VISUALIZATION, EXPLANATION AND REASONING STYLES IN MATHEMATICS

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VISUALIZATION, EXPLANATION AND REASONING STYLES IN MATHEMATICS

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P. MANCOSU, K.F. JØRGENSEN AND S.A. PEDERSEN

INTRODUCTION

In the 20th century philosophy of mathematics has to a great extent been dominated by views developed during the so-called foundational crisis in the beginning of that century. These views have primarily focused on questions pertaining to the logical structure of mathematics and questions regarding the justification and consistency of mathematics. Paradigmatic in this respect is Hilbert's program which inherits from Frege and Russell the project to formalize all areas of ordinary mathematics and then adds the requirement of a proof, by epistemically privileged means (finitistic reasoning), of the consistency of such formalized theories. While interest in modified versions of the original foundational programs is still thriving, in the second part of the twentieth century several philosophers and historians of mathematics have questioned whether such foundational programs could exhaust the realm of important philosophical problems to be raised about the nature of mathematics. Some have done so in open confrontation (and hostility) to the logically based analysis of mathematics which characterized the classical foundational programs, while others (and many of the contributors to this book belong to this tradition) have only called for an extension of the range of questions and problems that should be raised in connection with an understanding of mathematics. The focus has turned thus to a consideration of what mathematicians are actually doing when they produce mathematics. Questions concerning concept-formation, understanding, heuristics, changes in style of reasoning, the role of analogies and diagrams etc. have become the subject of intense interest. These historians and philosophers agree that there is more to understanding mathematics than a study of its logical structure and put much emphasis on mathematical activity as a human activity. How are mathematical objects and concepts generated? How does the process tie up with justification? What role do visual images and diagrams play in mathematical activity? In addition to these cognitive issues one might also investigate how mathematics interacts with the natural sciences, and how mathematical thinking might depend on the culture it is embedded in.

This book is based on the meeting "Mathematics as Rational Activity" held at Roskilde University, Denmark, from November 1 to November 3, 2001. The meeting focused on recent work in the study of mathematical activity understood according to the outline given above. The lectures, by some of the most outstanding scholars in this area, addressed a variety of issues related to mathematical reasoning. Despite the variety of the contributions there were strong unifying themes which recur in these lectures thereby providing a strong sense of unity and purpose to the present book.

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The title of the book “Visualization, Explanation, and Reasoning Styles in Mathematics” is indeed an accurate description of these recurring themes.

The volume is divided into two parts. The first part is called *Mathematical Reasoning and Visualization*.

One question which arises pertaining to mathematical reasoning is to what extent, if any, diagrams and visual imagery can provide us with mathematical knowledge. Most of the contributions in the book touch upon this question but the first part of the book is fully devoted to it. In “Visualization in Logic and Mathematics”, Paolo Mancosu provides a broad introductory discussion of visualization and diagrammatic reasoning and their relevance for recent discussions in the philosophy of mathematics. Mancosu begins by outlining how visual intuition and diagrammatic reasoning were discredited in late nineteenth century and twentieth century analysis and geometry. While diagrams and visual imagery were considered heuristically fruitful their role for justificatory purposes was considered to be unreliable and thus to be avoided. However, recent developments in mathematics and logic have brought back to the forefront the importance of visual imagery and diagrammatic reasoning. Mancosu describes how many mathematicians are calling for more visual approaches to mathematics and the recent developments in logic related to diagrammatic reasoning. In the final part of the paper he discusses how these recent developments affect the traditional foundational debates and describes some recent philosophical attempts to grant to visualization (Giaquinto) and diagrammatic reasoning (Barwise and Etchemendy) an epistemic status which goes beyond the mere heuristic role attributed to them in the past.

