property inscribed angles)

2. $\triangle FCD$ and $\triangle ECG$ are similar (follows from 1 – Property of similar triangles)

3. $FD/FC = EG/EC$ (follows from 2 – Property of similar triangles)

4. Construct point H on DF such that OH is perpendicular to DF, and construct point J on EG such that OJ is perpendicular to EG

5. $FD = 2FH$ and $EG = 2EJ$ (follows from 4 – Property of a chord)

6. $FH/FC = EJ/EC$ (follows from 3 and 5 – Substitution)

7. $\triangle FCH$ and $\triangle ECJ$ are similar (follows from 1 and 6 – Property of similar triangles)
Figure 2: Intermediate steps butterfly proof

(8) \( \angle EJC \) and \( \angle FHC \) are equal (Property corresponding angles)

(9) OCMH is cyclic (\( \angle FHO + \angle ACO = 180^\circ \) - Property of a cyclic quadrilateral)

OCNJ is cyclic (\( \angle EJO + \angle BCO = 180^\circ \) - Property of a cyclic quadrilateral)

(10) \( \angle MHC \) and \( \angle MOC \) are equal (follows from 9 – Property of inscribed angles)

\( \angle CON \) and \( \angle CJN \) are equal

(11) \( \angle MOC \) and \( \angle CON \) are equal (follows from 8 and 10 – substitution)

(12) \( \triangle OCM = \triangle OCN \) are equal (Follows from 10 and 11 – Property equal triangles)

(13) C is the midpoint of MN (Follows from 12)

Figure 3: Final steps proof butterfly theorem
We can also consider a proof that follows the top-down approach of reasoning and explanation. In that case, the butterfly theorem is a special case of the more general theorem, such as the two butterflies theorem (Jones, 1976).

**Two butterflies theorem:**

Let two self-intersecting quadrilaterals KLMN and K’L’M’N’ be inscribed into the same circle. Assume the two intersect chord AB at points P, Q, R, S and P’,Q’,R’,S’ respectively. Assume also that none of the vertices of the quadrilaterals coincide with either A or B. If some three of the points P, Q, R, S coincide with three corresponding points of P’, Q’, R’, S’, the remaining two points also coincide.

We can give an outline of the proof, deriving it from its generalization, as follows: if we suppose that one self-intersecting quadrilateral (of “butterfly quadrilateral”) is symmetric with respect to OC (in the butterfly theorem) and so that its intersecting sides meet at C. The second self-intersecting quadrilateral is bound to intersect AB at the same points as the first, and is in other words symmetrically with respect to C. This means that C is the midpoint of AB, as the butterfly theorem says. The second proof could be explanatory in the top-down approach. We have the two butterflies theorem, and by adding the characterizing property of the butterfly theorem we can prove this theorem. The characterizing property is the symmetry of the mathematical structure. Furthermore, since we can prove the two butterflies theorem, and we can derivate the butterfly theorem from the two butterflies theorem, the proofs have a unifying value.

The question remains whether the first proof is explanatory. This is a challenging question. The accounts of Steiner and Kitcher do not allow us to grant explanatory value to the proof. First of all, it is hard to see what the characterizing property of an entity mentioned in the theorem would be. The proof depends on properties of a circle, chords, triangles and quadrilaterals. The theorem fails to hold if we drop any of its conditions, but it is impossible to secure one property that the proof depends on. Furthermore, the proof is not deformable by varying on such a characterizing property.

The unification account does not yield improvement. The inferential steps of the proof cannot be modeled by Kitcher’s idea of argument pattern. At best we can think of argument patterns to justify a certain step. For example:

(1) \( \alpha \) and \( \beta \) are two angles of triangle \( A \).

(2) \( \gamma \) and \( \delta \) are two angles of triangle \( B \).
(3) \( \alpha = \gamma \)

(4) \( \beta = \delta \)

(5) If two angles of a triangle have measures equal to the measures of two angles of another triangle, then the triangles are similar.

