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# On the Growth and Nature of Mathematical Knowledge from a Practice-Sensitive Point of View

*By*

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# Publications & Submission

A version of Chapter 2 has appeared in *Philosophia Mathematica* in 2023 with the title "Mathematical Progress — On Maddy and Beyond." This is in the bibliography under (Weisgerber, 2023).

A version of Chapter 4 has appeared in 2022 in the peer-reviewed conference proceedings *Diagrammatic Representation and Inference. Diagrams 2022*, edited by Valeria Giardino, Sven Linker, Richard Burns, Francesco Bellucci, Jean-Michel Boucheix and Petrucio Viana as part of Springer's *Lecture Notes in Computer Science* with the title "Visual Proofs as Counterexamples to the *Standard View* of Informal Mathematical Proofs?." This is in the bibliography under (Weisgerber, 2022).

A version of Chapter 3 has been submitted to a journal. Date of submission: 27th January 2023.

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# Chapter 1

# Introduction

How does mathematics as a research discipline grow? Does each new mathematical discovery constitute mathematical progress? How do mathematicians decide which mathematical problems or questions to pursue? What, in general, justifies mathematical knowledge? These questions about the growth and nature of mathematical knowledge are the guiding questions of the present dissertation, which consists essentially of three independent research articles, each corresponding to a chapter of this thesis. In my work, I pay particular attention to mathematical practice, i.e., both to the works of (contemporary) mathematicians and to their actual research practice. In recent decades, a new trend has emerged in the philosophy of mathematics that moves away from a general idealized understanding of mathematics as a body of eternal truths and focuses on mathematics conceived as a practice with its own history and which is carried out by human beings. This trend or movement is widely known as *the philosophy of mathematical practice* to which the present work can be assigned.

After having said a bit more about this trend in Section 1.1, I briefly explain the main themes of this thesis and how my contributions fit into them in Section 1.2. Finally, in Section 1.3, I provide a more detailed overview of what to expect in the three chapters of the main body of this thesis.

## 1.1 The Philosophy of Mathematical Practice

"The Philosophy of Mathematical Practice" (PMP) is a rather new field in the philosophical research landscape.[1] As Jessica Carter explains, "PMP means a shift in focus as to what is the subject under study, not idealisations of mathematics, but mathematics in the multiple ways it presents itself to us in our various engagements with it" (2019, p. 28). Moreover, Silvia De Toffoli describes it in her role as editor of this specific category on the website `https://philpapers.org/` as a "branch of philosophy of mathematics starting with the assumption that mathematics is not only a body of eternal truths, but also a human activity with its specific dynamics of change and history" (n.d.).

An important early contribution to the philosophy of mathematical practice is Imre Lakatos's classic *Proofs and Refutations* (1976) which originates in a series of four papers from the 1960s. There, Lakatos provides a rational reconstruction of the history of Euler's formula for polyhedra which states that if $V$ denotes the number of vertices, $E$ the number of edges and $F$ the number of faces of a given polyhedron, then it is the case that $V - E + F = 2$. As Paolo Mancosu (2015, p. vii) explains in his preface to the *Cambridge Philosophy Classics edition* of *Proofs and Refutations*,

> The reconstruction, in strong contrast to a piece of axiomatic mathematics, features contradictions, monsters, counterexamples, conjectures, concept-stretchings, hidden lemmas, proofs, and a wide range of informal moves meant to account for the rationality of the process leading to concept-formation and conjectures/proofs in mathematical practice. [...] Whereas the latter [i.e., the formalist foundational approach dominant up to the 1960s; S.W.], inspired by Euclid's infallibilist dogmatic style, thought of mathematical theories statically as

[1] For some overviews of the field, see in particular (Van Bendegem, 2014), (Giardino, 2017), (Carter, 2019), and (Hamami and Morris, 2020).

> axiomatic systems, Lakatos was after *an account of informal mathematics as a fallible dynamic body of knowledge*. (emphasis added)

Aspray and Kitcher (1988) describe this work by Lakatos in their "opinionated introduction" of their edited volume *History and Philosophy of Modern Mathematics* as the origin of the "maverick" tradition which opposes the mainstream philosophy of mathematics that "appears to become a microcosm for the most general and central issues in philosophy," and which deals with tasks arising "either from the current practice of mathematics or from the history of the subject" (1988, p. 17).

De Toffoli (n.d.) mentions the volume *The Philosophy of Mathematical Practice* (2008) edited by Mancosu as a key work of PMP. It includes papers on topics such as mathematical explanation and understanding, computers in mathematical inquiry, visualization in mathematics and mathematical concepts and definitions. In his introduction to the volume, Mancosu explains that he sees himself and the other contributors to the volume as inheritors of several traditions, one of which is the "maverick." He explains that what these "maverick" philosophers—such as Kitcher (1984) and Tymoczko (1986)—called for

> was an analysis of mathematics that was more faithful to its historical development. The questions that interested them were, among others: How does mathematics grow? How are informal arguments related to formal arguments? How does the heuristics of mathematics work and is there a sharp boundary between method of discovery and method of justification? (Mancosu, 2008, p. 3)

As for the range of topics covered in PMP, Hamami and Morris (2020) differentiate in their overview article between what they take to be five "of the main trends and issues driving the philosophy of mathematical practice," namely *informal vs. formal proofs*, *visualization and artefacts*, *explanation and understanding*, *value judgments*, and *mathematical design* (2020, pp. 1116–1123).

The methods used in this new field range from case studies, over conceptual frameworks, such as Kitcher's (1984) account of the notion of mathematical practice (cf. Section 1.2), to empirical methods borrowed, for example, from social sciences, and imports from other fields, such as the import from cognitive science to Giaquinto's (2007) (cf. (Hamami and Morris, 2020, pp. 1115f.)). To support my arguments, especially those presented in Chapters 2 and 4, I provide my own case studies, use the results of empirical studies on various aspects of mathematical practice, and refer to individual statements by mathematicians.

## 1.2 On the Growth and Nature of Mathematical Knowledge

Besides Lakatos's *Proofs and Refutations*, another PMP classic that also deals with the growth of mathematical knowledge, among other things, is Philip Kitcher's *The Nature of Mathematical Knowledge* (1984). In this work, Kitcher develops an alternative account to mathematical apriorism, which he calls *mathematical empiricism*[2]. Crucial to his account is the supposition that "knowledge of an individual is grounded in the knowledge of community authorities" while "[t]he knowledge of the authorities of later communities is grounded in the knowledge of the authorities of earlier communities" (1984, p. 5). Kitcher's (1984) theory about mathematical knowledge centers, among other things, on the question of how mathematical knowledge grows. In this respect, he develops a theory of *mathematical change* in which the growth of mathematical knowledge is accounted for by *rational transitions* from one *mathematical practice* to another (cf. (1984, Chapters 7–10)). Note that for Kitcher, *mathematical practice* is a complex entity with five components: "a language, a set of accepted statements, a set of accepted reasonings, a set of questions selected as important, and a set of metamathematical

[2]Kitcher later prefers the name *naturalism* for this view (1988a, p. 321).

views" (1984, p. 163).[3]

Closely related to the issue of the growth of mathematical knowledge is the notion of *mathematical progress*. Kitcher himself has written an article dealing exclusively with this notion, namely (1988b), where he, inter alia, argues against ideas of the Fregean tradition in philosophy of mathematics and presents two stories about mathematical progress within a discussion of rationality and its relation to epistemic and non-epistemic goals.

The volume *The Growth of Mathematical Knowledge* edited by Emily Grosholz and Herbert Breger (2000), which is divided into three sections, also devotes one section consisting of six essays to "the question of progress." One of this essays is Penelope Maddy's (2000) where she presents her own account of mathematical progress. This essay is the starting point for my contribution to the topic of mathematical progress, presented in Chapter 2.

Crucial to *how* mathematics grows are mathematicians' choices about which problems to work on. An important, fairly recent contribution dealing with the practices of research mathematicians in selecting mathematical problems, based on an empirical study in the form of interviews with 13 mathematicians, is Misfeldt and Johansen's (2015). They identify three main themes around which clusters of criteria that are used by mathematicians to guide their choice of problem evolve: personal motivation and interest; suitable problems of the right difficulty following previous work; and the values of the community (2015, pp. 364ff.).

There have also been treatises written by mathematicians dealing, among other things, with the question of problem choice. A famous example is Jacques Hadamard's *An Essay on the Psychology of Invention in the Mathematical Field* (1954), in which he declares, with respect to the question of how to direct the choice of subjects of research, that "[t]he guide we must confide in is that sense

[3]This notion of mathematical practice in its context of accounting for the growth of mathematical knowledge has been criticized, for instance, by Kjeldsen and Carter (2012), who argue that there are aspects of the introduction of mathematical objects which cannot be expressed in Kitcher's account.

of scientific beauty, that special esthetic sensibility" and with regard to the fruitfulness of future results that "we *feel* that such a direction of investigation is worth following; we feel that the question *in itself* deserves interest, that its solution will be of some value for science, whether it permits further applications or not" (1954, p. 127).

Another example is G. H. Hardy's *A Mathematician's Apology* (1940/2012), where he is concerned with the question why it is worthwhile to make a serious study of mathematics. He argues that mathematics, as he understands it, must be justified as a *creative art*, "if it can be justified at all" (1940/2012, p. 139)—and with it the life of a professional mathematician in general. In particular, he argues that *beauty* and *seriousness* are the criteria by which the patterns of a mathematician should be judged (ibid., p. 98). Since the seriousness of a theorem lies in the significance of the ideas which it connects (ibid., p. 89) and significant ideas can be recognized by mathematicians when they see them (ibid., p. 103), this ability of recognition appears to be crucial in mathematical problem selection. Chapter 3 takes a closer look at Hardy's notion of seriousness mainly by discussing one example of a mathematical theorem that has been judged to be non-serious by Hardy.

The primary form of establishing or justifying mathematical truth and knowledge is *mathematical proof*. It is generally acknowledged (see, for instance, (Rav, 2007), (Avigad, 2021)) that most ordinary mathematical proofs, i.e., proofs written by mathematicians as part of their actual research practice, do not take the form of *formal proofs*, i.e., sequences of mathematical statements written in the language of a formal system, where each statement of a sequence is either an axiom of the formal system or the result of the application of a rule of inference of that system to one or more preceding statements of the sequence. There is an ongoing debate in PMP about the relationship between ordinary, informal mathematical proofs and formal proofs (cf. (Hamami and Morris, 2020)). In Chapter 4, I critically engage with an argument by Jody Azzouni against the views of Hamami (2022) and Avigad (2021) on this topic, which they both refer to as the *standard view*.

## 1.3 Outline

Each of the following three chapters of this thesis corresponds to an independent research article. Let us now go through what to expect in the three chapters in order.

In Chapter 2, a key question of the "maverick" tradition of the philosophy of mathematical practice—originating in Imre Lakatos's famous *Proofs and Refutations*—is addressed, namely what is mathematical progress. The investigation is based on Penelope Maddy's (2000) article devoted to this topic in which she considers only contributions "of some mathematical importance" as progress. With the help of a case study from contemporary mathematics, more precisely from tropical geometry, a few issues with her proposal are identified. Taking these issues into consideration, an alternative account of "mathematical importance," broadly within the framework of progress Maddy offers and for which I will introduce the technical notion of "valuable goals," is developed with a special focus on mathematicians' peer-review practice. On a very intuitive level, one could say that according to the resulting account of mathematical progress, only contributions that are "of some mathematical *interest*"—and not, as Maddy suggests, *importance*—will be considered progress. The chapter concludes by comparing the two accounts, inter alia in the context of mathematical reproofs.

Chapter 3 deals with Hardy's notion of *seriousness* which he introduces in his famous essay *A Mathematician's Apology* as one of the criteria by which a mathematician's patterns should be judged. A particular focus is on one of Hardy's examples of non-serious mathematical theorems, namely that 8712 and 9801 are the only numbers below 10000 which are integral multiples of their reversals, in the sense that $8712 = 4 \cdot 2178$, and $9801 = 9 \cdot 1089$. Hardy claims that there is nothing in this example which "appeals much to a mathematician" and that it is "not capable of any significant generalization." Interestingly, since the publication of the *Apology*, more than a dozen papers—including one by the renowned mathematician Neil Sloane—have been published that discuss generalizations of Hardy's example. It is argued that, contrary to the views of several researchers—again including Sloane—Hardy's claim regarding the non-capability of any significant generalization is still tenable. Furthermore, this case study is presented and discussed as an example of the multifaceted nature of mathematical interest.

In Chapter 4, a passage from Jody Azzouni's article "The Algorithmic-Device View of Informal Rigorous Mathematical Proof" (2020) in which he argues against Hamami and Avigad's *standard view* of informal mathematical rigor and proof with the help of a specific visual proof of $1/2 + 1/4 + 1/8 + 1/16 + \cdots = 1$, which is supposed to be a rigorous proof according to Azzouni, is critically examined. By reference to mathematicians' judgments about visual proofs in general, it is argued that Azzouni's critique of Hamami and Avigad's account is not valid. Nevertheless, by identifying a necessary condition for the visual proof to be considered a proper proof in the first place, and suggesting an appropriate way to establish its correctness, it is shown how Azzouni's assessment of the epistemic process associated with the visual proof can turn out to be essentially correct. From this, it is concluded that although visual proofs do not constitute counterexamples to the *standard view* in the sense suggested by Azzouni, at least the visual proof mentioned above shows that this view does not cover all the ways in which mathematical truth can be justified.

# Chapter 2

# Mathematical Progress – On Maddy and Beyond

## 2.1 Introduction

William Aspray and Philip Kitcher state in their "opinionated introduction" of their edited volume *History and Philosophy of Modern Mathematics* (1988), that philosophers in opposing to the mainstream philosophy of mathematics should also pose questions such as

> How does mathematical knowledge grow? What is mathematical progress? What makes some mathematical ideas (or theories) better than others? What is mathematical explanation? (1988, p. 17)

In this chapter, I return to "the origins" of PMP (cf. Section 1.1) and am mainly concerned with the second of these questions, also from a practice-sensitive point of view. That is to say, I will pay special attention to both the works of mathematicians and their actual research practice.

As I have already mentioned in Section 1.2, Kitcher himself wrote an article about the notion of mathematical progress. The current study, however, starts with a closer look at a more recent contribution concerning the notion of mathematical progress, namely Penelope Maddy's (2000). What is interesting, and prima facie intuitively appealing, about her account is that she builds it around the proposition that only contributions "of some mathematical importance" constitute progress. Her account consists of two pieces: first, a bit of mathematics counts as progress if and only if it is of some mathematical importance. Secondly, it is of some mathematical importance if it effectively furthers the goals of the practice in which it is embedded. She illustrates her ideas with two case studies from set theory. My own case study from contemporary mathematical research, more precisely from tropical geometry, will show, however, that there are some issues with Maddy's proposal that are either not addressed at all—although it seems worthwhile to do so—or not satisfactorily addressed by her. One issue that particularly stands out is the question what makes a goal a proper goal of a practice. By taking these issues into consideration, I will then develop an alternative account of what Maddy calls "mathematical importance," broadly within her framework of mathematical progress, for which I will introduce the technical notion of "valuable goals," and which will, inter alia, provide an answer to the question just raised about her proposal. My notion of "valuable goals" is based on mathematicians' evaluations of their own work and that of others regarding whether it is interesting or not, as found, for instance, in their peer-review practice. On a very intuitive level, one could say that according to the resulting account of mathematical progress, only contributions that are "of some mathematical *interest*"—and not, as Maddy suggests, "importance"—will be considered progress.

It is a rather delicate issue whether my elaboration of what should count as "mathematically important" (now understood again as a technical term) is in Maddy's sense or not. Nevertheless, especially in the last section before the summary, I will make some tentative attempts to answer this question, mainly within a discussion of the phenomenon of mathematical reproofs, for which I will introduce another example from mathematics—albeit shorter this time—namely Ivan Niven's famous reproof of the irrationality of $\pi$.

After having introduced, in Section 2.2, Maddy's main ideas about mathematical progress, the case study from tropical geometry is presented in Section 2.3. An alternative account of what Maddy calls "mathematical importance" is then developed in Section 2.4, with a special focus on mathematicians' peer-review process (2.4.1) and mathematicians' evaluations of serious or good quality mathematics (2.4.2). In Section 2.4.3, the notion of "valuable goals" is introduced and with it an explicit formulation of my resulting account of mathematical progress. Finally, Section 2.5 gives a comparison between this account and Maddy's.

## 2.2 Maddy on Mathematical Progress

In 2000, a short essay by Penelope Maddy on the nature of mathematical progress was published as part of the volume *The Growth of Mathematical Knowledge* edited by Emily Grosholz and Herbert Breger (2000). In this essay, she is interested in the question how mathematics as a discipline progresses. According to Maddy (2000), it is not enough to solve a problem one wanted to solve or to prove a theorem one wanted to prove if the problem has been solved or the theorem has been proved before and the mathematical community is aware of this. Furthermore, she does not want to count any mathematical performance such as a new calculation that has never been performed before, if carried out by familiar and mechanical techniques or a newly introduced concept, such as a "schmoup," together with some theorems concerning this new object, such as "no group is a schmoup," as progress (2000, p. 341). Only *important* bits of mathematics constitute progress:

> If my solution of a problem I want to solve or my proof of a theorem I want to prove is to count as mathematical progress, then, the problem, the theorem, the solution or the proof must be of some mathematical

importance. (ibid.)

So, she suggests to reduce the question about the nature of *mathematical progress* to the question about the nature of *mathematical importance*. She grants that the criteria for mathematical importance can vary from one epoch to another, from one area of mathematics to another, but she holds that there "remains objective content to the claim that a given bit of mathematics is or is not important in its context" (ibid.). She then goes on and gives a characterization of mathematical importance in terms of goals and practices, namely that "a given bit of mathematics is important if it effectively furthers the goals of the practice in which it is embedded" (ibid.). In line with her diagnosis that the criteria of importance can vary, she states that these goals "may vary from area to area, from time to time, but granting them, the effectiveness of a given means of advancing them is an objective matter" (ibid., p. 342).

Although the indented quote of Maddy presented above only implies that she understands mathematical importance as a necessary condition for progress, statements such as "mathematical progress is measured in terms of the goals of the practices involved and an analysis of effective means for achieving those goals" (ibid., p. 345) and "an understanding of mathematical progress as the employment of efficient means to achieve the goals of a particular practice" (ibid., p. 351) suggest that this condition can also be understood as a sufficient one. Therefore, Maddy's account of mathematical progress (hereinafter referred to as **(M)**) can be described by the following two propositions:

**(M1)** A given bit of mathematics constitutes mathematical progress if and only if it is of some mathematical importance.

**(M2)** A given bit of mathematics is important if it effectively furthers the goals of the practice in which it is embedded.

To elaborate further what kinds of goals she has in mind, Maddy presents two case studies from higher set theory. Her first example is a theorem by Dana Scott from 1961 which states that "the existence of a measurable cardinal implies the falsity of Gödel's axiom of constructibility (V=L)" (ibid., p. 342). In order to explain why this result constitutes mathematical progress, Maddy first identifies a goal of mathematical practice in general, namely the goal of being useful or helpful to science. She then goes on and—based on examples from the history of mathematics—suggests an effective way by which to accomplish this goal, namely the study and investigation of as many mathematical structures and mathematical theories "as seems mathematically fruitful" (ibid., p. 343). These general ideas about mathematical practice are followed by a discussion of set-theoretic practice in particular. One goal of set theory which she identifies is the foundational goal, i.e., the goal to provide a foundation for mathematics and thereby an ontological unity. She explains that it follows from the discussion of mathematical practice in general that set theory should provide realizations of as many isomorphism types of structures as possible. She calls this methodological advice MAXIMIZE. Maddy then states that since ZFC + V=L (i.e., Zermelo-Fraenkel set theory with the axiom of choice plus the axiom of constructibility) is from a specific point of view too restrictive, and should be rejected. Finally, she concludes that Scott's theorem constitutes progress since it "shows that adopting ZFC + MC [ZFC + 'there is a measurable cardinal'; S.W.] is an attractive way of rejecting V=L, attractive because MC is a promising axiom that extends the most fundamental methods of Cantorian set theory" (ibid., p. 345).

Her second set-theoretic example is a theorem by Martin and Steel of the mid-80s. To keep things shorter this time, let me only note that she identifies the set-theoretic goal to provide an account of sets of reals and the goal of set-theoretic axiomatics to provide consistent theories and explains why these goals are furthered by the theorem (ibid., pp. 346–351).

Regarding these two case studies, Maddy notes that not all of the goals and arguments appear explicitly in the historical record, but are rather obtained by "something of a rational reconstruction." She holds, however, that "the general lines of thought are implicit in the practice of the set theoretic community" (ibid., p. 342).

Note that this and Maddy's other contributions to the philosophy of mathematics, especially her *Naturalism in Mathematics* (1997), which was her main contribution to the philosophy of mathematics of the time her essay on progress was published, suggest that she generally uses "practice" not as a technical term (unlike Kitcher (1984), for example, who sees it as a complex entity with five components (cf. Section 1.2)) but in a rather rough-and-ready way. Furthermore, when she talks about the foundational goal as a goal *of* set-theoretic practice (and therefore presumably also in general, when she talks about "the goals of a practice"), she seems not to suggest that all set theorists intend to achieve this one goal. In (1997), she explains that besides its larger, foundational role, set theory, from its beginnings, "has also raised problems of its own, like any other branch of mathematics" (1997, p. 22). Maddy is rather making a claim about the *structure* of set-theoretic practice (cf. her "naturalized methodology" (ibid., pp. 193ff.)). She argues that recognizing the goal of set theory to provide a foundation helps to explain the rationality of set-theoretic principles such as the methodological maxims UNIFY or MAXIMIZE which are present in actual methodological decisions (ibid., pp. 208ff).

## 2.3 Mathematicians' Goals and Mathematical Practices

The purpose of this section is to analyze Maddy's account of mathematical progress with the help of a further case study. My point in considering the case study is not to determine whether or to what extent it constitutes progress according to Maddy's account. Rather, I want to point out some issues that I think are either not addressed at all—although it seems worthwhile to do so—or not satisfactorily addressed by Maddy. Taking this discussion into consideration, I will then, in Section 2.4, develop an alternative account of what should count as "mathematically important" in the context of progress broadly within Maddy's framework.

Since Maddy's account is about mathematical progress *in general*, it seems worthwhile to look at examples outside set theory as well. The area from which my case study comes is called *tropical geometry*, a fairly new field of study. In general, the wide range of contemporary mathematical research is striking. Since it will play a role later on, let me briefly say something about the current subject classification system used by mathematicians in order to locate their work. The editorial groups of *Mathematical Reviews* and *zbMATH* maintain the so-called "Mathematics Subject Classification System (MSC)." Its latest version is from 2020 (MSC2020, 2020) and lists 63 main categories (see Table 2.1). Each of these main categories is divided into several subcategories which themselves are divided again into sub-subcategories. Set theory, for instance, is a subcategory of the main category 03, "Mathematical logic and foundations" while tropical geometry is a subcategory of the main category 14, "Algebraic geometry." Tropical geometry is in turn divided into six sub-subcategories, namely: "Foundations of tropical geometry and relations with algebra," "Combinatorial aspects of tropical varieties," "Geometric aspects of tropical varieties," "Arithmetic aspects of tropical varieties," "Applications of tropical geometry," and "None of the above, but in this section." Note that this last sub-subcategory is part of the classification of each subcategory. In total, MSC2020 contains 6006 sub-subcategories.

### The Classification of Mathematics, 2020 (From the *Mathematical Reviews* and *zbMATH*)

| | | | |
|---|---|---|---|
| 00 | General and overarching topics; collections | 45 | Integral equations |
| 01 | History and biography | 46 | Functional analysis |
| 03 | Mathematical logic and foundations | 47 | Operator theory |
| 05 | Combinatorics | 49 | Calculus of variations and optimal control; optimization |
| 06 | Order, lattices, ordered algebraic structures | 51 | Geometry |
| 08 | General algebraic systems | 52 | Convex and discrete geometry |
| 11 | Number theory | 53 | Differential geometry |
| 12 | Field theory and polynomials | 54 | General topology |
| 13 | Commutative algebra | 55 | Algebraic topology |
| 14 | Algebraic geometry | 57 | Manifolds and cell complexes |
| 15 | Linear and multilinear algebra; matrix theory | 58 | Global analysis, analysis on manifolds |
| 16 | Associative rings and algebras | 60 | Probability theory and stochastic processes |
| 17 | Nonassociative rings and algebras | 62 | Statistics |
| 18 | Category theory; homological algebra | 65 | Numerical analysis |
| 19 | K-theory | 68 | Computer science |
| 20 | Group theory and generalizations | 70 | Mechanics of particles and systems |
| 22 | Topological groups, Lie groups | 74 | Mechanics of deformable solids |
| 26 | Real functions | 76 | Fluid mechanics |
| 28 | Measure and integration | 78 | Optics, electromagnetic theory |
| 30 | Functions of a complex variable | 80 | Classical thermodynamics, heat transfer |
| 31 | Potential theory | 81 | Quantum theory |
| 32 | Several complex variables and analytic spaces | 82 | Statistical mechanics, structure of matter |
| 33 | Special functions | 83 | Relativity and gravitational theory |
| 34 | Ordinary differential equations | 85 | Astronomy and astrophysics |
| 35 | Partial differential equations | 86 | Geophysics |
| 37 | Dynamical systems and ergodic theory | 90 | Operations research, mathematical programming |
| 39 | Difference and functional equations | 91 | Game theory, economics, social and behavioral sciences |
| 40 | Sequences, series, summability | 92 | Biology and other natural sciences |
| 41 | Approximations and expansions | 93 | Systems theory; control |
| 42 | Harmonic analysis on Euclidean spaces | 94 | Information and communication, circuits |
| 43 | Abstract harmonic analysis | 97 | Mathematics education |
| 44 | Integral transforms, operational calculus | | |

Table 2.1: MSC2020-Mathematics Subject Classification System

### 2.3.1 An Example from Contemporary Mathematical Research

The example from tropical geometry in which I am interested here is a paper by Andreas Gathmann and Hannah Markwig on the tropical Kontsevich formula (2008). However, before we turn to it, a few general comments about the broader context of their work will be given.