With this background the reader can then move on to Marcus Giaquinto’s “From Symmetry Perception to Basic Geometry”. In Frege’s approach to the foundations of mathematics, Frege explicitly excluded that psychological investigations might be relevant to the foundational goal. This was basically motivated by the idea that experience, whether physical or psychological, could not warrant the generalizations drawn from it and thus in this way one could not account for the objectivity of mathematics. The Fregean approach had no account of how our psychological processes relate to our grasping of mathematical truths. Frege’s position rests on the assumption that the only role a perception or an experience of visual imaging can play is that of evidence for a further generalization. By contrast, Giaquinto proposes that experiences of seeing or visual imaging might play the role of “triggers” for belief-forming dispositions which in turn give us geometrical knowledge. He gives an account, meant to be empirically testable, of how we could come to the knowledge of a simple geometrical truth, such as that in a perfect square the two parts either side of the diagonal are congruent. The account proceeds by stages. First, a description of how we perceive squareness (and the role that visual detection of symmetries plays in the process) and how this results in a category representation (in the form of a description set) for the perceptual concept of square. Second, how a modification of the perceptual concept of square gives rise to the concept of a perfect square. Third, it is argued that the possession of these concepts is tantamount to having certain belief forming dispositions which can be triggered by experiences of visual seeing or imaging. However, the role of the visual or imaging experience is not that of evidence but rather of "trigger" for the belief-forming dispositions. Finally, if the mode of acquisition of such triggered beliefs is reliable and there is no violation of epistemic rationality, Giaquinto claims that the beliefs thus obtained constitute knowledge. What type of knowledge? Being non-logical and non-empirical (since the role of experience is not that of providing evidence) the beliefs thus obtained are synthetic a priori. Whether any individual or we as a community of mathematical learners come to have the beliefs in question through the process described ought, according to Giaquinto, to be subject of empirical investigation. If he is right the gap between experience and mathematical knowledge would finally be filled by an account that does justice both to the role of psychological processes and to the objectivity of mathematics.

In a more traditional philosophical context, namely the perennial dispute between platonism and naturalism, James Robert Brown also addresses the role of "seeing" and "intuition" in mathematics and the relevance of diagrams in this context. While providing a defense of Platonism, Brown agrees that with respect to epistemological questions traditional Platonism has always been problematic: How are we to have access to the mathematical entities which exist in an abstract non-causal world? Modern Platonists typically claim that we can "see" or "intuit" the mathematical entities with a special non-sensible intuition. K. Gödel and G. H. Hardy are two of the most well known mathematicians holding this view. Thus, Hardy for instance, sees all mathematical evidence as some sort of perception. But in this respect, according to Brown, he goes too far. We only need, Brown says, to commit ourselves to the perception of some basic mathematical objects and facts. These can then serve as grounds for more advanced mathematics which we cannot directly "see" and this is quite close to Gödel's position. By stressing the fact that much of what is called "seeing" in natural science is quite remote from visual perception, Brown goes on to describe how we can have "seeing" in mathematics through diagrams. Using a simple example of a picture-proof, Brown claims that a diagram can function as a "telescope" that allows us to "see" into the Platonic realm. The diagram displays only a specific case but through the diagram we are able to intuit a general truth and this intuition cannot be confused with the sensory information given by the diagram. Brown's position on intuition is then elaborated further through an analysis of how Freiling's well known informal disproof of the Continuum Hypothesis (by throwing darts at the real line) affects Maddy's naturalism in philosophy of mathematics. The resulting claim is that, according to Brown, there is "some sort of mathematical perception which cannot be reduced to either physical perception or to disguised logical inference".

In his second contribution, Marcus Giaquinto addresses the varieties of mathematical activities which are encountered in mathematical practice. These include, to name only some paradigmatic examples, discovery, explanation, formulation, application, justification and representation. All of these activities provide rich material for a philosophical analysis of mathematics. Unfortunately, until recently philosophers of mathematics have mainly paid attention to only a few of these and, moreover, the of attention has often been too narrowly focused. The extension proposed by Giaquinto concerns not only the proposal to take into account the above mentioned activities but also the various aspects in which mathematics is done and communicated (making, presenting, taking in).

Three important ingredients of mathematical activity are discovery, explanation and justification. The discussion of discovery through visual imaging nicely ties up with the previous material on visualization and again Giaquinto points out that although we might reach knowledge by such means this need not be a proof. Explanation is also a theme touched upon by many contributors in the book. Giaquinto points out that there are proofs which are not explanations and explanations which are not proofs. Of course, there are also examples of proofs which are explanations, and Giaquinto refers to Chemla's paper for an important historical example. Moreover, explanations might play a role in motivating definitions, as illustrated by the moving particle argument which gives a satisfactory account of the use of Euler's formula

$$e^{i\pi} = \cos\pi + i\sin\pi.$$

as a definition in extending the exponential function to complex numbers. Motivating a definition through an explanation is thus an important type of mathematical activity and it can be seen as a form as justification which is distinct from proving a theorem. Another such activity is motivating or 'justifying' the axioms. Giaquinto concludes that extending the philosophical analysis of mathematics to all these aspects, and the many more discussed in his paper, 'would restore to philosophy of mathematics its ancient depth and succulence'.