(6) Triangles A and B are similar.

But it is not the case that such a pattern of derivation could represent the whole proof. Step 10 in the proof is justified by the production of two new circles in step 9. Such steps are not transferable to an argument pattern. And even if such a pattern would be manipulated to represent the whole proof, the unifying value would be enormously low since it would only be possible to derivate the butterfly theorem.

4.5 Mathematical mechanism

According to Friedman, a satisfactory account of explanation must have a connection between explanation and understanding (Friedman, 1974). Explanations should provide understanding, and I will argue the idea of a mechanistic explanation helps us to achieve such understanding. First of all one has to understand each intermediate step of the proof in isolation, to see how a step is justified by an accepted property or previous step. But to understand the complete proof, one does more than the assembly of the understanding of justified steps. In order to gain full understanding, one has to see the structure of the proof and which tactics are used in order to make certain steps.

I have mentioned that mechanistic explanation holds the identification of variables that are situated and make a difference in the mechanism. The idea of difference-makers is linked with notions of causality. Lewis (Lewis, 1973) introduces a counterfactual theory of causation. Woodward (Woodward, 2003) also states that scientists causally explain when they know how to manipulate. Such manipulations are understood as counterfactual experiments, which give an answer to a ‘what-if-things-had-been-different-question’.

I will not defend that mathematical proofs contain causal relations, but a model that interprets it as a mechanism does. Perhaps full-blooded platonists will defend that there is a causal process between the magnitudes of a triangle and the similarity of the triangles, but I do not wish to defend this claim. The key is that we can imagine a mechanism where such a difference-making relations are the case. Similarly to decomposition in science, a circle can be decomposed in chords, segments between the midpoints and chords, triangles formed within the triangle, quadrilaterals and other mathematical structures. Such a decomposed structure can be approached by imaginary counterfactual experiments. If we imagine that the magnitude of an angle in a triangle
changes, its relation with other decomposed parts will change. The triangle will no longer be similar to another triangle. If we can justify each step by giving an answer to a “what-if-things-had-been-different-question”, it is possible to see how every imaginary manipulation of a property of a part of the structure has effect on other steps, and finally the truth of the proven theorem. The properties of the decomposed parts are in relation with properties of the initial mathematical figure. This is a trivial observation for every mathematician, but it shows that such proofs can be considered explanatory. The proof localizes a set of difference-makers in an imaginary mechanism that establishes the truth of the theorem. If someone can give such a set of difference-makers that lead to the truth of the theorem, it is hard to argue that the understanding of this theorem is not increased.

4.6 Pragmatism and pluralism

I have suggested that the butterfly proof can be approached as a model of an imaginary mechanism, where difference-makers are identified and establish the truth of the theorem. This increases understanding, and can thus be seen as explanatory. But in order to achieve this understanding, one has to have insight in how each step makes a difference in the imaginary mechanism. As a consequence, in order to understand the theorem and to have an explanatory proof, understanding is demanded from the proof reader. Not everyone will see how difference makers make the proof work. It depends on the context of the proof and the person whether a full understanding will be reached. This means that a proof can be explanatory, but it will depend on the context whether it explains. This pragmatic context is crucial, and mostly overseen in debates of mathematical explanation.

This also addresses the question whether philosophers should aim for a notion of mathematical explanation that suits mathematical research or mathematical education. Mancosu (Mancosu, 2001) suggests that philosophy needs to investigate what counts as explanatory for mathematicians. I do not agree with this assumption. The aim of philosophy should be to address how proofs can be explanatory. Whether someone will experience the proof as explanatory, depends on the context. Mathematicians can find some proofs, understood as an imaginary mechanical explanation that identifies difference-makers, illuminating and others not because it does not increase their understanding. On the other hand, students can find several proofs interpreted in this way illuminating, but other proofs will demand to much background knowledge to acknowledge the difference-makers.