The basic question, with which enumerative geometry is concerned, looks in its most general form as follows (cf. (Katz, 2006, p. 1)): "How many geometric structures of a given type satisfy a given collection of geometric conditions?" The only requirement for the geometric conditions is that they are chosen so that one can expect a finite number of geometric structures satisfying them. An easy example for an enumerative problem is the question concerning the number of all straight lines passing through two given points in the Euclidean plane. The answer is obviously 1, at least if the two points are not identical. This incidence is excluded if one requires that they are "in general position." This answer is also true for projective lines—i.e., projective algebraic curves of degree 1—in the projective plane $\mathbb{P}^2$. The easy example leads to the problem of determining the number $N_d$ of rational curves of degree $d$ passing through $3d-1$ points in general position in the complex projective plane $\mathbb{P}^2$ (where $3d-1$ is the right number of points for which one would expect that $N_d$ is finite). In general, rational curves of degree $d$ in $\mathbb{P}^2$ constitute a special class of projective algebraic curves. Each such rational curve is defined as the zero set of a homogeneous polynomial of degree $d$ in three variables. By the end of the 19th century, only the first four numbers had been determined and, although there were great advances in intersection theory in the 20th century which helped to verify classical results and solve many old enumerative problems, the determination of the numbers $N_d$ "turned out to be very difficult" (Kock and Vainsencher, 2007, pp. 1f.). Maxim Kontsevich (1994) finally presented a recursive formula to calculate the numbers $N_d$. The paper uses techniques from theoretical physics (spring theory) and enumerative geometry.[1]

**Kontsevich's formula.** *The numbers $N_d$ for $d \geq 1$ of rational curves of degree $d$ in the complex projective plane $\mathbb{P}^2$ passing through $3d-1$ points in general position are given recursively by the initial value $N_1 = 1$ and the equation*

$$N_d = \sum_{\substack{d_1+d_2=d\\ d_1,d_2>0}} \left( d_1^2 d_2^2 \binom{3d-4}{3d_1-2} - d_1^3 d_2 \binom{3d-4}{3d_1-1} \right) N_{d_1} N_{d_2}.$$

The first six numbers, for instance, are as follows:

$$N_1 = 1,\ N_2 = 1,\ N_3 = 12,\ N_4 = 620,\ N_5 = 87304,\ N_6 = 26312976.$$

Tropical geometry[2] is a rather new area in mathematics (one of the origins of this field lies in work done within group theory starting in the late 1970s (cf. (Maclagan and Sturmfels, 2015, p. 25))), but the breakthrough which brought tropical methods to the attention of geometers only happened with a pioneering paper by Grigory Mikhalkin from 2005 (ibid., p. 31), namely (Mikhalkin, 2005). There Mikhalkin proves the first "correspondence theorem" between algebraic geometry and tropical geometry which, in general, correlates enumerative numbers of classical algebraic geometry with their tropical counterparts. Many other correspondence theorems have been proven since then. Mikhalkin's correspondence theorem says, among other things, that the numbers $N_d$ equal their tropical counterparts.

[1] See Appendix A to this chapter for some further information on Kontsevich's contribution.

[2] In case the reader wonders why this geometry is called "tropical": Maclagan and Sturmfels (2015, p. 1) explain that

> The adjective "tropical" was coined by French mathematicians, notably Jean-Eric Pin [...], to honor their Brazilian colleague Imre Simon [...], who pioneered the use of min-plus algebra in optimization theory. There is no deeper meaning to the adjective "tropical". It simply stands for the French view of Brazil.

Maclagan and Sturmfels (2015, p. ix) describe tropical geometry as a "field at the interface between algebraic geometry and combinatorics with connections to many other areas" which is "at its heart" geometry over the tropical semiring $(\mathbb{R}\cup\{\infty\}, \oplus, \odot)$ with the operations defined by $a \oplus b = \min(a, b)$ and $a \odot b = a + b$.[3] According to Hannah Markwig,

> *Tropical geometry* can be viewed as an efficient combinatorial tool to study degenerations in algebraic geometry. A degeneration referred to as *tropicalization* associates a combinatorial object to a given algebraic variety which surprisingly captures many important properties of the algebraic variety. For that reason, tropical geometry allows to incorporate methods from discrete mathematics into algebraic geometry. Vice versa, it is also possible to use methods from algebraic geometry in discrete mathematics using tropical geometry [. . . ]. (Markwig, 2020, p. 139)

In particular, *tropical curves* arise as the result of the process described by Markwig, namely *tropicalization*, when applied to algebraic curves. A *tropical line*, for instance, consists of three common half lines in the directions $(-1, -1)$, $(0, 1)$ and $(1, 0)$ which can emanate from any point in the plane. In Figure 2.1, a tropical line is shown on the left. On the right, a more complicated tropical curve is pictured. Now, the tropical counterparts to the numbers $N_d$ of Mikhalkin's correspondence theorem are the numbers $N_d^{\text{trop}}$ which are obtained by counting *rational tropical curves of degree $d$* in $\mathbb{R}^2$ passing through $3d - 1$ points in general position, where each such tropical curve is counted with a certain *multiplicity* assigned to it. The multiplicities coincide with the numbers of complex curves that are mapped to the given tropical curves.

In 2008, the paper "Kontsevich's formula and the WDVV equations in tropical

[3] A bit more about this semiring in particular and tropical geometry in general can be found in Appendix B to this chapter.

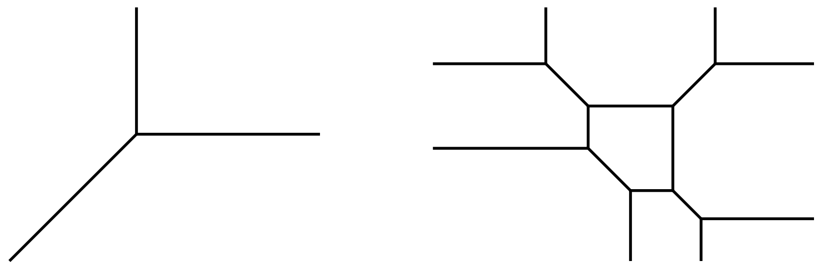

Figure 2.1: A tropical line and a biquadratic tropical curve in the tropical plane.

geometry" by Andreas Gathmann and Hannah Markwig (2008) was published in which the authors proved that the numbers $N_d^{\text{trop}}$ satisfy Kontsevich's recursive formula. This was not a new result, of course, since it is already an immediate consequence of Mikhalkin's correspondence theorem and Kontsevich's formula for complex algebraic curves. What *was* new about Gathmann and Markwig's work was that they presented a completely tropical proof of this fact with the help of tropical analogues of concepts from classical complex geometry involved in the original proof of Kontsevich's formula. In particular, they introduced tropical counterparts to the concepts of "moduli spaces of stable curves and maps, (evaluation and forgetful) morphisms, intersection multiplicities and their invariance under deformations" (Gathmann and Markwig, 2008, p. 537). They explain that their result

> shows once again quite clearly that it is possible to carry many concepts from classical complex geometry over to the tropical world [...]. Even if we only make these constructions in the specific cases needed for Kontsevich's formula we hope that our paper will be useful to find the correct definitions of these concepts in the general tropical setting. It should also be quite easy to generalize our results to other cases, e.g. to tropical curves of other degrees (corresponding to complex curves in toric surfaces) or in higher-dimensional spaces. Work in this direction is in progress. (ibid., p. 541)

In the introductory section of Hannah Markwig's dissertation in which she motivates her work which includes the paper currently under discussion and where she puts it in a broader context, she further explains that, since there are no tropical analogues of the theory of divisors and intersection theory yet "[w]e hope that our methods will help to understand tropical geometry in a more general context and maybe to define tropical divisors or intersection theory later" (Markwig, 2006, p. 4). She concludes that section with the belief that "our work is a step towards a better understanding of tropical geometry, which will in our opinion result in a better understanding of classical enumerative geometry" (ibid., p. 5).

### 2.3.2 Maddy's Proposal Revisited

If one wanted to decide whether Gathmann and Markwig's paper (interpreted as a "bit of mathematics") constitutes progress according to Maddy's account, one would first need to identify the practice in which it is embedded. As we have seen in Section 2.2, Maddy identifies three different "practices" for her two case studies, namely mathematical practice in general, and set theory and set-theoretic axiomatics in particular, which she does not, however, specify any further. As I have suggested, she appears to use them in a rather rough-and-ready way. This makes it difficult to decide which "practice" her characterization **(M2)** would refer to in our current case. A good, first candidate might be "tropical geometry" itself. However, since it is often viewed as a *tool* to study degenerations in algebraic geometry (see the quote from Markwig above) another, more apt candidate for "the" practice we are looking for might be "algebraic geometry." However, even this broad area might be too restrictive. This seems to be the case insofar as Gathmann and Markwig's paper could be understood as a preliminary step towards a coherent tropical intersection theory (which appears to be confirmed by their subsequent joint work with Michael Kerber (Gathmann et al., 2009)) and the fact that first parts of such a theory have by now been used also for purposes "outside" of algebraic geometry. In (Hampe, 2017), for instance, the author introduces the concept of an *intersection ring of matroids* by using ideas of tropical intersection theory to learn something about *matroids*. In general, matroids are structures which generalize the notion of linear independence of vector spaces from linear algebra and have numerous applications for issues of combinatorial nature. As Hampe explains, "[m]atroids have played an important role in tropical geometry ever since Sturmfels discovered that the tropicalization of a linear space only depends on the underlying matroid" with reference to a book by Bernd Sturmfels (2002) on solving systems of polynomial equations (Hampe, 2017, p. 579). So, by using geometric intuitions behind tropical geometry in order to study matroids, he is turning the tables. This example shows that a rather narrow practice, or "project"—such as the project of establishing a general tropical intersection theory—can not only be assigned to a "practice" it was originally meant for (namely "solving problems of enumerative algebraic geometry," or "algebraic geometry" more generally), but can be useful for other purposes, too. For this reason, identifying "the practice" referred to in **(M2)** in general with that to which the mathematicians who introduced the bit of mathematics would assign their work, along with whatever local and higher goals—more on that shortly—they had in mind, seems too restrictive. An effective furthering, by a given bit of mathematics, of goals (possibly of another "practice") which were not intended in the first place should have an effect on the importance of that given bit, too. With regards to this last point, it also seems valuable for an account of mathematical progress to address not only the question of whether a bit of mathematics constitutes progress at all, but also the extent to which it does so.

Interestingly, Maddy herself gives a nice example in (1997) where methods and concepts from one area of mathematics, namely set theory, were used to answer a question from another area, namely algebra. The talk is of Saharon Shelah's proof that Whitehead's problem, i.e., the question whether every abelian group $A$ satisfying a specific homological condition (namely $\mathrm{Ext}^1(A, \mathbb{Z}) = 0$) is a free group, is undecidable on the basis of ZFC (cf. (Maddy, 1997, pp. 69f.)). Paul Eklof describes this result in his review of Shelah's "significant paper" in which it appears and which "employs methods and results from set theory in order to solve some long-standing problems in abelian group theory" as "[t]he most striking" one (Eklof, 1975).[4]

Note that these last considerations should not necessarily be taken as objections to Maddy's proposal for the notion of mathematical progress. There is room within Maddy's theory to account for them. For instance, she never explicitly states in (2000) that a given bit cannot be embedded in several practices. Instead, one of the main purposes of these considerations is to show that an account of mathematical progress should be sensitive to the phenomenon that a bit of mathematics may further (unintended) goals from a "different practice," too.

Let us now take a closer look at the goals at stake in our case study. As Maddy has already pointed out in *Naturalism in Mathematics*, "some goals will take the shape of means toward higher goals" (1997, p. 194). This claim is nicely illustrated in Gathmann and Markwig's work on the tropical Kontsevich's formula. One can identify many different goals of the two mathematicians. The more local goals of their work are to introduce tropical counterparts of some concepts from classical algebraic geometry. These goals, however, might be seen

[4]The general theme of mathematicians carrying ideas or methods from one area to another in order to solve old or new problems appears in the works of several other philosophers. For instance, in her book *Representation and Productive Ambiguity in Mathematics and the Sciences*, Emily Grosholz (2007) not only discusses applications of mathematical methods to other sciences, such as representation theory to chemistry in the study of molecules, but also the application of (parts of) one mathematical area to (parts of) another, such as logic to general topology in the work of Tarski and Stone (cf. Chapter 10). Another example is Irina Starikova's (2010) discussion of Cayley graphs that offer a geometric representation of groups and which yield a "fruitful link" between group theory and graph theory. As a final example, I would like to mention (Manders, 1987) in which Kenneth Manders addresses conceptual relationships in mathematics and in particular the relationships of a mathematical area to others. He refers, for instance, to the classic example of the Cartesian algebraic approach as an application to geometry.

as means towards the higher goal of reproving the fact that the numbers $N_d^{\text{trop}}$ satisfy Kontsevich's recursive formula with purely tropical methods. Although this might be the highest goal achieved in the paper, it is not the highest goal that can be identified. As we have already seen at the end of the last section, they hope that their constructions restricted to the specific cases needed will be useful in finding the correct definitions in a more general tropical setting, that the methods will help understanding tropical geometry in a more general context and defining tropical theory of divisors or intersection theory. These were at least Gathmann and Markwig's more explicitly stated goals.

One might wonder why tropical geometers would even want, for instance, a general tropical intersection theory in the first place. "Simply" to reprove more, already known, facts from algebraic geometry with tropical methods? Of course not. They clearly want, among other things, to establish new results and solve (old) problems from enumerative algebraic geometry. It seems, however, that these results themselves should be of some mathematical importance in order to justify the effort. But what makes (enumerative) problems and thereby the goal of solving them "important" or worth pursuing in the first place? What if a problem can be solved in an easy and straightforward way? What tells us that this is not just the kind of problem Maddy intuitively does not want to count as mathematical progress? As a reminder, she wants to avoid counting any mathematical performance such as a new calculation that has never been performed before if carried out by familiar and mechanical techniques as progress (see Section 2.2). This train of thought leads us to a general question about Maddy's proposal: what makes a goal a goal *of a practice* (a practice **(M2)** is referring to)? Although Maddy does not address this general question, it seems that one can find a possible approach to it in her own work, namely (1997), which, as mentioned earlier, was her main contribution to the philosophy of mathematics at the time her essay on progress was published. However, in her latest book concerning mathematical naturalism, namely *Defending the Axioms* (2011), she introduces the notion of *mathematical depth* into the picture which appears to imply a different understanding of mathematical importance in general—and with it a different understanding of proper goals of a practice in particular. These issues will be addressed in more detail in Section 2.5.1, after I have developed in the next section an alternative account of what should count as "mathematically important," broadly within Maddy's framework of mathematical progress which will, inter alia, also provide an answer to the question just raised about her proposal.

## 2.4 The Pursuit of *Valuable* Goals

In general, one might wonder whether one should count each goal pursued by any mathematician as a "proper goal of a practice." When we look at mathematicians' research practice, however, this seems rather questionable: papers get rejected on the grounds of not being interesting. That this actually happens is confirmed by mathematicians talking about their experiences as researchers and referees. For instance, one of the mathematicians interviewed by Leone Burton for her book about the research practices of mathematicians states that

> I am quite a harsh referee. I wish I didn't reject so many. I like to think that I just don't get the good ones. I like to see a result which is non-trivial (another subjective category). (Burton, 2004, p. 147)

Or, as the mathematician Orr Shalit explains in a blog post on his web log, he understands that papers get rejected on the grounds that they are "not interesting," since he has seen some

> that were written only because they could be written. Nobody ever wrote that particular proof to that particular proposition, with this set of assumptions, so this is a "new contribution". But sometimes, a

> paper contains nothing which has appeared before, but does not really contain anything new. If there is nothing new, then it is boring, not interesting. (Shalit, 2016, June 12)

These cases show that not all goals pursued by some mathematicians are evaluated as being of "some mathematical importance" by others. In their paper "What to do to have your paper rejected," Mohammad Sal Moslehian and Rahim Zaare-Nahandi give some examples of how trivial mathematical contributions might look and which answers of the referees should be expected. For instance, if one were to base the core of an article on trivial generalizations, such as by adding or eliminating a parameter in an equation, or, if a property is proved for two elements, by stating and proving it for three elements, then one statement which should be expected from the referee might read as follows: "The authors simply extend a known inequality involving some double integrals to another inequality for triple integrals. Almost no new techniques have been presented. We would not be surprised if the authors will next try to publish a paper for multiple integrals!" (Moslehian and Zaare-Nahandi, 2016, p. 23).

I take these findings to reinforce the need to answer the question what qualifies a goal as a proper goal of a practice, at least if one wants to stay largely within the framework suggested by Maddy (as I do). In the current section I want to elaborate an alternative account of what Maddy calls "mathematically important" in the context of mathematical progress which provides an answer to the question just raised. In this regard, I will focus on the notion of "being interesting." The resulting account of progress will also be sensitive to other points that came up in our discussion of Gathmann and Markwig's work in Section 2.3.2.

Since in practice a crucial decision whether a bit of mathematics is interesting or not is often made (by some few or even only one representative) in the context of a peer-review process, we will now have a closer look at this kind of process.

### 2.4.1 Peer Review and Hardy's Three Questions

In (Geist et al., 2010), the authors write, as part of their description of the mathematical refereeing process "largely based on" books by Steven Krantz on mathematical writing and publishing and the personal experiences of one of the authors (namely Benedikt Löwe):

> In mathematics, papers are mostly published in journals; conferences and their proceedings volumes play a subordinate role. In the case of journal publishing, the journal editor typically asks a single referee to write a report on a given submission; these referees are experts and have often published on material very closely related to the material in the submission. (Geist et al., 2010, pp. 160f.)

Geist, Löwe and Van Kerkhove further reproduce "Littlewood's three precepts"—according to Krantz—which a referee should address: "(1) Is it new? (2) Is it correct? (3) Is it surprising?" (ibid.). Other mathematicians attributed three similar questions not to Littlewood but to G. H. Hardy, such as Ralph P. Boas Jr. who knew Hardy personally. He writes that Hardy "used to tell referees to ask three questions: Is it new? Is it true? Is it interesting?" (Boas, 1995, p. 10).[5] Curiously, Geist, Löwe and Van Kerkhove seem to change Littlewood's third precept themselves into "is it interesting" later in their paper (Geist et al., 2010, pp. 163f.). I will stick to Hardy's three questions here, since the term "interesting" appears to be broader and occurs more often in the context of whether a

[5]The entire paragraph reads as follows:

> At that time Hardy was an editor of the *Journal of the London Mathematical Society*. He used to tell referees to ask three questions: Is it new? Is it true? Is it interesting? The third was the most important. I would now add: Is it decently written? I think Hardy took that as a given. If he got a paper that was interesting but badly written, he would ask a graduate student to rewrite it. I know, because I did a couple of such rewrites for Hardy, for authors who subsequently became very well known. (Boas, 1995, p. 10)

I think that the second part of the quote makes it clear that this is a personal recollection and not just a second-hand report.

paper should be published. For instance, the mathematician and current editor of *Philosophia Mathematica* Robert Thomas argues that "interesting" is an important aesthetic category (more on this later) and states—mainly based on his ten years of experience as a managing editor of a mathematical research journal—that it is a "*sine qua non* of publishable mathematical research" (Thomas, 2017, p. 118). The following quote shows that he also subscribes to Hardy's three questions:

> For the whole of that time, the criteria that I asked referees to use in recommending acceptance of a manuscript were whether it was original, correct, and interesting. One does not want to publish what has already been published or what is wrong or what is new and correct but of no interest. (ibid., pp. 118f.)

Geist, Löwe and Van Kerkhove (2010) concentrate on the second question, since they are interested in the role the mathematical refereeing process is playing in ascertaining the correctness of results that are published. For our purposes, the third question is of main interest. I take it to be the case that it is necessary for a bit of mathematics to be new—or at least new to the vast majority of relevant experts in the mathematical community at the time of evaluation—and correct in order to constitute progress at all.[6] This seems to be consistent with Maddy's considerations (in this context, see also Maddy's explicit statement on reproofs, reproduced in Section 2.5.2). Before moving on, two remarks about the quality "new" should be made. First, I will stick roughly with Maddy's notion of newness here, which appears to differ somewhat from how Shalit is using it. As a reminder, he writes in his blog post considered above that "sometimes, a paper contains nothing which has appeared before, but does not really contain anything new."

[6] Note that, in my understanding, "bits" can also refer to "parts" of another bit of mathematics. That is, for example, a paper or a proof of a theorem may not be completely correct, but parts of it might be, such as some lemmas together with their proofs that appear in the paper or the general proof idea of the theorem, and thus might qualify as candidates for mathematical progress.

In line with at least some of Maddy's intuitive ideas in (2000), I take something, such as a calculation, as new if it has never been performed before. It seems that Shalit already introduces an evaluative dimension in the sense of Hardy's third question into the concept of newness.

Secondly, in mathematics, it can happen that a formerly established result is (more or less) forgotten and established anew at a later point in time. For instance,

Since "interesting" obviously comes in degrees, one might expect that there are differences between higher- and lower-ranked journals with respect to their standards concerning the degree of "being interesting" required of a mathematical work. This expectation is strengthened by the empirical findings presented in (Geist et al., 2010). The authors asked 27 editors of mathematical journals, 9 editors each of top-, mid- and lower-level journals (according to the authors' estimation) to rate, inter alia, the importance of Littlewood's precepts—although they now take the third precept to be "is it interesting"—and found that editors of higher-ranked journals gave higher scores to all categories than editors of lower-ranked journals while the biggest difference of scoring between top-ranked and lower-ranked journals was in the category "is it interesting" (Geist et al., 2010, p. 163).

As mentioned above, Thomas (2017) argues that "interesting" is an important aesthetic category. In order to explain what he has in mind when talking about aesthetics, Thomas refers to Francis Sparshott who himself mentions a work from 1735 by Alexander Gottlieb Baumgarten where Baumgarten coined the term "aesthetik" "to cover the non-logical side of philosophical considerations, matters of more and less rather than yes and no" (Thomas, 2017, p. 118). It is worth noting that Thomas distinguishes between "interesting" and "important." While he takes it to be the case that what is interesting in mathematics and how interesting it is "are very much matters of trained taste," he characterizes the property important to be more objective, “not so subject to disagreement” (ibid., p. 122). He explains that “a piece of mathematics can be of importance either for application outside mathematics or for mathematical use, but it can be interesting without having either of those non-aesthetic values” (ibid.). In general, it appears to be quite plausible to assume that some papers might be (and probably have been) accepted based on more objective reasons, on their “importance” in Thomas’s sense. Since he wants to understand “interesting” as a “*sine qua non* of publishable mathematical research,” it seems that he then should characterize important mathematics—at least in the above mentioned context—as being of some interest, too. I am not so much concerned here with a (clear) distinction between these two notions, but more with the question of what makes a piece of mathematics valuable according to mathematicians. Although my account of what Maddy calls “mathematically important” does not depend on these specific qualifications, they help specifying what is at issue here and they raise an intriguing question concerning Maddy’s ideas which will be addressed in Section 2.5.2. This is why we now look at what some mathematicians have to say about serious, important, or good quality mathematics in general.

### 2.4.2 Interesting Mathematics

In the following, we have a brief look at what three prominent mathematicians tell us about serious or good quality mathematics. Since the point here is just to get a rough idea of what they think is valuable in a mathematical publication or a work in general, there will be no critical examination of these statements.

In the paper “What is good mathematics?” (2007), Fields medalist Terence Tao presents his personal thoughts and opinions about “good quality mathematics” and gives a list of twenty-one aspects of mathematical quality (but he clarifies that this list is not meant to be exhaustive). Besides aspects concerning the educational or publicly representative side of mathematical practice, he mentions, inter alia, the following points:

(i) Good mathematical *problem solving* (e.g. a major breakthrough on an important mathematical problem);

(ii) Good mathematical *technique* (e.g. a masterful use of existing methods or the development of new tools);

[. . . ]

(iv) Good mathematical *insight* (e.g. a major conceptual simplification or the realisation of a unifying principle, heuristic, analogy, or theme);

(v) Good mathematical *discovery* (e.g. the revelation of an unexpected and intriguing new mathematical phenomenon, connection, or counterexample);

(vi) Good mathematical *application* (e.g. to important problems in physics, engineering, computer science, statistics, etc., or from one field of mathematics to another);

[. . . ]

(xiii) *Rigorous* mathematics (with all details correctly and carefully given in full);

(xiv) *Beautiful* mathematics (e.g. the amazing identities of Ramanujan, results which are easy (and pretty) to state but not to prove);

(xv) *Elegant* mathematics (e.g. Paul Erdős' concept of "proofs from *The Book*", achieving a difficult result with a minimum of effort);

(xvi) *Creative* mathematics (e.g. a radically new and original technique, viewpoint, or species of result);

(xvii) *Useful* mathematics (e.g. a lemma or method which will be used repeatedly in future work on the subject);

> [. . . ]
>
> (xix) *Deep* mathematics (e.g. a result which is manifestly non-trivial, for instance by capturing a subtle phenomenon beyond the reach of more elementary tools); [. . . ]. (Tao, 2007, pp. 623f.)