Part II of the book is entitled *Mathematical Explanation and Proof Styles*.

Jens Høyrup in "On Reasoning Styles in Early Mathematics" discusses aspects of reasoning in Babylonian and Greek mathematics. Høyrup's essay takes its start from a criticism of those historians of mathematics who formulate a distinction between Babylonian and Greek mathematics claiming that the former is basically a collection of ad hoc rules whereas the latter is a reasoned discipline. In addition to show that this is an incorrect characterization of the situation, Høyrup also wants to characterize the reasoning involved in Old Babylonian mathematics in constant comparison with Greek mathematics. What he points out is that there are certainly mathematical tablets in which solutions to problems are given where "no attempt is made to discuss why or under which conditions the operations performed are legitimate and lead to correct results". The situation is made worse by the fact that most of these clay tablets contain no diagrams. But obviously there must have been more that accompanied the process of instruction and learning. Høyrup argues that a few remaining texts from Susa allows us to see the kind of explanations that would have been given orally in a learning context. Moreover, these explanations are 'critical', i.e. provide reasons for the extent of the validity of the procedure under discussion and for why the procedure works. Thus, Old Babylonian mathematics displays its own characteristic style of thought. The real difference with Greek mathematics is that in the Old Babylonian school "the role of critique had been peripheral and accidental; in Greek theoretical mathematics it was, if not the very centre then at least an essential gauge". Høyrup concludes that we cannot count as mathematics any activity that is devoid of understanding and that when the historian works on a mathematical culture for which the sources do not reveal an appeal to reasoning then either we are not understanding the sources or the sources are not an accurate mirror of the mathematical practice.

Another area in history of mathematics which has traditionally been judged against the yardstick of Greek mathematics is Chinese mathematics. Karine Chemla's "The Interplay between Proof and Algorithm in 3-rd Century China" dovetails well with Høyrup's contribution by showing that Chinese mathematics also presents reasoning styles which differ from Greek mathematics but should nonetheless be seen as part of the history of proof. A key case study in this connection is Liu Hui's commentary on *The nine chapters on mathematical procedures*. In particular, Chemla focuses on Liu Hui's commentary on the measurement of the circle. The commentary, made up of two parts, reveals Liu Hui's concerns for explaining why a certain algorithm is correct and it functions as an explanation of the algorithm. The work bears witness to a sophisticated practice of proving mathematical results in ancient China which differs from proving practices in Greek mathematics. Liu Hui was a commentator and this is significant as proofs seem to emerge in Chinese mathematics as the result of such activity. Chemla claims that whereas in Greek mathematics proofs seem mainly aimed at establishing the truth of propositions in the case of Liu Hui what is at stake is the establishment of the correctness of a certain algorithm or possibly showing why the algorithm is correct. Without entering into the details of the analysis let us point out that we have here another interesting case study of mathematical practice which highlights two important facts. First, it shows that the role of proof might be explanatory, in addition to that of certifying a result. Moreover, the reasoning style displayed in these texts represent a characterizing feature of Chinese mathematics and thus it reminds us about the importance of the mathematical culture in which different proof practices are embedded.

Thus, Høyrup's and Chemla's case studies, in addition to their intrinsic importance for the historiographical characterization of Old Babylonian mathematics and Chinese mathematics vs. Greek mathematics, raise important issues concerning mathematical understanding and mathematical explanation and show that these notions are also context-dependent.