Next to the pragmatic context, I also suggest a pluralist approach to mathematical explanation. First of all, several accounts of explanation remain applicable. The butterfly
A theorem can have a bottom-up and a top-down explanation. A theorem can have several proofs that are explanatory, following different accounts of explanation. Again, the context will determine which proof is more explanatory for the person. Choosing for the pluralist approach to mathematical explanation, based on providing understanding, holds that much work has to be done. I have suggested that the notion of an imaginary mechanism, difference-makers, decomposition and localization can increase one’s understanding of a mathematical proof in Euclidean geometry. Contemporary mathematics is naturally much richer than this field. The question we should ask is how we define mathematical understanding, relative to its subfield. If we tackle the question of mathematical explanation on this pluralist and pragmatist way, we may lose a strict defined black-white picture of explanatory and non-explanatory proofs. But it can be more promising to enter the grey zone of proofs that are potentially explanatory, but do not fit the strict criteria of the accounts that are now available.

4.7 Conclusion

Several account of explanatory proofs in mathematics are available, most notably the accounts of Steiner, unification and the pragmatic account of why-questions. If we look into mathematical explanation from both a bottom-up and a top-down approach, it becomes clear that the account of Steiner and the unification account tend towards top-down explanations.

I have suggested, using an example from Euclidean geometry, mathematical explanation can be bottom-up as well. Similarly to mechanical explanation in science, we can approach the proof as providing a model of an imaginary mechanism. By using the concepts of difference-makers, decomposition and localization, we can see how properties of decomposed parts are related to properties of the initial figure. Recognizing how each decomposed part is manipulable in an imaginary way, and the difference this makes for other parts and the conclusion of the proof, increases the understanding of the proven theorem.

This approach demands for a pragmatic and pluralistic notion of mathematical explanation. It is pragmatic since it depends on the proof reader whether the proof is considered explanatory or not. Pluralism opens the way for further finding and elaborating how proofs can be explanatory in particular mathematical fields.
5. Conclusion

The aim was to provide questions for philosophy of mathematics that, in my humble opinion, are not sufficiently addressed. I started from several discussions and notions in philosophy of science, and made clear that certain questions are interesting for mathematical practice as well. This does not mean that the notions of philosophy of science are applicable to mathematics. Rather it means that philosophy of mathematics should internalize these concepts for its own research.

I have suggested that mathematical peer review should be more systematically investigated, in order to detect and suggest solutions for the reliability of mathematical proofs. This is needed in order to justify the high level of trust and testimony from authors that present new mathematical publications. A more credit-awarding approach to the checking of mathematical proofs could be promising.

The notion of pursuit is interesting for mathematics, since mathematicians as well ask questions whether a certain research directive is worthy of pursuit. Heuristics, proposed by Polya and Lakatos, already cover a certain amount of the notion of pursuit worthiness in mathematics. From a historical point of view I stressed it could nonetheless be interesting to see why mathematicians followed a certain research path while the dominating opinion among mathematicians contradicts such a path at that time. Such approaches could give a more nuanced view on mathematical developments than it is provided in the discussions on mathematical evolutions and revolutions.

I suggested that the notion of mechanical explanation can be fruitful for explanatory proofs. The use of this notion can provide understanding, since the imagination of a mechanism shows that we localize properties of decomposed parts that make a difference in the property of the mathematical figure. If we have a proof of this property, we see how each part contributes to the truth of the theorem by this imaginary mechanism and difference-makers. Such an approach to mathematical explanation asks for a pragmatic view on explanation, and I argued pluralism is desirable as well.

For each of these three questions, there is a lot of work that needs to be done. It is my opinion that a thorough study of the questions that I raised will lead to a more nuanced view of mathematical practice. Furthermore, each research question also promises interesting results for interdisciplinary research: sociology of mathematics (peer review), history of mathematics (pursuit worthiness) and pedagogy (explanation).
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