Tao makes it clear that although the different qualities appearing in his list are important for measuring the value of a mathematical work, it is also quite important whether and how the work "fits in with other pieces of good mathematics" (ibid., p. 632).

"Beautiful" and "deep" mathematics was already discussed by G. H. Hardy in his famous *Apology*. There (1940/2012, p. 89) he explains that a mathematician's patterns of ideas should be judged by their beauty and seriousness while seriousness itself is closely related to depth (ibid., p. 103). The notion of "depth," however, is an elusive one even for mathematicians capable of recognizing it (ibid., p. 112). As a first approximation, Hardy states:

> It seems that mathematical ideas are arranged somehow in strata, the ideas in each stratum being linked by a complex of relations both among themselves and with those above and below. The lower the stratum, the deeper (and in general the more difficult) the idea. (ibid., p. 110)

When concerned with the question what makes a mathematical problem a "good" problem, the Fields medalist Sir Michael Atiyah states that "the search for its solution should generate new powerful techniques which have a wide range of applicability" (1985, p. 354). One criterion of progress he explicitly mentions is "innovation." He explains that the most common form that it takes is "the invention of new techniques for the solution of problems" (ibid., p. 355). In general, "all mathematicians, whatever their speciality, are essentially craftsmen and appreciate the technical skills involved in the solution of long-standing problems" (ibid.). In addition to these qualities, Atiyah also stresses the importance of the aesthetic component of the mathematical enterprise. He says:

> Most mathematicians, but particularly those of the 'pure' variety, far removed from applications, have a clear idea of what is meant by a 'beautiful argument'. This is a term of great praise indicating elegance of style, economy of effort, clarity of thought, perfection of detail and balance of form adding up to give a feeling of overwhelming conviction. Naturally, these heights are rarely reached in full, and *they represent goals to be striven for*, but they exercise a very strong influence. (ibid., p. 356, emphasis added)

In order to justify the aesthetic component of mathematical research, Atiyah explains that since "[a]lmost any branch of knowledge has aspects which can be analysed mathematically," the essential feature of mathematics is the development of an "abstract language, flexible and powerful enough to serve a multitude of possible purposes" (ibid., p. 357). In this regard, simplicity and elegance are of primary importance. He concludes that "the development of simple and concise arguments is absolutely indispensable for progress in mathematics" (ibid.).

Without doubt more can be (and partly has been) said about all of these aspects of serious or good quality mathematics mentioned above, also from a philosophical point of view (as I do myself in the following chapters, especially with regard to Hardy's notion of *seriousness* and the notion of *mathematical rigor*). For instance, with respect to the notion of mathematical depth this has been done in a special issue of *Philosophia Mathematica* (Vol. 23, No. 2) dedicated exclusively to this notion. Although I will not conduct a deeper investigation of these aspects in this chapter, I will make some remarks about the more obvious aesthetic values mentioned above, namely beauty and elegance, with special attention to Maddy's notion of mathematical importance in Section 2.5.2. Let us now move on to the most important part of this section.

### 2.4.3 Valuable Goals and Mathematical Progress

In the initial paragraphs of the current section I mentioned that mathematical papers get rejected on the grounds of not being interesting. The quotes I have provided to support this claim might give the impression that "being of no interest" could be equated with "rather trivial" in the context of peer review. However, this is not the case. For instance, in the article (2015), for which Misfeldt and Johansen interviewed 13 pure mathematicians in order to investigate mathematicians' problem choice, the authors report that

> Several of our interviewees had experienced choosing research agendas that did not catch the interest of other mathematicians. One of them [...] had spent a lot of time on something only to discover that nobody found it interesting, "and that was not much fun." Another [...] had experienced difficulties having a result published, and consequently, he had put a line of research on hold, even though he and his collaborator found it very interesting. (Misfeldt and Johansen, 2015, p. 367)

I take this to indicate that there are situations where individual mathematicians pursue non-trivial mathematical goals that are nevertheless classified as not interesting (enough) by their peers.

In general, I think it is perfectly reasonable not to include trivial or non-trivial but uninteresting goals (characterized in this way by most mathematicians working in the relevant research field) among the "*proper* goals of a practice." A rationale behind the exclusion of the second type of goals is the following: Mathematics is hard. Researchers need to invest much time in order to understand a paper. However, if a paper is of no interest to other researchers in the field, it will not be read and it might as well have not been written (at least for the time being). With respect to mathematicians' research practice, something similar is expressed by one interviewee's supervisor of Misfeldt and Johansen's study:

> [My supervisor] told me several times that it's very important to think about what other people are interested in. Because that's the only way anyone is ever going to know about the results you get. Because even if you do something and you get some results, it's hard to understand them, nobody is going to invest time in them. They are not interested. And so you might as well not have done it. (Misfeldt and Johansen, 2015, p. 367)

Note that although "interesting" has been used so far to characterize mathematical papers or contributions in general, it might nevertheless be applied to goals, since the ideas and results of a contribution or paper can be reformulated in terms of them. Now, a goal pursued by a mathematician shall be called "valuable," if this goal would be evaluated as interesting in the sense of Hardy's third question by the majority of practising mathematicians who pursue (have pursued) similar or related goals, or work (have worked) on similar or (closely) related problems. In modern times, the "Mathematics Subject Classification System" (MSC) serves as a good orientation for the identification of mathematicians who pursue or have pursued similar or related goals. For instance, the primary classifications of Gathmann and Markwig's paper we discussed in Section 2.3 are 14N35 and 51M20 that refer to "Gromov-Witten invariants, quantum cohomology, Gopakumar-Vafa invariants, Donaldson-Thomas invariants (algebro-geometric aspects)" and "Polyhedra and polytopes; regular figures, division of spaces," respectively; its secondary classification is 14N10 which reads as "Enumerative problems (combinatorial problems) in algebraic geometry." As a reminder of what has been said earlier, at the time of publication, "tropical geometry" was not yet listed as a subject in the then up-to-date version of the MSC whereas now it is.[7]

For instance, a *potential* valuable goal of Gathmann and Markwig's work which is clearly effectively furthered is the goal of reestablishing Kontsevich's

[7] In fact, it already appeared in the previous, i.e., the 2010 version of the MSC.

formula for tropical curves entirely in the language of tropical geometry. Note that mathematicians might value this goal or find it interesting for different reasons. Some mathematicians might value it more for aesthetic reasons (perhaps because of its *methodological purity* in contrast to a proof using Mikhalkin's "correspondence theorem" (see Section 2.3.1));[8] others more for pragmatic ones (perhaps because its effective furthering might help, for instance, to define tropical intersection theory later) and still others for a combination of both types of reasons.

Based on the idea of valuable goals, we can finally characterize mathematical progress as follows:

**(P*I*)** A given bit of mathematics constitutes *mathematical progress* if and only if it is original, accessible and effectively furthers at least one valuable mathematical goal.

"Correctness" should be understood as being part of the notion of "effectively furthering" (see also Footnote 6). With this and Geist, Löwe and Van Kerkhove's characterization of referees of the mathematical peer-review process from above as experts who "have often published on material *very closely related* to the material in the submission" (emphasis added), this account of progress might be regarded as an abstraction of the peer-review process based on Hardy's three questions.

In view of my definition, what makes a goal a "valuable" goal is based on a positive assessment by the *majority* of a specific mathematical community. In practice it could happen that although a given bit of mathematics effectively furthers a valuable goal, it might nevertheless get rejected by a referee of a journal who does not qualify the goal as being interesting, and as a consequence the bit might not be accepted for publication (by that particular journal). Since I

[8] Dawson (2006, p. 279) calls the concern for methodological purity "the most important aesthetic reason for devising new proofs." Proofs are "pure," according to his understanding, if they "do not invoke notions extraneous to the concepts involved in the statement to be proved" (ibid.). In light of this characterization, a proof that the numbers $N_d^{\text{trop}}$ satisfy Kontsevich's formula using the correspondence theorem which relates these numbers to their complex counterparts would then clearly qualify as impure.

am concerned here with how mathematics progresses *as a discipline*—following Maddy—the term "accessible" has been added to **(P$I$)**: at least the relevant mathematical community should be able to access this bit of mathematics.

Note that there are several factors that make the notion of valuable goals in particular, and because of **(P$I$)**, the notion of mathematical progress in general, time-dependent (which is in accordance with Maddy's general diagnosis of her account of progress). An obvious factor is its characterization in terms of the evaluation of *practising* mathematicians. Furthermore, the fact that mathematical goals (especially if appreciated for pragmatic reasons) may take the shape of means towards higher goals—as I have illustrated in my example above—also has as a consequence that the notion of valuable goals is time-dependent. Another factor appears to be the commitment to a majority vote of a specific community. For instance, many mathematicians might not recognize the "value" of one of the achieved goals of a paper at the time of publication, for example, because of a lack of information, which, however, will be remedied in the course of time.

The expression "at least one valuable mathematical goal" occurs in **(P$I$)** to account for two different things that came up in our discussion of Gathmann and Markwig's work in Section 2.3.2: first, it allows for the possibility that the effective furthering of goals of a practice by a given bit of mathematics to which it was not originally assigned may also have an impact on its general progress status. And secondly, it leaves room not only for the question of whether a bit of mathematics constitutes progress at all, but also *to what degree* this is the case.

With regard to the determination of the degree of mathematical progress one can specify several different factors. First of all, as I have just mentioned and as we have seen in Section 2.3.2 when discussing Hampe's paper, it might turn out that a goal is retrospectively furthered by the work of mathematicians (possibly from another "practice") which these mathematicians did not originally have in mind. There should be room for this circumstance to have a positive effect on the degree of progress constituted by the work at the time it is evaluated. Furthermore, an important factor should be, of course, the level of effectiveness to which a given bit of mathematics furthers particular valuable goals. Finally, the importance of each valuable goal itself, according to the relevant community (as specified in my definition of these goals), should have an impact on the degree of progress. To avoid confusion with Maddy's technical understanding of mathematical importance, the level of importance of a goal will be called its *weight*. These considerations could then be summed up as follows:

**(P*II*)** The *degree of progress* of a given original (and accessible) bit of mathematics is constituted by the weights of *all* valuable mathematical goals that are furthered and the level of effectiveness to which they are furthered by the given bit.

As it stands now, one would need to set the weight of a valuable goal to zero if its "value" exclusively arises from its relation to some higher goals to avoid multiple counting. In general, however, proposition **(P*II*)** is not meant to suggest that there could actually be a functioning evaluation system with which we could assign an actual number to a bit of mathematics expressing the degree of progress it constitutes. It should rather be understood as a simple suggestion of which factors should have a *positive effect* on the level of progress a given bit of mathematics is constituting.

Note that the way Maddy generally talks about importance and progress—"of some mathematical importance" (Maddy, 2000, p. 341) and "important mathematical progress" (ibid., p. 351)—seems to suggest that she also endorses the idea that progress comes in degrees.

## 2.5 Maddy and the Idea of Valuable Goals

In this final section, in addition to the few comments I have already made in Section 2.4, I would like to compare further the ideas I developed there with those of Maddy. In this regard, I will first compare my concept of "valuable goals" with some general remarks made by Maddy in her other works on the philosophy of mathematics, and then turn to the more specific phenomenon of mathematical reproofs and examine how it relates to Maddy's and my understandings of mathematical progress.

### 2.5.1 Maddy and the Assessment of Mathematical Goals

As I mentioned at the end of Section 2.3.2, it seems that there are two (apparently) different approaches to answering the pressing question what makes a goal a proper goal of a practice which could be extracted from two of Maddy's main contributions to mathematical naturalism themselves, namely from *Naturalism in Mathematics* (1997) and *Defending the Axioms* (2011), respectively.

In *Naturalism in Mathematics*, Maddy explains with reference to the changes in mathematics during the course of the nineteenth century (e.g. the advent of non-Euclidean geometry, abstract algebras, and $n$-dimensional geometry):

> As a result of this development, contemporary pure mathematics is pursued on the assumption that mathematicians should be free to investigate any and all objects, structures, and theories *that capture their mathematical interest.* This is not to say, with the Glib Formalist, that all theories are of equal interest—mathematicians, for all their freedom, do concentrate on particular lines of development—but it is to say that some of that concentration is determined by purely mathematical, as opposed to scientific, goals. (Maddy, 1997, p. 210, emphasis added)

Similarly, in her book *Second Philosophy*, where Maddy develops her naturalistic method within a broader context, she states, after referring to the same development, that "mathematicians broke away from the necessity of application, asserting their freedom to pursue their own goals, to investigate whichever mathematical structures *they found interesting and fruitful*" (2007, p. 345, emphasis added).

One might take these two passages from Maddy's own work to suggest a possible way to approach the pressing question above, namely by taking mathematicians' evaluations of their mathematical goals themselves into account. This general attitude towards the evaluations of mathematicians themselves appears to be very similar to the one I express with the help of the notion of valuable goals.

However, in her book *Defending the Axioms* (2011), Maddy has changed her view about the relation between the concept of mathematical importance and goals of mathematical practices, i.e., about **(M2)**, by introducing the notion of "mathematical depth" into her naturalistic philosophy of mathematics. This notion is meant to depict a phenomenon for which "[a] generous variety of expressions is typically used to pick out": "mathematical depth, mathematical fruitfulness, mathematical effectiveness, mathematical importance, mathematical productivity, and so on" (Maddy, 2011, p. 81). She further explains in the afterword of the special issue of this journal on the notion of mathematical depth (already mentioned above) of which she was a guest editor, that all of these notions including importance are taken in a "roughly consequentialist spirit" (Ernst et al., 2015, p. 245). Moreover, she takes it to be the case that judgments about mathematical depth are not subjective, but entirely objective (Maddy, 2011, p. 81). She states that "our mathematical goals are only proper insofar as satisfying them furthers our grasp of the underlying strains of mathematical fruitfulness [i.e., mathematical depth; S.W.]" (ibid., p. 82), or, as she puts it in the afterword, "[w]e value those bits of mathematics that allow us to meet our mathematical goals, but in the end, the legitimacy of those goals depends on their actually uncovering depth" (Ernst et al., 2015, pp. 246f.).

So, according to this picture, it is not the furthering of goals of a practice that makes a bit of mathematics important—mathematicians can be wrong in their assessment of the value of the goals they are pursuing—but it is an objective fact whether it is important/deep/fruitful at all. Thus, the task of assessing mathematical goals is based on an objective ground that is clearly not part of my conception of a valuable goal. However, the view that mathematical depth is objective, if understood in this consequentialist spirit, is not uncontroversial (see, for instance, (Arana, 2015) for some critique). Even Maddy admits in the afterword of the special issue that in (Maddy, 2011) the objectivity of mathematical depth is perhaps asserted with "more bluster than argument" (Ernst et al., 2015, p. 247).

### 2.5.2 Reproofs

Now, let us turn to a rather delicate issue concerning Maddy's proposal of mathematical importance and her general stance on reproofs. We start with a closer look at what she explicitly says about reproofs:

> Does mathematics as a discipline progress when someone solves a problem she wanted to solve or proves a theorem she wanted to prove? Well, not if the problem had already been solved or the theorem had already been proved by someone else, at least as long as that previous solution or proof is well known to the mathematical community. (Maddy, 2000, p. 341)

If this is meant to rule out the case that any reproof of a theorem whatsoever—at least if the original proof is well known to the community—could be considered progress, then I think this view is too extreme and unconvincingly restrictive. Reproofs not only show that a (probably well-known) theorem holds, but may fulfill other purposes, too. Gathmann and Markwig's reproof of the fact that the numbers $N_d^{\text{trop}}$ satisfy Kontsevich's recursive formula discussed in Section

2.3.1 showed that many concepts from classical algebraic geometry in general and intersection theory in particular can be carried over to the "tropical world" and demonstrated the (potential) power of tropical methods for concerns of enumerative nature in algebraic geometry.[9] Furthermore, I think that exactly because creating new proofs to old theorems is an integral part of mathematical practice (see also (Dawson, 2006)), tokens of this phenomenon might satisfy Maddy's own requirement **(M2)** to ultimately qualify as mathematical progress. So perhaps the above quote should be interpreted as Maddy simply wanting to exclude non-original work—at least when the original is well known to the community—from being considered progress, so that new proofs of old theorems are not affected by this in principle.

But that is not all. As the statements by Tao, Hardy and Atiyah about serious or good-quality mathematics presented in Section 2.4.2 suggest, many mathematicians value beautiful and elegant mathematics. Sometimes this comes together in a simpler proof of an already established mathematical fact, such as in Ivan Niven's famous one-page paper "A simple proof that $\pi$ is irrational" (Niven, 1947). The irrationality of $\pi$ was first proved by Johann Heinrich Lambert in 1761, but as Miklós Laczkovich (1997, p. 439) explains—from today's perspective—Lambert's proof is seldom reproduced in books on number theory, since it presupposes knowledge about the convergence of continued fractions "and if our aim is just to prove the irrationality of $\pi$ then this digression is not worth the effort." Laczkovich explains the general situation as follows: "The 'usual' proofs avoid continued fractions and use variants of Hermite's idea: if $\pi$ were rational then certain sums or integrals would be integers, contradicting estimates showing that the actual values lie in $(0, 1)$" (ibid.). Charles Hermite's proof which gets along with basic calculus was published in 1873. Niven's proof falls under

[9] Note that this last-mentioned merit of Gathmann and Markwig's work falls into the category of Dawson's third reason why mathematicians give alternative proofs, namely "*to demonstrate the power of different methodologies*" (Dawson, 2006, p. 277). In addition to this and the one already mentioned in Footnote 8, he identifies six further reasons in his article.

the category of "usual proofs," but is additionally characterized in the review by Jan Popken (1948) as probably the simplest one. The proof is also part of Aigner and Ziegler's "Proofs from THE BOOK" (2018) which is meant to be a "first approximation" to Paul Erdős's concept of The Book "in which God maintains the perfect proofs for mathematical theorems" (2018, p. v)—also mentioned by Tao in his list as an example of elegant mathematics quoted in Section 2.4.2—and is described as an "extremely elegant one-page proof" (ibid., p. 47). In an interview with Donald Albers and Gerald Alexanderson, Niven explained why he worked on the problem of finding a simpler proof for the irrationality of $\pi$:

> I had worked on this problem for a specific reason: in the first edition (1938) of what is now a great classic, *Introduction to the Theory of Numbers*, by G. H. Hardy and E. M. Wright, the authors made the observation that "There is no simple proof of the irrationality of $\pi$." I wondered why this should be so. Following the publication of my proof, Hardy and Wright replaced that observation with a simple proof in subsequent editions of their book. (Albers et al., 1991, p. 385)

Would Maddy from the 2000 paper consider Niven's proof mathematical progress (at the time of its publication)? On the one hand, similar to Gathmann and Markwig's reproof, this is original and, if we accept "reproving an already known fact in a simpler, more elegant way" as a goal of number-theoretic practice (at that time), we could argue within Maddy's proposal **(M)** that Niven's proof constituted progress.

However, is this consistent with Maddy's general intuitions about the notion of mathematical progress? I claim that the answer depends on how one understands the ideas presented in *Defending the Axioms*. Let me explain. As we have seen in Section 2.5.1, Maddy considers in her later work only those goals as proper mathematical goals which actually track mathematical depth whereby this latter term is taken to be an umbrella term of a phenomenon for which a "generous variety of expressions is typically used to pick out" such as mathematical fruitfulness/effectiveness/importance/depth/productivity (2011, p. 81). Furthermore, she takes all these different notions and with it the phenomenon in general in a consequentialist spirit. This suggests that from the point of view of her later work, she would not consider the goal of reproving an already known fact in a simpler, more elegant way regardless of whether it is mathematically fruitful or not as a proper goal of a practice. Coming back to our example, although Niven himself used the idea of his proof to establish other mathematical facts such as that for any rational number $r \neq 0$, $\cos(r)$ must be irrational (1956/2006, pp. 17ff.) and other mathematicians presented generalizations of his proof to prove further theorems, like Alan Parks (1986), it appears that (the idea of) Niven's proof has not been used to establish previously unknown facts. If this is correct, then Maddy from 2011 would most likely not consider Niven's proof to be mathematical progress. Thus, deciding whether Maddy of 2000 would regard Niven's proof as progress (although it fits into her general framework—as mentioned above) depends on how one understands her general attitude towards mathematical importance and its connection to fruitfulness from *Defending the Axioms*, i.e., either as broadly consistent with her earlier view or as a change of mind.

Although a definite answer to this will not (and perhaps cannot) be given here, let me conclude this section with some remarks about how my account of progress deals with proofs such as Niven's and why I think this is a welcome result.

The account **(P*I*)** does not disqualify reproofs from being considered progress on the basis that they do not and probably will never lead to new theorems. A proof and thereby the goal of creating it can also be valued for aesthetic or pedagogical and other sociological reasons. Niven's proof can be valued for all these kinds of reasons: many mathematicians describe it as a clever and elegant proof; his proof can be presented to students who are only familiar with fairly elementary mathematics;[10] and as we have seen in the quote above, Niven valued his proof because a simple argument for the irrationality of $\pi$ was not known even by great mathematicians like Hardy and Wright.

Mathematics is a social practice, a human activity, and "not only a body of eternal truths" (as Silvia De Toffoli (n.d.) puts it). This is why I consider it a plus of my account of progress that I leave room for bits of mathematics to constitute mathematical progress based on pedagogical or other sociological reasons. Furthermore, since mathematics is viewed by many mathematicians as a creative *art* (as, e.g., by Hardy, famously expressed in his *Apology*), I also take it as a merit of my account to allow for the possibility that bits of mathematics constitute mathematical progress (solely) on the basis of their aesthetic appeal.

## 2.6 Summary

I started the investigation into the nature of mathematical progress with a closer look at Maddy's work on this topic. As we have seen, she builds her theory around the notion of mathematical importance and characterizes a bit of mathematics as important if it effectively furthers the goals of the practice in which it is embedded. My case study from tropical geometry showed that a bit of mathematics can also further goals of a practice to which it was not originally assigned. Furthermore, it helped to identify an important question about Maddy's proposal which she does not (at least explicitly) address, namely what makes a goal a proper goal of a practice. With these issues in mind, I then developed an alternative account of "mathematical importance," broadly within the framework of progress Maddy

[10] Parks explicitly mentions this as a value of his generalization of Niven's proof:

> The novelty of our argument is not so much the conclusion, but that our proof is elementary and can be effectively presented to students of calculus [. . . ]. Then again, at the beginning level, the fact that certain real numbers which occur naturally are irrational is interesting enough to present for its own sake. (1986, p. 722)

offers, for which I introduced the notion of "valuable goals." The "value" of a goal was roughly characterized by a positive evaluation regarding its being interesting to the majority of mathematicians pursuing similar goals or working on closely related problems. My resulting account of mathematical progress then characterized a given bit of mathematics as progress if and only if it is original, accessible and effectively furthers at least one valuable mathematical goal.

We have also seen that the attitude towards the evaluations of mathematicians themselves expressed by Maddy in some general remarks of her *Naturalism in Mathematics* and *Second Philosophy* appears to be similar to the one I expressed with the help of the notion of valuable goals. However, the introduction of the notion of mathematical depth into her naturalistic philosophy of mathematics in *Defending the Axioms* seems to change this.

Finally, with respect to the phenomenon of reproofs, I have argued that it is a welcome result of my account of progress—in contrast to Maddy's (at least) later ideas—that it allows for the possibility that these proofs might be considered progress even on non-consequentialist grounds.