Jamie Tappenden's article "Proof style and Understanding in Mathematics I" touches on several topics central to the book such as visualization, explanation, justification, and concept formation. The article focuses on the different styles within complex analysis represented by Weierstrass and Riemann. Weierstrass's methodology was computationally motivated: it aimed at finding explicit representations of functions and algorithms to compute their values. Riemann, by contrast, was more abstract in his approach, more "conceptual". With the introduction of the concept of a Riemann surface, Riemann not only reorganized the subject matter of complex analysis but introduced a whole new style in the area. This new approached yielded new discoveries, new proofs and it deepened our understanding of the subject in unexpected ways. Tappenden explores here the important role that the visualization allowed by Riemann's approach played in this reconfiguration of the subject. The unification yielded by Riemann's approach is also analyzed by Tappenden with reference to contemporary debates on the nature of unification, understanding, and explanation (Friedman, Toulmin, Kitcher). The topic of unification is intimately tied to the discussion of 'fruitful' concepts. Fruitful concepts have unifying and explanatory roles but it is often difficult to say what makes a concept fruitful in mathematics. Tappenden mentions the unification of the theory of algebraic functions of one variable and the theory of algebraic numbers given by Dedekind and Weber. In this case, notions like "ideals" and "fields" turned out to be extremely fruitful concepts: they help us understand "what is going on" and as such discharge an explanatory role. In connecting the topics of visualization, explanation, and unification, Tappenden notes that often, as in the case of Riemann's approach to function theory and Artin's *Geometric Algebra*, a contributor to the "fruitfulness" or "naturalness" of the approach is that the arguments and categories characterizing the approach can be visualized. These aspects of mathematical practice (visualization, explanation, fruitfulness) are often relegated to 'subjective' matters of taste but Tappenden makes a strong case that they can be the topic of fruitful philosophical and methodological analysis. However, he concludes, one should not hope to provide an a priori account of such notions; rather, only detailed case studies of mathematical practice will be able to enlighten us on these complex issues.

The notion of explanation in mathematics, which has appeared in many of the articles discussed above, is the focus of Johannes Hafner and Paolo Mancosu's "The Varieties of Mathematical Explanations". While Hafner and Mancosu emphasize that explanations in mathematics need not be proofs (for instance, theories as a whole might be explanatory), in this paper they restrict attention to proofs. They begin by providing evidence for the claim that mathematicians seek explanations in their ordinary practice and cherish different types of explanations (for instance, many mathematicians are often critical of proofs that only show *that* something is true but do not give an hint of *why* it is true). They go on to suggest that a fruitful approach to the topic of mathematical explanation would consist in providing a taxonomy of recurrent types of mathematical explanation and then trying to see whether these patterns are heterogeneous or can be subsumed under a general account. In the literature on explanation in mathematics there are basically only two philosophical theories on offer. One proposed by Steiner (1978) and an account based on unification due to Kitcher (discussed in the previous article by Tappenden). Mancosu and Hafner provide a case study of how to use mathematical explanations as found in mathematical practice to test theories of mathematical explanation. The case study focuses on Steiner's theory of mathematical explanation. This theory singles out two criteria for a proof to count as explanatory: dependence on a characterizing property and generalizability through varying of that property. The authors argue that Pringsheim's explanatory proof of Kummer's convergence criterion in the theory of infinite series defies both criteria and thus cannot be accounted for by Steiner's model of explanation. This can be seen, as it were, as a case study of how to show that the variety of mathematical explanations cannot be easily reduced to a single model.

The last article of the book is devoted to a very neglected part of mathematical activity: the role of aesthetical factors in mathematics. Obviously, mathematics displays aesthetic features. Mathematicians often talk about the elegance of certain constructions or the beauty of various geometrical figures. But is the 'aesthetic dimension' a rational feature of mathematical activity or a completely subjective and non-analyzable aspect of the mathematical experience? Reviel Netz's "Towards an aesthetic of mathematics" develops several analytical tools required for a productive discussion of this difficult topic. Netz argues from the outset that every type of human expression possesses an aesthetic dimension. Moreover, the aesthetic dimension is an objective fact, although a difficult one to analyze. Whereas most driving factors in mathematical activity are epistemic in the case of beauty we have a clear case of a non-epistemic value which is intrinsic to mathematical research. Netz thus expands the range of topics addressed in the other contributions of the book: "The thrust of the articles collected in this volume is, I believe, to widen our picture of the field of mathematical practice as a rational activity: one that appeals to the visual and not merely to the symbolic, that aims at explanation and not merely at proof. It also appeals, I suggest, to the aesthetic. Among other things – and still as rational practitioners – mathematicians aim at beauty." Netz's paper proceeds by giving a typology of sources of mathematical beauty. Mathematical beauty can be predicated of states of minds, of the products of mathematical activity (say theorems as embodied in texts), and of the objects studied in the previous categories. Netz's analysis focuses on mathematical texts and he proposes to bring to bear for the task a body of theory already developed in poetics. In order to limit the scope he discusses Greek mathematical texts and explores the sense in which techniques of "narrative" and "prosody" can be fruitfully exploited for an analysis of the aesthetic dimension of these texts. In this approach "narrative" will account for the content and "prosody" for the form of the mathematical text. Netz claims that just as in literature one source of beauty in mathematics is the interaction (he calls it "correspondence") between form and content.