# Appendix.

## A Kontsevich's Formula

As a reminder, Maxim Kontsevich (1994) has shown that the numbers $N_d$ can be determined using the following recursive formula:

**Kontsevich's formula.** *The numbers $N_d$ for $d \geq 1$ of rational curves of degree $d$ in the complex projective plane $\mathbb{P}^2$ passing through $3d-1$ points in general position are given recursively by the initial value $N_1 = 1$ and the equation*

$$N_d = \sum_{\substack{d_1+d_2=d \\ d_1,d_2>0}} \left( d_1^2 d_2^2 \binom{3d-4}{3d_1-2} - d_1^3 d_2 \binom{3d-4}{3d_1-1} \right) N_{d_1} N_{d_2}.$$

In order to prove this formula, Kontsevich introduced the notion of a *stable map* which is a triple $(C, \{x_1, \ldots, x_n\}, f)$ consisting of an algebraic curve of a specific type, $n$ distinct marked points on this curve and a map $f : C \to X$ from $C$ to some ambient space $X$. The numbers $N_d$ then arise as intersection numbers on the *moduli space* or *parameter space* of stable maps to $\mathbb{P}^2$. So, in this respect his work can be characterized as intersection theory on the moduli space of stable maps. Parameter spaces play an important role for solving enumerative problems, since, as Eisenbud and Harris (2016, p. 289) express it, "[a]ll the applications of intersection theory to enumerative geometry exploit the fact that interesting classes of algebraic varieties—lines, hypersurfaces and so on—are themselves parametrized by the points of an algebraic variety, the *parameter space*, and our efforts have all been toward counting intersections on these spaces." As an easy example, lines in $\mathbb{P}^2$ can be parametrized as points of $\mathbb{P}^2$ and this fact can be used to calculate $N_1$ (cf. (Katz, 2006, pp. 17f.)): A *line* in $\mathbb{P}^2$ which is an algebraic curve of degree 1 can be described by the zero set of a polynomial $f = a_0x_0 + a_1x_1 + a_2x_2$. Since a second polynomial $g = b_0x_0 + b_1x_1 + b_2x_2$ describes the same line if and only if the two polynomials are multiples of each other, any line can be represented or parametrized by a triple $(a_0, a_1, a_2) \in \mathbb{C}^3 - \{(0, 0, 0)\}$ modulo the equivalence relation of scalar multiplication. The resulting space, however, is just another copy of the projective space $\mathbb{P}^2$, the parameter space of projective plane lines and is denoted here by $\mathbb{P}^2_{(a_0,a_1,a_2)}$ to distinguish it from the original $\mathbb{P}^2$. The enumerative question about the number $N_1$ of projective lines which contain each of two distinct points $p_1$, $p_2$ can now easily be answered by intersection theory: Let $C_i \subset \mathbb{P}^2_{(a_0,a_1,a_2)}$ denote the set of all lines passing through $p_i = (y_{i_1}, y_{i_2}, y_{i_3})$, i.e.,

$$\begin{aligned} C_i &= \{\text{lines } l \subset \mathbb{P}^2 \mid p_i \in l\} \\ &= \{(b_0, b_1, b_2) \in \mathbb{P}^2_{(a_0,a_1,a_2)} \mid b_0 y_{i_0} + b_1 y_{i_1} + b_2 y_{i_2} = 0\}. \end{aligned}$$

Written in this manner, it becomes clear that $C_1$ and $C_2$ define lines themselves in the parameter space $\mathbb{P}^2_{(a_0,a_1,a_2)}$. Therefore, the number $N_1 = \#(C_1 \cap C_2)$ equals the intersection points of two lines in the projective space which is, as stated in Section 2.3.1 above, 1.

## B Tropical (Algebraic) Geometry

Tropical geometry[11] can be understood as algebraic geometry over the tropical semiring $(\mathbb{R} \cup \{\infty\}, \oplus, \odot)$ with the operations defined by

$$a \oplus b = \min(a, b), \ a \odot b = a + b.$$

It is clear that infinity is the identity element for tropical addition and zero is the identity element for tropical multiplication, since

$$x \oplus \infty = x \ \text{ and } \ x \odot 0 = x$$

[11]This short presentation of some ideas of tropical geometry is based on (Maclagan and Sturmfels, 2015, Chapter 1).

for any $x \in \mathbb{R} \cup \{\infty\}$. Working over the tropical semiring transforms questions about algebraic varieties into questions about polyhedral complexes.

Let $x_1, x_2, \ldots, x_n$ be $n$ variables representing elements in the tropical semiring $(\mathbb{R} \cup \{\infty\}, \oplus, \odot)$. Any product of these variables is called a *monomial* and we write

$$x_3 \odot x_1 \odot x_1 \odot x_2 \odot x_3 \odot x_2 \odot x_4 \odot x_3 = x_1^2 x_2^2 x_3^3 x_4.$$

A *tropical polynomial* is then defined as a finite linear combination of these monomials:

$$p(x_1, \ldots, x_n) = a \odot x_1^{i_1} x_2^{i_2} \cdots x_n^{i_n} \; \oplus \; b \odot x_1^{j_1} x_2^{j_2} \cdots x_n^{j_n} \; \oplus \cdots$$

with real numbers $a, b, \ldots$ and integral numbers $i_1, j_1, \ldots$. In classical arithmetic this polynomial describes the minimum of a finite collection of linear functions, namely

$$p(x_1, \ldots, x_n) = \min(a + i_1 x_1 + \cdots + i_n x_n, b + j_1 x_1 + \cdots + j_n x_n, \ldots).$$

This represents a continuous, piecewise linear (with a finite number of pieces) and concave function $p : \mathbb{R}^n \to \mathbb{R}$. As an example, let us look at the polynomial $f(x) = 1 \oplus x \oplus 1x^2$ which describes the function

$$f : \mathbb{R} \to \mathbb{R}, \; x \mapsto \min(1, x, 1 + 2x).$$

In Figure 2.2, we can see the graph of this polynomial, interpreted as a function from $\mathbb{R}$ to $\mathbb{R}$ (on the right hand side), and how we got there (on the left hand side). The lines $f_1, f_2$ and $f_3$ represent the graphs of $f_1(x) = 1$, $f_2(x) = x$ and $f_3(x) = 1 + 2x$ respectively.

The *hypersurface* $V(p)$ of a tropical polynomial function $p : \mathbb{R}^n \to \mathbb{R}$ is defined to be the set of all points $\mathbf{w} \in \mathbb{R}^n$ at which the minimum given by $p$ is attained

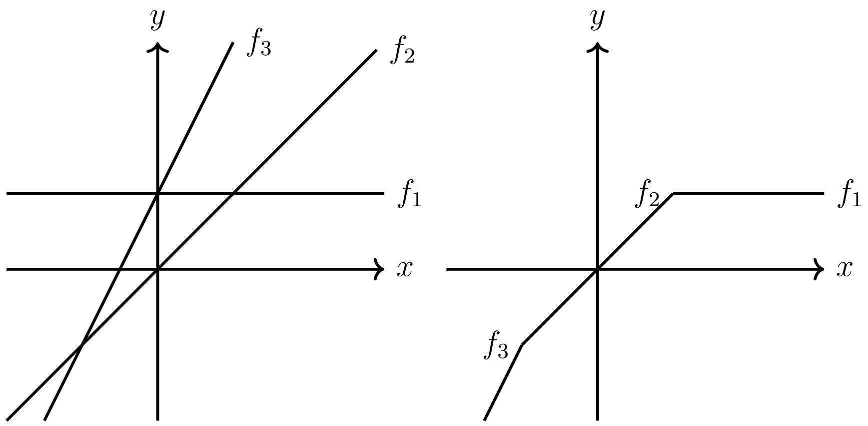


Figure 2.2: Graph of $f(x) = 1 \oplus x \oplus 1x^2$.

at least twice. The elements of $V(p)$ are called *roots* of the polynomial $p$. This concept might remind the reader familiar with algebraic geometry of algebraic varieties which are defined as the sets of solutions of a system of polynomial equations.

The tropical hypersurface $V(p)$ of a polynomial in two variables, i.e.,

$$p(x, y) = \bigoplus_{(i,j)} c_{ij} \odot x^i \odot y^j,$$

is called a *plane tropical curve*. Figure 2.1 from Section 2.3.1 shows an example of a tropical curve corresponding to a polynomial in $x$ and $y$ of degree 1 and one of degree 4.

# Chapter 3

# Value Judgments in Mathematics: G. H. Hardy and the *(Non-) Seriousness* of Mathematical Theorems

## 3.1 Introduction

In his famous essay *A Mathematician's Apology* from 1940, the English mathematician Godfrey Harold Hardy (1877–1947) provides a justification for a serious study of mathematics and, with it, for the life of a professional mathematician. Hardy argues that mathematics must be justified as a *creative art*. As he famously puts it: "Beauty is the first test: there is no permanent place in the world for ugly mathematics" (1940/2012, p. 85). The general criteria Hardy identifies and discusses in his essay by which a mathematician's patterns must be judged are *beauty* and *seriousness*.

References to Hardy's thoughts presented in his essay readily appear in the works of philosophers and researchers in mathematics education when investigating, for instance, mathematical problem choice (Ashton, 2018), whether mathematics can be treated as an art (Tymoczko, 1993), values in mathematics (Ernest, 1998), and aesthetic considerations in mathematical inquiry more generally (Sinclair, 2004, 2006, 2011) or mathematical beauty ((Dutilh Novaes, 2019), (Sa et al., 2023)) and depth (Urquhart, 2015) in particular.

In this chapter, I focus on Hardy's notion of seriousness and in particular on a specific example proposed by him (example $(a)$ below) as an example of a *non-serious* mathematical theorem. Roughly speaking, a serious theorem is a theorem which connects *significant* mathematical ideas, while a significant idea is an idea which "can be connected, in a natural and illuminating way, with a large complex of other mathematical ideas" (Hardy, 1940/2012, p. 89).

One quality that Hardy says is essential to the significance of a mathematical idea or the seriousness of a theorem is *generality*. While discussing this quality, he presents the following two examples that (allegedly) lack this quality "conspicuously" (in ways we will discuss in more detail later) and therefore cannot be serious (ibid., pp. 104f.):

> $(a)$ 8712 and 9801 are the only four-figure numbers which are integral multiples of their 'reversals':
>
> $$8712 = 4 \cdot 2178, \; 9801 = 9 \cdot 1089,$$
>
> and there are no other numbers below 10,000 which have this property.
>
> $(b)$ There are just four numbers (after 1) which are the sums of the cubes of their digits, viz.
>
> $$153 = 1^3 + 5^3 + 3^3, \; 370 = 3^3 + 7^3 + 0^3,$$
> $$371 = 3^3 + 7^3 + 1^3, \; 407 = 4^3 + 0^3 + 7^3.$$

After having introduced these examples, Hardy states that (hereinafter referred to as ($\mathcal{D}$), where "$\mathcal{D}$" is meant to remind of "diagnosis")

> ($\mathcal{D}$) These are odd facts, very suitable for puzzle columns and likely to amuse amateurs, but there is nothing in them which appeals much to a mathematician. The proofs are neither difficult nor interesting—merely a little tiresome. The theorems are not serious; and it is plain that one reason (though perhaps not the most important) is the extreme speciality of both the enunciations and the proofs, which are not capable of any significant generalization. (ibid., p. 105)

Interestingly, since the publication of Hardy's *Apology*, more than a dozen papers—including one by the renowned mathematician Neil Sloane—have been published on the phenomenon of *reverse multiples*, i.e., on numbers with the special property described in example ($a$), that discuss nontrivial (in terms of difficulty) generalizations.

My main goal in this chapter is to clarify what Hardy meant by his diagnosis and to examine it for accuracy, especially in light of the research that has been done since his claims were made. In doing so, I will, among other things, identify and argue for what I believe to be the most important aspect of Hardy's notion of generality. Taking this result into account, I will further argue that his upshot that example ($a$) is "not capable of any significant generalization" is still tenable, which contradicts several authors dealing with the phenomenon of reverse multiples—again including Sloane—who explicitly refer to (part of) ($\mathcal{D}$). Finally, I will present and discuss this phenomenon as an example of the multifaceted nature of mathematical *interest*.

In Section 3.2, some of the main research results on the phenomenon of reverse multiples are presented. After having introduced, in Section 3.3, Hardy's main ideas on seriousness relevant to the present study, a closer look is taken at his notion of generality in Section 3.4. First, Hardy's proposed ways in which (*a*) lacks generality are discussed (3.4.1). Then, the most important aspect of Hardy's notion of generality is identified (3.4.2). Finally, in Section 3.5, diagnosis ($\mathcal{D}$) is discussed by, among other things, evaluating researchers' interpretations (3.5.1) and by examining mathematical interests in reverse multiples (3.5.2).

## 3.2 The General Phenomenon of Reverse Multiples

In this section, I present some of the main research that has been carried out on the general phenomenon of reverse multiples. In order to provide some self-containment, I will cover Ludington Young's (1992a) contribution and, to a certain degree that of Sloane (2014), in some detail.

Sloane listed in (2014) all the work on the general phenomenon of reverse multiples of which he was aware at the time. His earliest references are two brief responses published in volume XV of *L'Intermédiaire des mathématiciens* in 1908 (Laisant et al., 1908, pp. 132f., 278f.) to a problem that has been posed in volume VI of the same journal published in 1899. There, J. Jonesco asks, among other things, "[w]hat are then the numbers $n$ smaller than the base of the system, such that there are products $n$N which are the reversals of N?"[1] (Laisant and Lemoine, 1899, p. 200, problem 1622). So, Jonesco was not just interested in the base-10 case, as in Hardy's example (*a*). One of the respondent gave two examples of this general case, namely 1015 in base 8, since $5 \cdot 1015 = 5101$, and 18 in base 13, since $5 \cdot 18 = 81$.

Alan Sutcliffe's (1966) *Integers that are multiplied when their digits are reversed* published in *Mathematics Magazine*, appears to be the first paper after the

[1] "Quels sont alors les nombres $n$ plus petits que la base du système, tels qu'il existe des produits $n$N qui soient les renversés de N?"

publication of Hardy's *Apology* that discusses generalizations of reverse multiples (see also (Sloane, 2014, p. 99)). In this work, Sutcliffe is interested in the general phenomenon of base-$g$ reverse multiples, i.e., he is interested in the solution of

$$k(a_{n-1}g^{n-1}+a_{n-2}g^{n-2}+\cdots+a_1g+a_0) = a_0g^{n-1}+a_1g^{n-2}+\cdots+a_{n-2}g+a_{n-1} \quad (3.1)$$

where $k, a_0, \ldots, a_{n-1}$ are all less than $g$ and $k > 1$, $a_0 > 0$ and $a_{n-1} > 0$.[2] He is mainly concerned with a characterization of 2- and 3-digit numbers. For 2-digit numbers he proves, for instance, that there is a solution of Equation (3.1) in base $g > 3$ if and only if $g + 1$ is non-prime. He notes that things are problematic for numbers with more digits since "for each additional digit there is one new equation but two new variables" (1966, p. 287). He also conjectured that if there exists a 3-digit solution for the base $g$, then there also exists a 2-digit solution for $g$, but explains that "no general proof has been found" (ibid., p. 285). T. J. Kaczynski (1968)—who is, as Lara Pudwell (2007, p. 129) puts it, "[b]etter known for other work"—proved that this conjecture is correct. Based on Sutcliffe and Kaczynski's results, Pudwell (2007) asks whether there is any value of $g$ for which there is a 5-digit solution but no 4-digit solution which she answers in the negative.

In Anne Ludington Young's (1992a; 1992b) two articles on $k$-reverse multiples, which appeared in the same issue of *The Fibonacci Quarterly*, she focuses on the task of determining *all* $k$-reverse multiples in base $g$, for given $k$ and $g$, once those with a small number of digits are known. To this end, she introduces a graphical notation in the form of specific rooted trees (see as an example Figure 3.1). By comparing corresponding digits of Equation (3.1), she gets the following set of equations:

[2] Note that the letters that I have used in the statement of the general equation in which Sutcliffe is interested in, have been changed to be consistent with (Ludington Young, 1992a,b) and (Sloane, 2014).

$$
\begin{array}{lllllll}
ka_0 & & & = & a_{n-1} & + & r_0 g \\
ka_1 & + & r_0 & = & a_{n-2} & + & r_1 g \\
 & & & \dots & & & \\
ka_i & + & r_{i-1} & = & a_{n-1-i} & + & r_i g \\
 & & & \dots & & & \\
ka_{n-2} & + & r_{n-3} & = & a_1 & + & r_{n-2} g \\
ka_{n-1} & + & r_{n-2} & = & a_0 & &
\end{array}
\tag{3.2}
$$

where $0 \le r_i < g$ for $i = 0, \dots, n-2$. The $r_i$'s are called the "carry numbers" or simply "carries." For instance, if we think of Hardy's first mentioned reverse multiple, namely 2178, we have $(r_2, r_1, r_0) = (0, 3, 3)$; for example, the first equation of (3.2) is $4 \cdot 8 = 2 + \mathbf{3} \cdot 10$. Now, Ludington Young considers two equations of (3.2) at a time. She sets $r_{-1} = r_{n-1} = 0$ for convenience. At the $(i+1)$-st step for $i = 0, 1, \dots,$ one has

$$
\begin{array}{lllllll}
ka_i & + & r_{i-1} & = & a_{n-1-i} & + & r_i g \\
ka_{n-1-i} & + & r_{n-2-i} & = & a_i & + & r_{n-1-i} g
\end{array}
\tag{3.3}
$$

where $r_{i-1}$ and $r_{n-1-i}$ are known from the previous step. If the non-negative numbers in (3.3) also satisfy

$$
a_0, a_{n-1} > 0,\ a_i < g \text{ for } i = 0, 1, \dots, n-1,\ r_0 > 0 \text{ and } r_i < k \text{ for } i = 0, \dots, n-2,
\tag{3.4}
$$

then Ludington Young writes

$$
\begin{array}{l}
(r_{n-1-i}, r_{i-1}) \\
\qquad\Big|\,(a_{n-1-i}, a_i) \\
(r_{n-2-i}, r_i)
\end{array}
$$

and conversely. That is to say that the existence of non-negative solutions to (3.3) and (3.4) is equivalent to the existence of a tree of the form

$$
\begin{array}{l}
(0,0) \\
\quad \big| \, (a_{n-1}, a_0) \\
(r_{n-2}, r_0) \\
\quad \big| \, (a_{n-2}, a_1) \\
(r_{n-3}, r_1) \\
\quad \big| \\
\quad \cdots \\
\quad \big| \\
(r_{n-1-i}, r_{i-1}) \\
\quad \big| \, (a_{n-1-i}, a_i) \\
(r_{n-2-i}, r_i)
\end{array}
\tag{3.5}
$$

As an example, Ludington Young (1992a, p. 128, Example 1) considers the case for $g = 10$ and $k = 4$ for which she gets the following graph or rooted tree:

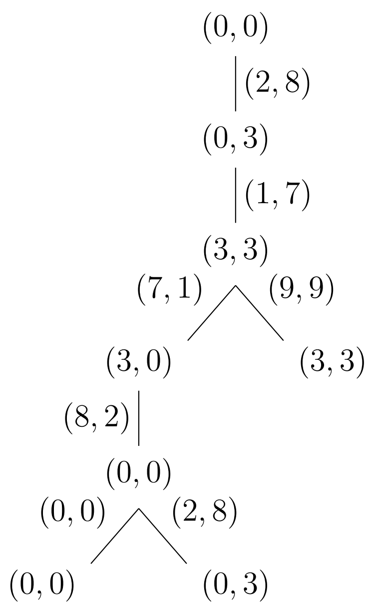


Figure 3.1: Pruned rooted tree for $g = 10$ and $k = 4$.

Note that although these trees are in essence infinite, the tree in Figure 3.1 is pruned after the nodes from which no new information would emerge.

Now, how do we use these trees to determine $k$-reverse multiples? Ludington Young answers this with the help of two theorems:

THEOREM 1 (Ludington Young, 1992a, Theorem 1, p. 129): For a given $g$ and $k$, suppose a tree of the form (3.5) exists. Then there is an $n = 2i + 2$-digit number $x$ satisfying (3.1) if and only if $r_{n-2-i} = r_i$. In this case, $x$ is given by

$$x = a_{n-1}a_{n-2} \ldots a_{n-1-i}a_i \ldots a_1a_0.$$

The theorem says that if there is a tree of the form (3.5) with a path of length $n/2$ for an even number $n$ from the root to a node of the form $(u, u)$, then there exists an $n$-digit reverse multiple. Given a tree with such a node, we can then simply read off the corresponding reverse multiple by first listing all the left entries of the labels of the edges while following the path until we reach the node of the form $(u, u)$, and then, while going back to the root, listing all the right entries of these labels.

As an example, consider Figure 3.1. The path from the starting node $(0, 0)$ to the first appearance of the node $(3, 3)$ gives us the number 2178, which is one of the four-digit reverse multiples from Hardy's example *(a)*. We could also pick the path from the root to the second appearance of the node $(3, 3)$. In this case, we would get the reverse multiple 219978. Note that the tree in Figure 3.1 is pruned; there are infinitely many more 4-reverse multiples in base 10.

THEOREM 2 (Ludington Young, 1992a, Theorem 3, p. 130): For a given $g$ and $k$, suppose a tree of the form (3.5) exists. Then there is an $n = 2i + 3$-digit number

$x$ satisfying (3.1) if and only if the graph contains

$$\begin{array}{l} (r_{n-1-i}, r_{i-1}) \\ \quad \Big| \, (a_{n-1-i}, a_i) \\ (r_{n-2-i}, r_i) \\ \quad \Big| \, (a_{n-2-i}, a_{i+1}) \\ (r_i, r_{n-2-i}). \end{array}$$

Further, when this occurs, $a_{n-2-i} = a_{i+1} = M = (r_{n-2-i}g - r_i)/(k-1)$. The desired $n$-digit number $x$ is given by

$$x = a_{n-1}a_{n-2} \ldots a_{n-1-i}Ma_i \ldots a_1a_0.$$

In Figure 3.1, there is an edge from $(3,3)$ to $(3,3)$ which ensures the existence of reverse multiples with an odd number of digits according to the previous theorem, such as 21978. In this case, $a_{n-2-i} = a_{i+1} = M = 9$.

In her follow-up article (1992b), Ludington Young examines these trees in more detail. Her main results are essentially of the type that if one or more labelled edges are known along with their nodes, then other parts of the tree are also known in principle.

The mathematician Neil Sloane, who is best known for his role as the founder and maintainer of the *On-Line Encyclopedia of Integer Sequences* (OEIS), extends Ludington Young's work in (Sloane, 2014), which also appeared in *The Fibonacci Quarterly*. He introduces, inter alia, modified versions of Ludington Young's trees, which are finite, directed graphs and which he calls *Young graphs*, and determines the possible values of $k$ for bases $2 \leq g \leq 100$. Further, he shows how to use the *transfer-matrix method* from combinatorics (by applying it to the Young graphs) in order to enumerate the $(g,k)$-reverse multiples with a given number of base-$g$ digits. The Young graphs result from Ludington Young's pruned rooted trees by essentially identifying equal nodes, except for the starting node and other occurrences of $(0,0)$. For instance, the pruned tree of Figure 3.1 gives the following Young graph (2014, p. 104, Figure 4):

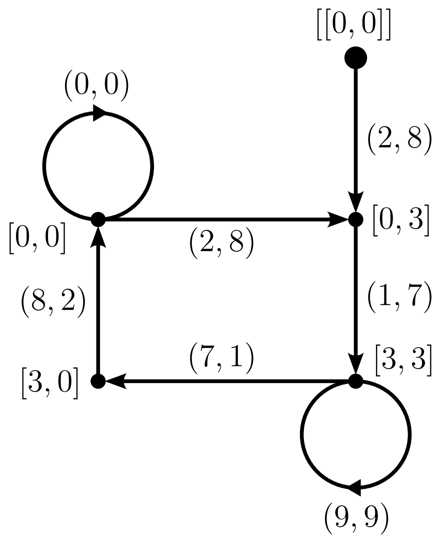


Figure 3.2: The $(10,4)$ Young graph.

Note that Sloane uses square brackets for the nodes and double brackets for the starting node.

Sloane (2014) shows, among other things, how to apply the transfer-matrix method to these Young graphs in order to determine their *generating functions*, i.e., formal power series of the form

$$\mathcal{C}(x) = \sum_{t \geq 0} c_t x^t,$$

where the $c_t$'s denote the numbers of $(g,k)$-reverse multiples with $t$ digits. One of his concrete examples is the generating function for the number of *all* base-10 reverse multiples, regardless of the multiplier. He gets (2014, p. 109)

$$\begin{aligned} \mathcal{C}(x) = 2\sum_{t=4}^{\infty} F_{\lfloor \frac{t}{2} \rfloor - 1} x^t =& 2x^4 + 2x^5 + 2x^6 + 2x^7 + 4x^8 + 4x^9 + \\ & 6x^{10} + 6x^{11} + 10x^{12} + 10x^{13} + \cdots , \end{aligned} \tag{3.6}$$

where $F_{\lfloor \frac{t}{2} \rfloor - 1}$ is the $(\lfloor \frac{t}{2} \rfloor - 1)$-st Fibonacci number. So, in the base-10 case, the reverse multiples are basically enumerated by the Fibonacci numbers.[3] Note that the first 2, i.e., the coefficient of $x^4$, corresponds to the two numbers in Hardy's example $(a)$.

In general, Sloane (2014) identifies and characterizes in more detail three particular families of Young graphs. For example, one such family is the isomorphism class of the $(10, 4)$ Young graph, which also contains the $(10, 9)$ Young graph. Note that two Young graphs are isomorphic if and only if their underlying directed graphs are the same and some particular nodes of both graphs, each playing the same role in identifying the reverse multiples, correspond to each other.

Furthermore, he presents some computer-generated results for bases $g \leq 20$. For example, all $(g, k)$ values for which reverse multiples exist in this case are listed along with a specification of the respective Young graphs (see (ibid., p. 115, Table 1)).

Sloane also raises some open questions and makes several conjectures, some of which have been addressed by Lev Kendrick (2015). As Kendrick himself explains in the abstract of his article:

> We prove Sloane's isomorphism conjectures for 1089 graphs [i.e., graphs that are isomorphic to the $(10, 4)$ and $(10, 9)$ Young graphs; S.W.] and complete graphs, namely that the Young graph for $g$ and $k$ is a 1089 graph if and only if $k + 1$ — $g$ and is a complete Young graph on $m$ nodes if and only if $\lfloor \gcd(g - k, k^2 - 1)/(k + 1) \rfloor = m - 1$. We also extend his study of cyclic Young graphs and prove a minor result on

[3] Sloane (2014, p. 100) states in the introduction that "the first mention of this fact appears to have been by D. W. Wilson" in his comment in OEIS on the sequence A001232 (1997, December 15). However, already in a talk presented at a meeting of the *Berliner Mathematischen Gesellschaft* in 1915, the member Robert Burg made the connection between the numbers of base-10 reverse multiples and the Fibonacci numbers (Burg, 1916, pp. 15f.). There, Burg also noticed that $(10, 9)$-reverse multiples are precisely the numbers $\gamma \cdot \beta$, where $\gamma = 99$ and $\beta$ is any positive palindromic decimal number consisting only of the digits 1 and 0, but where a single 1 or 0 may never occur (1916, p. 18). This fact is again attributed by Sloane (2014, p. 100) to Wilson (1997, December 15).

isomorphism and the nodes adjacent to the node $[0, 0]$. (2015, p. 1)

Besides these studies on reverse multiples, Benjamin Holt's work should also be mentioned. In a series of three papers (2014; 2016; 2017) that have all been published in the electronic journal *Integers*, he also investigates the general phenomenon of reverse multiples. In (Holt, 2014), he establishes some general properties of reverse multiples of any base with an arbitrary number of digits using only elementary methods. In (2016), Holt builds on Sloane and Kendrick's work and is concerned with several families of *derived* reverse multiples, i.e., reverse multiples where the carry numbers themselves are digits of lower-base reverse multiples. Finally, in (Holt, 2017), he generalizes results on reverse multiples to an arbitrary permutation, i.e., he investigates numbers which are integer multiples of some permutation of their digits, which he calls *permutibles*.

## 3.3 Hardy on *Serious* Mathematics

As mentioned earlier, Hardy's *Apology* is meant to provide a justification of the study of mathematics and that of a professional mathematician's life, in particular that of his own. He thinks that the "real" mathematics, i.e., the mathematics of the best mathematicians (pure or applied), such as Fermat, Euler Gauss, Abel and Riemann (p. 119)[4], but also Maxwell, Einstein, Eddington and Dirac (p. 131), is "almost wholly 'useless'" (p. 119), so that one cannot justify it by its practical utility. According to Hardy, the real mathematics must be justified as (a creative) art.

I have already pointed out in the introduction to this chapter that Hardy introduces examples *(a)* and *(b)* in his *Apology* as examples of *non-serious* mathematical theorems. They appear together with his diagnosis $(\mathcal{D})$ in § 15, where he starts to explain a little more precisely what he means by a "serious" theorem

[4]Page references without additional information in the remainder of this chapter always refer to (Hardy, 1940/2012).

and a "significant" mathematical idea. In the remainder of this section, I present Hardy's main thoughts on these qualities which he sets forth in his *Apology*.

The first time Hardy mentions the *seriousness* of a theorem and the *significance* of a mathematical idea is in § 11, where he explains quite at the beginning that contrary to chess problems which are "genuine mathematics," but "in some way 'trivial' mathematics," since "there is something essential lacking," namely importance, "[t]he best mathematics is *serious* as well as beautiful—'important' if you like, but the word is very ambiguous, and 'serious' expresses what I mean much better" (pp. 88f.). To further illustrate his ideas, he says that (hereinafter referred to as (*S1*))

> (*S1*) The 'seriousness' of a mathematical theorem lies, not in its practical consequences, which are usually negligible, but in the *significance* of the mathematical ideas which it connects. We may say, roughly, that a mathematical idea is 'significant' if it can be connected, in a natural and illuminating way, with a large complex of other mathematical ideas. Thus a serious mathematical theorem, a theorem which connects significant ideas, is likely to lead to important advances in mathematics itself and even in other sciences. No chess problem has ever affected the general development of scientific thought; Pythagoras, Newton, Einstein have in their times changed its whole direction. (pp. 89f.)

He continues by emphasizing that "[t]he seriousness of a theorem, of course, does not *lie in* its consequences, which are merely the *evidence* for its seriousness" (p. 90).

He then goes on and gives some examples of serious mathematical theorems. He also provides proofs for two of them, namely Euclid's proof that there are infinitely many prime numbers (§ 12) and Pythagoras's proof that $\sqrt{2}$ is irrational (§ 13).[5]

[5] The two proofs can be found in the Appendix to this chapter.

He concludes these two sections by pointing out that compared to "Dudeney's most ingenious puzzles" or "the finest chess problems that masters of that art have composed," the superiority of both Euclid's theorem and that of Pythagoras stands out and that "there is an unmistakable difference of class" (p. 98). The two ways in which their superiority stands out is in their *beauty* and their *seriousness*. In § 14, he then starts to discuss the latter notion in more detail. There he says that this superiority is "obvious and overwhelming" (p. 99) and explains that (hereinafter referred to as (*S2*))

> (*S2*) The chess problem is the product of an ingenious but very limited complex of ideas, which do not differ from one another very fundamentally and have no external repercussions. We should think in the same way if chess had never been invented, whereas the theorems of Euclid and Pythagoras have influenced thought profoundly, even outside mathematics. (ibid.)

With respect to Pythagoras's proof, Hardy points out that it is

> capable of far-reaching extension, and can be applied, with little change of principle, to very wide classes of 'irrationals'. We can prove very similarly (as Theodorus seems to have done) that
>
> $$\sqrt{3},\ \sqrt{5},\ \sqrt{7},\ \sqrt{11},\ \sqrt{13},\ \sqrt{17}$$
>
> are irrational, or (going beyond Theodorus) that $\sqrt[3]{2}$ and $\sqrt[3]{17}$ are irrational. (p. 100)

In general, he emphasizes the importance of both theorems in the development of (modern) mathematics. While "Euclid's theorem is vital for the whole structure of arithmetic" (p. 99), "Pythagoras's theorem and its extensions tell us that, when we have constructed this arithmetic, it will not prove sufficient for our needs, since there will be many magnitudes which obtrude themselves upon our attention and which it will be unable to measure" (p. 100).

Hardy begins § 15 by reiterating that "[a] 'serious' theorem is a theorem which contains 'significant' ideas" (p. 103). Since he thinks that "[w]e can recognize a 'significant' idea when we see it," he tries to give "some sort of analysis" (ibid.). He mentions "two things at any rate which seem essential" to a significant idea, namely "a certain *generality* and a certain *depth*" (ibid.). With respect to the notion of generality, he gives the following, tentative characterization:

**Characterization of *Generality*:** "A significant mathematical idea, a serious mathematical theorem, should be 'general' in some such sense as this."

(*G1*) "The idea should be one which is a constituent in many mathematical constructs, which is used in the proof of theorems of many different kinds."

(*G2*) "The theorem should be one which, even if stated originally (like Pythagoras's theorem) in a quite special form, is capable of considerable extension and is typical of a whole class of theorems of its kind."

(*G3*) "The relations revealed by the proof should be such as connect many different mathematical ideas." (p. 104)[6]

I speak of a "tentative" characterization, since immediately after he has presented the ideas above, Hardy states that "[a]ll this is very vague, and subject to many reservations" (ibid.). He continues, however, as follows (hereinafter referred to as ($\mathcal{A}$), where "$\mathcal{A}$" is meant to remind of "assessment"):

> ($\mathcal{A}$) But it is easy enough to see that a theorem is unlikely to be serious when it lacks these qualities conspicuously; we have only

[6] Note that Hardy does not present this within a "characterization" environment in the original text, along with an enumeration (*G1*)–(*G3*). I have introduced this for ease of reference in my later discussion. In the *Apology*, the individual sentences that appear in my "characterization" simply occur as a continuous text.

> to take examples from the isolated curiosities in which arithmetic abounds. I take two, almost at random, from Rouse Ball's *Mathematical Recreations*. (ibid.)

He then presents his two examples $(a)$ and $(b)$, immediately followed by his diagnosis $(\mathcal{D})$ which closes § 15.

In § 16, Hardy discusses the fact that in some sense, all mathematics is equally general, or *abstract*, which should not be confused with his notion introduced in his previous section. As he emphasizes, "[w]e are looking for *differences* of generality between one mathematical theorem and another" (p. 107).

Finally, in § 17, he addresses the notion of *depth*, which a significant idea should have in addition to its generality. The notion of depth, however, "is still more difficult to define" (p. 109) and is an elusive one even for mathematicians capable of recognizing it (p. 112). He explains that "[i]t has *something* to do with *difficulty*; the 'deeper' ideas are usually the harder to grasp: but it is not at all the same" (p. 109). Presumably as a kind of first approximation, Hardy states:

> It seems that mathematical ideas are arranged somehow in strata, the ideas in each stratum being linked by a complex of relations both among themselves and with those above and below. The lower the stratum, the deeper (and in general the more difficult) the idea. (p. 110)

This is why he considers the "idea of an 'irrational'" to be "deeper than that of an integer"; and, because of that, Pythagoras's theorem as deeper than Euclid's (ibid.). Moreover, he regards Euclid's theorem to be "very important, but not very deep" since the deepest notion involved in the proof is that of divisibility (p. 111).

Taking into account the research that has been done on the general phenomenon of reverse multiples, which I discussed at least in part in Section 3.2, I would now like to take a closer look at Hardy's assessments of this phenomenon, which he makes in ($\mathcal{A}$) and ($\mathcal{D}$), in terms of their accuracy.

## 3.4 A Closer Look at Hardy's Notion of *Generality*

We will now begin with evaluating Hardy's comments about his example (*a*). In this regard, we will first discuss his assessment ($\mathcal{A}$) in which he addresses his qualities (*G1*) to (*G3*). After that, I will identify and argue for what I think is the most important aspect of Hardy's notion of generality. This will be crucial to my evaluation of Hardy's diagnosis ($\mathcal{D}$), which I will turn to in Section 3.5.

### 3.4.1 Example (a) and the Three Qualities of *Generality*

Let us now take a closer look at Hardy's assessment of example (*a*) made in ($\mathcal{A}$) in which he refers to the statements (*G1*)–(*G3*), by going through all these properties one by one (in reverse order). Although it is not easy to evaluate his assessment—not least because all the qualities he proposes in his characterization are "very vague, and subject to many reservations"—I think it is worth doing anyway, partly because it will help bring some clarity to what I think is the most important aspect of *generality*. In assessing his statements, we should also keep in mind the context in which they appear in his *Apology*: Hardy tries to characterize the *best* mathematics, which is both beautiful *and* serious. In this regard, I will compare the respective findings with his two most elaborated examples of serious theorems, namely those of Pythagoras and Euclid.

Although Hardy does not give a specific proof of example (*a*) in his *Apology*, he must have had a rather straightforward one in mind. A proof in this category can be found, for instance, in (Webster and Williams, 2012/2013). To avoid some repetitive work, one can first establish the fact that in base 10, the only possible values for $k$ are 4 and 9. A proof of this must not be restricted to the four-digit case, since it can be essentially the same for an arbitrary number of digits—as the one proposed by Webster and Williams. For all the other possible values for $k$, i.e., 2, 3, 5, 6, 7 or 8, one can arrive at contradictions with the help of the observation that $ka_{n-1}$ must be less than or equal to 9 (to retain the number of digits) and (in the more "difficult" cases) by comparing possible values for $a_{n-1}$ and $a_0$. After this, one can convince oneself that there are no 4 and 9-reverse multiples with less than four digits by obtaining some simple contradictions and similarly that 1089 and 2178 are *the only* two four-digit reverse multiples.

Now, if we admit that Hardy is right in $(\mathcal{A})$ that example $(a)$ lacks quality *(G3)* conspicuously, one might wonder whether his two examples from Greek mathematics do not also lack this quality, especially since, as Hardy himself points out, "[t]hey are 'simple' theorems, simple both in idea and in execution" (p. 92). As I noted above, the corresponding proofs of these two "simple" theorems can be found in the Appendix to this chapter.

With respect to *(G2)*, possible examples of theorems of the kind of example $(a)$ are theorems of the form

- $a$, $b$, $c$, ... are the only $n$-digit reverse multiples in base $g$;
- there are only $m$ $k$-reverse multiples in base $g$ with less than $n$ digits;
- all $k$-reverse multiples in base $g$ are of the form $ABCD\dots EFGH$ (see, for instance, Theorem 2.10 in (Webster and Williams, 2012/2013));
- the generating function for $(g,k)$-reverse multiples is given by $\mathcal{C}(x) = \sum_{t\geq 0} c_t x^t$;
- the $(g,k)$ Young graph is isomorphic to the $(g',k')$ Young graph;
- the digits of the reverse multiple $x$ of the family $\mathcal{F}$ of reverse multiples correspond to the carry numbers of the higher-base reverse multiple $y$;

- etc.

While I do think that Hardy was well aware that one could produce countless theorems of the form listed in the first two to three points above (that are not only concerned with the base-10 case)—which is why he probably added the restriction "considerable" to *(G2)*—I nevertheless think that he might have underestimated the potential of *(a)* with respect to *(G2)*, given the other items of the enumeration.

Besides the particular examples of irrationals to which Pythagoras's proof can be applied "with little change of principle" which I have reproduced in Section 3.3, Hardy also refers in a subsequent footnote to Chapter IV of *An Introduction to the Theory of Numbers* (1938) which he co-authored with E. M. Wright "where there are discussions of different generalizations of Pythagoras's argument" (p. 100). More precisely, Hardy and Wright present two proofs of the theorem that

$\sqrt[m]{N}$ *is irrational, unless* $N$ *is the* $m$*-th power of an integer* $n$.

one of which is a (close) generalization of Pythagoras's proof that Hardy provides in the *Apology*, and one proof of the theorem that

> *If* $x$ *is a root of an equation* $x^m + c_1x^{m-1} + \ldots + c_m = 0$*, with integral coefficients of which the first is unity, then* $x$ *is either integral or irrational.*

which is a generalization of a "very similar" argument to that of Pythagoras (1938, pp. 39–41). However, compared to these extensions of Pythagoras's proof or to the case of Euclid's theorem, where Hardy mentions no extension at all, it seems that *(a)* is not worse off.

To sum up, I take it to be the case that example *(a)* is not (decisively) worse off than Hardy's two serious theorems with respect to the qualities *(G3)* and *(G2)*.

However, I think this is different for quality (*G1*). Although again he does not elaborate on how exactly example (*a*) conspicuously lacks this quality, I think that "the idea" he speaks of in (*G1*) refers in this case to the concept of a reverse multiple itself.[7]

First, while there are—as we saw in Section 3.2—some possible "constructs" in which this idea is a constituent, such as Ludington Young's rooted trees, Sloane's Young graphs, and Holt's permutibles, I do not think there are enough to count as "many" (as required by (*G1*)). Secondly, and, what I take to be much more important, is the fact that the idea of a reverse multiple—even considering all the research that has been carried out on this phenomenon since the publication of Hardy's *Apology*—does not (yet) occur in proofs of theorems of *many different kinds*. For instance, (so far) Young graphs do not appear in proofs of theorems of other areas of research.

This is in stark contrast to Hardy's two main examples of serious theorems. Because of the fundamental character of the ideas contained in the theorems of Euclid and Pythagoras for the development of mathematics, namely the idea of

[7] If this seems obvious to the reader, so much the better for my argumentation. There are, however, some complications in trying to evaluate his assessment made in ($\mathcal{A}$) in terms of the quality (*G1*) in thinking about example (*a*). As we now know, for Hardy a serious theorem is one that *connects* or *contains* significant ideas. The question that arises in this context is whether we should treat theorems and their proofs separately. The passage of the *Apology* in which Hardy discusses the "purely aesthetic" qualities of the theorems of Euclid and Pythagoras seems to contradict such an interpretation. There, indeed, the theorems and proofs are considered as a kind of unity. As Hardy puts it: "In both theorems (and in the theorems, of course, I include the proofs) there is a very high degree of *unexpectedness*, combined with *inevitability* and *economy*" (p. 113). In fact, (*G3*) itself indicates that a proof of a theorem can also play a role in evaluating theorems in terms of their seriousness. In general, we might wonder whether a theorem, which is about rather "non-general" (with respect to (*G1*)), "superficial" or non-significant ideas, but which requires a highly nontrivial proof that involves a huge amount of significant ideas might nevertheless turn out to be serious. In this context, one might also wonder whether different proofs of a theorem have different effects on the seriousness of a theorem. With respect to the former question, a possible indication that it would have been answered by Hardy in the negative can be found in his discussion of the notion of depth where he compares this notion to *difficulty* and explains that there are not only cases of deep theorems which are not difficult, but also those which "may be essentially superficial and yet quite difficult to prove" (p. 110). Nevertheless, we cannot answer these questions satisfactorily with what Hardy gives us in his *Apology*.

the infinity of primes or the notion of a prime number itself and the idea of an irrational number, it is clear that they score highly in terms of quality (*G1*).

### 3.4.2 The Most Important Aspect of *Generality*

We should now take a step back and have a look at qualities (*G1*)–(*G3*) themselves and how they relate to each other. While quality (*G1*) is explicitly about the generality of mathematical ideas themselves, (*G2*) addresses theorems in general and (*G3*) their proofs.

When comparing these qualities of generality—which itself is introduced as an essential property of a significant mathematical idea and a serious theorem—we find that a general theme present in all of them is the connectedness with more mathematics. As the reader may recall (cf. especially (*S1*)), according to Hardy, the seriousness of a mathematical theorem lies in the significance of the ideas which it connects or contains, which makes quality (*G1*) the most fundamental and important one out of the three, even if one is interested in the generality (and ultimately in the seriousness) of a theorem and its proof. This is also immanent in the discussion of Section 3.4.1 above, for as we have seen, it is precisely this quality that distinguishes the serious (and thus general(!)) theorems of Euclid and Pythagoras.

As a reminder, in Hardy's first, rough characterization of *significance* (cf. (*S1*)), he explains that a mathematical idea is significant "if it can be connected, in a natural and illuminating way, with a large complex of other mathematical ideas." I think that part of this characterization is that these "other mathematical ideas" should also be distinctly *different* from each other in a large number. This is supported, first and most importantly, by the fundamental quality (*G1*) itself, where he explicitly talks about "many *different* kinds" (emphasis added) (and for that matter also by (*G3*), where the emphasis is on a connectedness to many different mathematical ideas). Secondly, indirectly by his comparison of serious mathematical theorems with chess problems: recall that in § 14 of the *Apology*, Hardy discusses the seriousness of Euclid's theorem and that of Pythagoras as one important way in which their superiority lies when compared to chess problems. There, he emphasizes that a chess problem "is the product of an ingenious but very limited complex of ideas, which do not differ from one another very fundamentally and have no external repercussion" (cf. (*S2*)). Finally, I think that the aspect of connectedness to other, different mathematics is also indirectly emphasized in ($\mathcal{A}$), when Hardy uses its antonym to refer to his examples ($a$) and ($b$) by calling them *isolated* curiosities—especially since I am convinced that in the case of ($a$) he was well aware that one could produce innumerable theorems of the form given in the first two items of my list presented in Section 3.4.1.

For these reasons, I believe that *connectedness to a large complex of appreciably different mathematical ideas* is not only the most important aspect of Hardy's notion of generality, but also a necessary condition for an idea to be considered significant or a theorem to be serious.

Before moving on to the next section where we will have a look at Hardy's diagnosis here referred to as ($\mathcal{D}$), there are two points which should be made.

First, one should notice that, according to my conclusion, scoring well on quality (*G2*) while lacking the other two, makes a theorem not general, at least not in the sense required for it being serious. I think this is a welcome result, since a rather isolated mathematical theorem is not made much less isolated, at least not if it is understood as part of the broad mathematical research landscape, when only other results *of its kind* are added. Moreover, it seems that such a theorem is very unlikely to lead to important advances in mathematics, unlike serious theorems (cf. (*S1*)).

Secondly, while I believe that Hardy admits that the seriousness of a theorem and the significance of a mathematical idea can come in degrees, I do not think that this compromises the correctness of my conclusion above, especially with respect to its second part that the most important aspect I have identified is also a *necessary condition* for significance and seriousness. To begin with, that these notions can actually come in degrees is in my opinion contained in Hardy's conclusion of the *Apology*. While he focuses on examples of the *best* mathematics in his discussion of the seriousness of a theorem, in his ultimate conclusion on the value of his own mathematical life he declares that his work differs only in degree, *not in kind*, from that of the great mathematicians:

> The case for my life, then, or for that of any one else who has been a mathematician in the same sense in which I have been one, is this: that I have added something to knowledge, and helped others to add more; and that these somethings have a value which differs in degree only, and not in kind, from that of the creations of the great mathematicians, or of any of the other artists, great or small, who have left some kind of memorial behind them. (p. 151)

That he does not think, however, that all mathematics is of this kind with respect to seriousness is shown, among other things, by his demarcation of chess problems, which are "genuine mathematics," *but lack something essential*, namely importance or seriousness, and "real" mathematics (cf. Section 3.3); and his examples *(a)* and *(b)* "in which arithmetic abounds" themselves *as proposed examples* of mathematical(!) theorems *which are not serious*.[8] I think my discussion above makes it is perfectly reasonable to call theorems—within Hardy's framework—not of that *kind* that are conspicuously lacking in the most important aspect of generality, as I have identified it.

[8]Note that Hardy's distinction between real mathematics and "trivial" mathematics which he addresses in § 25–§ 28 is primarily based on their "aesthetic merit" (p. 134), i.e., on the second criterion, besides seriousness, by which the patterns of mathematicians ought to be judged.

## 3.5 Hardy's Diagnosis Revisited

As a reminder, in § 15–§ 17 of the *Apology* (cf. Section 3.3), Hardy explains a little more precisely what makes a mathematical idea *significant*, or a theorem *serious*. Specifically, in § 15, he focuses on the notion of generality in the sense that this quality is essential to a serious theorem or a significant idea. After having introduced his tentative characterization of generality consisting itself of the qualities (*G1*)–(*G3*), he presents (*a*) and (*b*) as examples which are said to be conspicuously lacking in these qualities (cf. ($\mathcal{A}$)). He then concludes the section with diagnosis ($\mathcal{D}$).

### 3.5.1 Example (a) and its Generalizations

Let us first evaluate the second part of ($\mathcal{D}$), namely Hardy's statement that "[t]he theorems are not serious; and it is plain that one reason (though perhaps not the most important) is the extreme speciality of both the enunciations and the proofs, which are not capable of any significant generalization," with respect to example (*a*).

To begin with, I just want to take a quick look at Hardy's insertion that the reason he gives is "perhaps not the most important" one, because he does not further elaborate on this. As mentioned in Section 3.3, for Hardy a certain generality is not the only thing which seems essential for a significant idea. The other one he mentions in § 15 and elaborates in § 17 is depth. So perhaps he meant to imply that the main reason why the examples (*a*) and (*b*) are not serious is that they are not deep. However, I will refrain from any further speculations on this issue and turn to the more interesting part of his statement, at least with regards to my concerns here.

First, I think that we should not confuse his use of the word "significant" in this sentence with his rather technical notion of *significance*, which he uses in the context of a "significant idea." This is because at this point in the *Apology* he has only begun a more detailed analysis of his technical notions of significance and seriousness, which will occupy him up to and including § 17. A reference to this notion in the conclusion of § 15, i.e., in the last sentence of ($\mathcal{D}$), would then in principle transform it into a statement such as "These are not serious theorems, because they are not serious."

Moreover, when we try to make sense of this sentence, we must of course consider the context in which it appears. It is the last sentence of § 15—in which Hardy is mainly concerned with the notion of generality—and should be understood as such, namely as his concluding remark about why examples ($a$) and ($b$) are not serious, given his assessment of their generality. While he has already stated in ($\mathcal{A}$) that these examples are conspicuously lacking in qualities (*G1*)–(*G3*), I think he added the phrase "which are not capable of any *significant* generalization" (emphasis added) to emphasize that there are also no generalizations of ($a$) and ($b$) that would qualify as "general" themselves—at least not in the sense or to the extent necessary to count as significant ideas or serious theorems themselves. In other words, he wanted to make clear that not only examples ($a$) and ($b$) show an extreme speciality in both their enunciations and their proofs, but also that there are no generalizations which would make them much less isolated, to the point where one might start doubting whether his two examples are in fact examples of non-serious theorems.

It is necessary to stress this point, since I think it has been misinterpreted by several of the researchers who have recently contributed to the general phenomenon of reverse multiples. So, before I will evaluate whether Hardy's upshot is still tenable after all the research that has been carried out on this phenomenon, let us first have a look at what some of the researchers have to say about this.