Given the emphasis on the heterogeneity of mathematical practice displayed in most of the articles in the present collection, the outcome of the work is not that of claiming that some unique model or theory will account for the great wealth of mathematical activities. Even if such a theory were to be found in the future, it would be premature to suggest anything of the sort at this stage. Rather, through their mathematical, historical, and philosophical richness, these contributions show that there is a wide virgin territory open to investigation. Our hope is that others will also embark in its exploration.

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PART 1

MATHEMATICAL REASONING AND VISUALIZATION

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VISUALIZATION IN LOGIC AND MATHEMATICS

In the last two decades there has been renewed interest in visualization in logic and mathematics. Visualization is usually understood in different ways but for the purposes of this article I will take a rather broad conception of visualization to include both visualization by means of mental images as well as visualizations by means of computer generated images or images drawn on paper, e.g. diagrams etc. These different types of visualization can differ substantially but I am interested in offering a characterization of visualization that is as broad as possible. The article describes and explains (1) the way in which visual thinking fell into disrepute, (2) the renaissance of visual thinking in mathematics over recent decades, (3) the ways in which visual thinking has been rehabilitated in epistemology of mathematics and logic.

This renaissance of interest in visualization in logic and mathematics has emerged as a consequence of developments in several different areas, including computer science, mathematics, mathematics education, cognitive psychology, and philosophy. When speaking of renaissance in visualization there is an obvious implication that visualization had been relegated to a secondary role in the past. One usually refers to the fact that the development of mathematics in the nineteenth century had shown that mathematical claims that seemed obvious on account of an intuitive and immediate visualization turned out to be incorrect on closer inspection. This went hand in hand with a downgrading of *Anschauung* and specifically visuo-spatial thinking from the exalted status it had in Kant's epistemology of mathematics. The effects were also felt in pedagogy with a shift of emphasis away from visualization (for instance, in Landau's diagram-free text on calculus).

1. DIAGRAMS AND IMAGES IN THE LATE NINETEENTH CENTURY

Some of the standard cases mentioned in this connection are the belief that every continuous function must be everywhere differentiable except at isolated points, or the tacit assumption in elementary geometry that the two circumferences drawn in the construction of the equilateral triangle over any given segment in Euclid's *Elements* I.1 meet in one point (the vertex of the equilateral triangle).[1]

In both cases the claim seems to be obvious from the visual situation (diagrams or mental imagery) but turns out to be unwarranted. In the former case this is the consequence of the discovery of continuous nowhere differentiable functions. In the latter case, this was due to the realization

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that only a continuity axiom can guarantee the existence of the intersection point of the two circles. Such results and many other concomitant factors, led mathematicians to formulate more rigorous approaches to mathematics that excluded the recourse to such treacherous tools as images and diagrams in favor of a linguistic development of mathematics. Of course, the use of images and diagrams was still allowed at a heuristic level. The careful mathematician was however supposed to resist the chant of the visual sirens when it came to the context of justification:

> For the appeal to a figure is, in general, not at all necessary. It does facilitate essentially the grasp of the relations stated in the theorem and the constructions applied in the proof. Moreover, it is a fruitful tool to discover such relationships and constructions. However, if one is not afraid of the sacrifice of time and effort involved, then one can omit the figure in the proof of any theorem; indeed, the theorem is only truly demonstrated if the proof is completely independent of the figure. (Pasch, 1882/1926, 43).

In short, visualization seemed to lose its force in the context of justification while being allowed in the context of discovery and as something that simplifies cognition (but cannot ground it). Pasch is well known for being one of the pioneers of a development of geometry characterized by the rejection of diagrams as relevant to geometrical foundations. In the *Foundations of Geometry* (1899) Hilbert is not explicit about the role of diagrams in geometry. However, in a number of unpublished lectures he raises the issue. In lectures on the foundations of geometry from 1894 we read:

> A system of points, lines, planes is called a diagram or figure [Figur]. The proof [of the theorem he is discussing] can indeed be given by calling on a suitable figure, but this appeal is not at all necessary. [It merely] makes the interpretation easier, and it [the appeal to diagrams] is a fruitful means of discovering new propositions. Nevertheless, be careful, since it [the use of figures] can easily be misleading. A theorem is only proved when the proof is completely independent of the diagram. The proof must call step by step on the preceding axioms. The making of figures is [equivalent to] the experimentation of the physicist, and experimental geometry is already over with the [laying down of the] axioms. (Hilbert, 1894, 11).