For instance, Lara Pudwell (2007, p. 129) states that

> In *A Mathematician's Apology* [(Hardy, 1993); S.W.] G. H. Hardy

> states, "8712 and 9801 are the only four-figure numbers which are integral multiples of their reversals"; and, he further comments that "this is not a serious theorem, as it is not capable of any significant generalization." However, Hardy's comment may have been short-sighted. In 1966, A. Sutcliffe [(1966); S.W.] expanded this obscure fact about reversals. Instead of restricting his study to base 10 integers and their reversals, Sutcliffe generalized the problem to study all integer solutions of
>
> $$k(a_h n^h + a_{h-1} n^{h-1} + \cdots + a_1 n + a_0) = a_0 n^h + a_1 n^{h-1} + \cdots + a_{h-1} n + a_h$$
>
> with $n \geq 2$, $1 < k < n$, $0 \leq a_i \leq n-1$ for all $i$, $a_0 \neq 0$, $a_h \neq 0$.

Benjamin Holt (2014, pp. 1f.) claims that

> The most well-known examples of base-10 palintiples include 87912 and 98901 since $87912 = 4 \cdot 21978$ and $98901 = 9 \cdot 10989$. [...] At first glance it may seem that such numbers are merely curiosities that only make for cute puzzle problems. Such was the belief of G. H. Hardy who, in his classic essay *A Mathematician's Apology* [(Hardy, 1993); S.W.], cited the fact that "8712 and 9801 are the only four-figure numbers which are integral multiples of their 'reversals'" as an example of a theorem that is not "serious." Furthermore, "[this fact is] very suitable for puzzle columns and likely to amuse amateurs, but there is nothing in them which appeals much to a mathematician" and is "not capable of any significant generalization." Sutcliffe [(1966); S.W.], Pudwell [(2007); S.W.], and Young [(1992a); S.W.] demonstrate otherwise; in spite of Hardy's comments the palintiple problem generalizes quite naturally.

And Lev Kendrick (2015, pp. 1f.) says that

> Ironically, the most prominent mention of these numbers may well have curtailed their prominence: G. H. Hardy, in *A Mathematician's Apology*, refers to the fact that 1089 and 2178 are the only four-digit $(10, k)$ reverse multiples as one "very suitable for puzzle columns and likely to amuse amateurs," but "not serious" and "not capable of any significant generalization." While we abstain from judgment on the first two counts, it is clear, after the intervening decades, that Hardy misjudged regarding the third. [footnote: The only refuge for his position being in the nebulous qualifier "significant."] A number of works generalize the problem, a list of most of which may be found in Sloane's 2013 paper [(2014); S.W.] on the topic [cf. Sloane's comment below; S.W.]; [. . . ].

Despite what these researchers say, I do not think that the work done so far on reverse multiples, and especially not the papers explicitly or, in Kendrick's case, implicitly mentioned by them (and by Sloane in his comment below), namely (Sutcliffe, 1966), (Pudwell, 2007) and (Ludington Young, 1992a), show that Hardy was wrong. In general, I believe their diagnoses are ultimately due to a misinterpretation of the "nebulous qualifier 'significant'" (as Kendrick puts it).

As I have already explained above, the context in which diagnosis ($\mathcal{D}$) appears in the *Apology*—and hence the phrase "which are not capable of any significant generalization"—suggests that this qualifier refers to Hardy's assessment of the generality of the "isolated curiosities" $(a)$ and $(b)$, and to whether possible generalizations would call into question their (alleged) status of being isolated. I have argued in Section 3.4.2 that the most important aspect of generality and at the same time a necessary condition for an idea to be considered significant or a theorem to be serious is the connectedness to a large complex of appreciably different mathematical ideas. Although the research done so far has shown that the general phenomenon of reverse multiples is anything but trivial (in terms of difficulty), I do not think it has shown that this phenomenon is connected to so much other, different mathematics that it would no longer be considered "isolated." In particular, in the case of Ludington Young's (1992a) work, presented in the comments by the researchers above as a counterexample to Hardy's claim, the reader can easily see for themselves, as I discussed it quite extensively in Section 3.2, that while Ludington Young has introduced a new technique (in the form of graphical representations) for studying reverse multiples, this technique does not in itself show that this phenomenon is connected to a much larger complex of other, *different* mathematical ideas, which would make it much less isolated. Moreover, as I have already mentioned in Section 3.4.1 in the context of evaluating example *(a)* with respect to quality *(G1)*, even the follow-up work on these graphical representations in form of Sloane's Young graphs has not (yet) shown that the phenomenon of a reverse multiple is connected to other areas of research.

In my opinion, Neil Sloane has misinterpreted the phrase "not capable of any significant generalization" in a similar way to Pudwell, Holt, and Kendrick. But in doing so, he also presents another interpretation of the entire passage. He says (2014, p. 99):

> In 1940, G. H. Hardy [(Hardy, 2000); S.W.] famously remarked that the existence of these two numbers was "likely to amuse amateurs", but was not of interest to mathematicians, since this result is "not capable of any significant generalization". It seems fair to say that Hardy was wrong, since references [(Grimm and Ballew, 1975-76), (Laisant et al., 1908), (Kaczynski, 1968), (Klosinski and Smolarski, 1969), (Pudwell, 2007), (Sutcliffe, 1966), (Webster and Williams, 2012/2013), (Ludington Young, 1992a), (Ludington Young, 1992b); S.W.] discuss generalizations.[9]

[9] Note that two of the papers listed by Sloane have not been mentioned in Section 3.2. Both of them are rather short papers, not longer than three pages. After characterizing all base-10 reverse multiples, Klosinski and Smolarski (1969) determine two specific families of reverse

Similarly, the mathematician Solomon Golomb (2014) explains in his review of Sloane's article that

> Using the special case of the two numbers 1089 and 2178, G. H. Hardy gave this problem notoriety in his *A mathematician's apology* [(Hardy, 1940); S.W.], by commenting that it was "likely to amuse amateurs" but was not of interest to mathematicians, since it was "not capable of any significant generalization". With the present paper, there are now at least ten works that discuss nontrivial generalizations. This furnishes yet another example (along with his statement that relativity and quantum mechanics were unlikely to have practical applications) of the unreliability of Hardy's assertions in his famous *Apology*.

However, in ($\mathcal{D}$), Hardy is only using the phrase "not capable of any significant generalization" in his upshot of why examples ($a$) and ($b$) are *not serious*. While Hardy might have been of the opinion that ($a$) is ultimately not of interest to mathematicians because it is not capable of any significant generalization, this is not what he says in ($\mathcal{D}$). In this context, one might also wonder whether Hardy thought that only serious theorems are, or rather *should be* of interest to a (real) mathematician. Especially the final conclusion of his *Apology* (cf. the end of Section 3.4.2), which occurs in the context of his justification of his own mathematical life, makes such a normative view on Hardy's part not implausible. Although I will not go into this any further here, I will return to mathematicians' interests more generally in Section 3.5.2.

Overall, while I do not want to completely rule out the possibility that at some point it might turn out that reverse multiples can be connected with a large complex of different mathematics, I still think Hardy's conclusion, expressed in

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multiples of bases not restricted to 10. Grimm and Ballew (1975-76) first list all base-10 reverse multiples less than $10^9$ which they have determined using a CDC 3400 (where they allow "reverse multiples" to have a zero as their first digit). Then, they present an algorithm for generating 3-digit reverse multiples of bases not restricted to 10.

the last sentence of diagnosis $(\mathcal{D})$ is tenable.

Note that some (if not all) of the above comments by researchers, especially those of Pudwell and Holt, seem to suggest that Hardy was not aware that his base-10 example $(a)$ can also be generalized to arbitrary bases and that one can also seek solutions to the general Equation (3.1). However, this assumption does not seem very convincing to me. Why should he not have been aware of this obvious, natural idea? In this respect, I think my interpretation, together with my evaluation of Hardy's assessment of $(a)$ with respect to $(G2)$ (cf. Section 3.4.1) is much more plausible.

### 3.5.2 Mathematical Interest is Multifaceted

In the first sentence of $(\mathcal{D})$, Hardy states that examples $(a)$ and $(b)$ "are likely to amuse amateurs, but there is nothing in them which appeals much to a mathematician." What about the accuracy of this statement in light of the research that has been conducted on the general phenomenon of reverse multiples since the publication of Hardy's *Apology*?

Section 3.2 shows that quite a few researchers are/were interested in reverse multiples over the last sixty years or so. While some of them might qualify as "amateur mathematicians"—meaning that they have not done much other, more traditional work at the university research level—and in this sense maybe qualify as "amateurs" in Hardy's understanding, such as Sutcliffe, Holt and Kendrick,[10] this does not apply to everyone. For example, not to Lara Pudwell, who, although still a doctoral student at the time of publication of her (2007), is now a full professor in the department of mathematics and statistics at Valparaiso University; and not to Anne Ludington Young, who is an emeritus professor of mathematics at Loyola

[10] The online bibliographic database *MathSciNet* mentions in the case of Alan Sutcliffe only one further article which has been published in the academic journal of mathematics education *The Mathematical Gazette* also in 1966; besides the three papers on reverse multiples only one from 2005 which also appeared in *Integers* in the case of Benjamin Holt; and no other work in Lev Kendrick's case.

University Maryland; and, of course, not to the renowned mathematician Neil Sloane. At least these examples suggest that, although, as Kendrick appropriately points out, Sloane's list of references "is rather short for a problem given such exposure as a mention in Hardy's book" (2015, p. 2), Hardy's diagnosis that there is nothing in example $(a)$ "which appeals much to a mathematician" is not true in all its generality. Concerning the "appeal" of some of the research carried out on reverse multiples, Sloane makes some interesting comments to which we will now turn.

After having determined the generating function for the number of all base-10 reverse multiples, regardless of the multiplier (Equation (3.6)), Sloane (2014, p. 109) asks: "Would this generating function have changed Hardy's opinion of the problem?," which, most likely, is meant to mainly address the appeal of example $(a)$ for Hardy, based on what Sloane said in a talk about this topic. In this talk which he gave at the *Rutgers Experimental Mathematics Seminar* at Rutgers University on October 10, 2013, this question also appeared on his slides after he has determined the generating function (3.6). Although he did not read it out loud, he said with respect to his result "This might even have impressed Hardy" but added immediately after "Though I doubt it" (2013, October 10, 22:18–22:22).

Furthermore, Sloane explains in the abstract of his article: "These Young graphs are *interesting* finite directed graphs, whose structure is not at all well understood" (2014, p. 99, emphasis added). And, in his talk, he calls Ludington Young's (1992a; 1992b) "two really interesting papers" (2013, October 10, 6:10–6:18).

These are all statements in which Sloane expresses his own appreciation of (some results on) the phenomenon of reverse multiples.

We could even go a step further and argue with the help of Robert Thomas's (2017), that the articles mentioned in Section 3.2, because they all have been published in journals, already show value, namely the aesthetic value of being interesting. As Thomas (2017, p. 118) claims—mainly based on his ten years of experience as a managing editor of a mathematical research journal—"'interesting' is a *sine qua non* of publishable mathematical research" and, consequently, that it "is an aesthetic feature seen in all published mathematics."

In this context, one might wonder, however, whether one should treat all publications in the same way. In mathematics, there is an (alleged) distinction that is occasionally brought up, namely that between serious and *recreational* mathematics, which is perhaps also partly reflected in Hardy's discussion. As a reminder, examples *(a)* and *(b)* were taken, by Hardy, "almost at random, from Rouse Ball's *Mathematical Recreations*" (cf. $(\mathcal{A})$). So, if a paper, such as Grimm and Ballew's (1975-76) is published in a journal devoted to recreational mathematics, is then its aesthetic feature of being interesting of the same kind as a paper published in journals exclusively devoted to "serious mathematics"? Or should not also all the other work done on reverse multiples be characterized as rather recreational? Furthermore, is there a general difference *in kinds* between interesting serious and interesting recreational mathematics?

The problem with these questions is that there is no sharp boundary between "serious" and "recreational" mathematics. For instance, a problem may start out as a mere recreation, but develop into quite useful mathematics. As Ian Hacking (2014, p. 77) explains:

> There are paths that lead on from mere amusements to things that Hardy might count as serious. The example of squaring the square is a case in point. Tutte's own (1958) account of the events was published in Gardner's column. The problem began in part with Dudeny's puzzle of Lady Isabel's Casket (§1.28). It turned into moderately deep questions with applications to electrical analysis. And Hardy's collaborator Littlewood used the proof, of the impossibility of a cube dissection, as an exemplar of good proof. A silly puzzle evolves into

> fairly good maths.

There would be much more to say about the merits of recreational mathematics and its connection to more "serious" mathematics, but that is beyond the scope of this article. Let us conclude this section by returning once again to Neil Sloane.

As for him, he is not much concerned with such a distinction. In an interview published in *Quanta Magazine* (Klarreich, August 6, 2015), Sloane was asked whether he felt "that there is a divide between 'serious mathematics' and 'recreational mathematics'," or whether he tended "not to think in those terms" to which he answered:

> I don't think in those terms. I don't think there's much difference. If you look hard enough, you can find interesting mathematics anywhere. (ibid.)

Therefore, I think Sloane's disagreement with Hardy on the appeal of reverse multiples to a mathematician is a genuine example of the multifaceted nature of mathematical interest.

## 3.6 Conclusion

As we have seen, some work has been done on the general phenomenon of research multiples since the publication of Hardy's *Apology.* Among other things, particular finite, directed graphs, called Young graphs, were introduced to study this phenomenon.

I have argued, however, that this work does not (yet) threaten Hardy's claim that his example *(a)* is "not capable of any significant generalization." To do this, I have first argued that when read in its context, this phrase should be understood as an emphasis that there are no generalizations of this example (and of example *(b)*) that would qualify as "general" themselves, at least not in the sense or to the extent necessary to qualify as candidates for serious theorems. Then, with the help of what I take to be the most important aspect of Hardy's notion of generality and a necessary condition for an idea to be considered significant or a theorem to be serious, namely its *connectedness to a large complex of appreciably different mathematical ideas*, I have argued that Hardy's upshot is still tenable, which is in contrast to what several researchers, who worked on the general phenomenon of reverse multiples, say.

Finally, it has been suggested that especially Sloane's appreciation of some results on reverse multiples, in contrast to Hardy's generally dismissive attitude towards it, makes this phenomenon a genuine example of the multifaceted nature of mathematical interest.

# Appendix.

# Hardy's Two Main Examples of Serious Theorems

Hardy gives several examples of serious mathematical theorems, such as *the fundamental theorem of arithmetic* (pp. 96f.), but mainly focuses in his essay on the two simplest ones stemming from Greek mathematics for which he also provides proofs. Since I compare these proofs in Section 3.4.1 with a proof of Hardy's example *(a)*, I will present them here in order, following closely Hardy's expositions.

**Euclid's Theorem.** (pp. 92–94) *There are infinitely many prime numbers.*

*Proof.* Let us suppose that there are only finitely many prime numbers and that the complete series of primes is given by

$$2, 3, 5, \ldots, P$$

so that $P$ is the largest prime. Let us now consider the number $Q$ defined as the successor of the product of all prime numbers, i.e.,

$$Q = (2 \cdot 3 \cdot 5 \cdot \ldots \cdot P) + 1.$$

The number $Q$ is not divisible by any of $2, 3, 5, \ldots, P$, since it leaves the remainder 1 when divided by any one of these numbers. Now, if $Q$ is not *itself* a prime number, it is divisible by *some* prime, which is why there is a prime greater than any of the other primes which contradicts the hypothesis that there is no prime greater than $P$.

**Pythagoras's Theorem.** (pp. 94–96) $\sqrt{2}$ *is irrational.*

*Proof.* Let us assume that $\sqrt{2}$ is a rational number, i.e., it can be expressed as a fraction of two integers $a$ and $b$,

$$\sqrt{2} = \frac{a}{b}. \tag{3.7}$$

We may assume that $a$ and $b$ have no common factor, since otherwise we could simply remove it. Equation (3.7) implies

$$2 = \left(\frac{a}{b}\right)^2$$

which is equivalent to

$$a^2 = 2b^2. \tag{3.8}$$

From this it follows that $a^2$ is even which implies that $a$ itself is even (since the square of an odd number is odd), so that $a = 2c$ for an integer $c$. With this, we have

$$2b^2 = a^2 = (2c)^2 = 4c^2$$

or

$$b^2 = 2c^2$$

which implies that $b$ must be even. Now both $a$ and $b$ are even, that is to say that they have the common factor 2. However, this contradicts our hypothesis, so that $\sqrt{2}$ cannot be rational.

# Chapter 4

# Visual Proofs as Counterexamples to the *Standard View* of Informal Mathematical Proof?

## 4.1 Introduction

The relation between informal mathematical proofs and formal derivations in (suitable) formal systems is a much debated topic in the philosophy of mathematics in general and the philosophy of mathematical practice in particular. Here I concentrate on the epistemological side of the topic and am concerned with the question whether and, if so, to what extent this relation has something to do with how mathematicians' informal proofs secure mathematical knowledge. The focus is on a specific "derivational account of informal mathematical proof" together with a particular critique of it. According to the derivationists, as named by Tanswell (2015), the rigor and correctness of informal proofs depend (in some sense) on associated formal derivations.

While Jody Azzouni has defended a derivational account of informal mathematical proof himself (2004), he has developed an alternative approach to informal rigorous proof in his recent article (2020) which he calls "the algorithmic-device view." In this article, he explains why derivational accounts of mathematical proofs do not work and argues for the "superiority" of his algorithmic-device view. He argues explicitly against a specific derivational account, namely Hamami and Avigad's *standard view* of informal mathematical rigor and proof, among other things with the help of the visual/diagrammatic proof of the fact that $1/2 + 1/4 + 1/8 + 1/16 + \cdots = 1$ shown in Figure 4.1.[1] Note that when the talk is of "Hamami and Avigad's *standard view*" I am referring to two separate papers, namely (Hamami, 2022) and (Avigad, 2021). Since Avigad's work can be understood as a kind of augmentation of Hamami's model of the "standard view of mathematical rigor" which we will see later, and for the purposes of this text, it is convenient to talk about "Hamami *and* Avigad's derivational account/*standard view*," as sometimes Azzouni himself does.

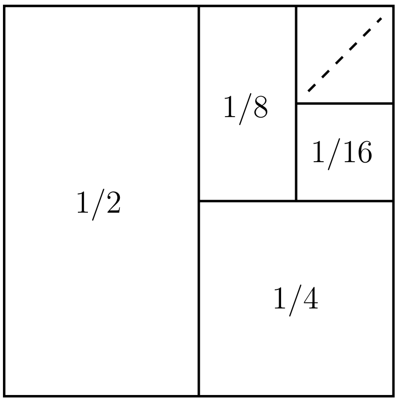


Figure 4.1: A visual proof of $1/2 + 1/4 + 1/8 + 1/16 + \cdots = 1$.

In the following, I will critically examine Azzouni's account of the visual proof and the conclusions he draws regarding Hamami and Avigad's *standard view*. In particular, using mathematicians' evaluations of visual proofs in general, which play a fundamental role in Hamami and Avigad's *standard view*, I will argue that

[1] A possible description of what is going on in the figure can be found in the initial paragraphs of Section 4.5.

Azzouni's criticism of their account is not valid. Nevertheless, by identifying a necessary condition for the visual proof to be considered a proper proof in the first place, and suggesting an appropriate way to establish its correctness, I will argue that his assessment of the epistemic process associated with the visual proof proves to be essentially correct. From this, I will conclude that although visual proofs do not constitute "counterexamples" to the *standard view* in the sense suggested by Azzouni, at least the one presented in Figure 4.1 shows that this view does not cover all the ways in which mathematical truth can be justified.

After having introduced, in Section 4.2, Azzouni's account of the visual proof and his critique towards Hamami and Avigad's *standard view*, their work is briefly discussed in Section 4.3. Section 4.4 is about how mathematicians themselves regard visual proofs. This is followed by a critical examination of Azzouni's critique in Section 4.5, which discusses, inter alia, the conditions under which the visual proof can turn out to be a proper proof (4.5.1) and what this means for the *standard view* (4.5.2).

## 4.2 Azzouni's Counterexample to the *Standard View*

In (2020), Azzouni uses the visual proof of $1/2 + 1/4 + 1/8 + 1/16 + \cdots = 1$ shown in Figure 4.1 to argue against Hamami and Avigad's *standard view*. In particular, he argues that this proof constitutes a counterexample to the "normativity thesis" which is "in a way" part of Avigad's *standard view*. We will see to what extent this is true in Section 4.3, where I will discuss their *standard view* in more detail. In Azzouni's own words:

> I've suggested in earlier work [(2009); S.W.] – in a way related to Avigad's [(2021); S.W.] approach to a normative role for formal derivations – that transcribability to a formal derivation has, in the contemporary

> setting, become a norm for informal rigorous proof. I want to end this section by revisiting considerations that cut against that idea. The problem is that there are informal rigorous mathematical proofs that are *counterexamples* to the normativity thesis. (Azzouni, 2020, p. 77)

Besides in this passage, he does not mention the expression "normativity thesis" again. Since he writes shortly after that whether "derivations correspond to informal proofs" is actually a norm is "ultimately, a sociological matter" (ibid., pp. 77f.), I take him to mean by the normativity thesis something like "the transcribability to a formal derivation *should* be considered a norm for informal rigorous proof." In the earlier work he is referring to in the indented quote above, he explains that

> The first point to observe is that formalized proofs have become the norms of mathematical practice. And that is to say: should it become clear that the implications (of assumptions to conclusion) of an informal proof cannot be replicated by a formal analogue, the status of that informal proof as a successful proof will be rejected. [. . . ] The norm is this: *There is* a formal analogue of a purported informal mathematical proof or else the latter fails to be a proof. (Azzouni, 2009, p. 14)

Based on this passage, I present the following characterization as a first attempt to specify what this thesis might amount to:

**(NT*)** Should it become clear that the implications (of assumptions to conclusion) of an informal rigorous proof cannot be replicated by a formal analogue, the status of that informal rigorous proof as a successful proof *should* be rejected.

His reasoning for the visual proof being a counterexample to the normativity thesis is as follows (cf. (Azzouni, 2020, p. 77)): **(i)** "Phenomenologically – notice –

this proof is *utterly convincing* as it stands" and **(ii)** "there is no sense in which it looks like it needs to be completed or filled in" from which he concludes that **(iii)** "neither epistemically nor normatively does this proof and many other informal rigorous proofs [. . . ] need supplementation of any sort." Finally, he states that **(iv)** this and many other informal rigorous mathematical proofs do not themselves "indicate the existence of formalizations that, in turn, justify why they're true: their content, that is, does nothing of this sort."[2]

Because of this—especially because of his statement **(iv)**—and the fact that the implications of the visual proof can in fact be captured and to a certain degree replicated by a formal analogue, which is also admitted by Azzouni himself,[3] the following appears to be a more appropriate characterization of the normativity thesis:

**(NT)** The transcribability to a formal derivation should be considered a *norm* for informal rigorous proof, i.e., an informal rigorous proof should indicate the existence of a formal counterpart that, in turn, justifies why it is true.

Besides the reference to an "indication relation" between informal rigorous proofs and their formal counterparts in the sense that an informal proof should indicate the existence of a formal analogue, there is a second essential component of Azzouni's "normativity thesis," namely that it deals exclusively with informal

[2]Notice that statement **(iv)** is closely related to one of Tanswell's five "minimal desiderata" of any derivational account of informal proofs, namely **(Content)** (cf. (Tanswell, 2015, pp. 297f.)), which are all approved by Azzouni. That is to say that any derivational account needs to provide an explanation for each of these aspects of mathematical practice in general or informal mathematical proofs in particular. In Azzouni's words, **(Content)** says that "[a]ny derivational explanation must explain how the perceived content of an informal rigorous mathematical proof – what the sentences of that proof are experienced to *say* – determines which formal proof(s) it indicates" (Azzouni, 2020, p. 10).

[3]In (2013), Azzouni explains with respect to this visual proof that he does not want to deny "that there is a sense in which the (visual) concepts involved in the pictorial-proof have been embedded or reconstrued in the $\epsilon$-$\delta$ proof" (2013, p. 330), where for the "$\epsilon$-$\delta$ proof" he has in mind the standard proof with $\epsilon$-$\delta$ techniques such as the one presented in (Brown, 1999). So, it seems that according to Azzouni, the now-standard proof is an appropriate semi-formal *analogue* of the visual proof, which, in turn, can be replicated rather straightforwardly by a formal analogue itself.

*rigorous* proofs. In accordance with this, he describes the visual proof shown in Figure 4.1 as a rigorous one. With respect to the (alleged) rigorousness of visual proofs, Azzouni explains in (2013) that

> it's been quite common, historically, to describe diagrammatic proofs as lacking 'in rigor'. This is so to the extent that, in the nineteenth century, if not before, it seemed reasonable to expunge diagrams altogether from mathematical proof along with reliance on 'intuition'. (2013, p. 324)

In this article, however, Azzouni argues that it is false to claim that diagrammatic proofs lack in rigor. He identifies several factors which people mistakenly regard as reasons to deny rigor to them. In particular, he argues against the suggestion that these proofs are not rigorous or defective because they involve unarticulated mathematical content. Although he does not give a very detailed account of mathematical rigor, I take him to implicitly assume something like the following sufficient condition for (informal) rigorousness that is related to his statement **(ii)** from above (cf. (Azzouni, 2013, p. 333), where also the following expressions in quotation marks come from): If there are no missing steps in the mathematical content of a proof—where the content does not need to be "explicit" (e.g. "explicated by axioms"), but which is nevertheless "playing a role enabling the proof procedure"—then this proof should count as rigorous. Except for one qualification, I think that statements **(ii)** and **(iii)** show that Azzouni in (2020) still holds on to this view. Since he states in footnote 142 on page 77 of (2020), that if "language-based transcriptions of something we see visually" are treated to be more explicit or as making something explicit in the first place, then only *by fiat*, let me reformulate the condition as follows:

**(R)** If there are no missing steps in the content of a proof—where the content does not necessarily have to be presented in a language-based form, but which is nevertheless playing a role enabling the proof-procedure—then this proof counts as *rigorous*.

Furthermore, with respect to the visual proof, Azzouni argues that **(v)**

> The epistemic process, rather, is the exact *reverse* of what normative and descriptive derivational accounts hypothesize. The intuitively effective procedures such proofs exhibit right on their surfaces, when preserved formally, simultaneously preserve the epistemic qualities (the phenomenology) of those informal proofs. The formalization inherits, that is, what it is about the informal proof that convinces us – what *justifies* our being convinced of the result of the proof. It's not, that is, that the formalization reveals what's convincing about that proof or that the formalization justifies that proof. (Azzouni, 2020, p. 77)

He concludes that **(vi)** this "is enough to show that – at least with respect to *many* informal rigorous mathematical proofs – derivation accounts are intrinsically misleading" (ibid.).

The "intuitively effective (recognition) procedures" which are mentioned in statement **(v)**, lie at the heart of Azzouni's algorithmic-device view of informal rigorous mathematical proofs. These are procedures that "mathematicians grasp directly and not via formal transcriptions of those procedures into the medium of formal languages" (ibid., p. 20). The notion of "intuitive" involved in the characterization is meant to refer to something which is computable or executable by a human being while an "effective procedure" or "effective method" which is expressible as a finite set of precise instructions is closely related to the notion of an algorithm (ibid., pp. 11–17).

Let us now take a closer look at Hamami and Avigad's account(s).

## 4.3 The *Standard View* of Mathematical Rigor and Proof

In a recent article, Yacin Hamami (2022) offers an elaborated formulation of, what he calls, "the standard view of mathematical rigor" (2022, p. 411). He traces this view back to the work by Saunders Mac Lane and Bourbaki. Hamami differentiates between a descriptive and a normative part of the *standard view*. The descriptive part—which might be called an account of *informal mathematical rigor*—is meant to "provide a characterization of the process by which mathematical proofs are judged to be rigorous in mathematical practice, i.e., by which the quality of being rigorous is attributed to mathematical proofs in mathematical practice" (ibid., p. 420). His general characterization of a descriptive account of mathematical rigor is given as follows (ibid., pp. 420f.):

A mathematical proof $P$ is rigorous$_{\mathcal{M}}$

$\Leftrightarrow$

$P$ can be verified by a typical agent in mathematical practice $\mathcal{M}$, using the resources commonly available to the agents engaged in $\mathcal{M}$.

$\Leftrightarrow$

Every mathematical inference $I$ in $P$ can be verified by a typical agent in mathematical practice $\mathcal{M}$, using the resources commonly available to the agents engaged in $\mathcal{M}$.

Hamami's particular description of the descriptive part of the *standard view* expresses the last specification of the characterization above in terms of decomposition and verification processes. That is to say that when confronted with a proof $P$, agents in a practice $\mathcal{M}$ verify specific proof steps by decomposing these steps into smaller steps (if needed) until they can verify them with the help of mathematical inference rules, which Hamami calls *higher-level rules of inference*, that were acquired during their former studies (ibid., pp. 422ff.).[4]

Hamami explains that in general, "a *normative account* of mathematical rigor stipulates one or more conditions that a mathematical proof ought to satisfy in order to qualify as rigorous" (ibid., p. 411). The specific normative part of the *standard view* is now given by the following characterization (ibid., p. 428): A mathematical proof $P$ is rigorous in the normative sense if and only if it "can be *routinely translated* into a formal proof." It is with respect to this part of the view that Hamami claims that it is "almost an orthodoxy among contemporary mathematicians" (ibid., p. 409). He develops in his article a precise conception of the notion of "routine translation" by first differentiating between four "levels of granularity" and then elaborating three successive translations, i.e., algorithmic procedures, each from one level of granularity to the next finer level. At the coarsest level, which he calls the "vernacular level," the mathematical proof "is a sequence of inferences as commonly presented in the ordinary mathematical texts of mathematical practice $\mathcal{M}$" (ibid., p. 429).

With the help of the machinery presented in his article, Hamami can show that if a proof is informally rigorous, it is also rigorous in the normative sense, i.e., it can be routinely translated into a formal proof (ibid., pp. 433f.).

In (2021), in which the philosopher and mathematician Jeremy Avigad defends the *standard view*, he explains that according to this view, "an informal mathematical statement is a theorem if and only if its formal counterpart has a formal derivation" and that a judgment as to the correctness of a mathematical proof "is tantamount to a judgment as to the existence of a formal derivation, and whatever psychological processes the mathematician brings to bear, they are *reliable insofar as they track the correspondence*" (2021, p. 7379, emphasis added). Avigad further explains that informal proof texts are "high-level sketches that are intended to *indicate* the existence of formal derivations" (ibid., p. 7381, emphasis original)

[4]For more on these rules, see the Appendix to this chapter.

and that informal proofs "work" in this way (ibid., p. 7394). To be sure, when Avigad talks about "informal proofs" he is referring to informal mathematical proofs that are in line with mathematicians' *contemporary* proof practice. Since he explicitly mentions Hamami's work, among others, as an example in which his general viewpoint has been articulated (ibid., p. 7379) (another example is Burgess's (2015) account where he characterizes *rigor* as, among other things, "[t]he quality whose presence in a purported proof makes it a genuine proof by present-day journal standards" (2015, p. 2)), we may take him to be talking about informal *rigorous* proofs. Together with Avigad's statements above, one can see that the normativity thesis **(NT)** is indeed—at least "in a way"—part of his *standard view*, which is in accordance with Azzouni's assessment. (Notice that while Avigad speaks of the existence of formal derivations, Hamami instead speaks "only" of the existence of a routine translation which is able to turn a proof $P$ into a formal one (cf. (Hamami, 2022, p. 432, footnote 25)).)

Now, with respect to Hamami's model of informal rigor, i.e., the descriptive part of the *standard view*, Avigad, while broadly accepting it, nevertheless has some reservations. He believes that

> Hamami's model is essentially correct: when we read an informal mathematical proof, we really do try to expand inferences in order to gain confidence that a much more detailed version could be given, down to the kinds of basic inferences that twentieth century logic has shown can be reduced to axiomatic primitives. At the same time, we can be convinced by an informal proof without carrying out a fully detailed expansion, and it is too much to ask that we reach the point where each inference is an instance of a known theorem or an explicit rule we have stored in memory. (Avigad, 2021, pp. 7393f.)

In order to bridge this gap with respect to Hamami's criterion of informal rigor, Avigad discusses in his article several strategies, such as *modularize*, *generalize* and *visualize*, which are employed by mathematicians to ensure reliable and robust assessments concerning the correctness of informal mathematical proofs. In fact, he presents these "common features of mathematical practice" as "normative dictates, strategies that one might urge upon an aspiring young mathematician" (ibid., p. 7388).

Since the judgments and evaluations of (contemporary) mathematicians play a fundamental role in Hamami's (and Avigad's) descriptive part of the *standard view*, we will now have a look at how mathematicians themselves regard visual proofs such as the one shown in Figure 4.1.

## 4.4 Mathematicians on *Visual Proofs*

How do (contemporary) mathematicians regard visual proofs? Roger Nelsen, for instance, who has edited three volumes on "proofs without words" (PWWs) (as visual proofs are often referred to in the mathematical literature), states in the introduction of the first volume in which Figure 4.1 can be found on page 118, that "[o]f course, 'proofs without words' are not really proofs" (1993, p. vi). This statement is somewhat weakened in the introduction of the second volume, where he explains that "[o]f course, some argue that PWWs are not really 'proofs'" but goes on by quoting a passage by James Brown from his *Philosophy of Mathematics – An Introduction to the World of Proofs and Pictures* (Brown, 1999) in which Brown states that "pictures can prove theorems" (Nelsen, 2000, p. x).[5] However, this is again relativized in (Alsina and Nelsen, 2010), where Nelsen and his co-author Claudi Alsina describe PWWs as "pictures or diagrams that help the reader see *why* a particular mathematical statement may be true, and also to see *how* one might begin to go about proving it true" (2010, p. 118).

As another example, consider the two mathematicians Peter Borwein and Loki

[5] Brown (1999) argues for this from a Platonist point of view. Figure 4.1 is one of his examples of "picture-proofs," as he calls them (cf. (1999, Chapter 3)).

Jörgenson who write in their article "Visible Structures in Number Theory" (for which they won a *Paul R. Halmos – Lester R. Ford Award* from the *Mathematical Association of America* which recognizes "authors of articles of expository excellence published in *The American Mathematical Monthly*" (Paul R. Halmos - Lester R. Ford Awards, 2023)) that

> The value of visualization hardly seems to be in question. The real issue seems to be what it can be used for. Can it contribute directly to the body of mathematical knowledge? Can an image act as a form of "visual proof"? Strong cases can be made to the affirmative [they mention two references, one of which is an article again by James Brown; S.W.] (including in number theory), with examples typically in the form of simplified, heuristic diagrams such as Figure [4.2 (see below); S.W.]. These carefully crafted examples call into question the epistemological criteria of an acceptable proof. (Borwein and Jörgenson, 2001, pp. 898f.)

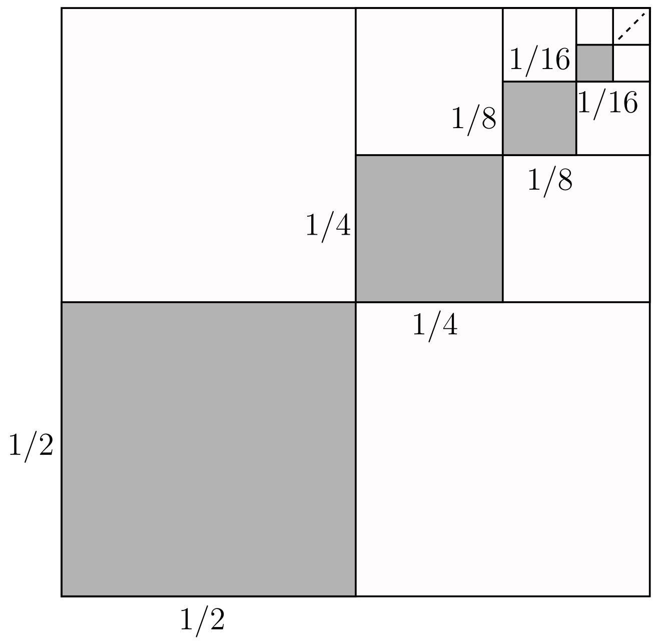


Figure 4.2: A visual proof of $(1/2)^2 + (1/4)^2 + (1/8)^2 + (1/16)^2 + \cdots = 1/3$.

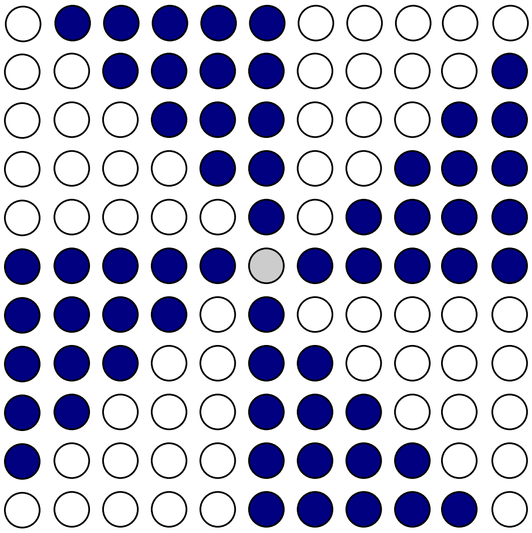

Figure 4.3: A visual proof that $n$ odd implies $n^2 \equiv 1 \pmod 8$ (Nelsen, 2008, p. 8).

Note that although Borwein and Jörgenson seem inclined to grant specific diagrams the status of "visual proofs" ("strong cases can be made"), they nevertheless refer to them as "heuristic diagrams."

Besides what these individual mathematicians have to say about visual proofs, there is also a survey study conducted by Weber and Czocher (2019) in which the executors asked ninety-four mathematicians from universities in the United Kingdom to judge the visual proof shown in Figure 4.3—in addition to an empirical, a computer-based and two "prototypical" proofs—regarding its validity. When the participants were asked "If you were forced to choose, would you say that this argument is a valid proof?," 38% chose "This is not a valid proof" (2019, pp. 259f.). Note that this visual proof is quite different from the proofs shown in Figure 4.1 and 4.2 which raises the question whether the evaluations of mathematicians concerning this specific visual proof should be considered representative of a whole class of proofs. However, the general handling of visual proofs, especially the fact that they are almost all listed in the mathematical literature under the heading of PWWs, suggests this.[6]

These findings suggest that the general status of visual proofs, i.e., whether they should count as *proper* or *valid* proofs, is (at least to some degree) controversial among (contemporary) mathematicians. Obviously, whether these "proofs" should count as *rigorous proofs*—insofar as one can legitimately (or wants to in the first place) distinguish between *valid* and *rigorous* proofs—is then not less controversial.[7] This assessment is consistent with Azzouni's own statement which we have already seen in Section 4.2 that "it's been quite common, historically, to describe diagrammatic proofs as lacking 'in rigor'" (2013, p. 324).[8]

[6]In the introduction of (Alsina and Nelsen, 2006), the authors write that

> Mathematical drawings related to proofs have been produced since antiquity in China, Arabia, Greece and India but only in the last thirty years has there been a growing interest in so-called "proofs without words." Hundreds of these have been published in *Mathematics Magazine* and *The College Mathematics Journal*, as well as in other journals, books and on the World Wide Web. Popularizing this genre was the motivation for the second author of this book in publishing the collections [(Nelsen, 1993, 2000); S.W.]. (2006, p. ix)

The visual proof shown in Figure 4.3 appears in this work on page 145 as a proof of $8T_n + 1 = (2n + 1)^2$, where $T_n = 1 + 2 + \cdots + n$ denotes the $n$th triangular number.

[7]One can speculate that Weber and Czocher's study shows even more than what has been said so far. Mathematicians were also asked to evaluate the visual proof shown in Figure 4.3 with respect to a "more fine-grained view of validity" as Weber and Czocher call it. In that regard, even 78% of the participants characterize this proof as invalid in at least some contexts. I think it is not too much of a stretch to suggest that probably many were thinking of mathematical contexts in which rigorous proof is required. But this is pure speculation, as participants were not asked to specify the contexts more precisely.

[8]The case of visual proofs or proofs without words discussed here is a rather extreme one. Many recent studies in the philosophy of mathematical practice do not focus on these particular diagrams, but on those that play an important role in (contemporary) mathematical reasoning and that can even be part of a published modern proof (see, for instance, (Carter, 2010), (De Toffoli and Giardino, 2015) and (De Toffoli, 2017)). In the Appendix to this chapter, with reference to Hamami's *standard view*, I say a little more about mathematical diagrams that form an integral part of the (proof) practices of some mathematical areas.

## 4.5 Azzouni's Critique towards the *Standard View* Revisited

In this section, I have a closer look at Azzouni's critique of the normativity thesis and Hamami and Avigad's *standard view* in general. Before I go into more detail on this, however, let me give one possible description of what is going on in Figure 4.1, which will play a role later on.

The numbers written on the rectangles suggest that these rectangles stem from a successive bisecting process. We might imagine two squares with the same area (for convenience, we may assume that they have an edge length of 1) where we use parts of the first square to cover parts of the second square. The process starts by bisecting the first square in order to get two rectangles and using one of them to cover the left side of the second square (this square is meant to be positioned as the one in Figure 4.1). Now, we halve the remaining rectangle of the first "square" to get two smaller squares, and use one of them to cover the lower right corner of the second square, and so on and so forth, so that the second square is covered more and more by the parts of the first "square." By referring to the areas of the rectangles, this process can be represented as follows (where the last summand in each case denotes the area of the remaining part of the first "square," respectively the part of the second square that is not yet covered):

$$
\begin{aligned}
1 &= \frac{1}{2} + \frac{1}{2} \\
&= \frac{1}{2} + \frac{1}{4} + \frac{1}{4} \\
&= \frac{1}{2} + \frac{1}{4} + \frac{1}{8} + \frac{1}{8} \\
&= \frac{1}{2} + \frac{1}{4} + \frac{1}{8} + \frac{1}{16} + \frac{1}{16} \\
&= \frac{1}{2} + \frac{1}{4} + \frac{1}{8} + \frac{1}{16} + \frac{1}{32} + \frac{1}{32}
\end{aligned}
\tag{4.1}
$$

and so forth, so that in the $n$th step we have $1 = (\sum_{k=1}^{n}(1/2)^k) + (1/2)^n$ or, equivalently, $1 - (1/2)^n = \sum_{k=1}^{n}(1/2)^k$. That is, in each step, the last summand of the previous step is expressed as the summation of its bisection with its bisection. This process could in principle be performed infinitely often, so that the last summand (or the remaining area of the first "square," respectively the part of the second square that is not yet covered) becomes infinitesimally small and the series $1/2 + 1/4 + 1/8 + 1/16 + \ldots$ should be assigned the value 1 (if any at all). Note that I propose here "to make the jump to an infinite summation" as a "modern reader is inclined to" do, as expressed by David Bressoud in his "radical approach" to real analysis (Bressoud, 2007, p. 11), while this would have been avoided, for instance, by the Greeks of the classical era, such as Archimedes (ibid., pp. 9ff.).

### 4.5.1 Figure 4.1 and the Corresponding Epistemic Process

I think that Azzouni's characterization of the epistemic process with respect to the visual proof shown in Figure 4.1 which he gives in **(v)**, in particular that it is not the case that a formalization would reveal what is convincing about the visual proof in the first place or that it justifies this proof, is essentially correct. However, I think there is an implicit assumption by Azzouni that needs to be made explicit and argued for in a proper way to give a full explanation of why his characterization in **(v)** is appropriate.

With that in mind, let us distinguish between an intuitive, pre-formal notion of something that could in principle be repeated infinitely often or that refers to infinity in one way or another and an exact, i.e., "rigorous" (with respect to modern standards) mathematical definition thereof, such as the modern definition of an infinite series or the sum of a convergent infinite series. Let us write the "mathematical theorem" suggested by the visual proof which corresponds to the first, pre-formal understanding as "$1/2+1/4+1/8+\cdots = 1$," where the dots "$\ldots$" are referring to the involved (potential) infinite process, and let "$\sum_{k=1}^{\infty}(1/2)^k = 1$"

denote the theorem which appears in modern mathematical textbooks or exercise sheets. The description of what is going on in Figure 4.1 which I gave above shows that it is quite reasonable to judge the figure as a proof of "$1/2+1/4+1/8+\cdots = 1$" or why one is inclined to judge it as "utterly convincing" as Azzouni does in **(i)**. But, of course, Azzouni wants the visual proof to be understood as a proof of "$\sum_{k=1}^{\infty}(1/2)^k = 1$" and his comments **(v)** and **(vi)** are to be interpreted in this sense (see also (Azzouni, 2013, pp. 329f.)). This means that he has to assume some sort of link between these two interpretations, probably something like the following:

**(Lim)** The (an) intuitive notion of the sum of the series involved in the intuitively effective procedure that Figure 4.1 exhibits right on its surface is compatible with the now-standard, "rigorous" definition of the sum as the limit of the sequence of partial sums using the $\epsilon$-$\delta$ terminology.

By "compatible with" I mean that the now-standard definition should not rule out the (an) intuitive notion of the sum of the series which one is inclined to read into the diagram.

So I claim that the correctness of **(Lim)** is a necessary condition for Figure 4.1 to constitute a (visual) proof of the mathematical theorem "$\sum_{k=1}^{\infty}(1/2)^k = 1$." If there were no such connection between the visual proof and the mathematical theorem that appears in current textbooks, I cannot see how the former could ever constitute a proof of the latter.

Now, how can we know that **(Lim)** is correct? One way would be to simply verify that the now-standard $\epsilon$-$\delta$ technique proves this series to be convergent. Azzouni claims in **(v)**, however, that it is not the formalization of the visual proof that justifies it, but the diagram itself. This would not follow if we justified **(Lim)**—which, as I have just pointed out, is a necessary condition for the visual proof to constitute a proof of the mathematical theorem "$\sum_{k=1}^{\infty}(1/2)^k = 1$"—with reference to the semi-formal version of it. Note that if this were the only way to establish **(Lim)**, the visual proof could not be considered a real proof either, since one would have to prove the theorem by another method before one could prove it using the diagram. I submit, however, that there is another way of justifying **(Lim)** which does not lead to this result so that Azzouni's evaluation of the epistemic process in (v) can still turn out to be valid: One can also establish **(Lim)** by showing that mathematicians in the development of the calculus tried to capture the (main) intuitions underlying the visual proof shown in Figure 4.1—possibly in the form of the Equations (4.1)—with their definitions of the sum of a series.

Even though the following two excerpts from the history of mathematics might not necessarily prove the correctness of **(Lim)** themselves, they strongly suggest that one can justify **(Lim)** in the way just described: For instance, in his *De seriebus divergentibus* from 1760 (which was roughly a century before "[w]ith Weierstrass, the now-accepted $\epsilon$-$\delta$ terminology became part of the language of rigorous analysis" (Burton, 2011, p. 620)), Leonhard Euler refers to the series $1 + 1/2 + 1/4 + 1/8 + \cdots = 2$ in his characterization of a convergent series as a clear example of this "phenomenon":

> And now, series are said to be convergent when their terms steadily become smaller and at length completely vanish, such as this one: $1 + 1/2 + 1/4 + 1/8 + 1/16 + 1/32 +$ *etc.*, whose sum is in fact $= 2$, without any doubt. For as you add in more terms, you draw closer to 2; thus the sum of 100 terms falls short of 2 by a very small amount, indeed a fraction with numerator 1 and a denominator made up of 30 digits. Therefore, with such a series, there is no doubt that it indeed has a sum and that the sum which is assigned in analysis is correct. (Barbeau and Leah, 1976, p. 143)

As another example, consider Cauchy's still "unrigorous" definition (with respect to modern standards) of the convergence of series presented in his *Cours* *d'Analyse* from 1821. Especially when the calculations or relations indicated by the visual proof are written as in (4.1), one can see how they fit nicely with this definition: Cauchy calls a series convergent if and only if its sequence of partial sums $s_n$ "tends to a certain limit $s$ for increasing values of $n$" (Birkhoff, 1973, p. 3) where by "limit" he means that "[w]hen the values successively attributed to the same variable approach indefinitely a fixed value, eventually differing from it by as little as one could wish, that fixed value is called the *limit* of all the others" (ibid., p. 2). Immediately after the presentation of his definition of the convergence of series, Cauchy briefly discusses "one of the simplest sequences" which is the geometric progression $1, x, x^2, x^3, \ldots$ for which one finds that

$$1 + x + x^2 + \cdots + x^{n-1} = \frac{1}{1-x} - \frac{x^n}{1-x}$$

and whose sum is $1/(1-x)$ if the magnitude of $x$ is less than unity (ibid., p. 3). If we start with the term $u_1 = x$ (or subtract the value 1) and set $x = 1/2$, we of course get the series that is currently being discussed.

Let me conclude this section with a comment on Azzouni's statement **(vi)**. Insofar as mathematicians throughout history tried to capture with their definitions of the convergence of series the intuitive notion of the sum of the series involved in the intuitively effective procedure that Figure 4.1 exhibits right on its surface as the historical findings from above suggest (at least to a certain degree), I think that an account of informal mathematical proof would in fact be "intrinsically misleading"—as Azzouni states in **(vi)**—if it suggested that the truth of the visual proof can be justified only by its formalization. However, that Hamami and Avigad's account is not susceptible to the accusation of being "intrinsically misleading" in this respect will be shown, inter alia, in the next section.

### 4.5.2 Visual Proofs as Counterexamples to the *Standard View*?

As we have already seen in Section 4.2, Azzouni (2020) argues against the normativity thesis **(NT)** with the help of the visual proof shown in Figure 4.1 which is meant to constitute a counterexample towards it. Insofar as **(NT)** is part of the *standard view* (which it actually appears to be as indicated in Section 4.3), does this imply that the visual proof constitutes a counterexample to this view itself? I claim that the answer is no, even if one were to agree with Azzouni's estimation expressed in **(iv)**, that the visual proof does not indicate a formalization—which seems not to be uncontroversial, since it is a general statement about all possible "indication relations" (where his own from his earlier work (2004) and Hamami's "routine translation" are only two of them).

The reason for this is that crucial to Hamami and Avigad's models of informal rigor is the assessment of proofs by mathematicians themselves (keyword "descriptive part"). The findings of Section 4.4—especially the quotations of the individual mathematicians—suggest that even the general status of visual proofs among mathematicians is controversial, i.e., whether Figure 4.1, for example, qualifies as a proof of "$\sum_{k=1}^{\infty}(1/2)^k = 1$" (referring to the interpretation introduced in Section 4.5.1) in the first place, not to mention whether they should be considered rigorous.[9]

As we have seen in Section 4.3, Hamami's descriptive part of the *standard view* is meant to characterize the process by which "a typical agent in mathematical practice $\mathcal{M}$" attributes the quality of being rigorous to mathematical proofs. But even if one granted that the creation of PWWs constituted one of these practices, Nelsen's statements above suggest that "a typical agent" of this practice would not

[9] My speculation in Footnote 7 even suggests that a criterion such as **(R)** and/or that visual proofs satisfy the necessary condition for **(R)** that there must be no missing steps in the content of the proofs—which Azzouni addresses with his statement **(ii)** regarding the visual proof shown in Figure 4.1—are not commonly accepted by mathematicians.

characterize these proofs as rigorous. Due to Avigad's generally affirmative attitude towards Hamami's account and his own focus on contemporary mathematical practice and its practitioners, I take this to mean that the visual proof shown in Figure 4.1 does not constitute a counterexample to **(NT)** from the perspective of the *standard view*, since this view is simply not concerned with this genre of proofs. This also means that if Figure 4.1 indeed constituted a counterexample to **(NT)** as claimed by Azzouni, this interpretation of the normativity thesis would not be part of the *standard view*, since it would deal with a different notion of informal *rigorous* proof, (partially) expressed, for instance, in **(R)**.

As I have argued in Section 4.5.1, the visual proof shown in Figure 4.1 can indeed be seen as a proper proof of the mathematical theorem "$\sum_{k=1}^{\infty}(1/2)^k = 1$," although one has to establish the correctness of the necessary condition **(Lim)** first without having to confirm that the now-standard $\epsilon$-$\delta$ technique proves this series to be convergent. In light of this, the corresponding epistemic process as described by Azzouni in (v) turns out to be essentially correct. However, this does not imply that Hamami and Avigad's account of the *standard view* is intrinsically misleading. This is due to the same reason that the visual proof is not a counterexample to the normativity thesis from the point of view of their account: It is not intrinsically misleading with respect to visual proofs, because it does not deal with that type of proof. What it shows, however, is that the *standard view* does not cover all the ways in which mathematical truth can be justified. Hamami starts his investigation with the words

> Mathematical proof is the primary form of justification of mathematical knowledge. But in order to count as a *proper* mathematical proof, and thereby to function *properly* as a justification for a piece of mathematical knowledge, a mathematical proof must be *rigorous*. (2022, p. 409)

However, as the discussion of the "epistemic process" of the visual proof of

Figure 4.1 has shown, an informal mathematical proof can be *proper*—in the sense that it functions properly as a justification for a piece of mathematical knowledge—without being *rigorous* (in Hamami's descriptive sense).[10]

## 4.6 Conclusion

We have seen that, according to Azzouni, the visual proof shown in Figure 4.1 constitutes a counterexample to the "normativity thesis" that is part of Avigad's *standard view* and which says that the transcribability to a formal derivation should be considered a norm or standard of correctness for informal rigorous proof. It has been argued, however, that from the point of view of Hamami and Avigad's *standard view*, the visual proof does not constitute a counterexample to this thesis and thereby no counterexample to their *standard view* in general. This is the case, because the *standard view* is not concerned with this genre of proofs: Crucial to the view are the judgments about the rigorousness of a mathematical argument by the mathematicians themselves. And many comments made by mathematicians and a survey study suggested that even the general status of visual proofs is controversial, not to mention whether they should be considered rigorous.

Furthermore, we have seen that from an evaluation of the epistemic process associated with the visual proof, Azzouni concludes that with respect to this specific one and many other informal proofs the *standard view* is "intrinsically misleading." This conclusion was rejected for the same reason that the visual proof is not a counterexample to the normativity thesis from the perspective of the *standard view*: It is not intrinsically misleading with respect to visual proofs,

[10]Note that although we have seen that visual/diagrammatic proofs are not too much of a problem for Hamami and Avigad's *standard view*, there is a legitimate concern, especially with respect to Hamami's model: His characterization of the *standard view* appears to preclude any mathematical diagram from being an essential part of a rigorous mathematical proof. That this would indeed be a real deficit confirms a quick look at, for instance, contemporary homological-algebraic, category-theoretical or knot-theoretical proof practice. I will address this issue in the Appendix to this chapter below.

because it does not deal with that type of proof. I further identified the need for a connection between the intuitive notion of the sum of the series one is inclined to read into the diagram and the now-standard, rigorous definition of it, as a necessary condition for the visual proof to constitute a proper proof, and suggested a way in which one can establish its correctness, namely with the help of the history of mathematics, that also proves Azzouni's assessment of the epistemic process to be essentially correct. From this, I concluded that although visual proofs do not show that the *standard view* is intrinsically misleading, at least the one mentioned above shows that this view does not cover all the ways in which mathematical truth can be justified.

# Appendix.

# Mathematical Diagrams and Hamami's *Standard View*

We have seen that visual proofs (or "proofs without words") are not too much of a problem for Hamami and Avigad's *standard view*, but what about other types of mathematical diagrams? A closer reading of Hamami's (2022) seems to suggest that he does preclude any mathematical diagram from being an essential part of an informal rigorous proof. In addition to the fact that he does not mention diagrams at all, he only talks of "ordinary mathematical texts of a given mathematical practice" (2022, p. 429) without specifying what he means by these "ordinary texts." Do they include diagrams? A look at his characterization of *higher-level rules of inference* (hl-rules) suggests that they do not. (Remember that these rules are possessed by mathematical agents of a mathematical practice $\mathcal{M}$ and acquired in the course of their former studies and that they are part of the descriptive account of mathematical rigor which is meant to characterize the process by which mathematical proofs are judged to be rigorous in $\mathcal{M}$ (cf. Section 4.3)). He explains that an hl-rule "is entirely determined by its *inference schema*, which is a pair composed of a *premiss schema* and a *conclusion schema* consisting, respectively, of a set of schemas for the premisses and a schema for the conclusion" where "a schema is a template or pattern composed of placeholders and of symbols from the vocabulary of the language of the mathematical practice $\mathcal{M}$, together with some specifications on how the placeholders are to be filled in to generate mathematical propositions in the language of $\mathcal{M}$" (2022, p. 424). As an example, he presents the inference schema for mathematical induction which can be given by

$$H(0),\ H(X) \to H(X+1) \Rightarrow H(Y),$$

"where $H$ is a placeholder for an expression involving an arbitrary variable ranging over $\mathbb{N}$, and $X$ and $Y$ are placeholders for arbitrary variables ranging over $\mathbb{N}$" (ibid., p. 425).

He further explains that his notion of inference schema "corresponds exactly to what Corcoran (2014) calls an *argument-text schema*" (2022, p. 425, footnote 16). These schemas consist of a *set of sentences* which are called the premises and a *single sentence* which is called the conclusion. This identification and the focus on texts and sentences suggest that Hamami excludes diagrams from playing any role in the characterization of higher-level rules of inference.

Now, when we look at some mathematical areas, such as category theory (cf. for example (Carter, 2021)), homological algebra (cf. for example (De Toffoli, 2017)) and knot theory (cf. for example (De Toffoli and Giardino, 2014, 2016)), one realizes that diagrams form an integral part of the (proof) practices. In the following, I would like to present two examples, each of a different diagram type, which have been discussed by Silvia De Toffoli in (2017) and (forthcoming), to emphasize that it would not only be a real shortcoming of the *standard view* if it excluded diagrams in general, but also if they played no role in the characterizations of hl-rules in particular.

The first example is from algebraic topology and is about *arrow diagrams* which, in general, (may) appear in the study of 2-dimensional manifolds (also called *surfaces*). For instance, William Massey (1991) makes use of these diagrams in the proof of the classification theorem for compact surfaces in his graduate textbook in algebraic topology. Here is an excerpt from his proof:

> We wish to show that our surface can be transformed so that any pair of edges of the second kind are adjacent to each other. Suppose we have a pair of edges of the second kind which are nonadjacent, as in Figure [4.4(a)—note that the figure below is a self-made copy of the original one; S.W.]. Cut along the dotted line labeled $a$ and paste

> together along $b$. As shown in Figure [4.4(b); S.W.], the two edges are now adjacent. (Massey, 1991, pp. 21f.)

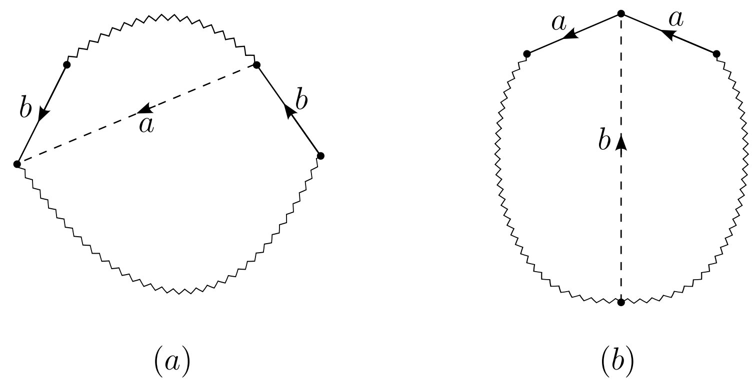


Figure 4.4: A step in the proof of the classification theorem for compact surfaces after Massey (1991).

The zigzag is meant to indicate that any combination of arrows could appear in its place. Moreover, note that the *cutting* and *gluing* or *pasting together* correspond to precise topological operations. As De Toffoli explains, "gluing corresponds to forming a quotient space, and cutting is the inverse operation of gluing" (forthcoming, p. 9). She argues that arrow diagrams (for surfaces) should not be seen as mere illustrations. They form a sound (diagrammatic) notational system regarding their interpretation, i.e., they can be essential to mathematical proofs precisely because "(i) they satisfy well-formedness conditions, and (ii) they are subject to precise rules of manipulation" (ibid., p. 21). That is to say that mathematicians can *reliably* manipulate these diagrams where these manipulations "correspond to specific mathematical operations" (ibid., 20).

Another, quite important type of diagrams are the so-called *commutative diagrams* (CDs) which play essential roles in homological algebra and category theory. In (2017), in which De Toffoli discusses *diagram chasing*—a proving technique that makes use of these diagrams—she presents, inter alia, the following theorem together with a proof of its first part (2017, pp. 171–173):

**Strong version of the Five Lemma.** *Let the diagram in Figure 4.5 be commutative and such that its rows are exact. If $f_2$ and $f_4$ are surjective and $f_5$ is injective, then $f_3$ is surjective. Symmetrically, if $f_2$ and $f_4$ are injective, and $f_1$ is surjective, then $f_3$ is injective.*

$$\begin{tikzcd}
A_1 \arrow[r, "\alpha_1"] \arrow[d, "f_1"] & A_2 \arrow[r, "\alpha_2"] \arrow[d, "f_2"] & A_3 \arrow[r, "\alpha_3"] \arrow[d, "f_3"] & A_4 \arrow[r, "\alpha_4"] \arrow[d, "f_4"] & A_5 \arrow[d, "f_5"] \\
B_1 \arrow[r, "\beta_1"] & B_2 \arrow[r, "\beta_2"] & B_3 \arrow[r, "\beta_3"] & B_4 \arrow[r, "\beta_4"] & B_5
\end{tikzcd}$$

Figure 4.5: Commutative diagram for the 5-Lemma.

Note that the $A_i$'s and $B_i$'s denote abelian groups and the $\alpha_i$'s, $\beta_i$'s and $f_i$'s homomorphisms.

De Toffoli explains, that

> Through the display of the 5-lemma and of the FTHA [i.e., the Fundamental Theorem of Homological Algebra; S.W.], it is clear that CDs play an essential role not only in proofs, but also in defining the relevant content-matter in the statement of a theorem. In fact, without diagrams, even stating the results would be difficult. (ibid., p. 175)

In Chapter 3, we have encountered a different type of diagram, namely Ludington Young's rooted trees (or Sloane's Young graphs for that matter). So, as an additional, final example where diagrams occur even in the statement of a theorem, let me mention a theorem by Ludington Young (1992b, p. 169):

$$\textit{Theorem 2:}\ \text{If}\quad \begin{array}{l} (r,s) \\ \big|\,(a,b) \\ (u,v) \end{array} \quad \text{then} \quad \begin{array}{l} (v,u) \\ \big|\,(b,a) \\ (s,r) \end{array} \quad .$$

I conclude that if we want hl-rules to be part of a faithful characterization of the process by which proofs are judged to be rigorous in practice, there should be room for mathematical diagrams to appear in their characterizations and formulations. As a possible example for such an hl-rule, consider Ludington Young's theorem from above. An hl-rule that a typical agent dealing with the general phenomenon of reverse multiples possesses after encountering this theorem can then be given by

$$\begin{array}{c} (R,S) \\ \Big|\,(A,B) \\ (U,V) \end{array} \;\Rightarrow\; \begin{array}{c} (V,U) \\ \Big|\,(B,A) \\ (S,R) \end{array}$$

where $A, B, R, S, U$ and $V$ are placeholders for expressions denoting non-negative integers less than an integer $g$ (the base of a given number system) and which satisfy (cf. Equations (3.3))

$$\begin{array}{ccccccccc} kB & + & S & = & A & + & Vg \\ kA & + & U & = & B & + & Rg \end{array}$$

for some integer $k$ with $1 < k < g$.

Insofar as it is generally the case that mathematical diagrams appearing in the contemporary mathematical literature in the statements of theorems and/or in proofs—possibly together with some manipulations of these diagrams—correspond to precise mathematical concepts and well-defined mathematical operations (cf. also (De Toffoli, 2021, 2022)), these do not seem to pose too much of a problem for Hamami's *standard view* in general and his characterization of a *routine translation* in particular. However, a detailed analysis of this issue is beyond the scope of this thesis.

# Chapter 5

# Conclusion

In this dissertation, several aspects of the growth and nature of mathematical knowledge were explored in more detail.

In Chapter 2, I returned to the origins of PMP and was mainly concerned with the question of what mathematical progress is. My investigation was based on an article by Maddy about this topic where she considers only contributions “of some mathematical importance” as progress. She characterizes a bit of mathematics as important if and only if it effectively furthers the goals of the practice in which it is embedded. With the help of my own case study—a contribution from tropical geometry—I identified several issues with Maddy’s proposal that are either not addressed at all or not satisfactorily addressed by her. The most important of which were

- that a bit of mathematics can be embedded in different practices;
- the question of what makes a goal a proper goal of a practice; and
- that it seems valuable for an account of mathematical progress to also address the question *of the extent* to which a bit of mathematics constitutes progress.

Taking these issues into consideration, I developed an alternative account of "mathematical importance," broadly within Maddy's framework of progress, for which I introduced the notion of a "valuable goal." The "value" of a goal was roughly characterized by a positive evaluation regarding its being interesting (in the sense of Hardy's third question of peer review) to the majority of mathematicians pursuing similar goals or working on closely related problems. With this notion at hand, I finally characterized mathematical progress as follows:

> A given bit of mathematics constitutes *mathematical progress* if and only if it is original, accessible and effectively furthers at least one valuable mathematical goal.

From this, it follows that it need not be the case that each new mathematical discovery constitutes mathematical progress. With respect to the extent to which a bit of mathematics constitutes progress, I submitted the following characterization:

> The *degree of progress* of a given original (and accessible) bit of mathematics is constituted by the weights of *all* valuable mathematical goals that are furthered and the level of effectiveness to which they are furthered by the given bit.

When discussing Ivan Niven's reproof of the irrationality of $\pi$, I pointed out that I consider it a merit of my account to allow for the possibility that bits of mathematics—unlike Maddy's (at least) later ideas from *Defending the Axioms*—constitute mathematical progress solely on the basis of their aesthetic appeal. Although I think that in general this fits well with Hardy's understanding of mathematics as creative *art*, it is questionable whether he would have wanted to qualify a bit of mathematics as progress only because of its aesthetic qualities. As a reminder, in his view, the criteria by which a mathematician's patterns should be judged are beauty *and* seriousness.

Chapter 3 mainly dealt with Hardy's notion of seriousness from his *A Mathematician's Apology*. The focus was on a particular example of a mathematical theorem which Hardy judged to be non-serious and which states, in essence, that 2178 and 1089 are the only base-10 reverse multiples below 10000. In general, according to Hardy, the seriousness of a theorem lies in the significance of the ideas which it connects, and the two things that seem to him essential to a significant idea are a certain generality and a certain depth. With regard to the example above, Hardy claims that there is nothing in it which "appeals much to a mathematician" and that it is "not capable of any significant generalization." Although some work has been done on the general phenomenon of research multiples since the publication of Hardy's *Apology*, by identifying what I take to be the most important aspect of Hardy's notion of generality, namely connectedness to a large complex of appreciably different mathematical ideas, I have argued that Hardy's claim regarding the non-capability of any significant generalization is still tenable, contrary to the views of several researchers. From this, it also follows that the phenomenon of reverse multiples does not (yet) constitute a counterexample to Hardy's claim that mathematicians have the ability to recognize significant ideas when they see them. In addition, I have suggested that the phenomenon of reverse multiples should be considered as a genuine example of the multifaceted nature of mathematical interest, given Neil Sloane's appreciation of some results on reverse multiples—in particular the generating function for the number of all base-10 reverse multiples, regardless of the multiplier—in contrast to Hardy's generally dismissive attitude towards it.

In Chapter 4, I addressed the general question of how mathematical knowledge is justified by critically examining Azzouni's argument against the views of Hamami and Avigad on the relationship between informal mathematical proofs and formal proofs, which they both refer to as the *standard view*. In particular, Azzouni argues that a specific visual proof of $1/2 + 1/4 + 1/8 + 1/16 + \cdots = 1$ constitutes a counterexample to the thesis that the transcribability to a formal derivation should be considered a norm or standard of correctness for informal rigorous proof, which is part of Avigad's *standard view.* I have argued, however, that Azzouni, on the one hand, and Hamami and Avigad, on the other, are dealing with different notions of informal rigor. For Hamami and Avigad, the judgments about the rigorousness of a mathematical argument by the mathematicians themselves are crucial. I referred to an empirical study and several individual statements by mathematicians to show that the general status of visual proofs is controversial among mathematicians, and concluded that visual proofs in general, and the one presented by Azzouni in particular, do not constitute counterexamples to the *standard view.* However, I have also argued that Azzouni's assessment of the epistemic process associated with the visual proof is essentially correct. To this end, I identified a necessary condition for the visual proof to be considered a proper proof in the first place, which is

> The (an) intuitive notion of the sum of the series involved in the intuitively effective procedure that the visual proof exhibits right on its surface is compatible with the now-standard, "rigorous" definition of the sum as the limit of the sequence of partial sums using the $\epsilon$-$\delta$ terminology.

and suggested an appropriate way to establish its correctness, namely with the help of the history of mathematics. From this I concluded that an informal proof can function properly as a justification for a piece of mathematical knowledge without being *rigorous* in Hamami's (and Avigad's) sense.

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# Abstract

The present cumulative dissertation which consists essentially of three independent research articles, each corresponding to a chapter of this thesis, is about several aspects of the growth and nature of mathematical knowledge. I pay particular attention to mathematical practice, i.e., both to the works of (contemporary) mathematicians and to their actual research practice. In recent decades, a new trend has emerged in the philosophy of mathematics that focuses on mathematics conceived as a practice with its own history and which is carried out by human beings. This trend is widely known as *the philosophy of mathematical practice* (PMP) to which the present work can be assigned.

In the first article, I return to "the origins" of PMP and address the question of what mathematical progress is. The investigation is based on an article by Penelope Maddy devoted to this topic in which she considers only contributions "of some mathematical importance" as progress. With the help of a case study from contemporary mathematics, more precisely from tropical geometry, a few issues with her proposal are identified. Taking these issues into consideration, an alternative account of "mathematical importance," broadly within the framework of progress Maddy offers, is developed with a special focus on mathematicians' peer-review practice. The second article focuses on Hardy's notion of *seriousness* from his *A Mathematician's Apology*, and in particular on one of Hardy's examples of non-serious theorems, namely that 8712 and 9801 are the only numbers below 10000 which are integral multiples of their reversals, in the sense that

$8712 = 4 \cdot 2178$, and $9801 = 9 \cdot 1089$. Hardy claims that there is nothing in this example which "appeals much to a mathematician" and that it is "not capable of any significant generalization." Although more than a dozen papers have been published that discuss generalizations of Hardy's example since the publication of the *Apology*, I argue that Hardy's claim regarding the non-capability of any significant generalization is still tenable, contrary to the views of several researchers. Furthermore, I present and discuss this case study as an example of the multi-faceted nature of mathematical interest. In the third article, a recent argument by Jody Azzouni, based on a specific visual proof of $1/2 + 1/4 + 1/8 + 1/16 + \cdots = 1$, against the views of Yacin Hamami and Jeremy Avigad on the relationship between ordinary, informal mathematical proofs and formal derivations is critically examined. By reference to mathematicians' judgments about visual proofs in general, I argue that Azzouni's critique of Hamami and Avigad's account(s) is not valid. Nevertheless, by identifying a necessary condition for the visual proof to be considered a proper proof in the first place, and suggesting an appropriate way to establish its correctness, it is shown how Azzouni's assessment of the epistemic process associated with the visual proof can turn out to be essentially correct.

# Zusammenfassung

Die vorliegende kumulative Dissertation, die im Wesentlichen aus drei unabhängigen Forschungsartikeln besteht, welche jeweils einem Kapitel dieser Arbeit entsprechen, befasst sich mit verschiedenen Aspekten des Wachstums und der Natur des mathematischen Wissens. Mein besonderes Augenmerk gilt dabei der mathematischen Praxis, d.h. sowohl den Werken von (zeitgenössischen) Mathematiker*innen als auch ihrer tatsächlichen Forschungspraxis. In den letzten Jahrzehnten hat sich in der Philosophie der Mathematik ein neuer Trend herausgebildet, der sich mit der Mathematik als eine Praxis, die ihre eigene Geschichte hat und die von Menschen ausgeübt wird, beschäftigt. Dieser Trend ist weithin bekannt als *die Philosophie der mathematischen Praxis* (PMP), dem die vorliegende Arbeit zugeordnet werden kann.

Im ersten Artikel kehre ich zu den „Ursprüngen“ der PMP zurück und gehe der Frage nach, was mathematischer Fortschritt ist. Die Untersuchung stützt sich auf einen Artikel von Penelope Maddy, der diesem Thema gewidmet ist und in dem sie nur Beiträge „von einiger mathematischer Wichtigkeit“ als Fortschritt betrachtet. Mit Hilfe einer Fallstudie aus der zeitgenössischen Mathematik, genauer gesagt aus der tropischen Geometrie, werden ein paar Aspekte ihres Vorschlags aufgezeigt, über die meiner Meinung nach mehr gesagt werden sollte. Unter deren Berücksichtigung wird ein alternatives Konzept der „mathematischen Wichtigkeit“ entwickelt, das sich weitgehend in den Rahmen der von Maddy vorgeschlagenen Theorie des Fortschritts einfügt, wobei ein spezieller Fokus auf der Peer-Review-

Praxis von Mathematiker*innen liegt. Der zweite Artikel konzentriert sich auf Hardys Begriff der *Ernsthaftigkeit* (*seriousness*) aus seiner *Apologie eines Mathematikers* (*A Mathematician's Apology*), und insbesondere auf eines von Hardys Beispielen nicht-ernsthafter Theoreme, nämlich, dass 8712 und 9801 die einzigen Zahlen unter 10000 sind, die ganzzahlige Vielfache ihres Wertes in umgekehrter Ziffernfolge geben, also in dem Sinne, dass $8712 = 4 \cdot 2178$ und $9801 = 9 \cdot 1089$. Hardy behauptet, dass es in diesem Beispiel nichts gibt, was „einen Mathematiker besonders anspricht" und dass es „keine bedeutende Verallgemeinerung zulässt". Obwohl seit der Veröffentlichung der *Apologie* mehr als ein Dutzend Arbeiten veröffentlicht wurden, die Verallgemeinerungen von Hardys Beispiel diskutieren, argumentiere ich, dass Hardys Behauptung bezüglich der Nichtfähigkeit einer bedeutenden Verallgemeinerung entgegen der Ansichten mehrerer Forscher*innen immer noch haltbar ist. Darüber hinaus präsentiere und diskutiere ich diese Fallstudie als ein Beispiel für die Vielseitigkeit des mathematischen Interesses. Im dritten Artikel wird ein neueres Argument von Jody Azzouni, das auf einem spezifischen visuellen Beweis von $1/2 + 1/4 + 1/8 + 1/16 + \cdots = 1$ beruht, gegen die Ansichten von Yacin Hamami und Jeremy Avigad über die Beziehung zwischen gewöhnlichen, informellen mathematischen Beweisen und formalen Ableitungen kritisch untersucht. Unter Bezugnahme auf Urteile von Mathematiker*innen über visuelle Beweise im Allgemeinen argumentiere ich, dass Azzounis Kritik an Hamamis und Avigads Darstellung(en) nicht gültig ist. Nichtsdestotrotz zeige ich, indem ich eine notwendige Bedingung dafür bestimme, dass der visuelle Beweis überhaupt als ein wahrer Beweis in Betracht kommen kann, und indem ich einen geeigneten Weg vorschlage, deren Korrektheit festzustellen, wie sich Azzounis Einschätzung des epistemischen Prozesses, welcher er mit dem visuellen Beweis assoziiert, als im Wesentlichen richtig erweisen kann.