And in other lectures from 1898 and 1902 Hilbert provides examples of how one can be misled by diagrams by going through a proof of the claim that “every triangle is equilateral”. In introducing the example he says:

> One could also avoid using figures, but we will not do this. Rather, we will use figures often. However, we will *never rely on them* [*niemals auf sie verlassen*]. In the use of figures one must be especially careful; we will always have care to make sure that the operations applied to a figure remain correct from a purely logical perspective. (Hilbert, 1902, 602).

These motivations, emerging from the foundational work in geometry and analysis, led to a conception of formal proof that has dominated logic in the past century (and it is usually attached to the names of Frege, Hilbert, and Russell). This conception of formal proof relies on a linguistic characterization of proofs as a sequence of sentences. We find the essential elements of such conception already in Pasch:

> We will acknowledge only those proofs in which one can appeal step by step to preceding propositions and definitions. If for the grasp of a proof the corresponding figure is indispensable then the proof does not satisfy the requirements that we imposed on it. These requirements are fulfillable; in any complete proof the figure is dispensable [...]. (Pasch, 1882/1926, 90).

These attitudes towards diagrammatic reasoning and visualization have thus a complex history, which still calls for a good historian. Certainly one would have to take into account the importance of the development of projective and non-Euclidean geometries in the nineteenth century and of the arithmetization of analysis.[2]

However, I am not convinced that we can tell a linear story where the heroes finally attained a level of rigor hitherto unprecedented, thus leaving the opposition in disarray. For instance, I think there is much to learn from taking a look at the opposition between Klein and the Weierstrass school or the debate that opposed the "rigorist" Segre to the "intuitionists" Severi and Enriques in algebraic geometry.[3]

I will limit myself to a remark on one of the main paradigmatic examples that were used to discredit the role of geometric intuition in analysis, e.g. Weierstrass' discovery of a continuous nowhere differentiable function. Weierstrass' result was announced in 1872 (and published by du Bois Reymond (1875, 29)). The function in question was given by the equation

$$f(x) = \sum b^n \cos(a^n x)\pi$$

with a odd, $b \in [0,1)$ and $ab > 1 + 3\pi/2$.

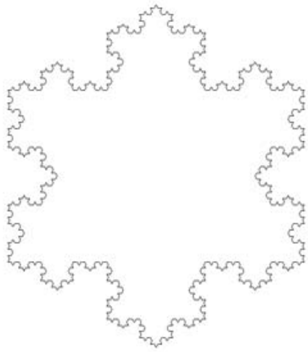

FIGURE 1. von Koch's snow-flake.

Weierstrass' result was given in a strictly analytic way (du Bois Reymond (1875, 29-31)) and it left obscure what the geometrical nature of the example might be. This was somehow characteristic of the school of Weierstrass, which – as Poincaré poignantly puts it – "ne cherche pas a voir mais à comprendre" (Poincaré, 1898, 16). However, there were mathematicians who did not accept this distinction between seeing and understanding. A case in point is Helge von Koch. Von Koch is now well known for his snowflake, one of the earliest examples of fractals and up to this day one of the paradigmatic examples of fractals.

What is not well known is the motivation that led von Koch to his discovery of the snowflake. In his 1906 von Koch begins by remarking that until Weierstrass came up with his example of a continuous nowhere differentiable curve it was a widespread opinion ("founded no doubt on the graphical representation of curves") that every continuous curve had a definite tangent (with the exception of singular points). But then he adds:

> Although Weierstrass' example has once and for all corrected this mistake, it is insufficient to satisfy our mind from the geometrical point of view; for the function in question is defined by an analytic expression which hides the geometrical nature of the corresponding curve so that one does not see, from this point of view, why the curve has no tangent; one should say rather that the appearance is here in *contradiction* with the reality of the fact established by Weierstrass in a purely analytic manner. (von Koch, 1906, 145-6).

Restoring geometrical meaning to the analytic examples was at the source of the work:

> This is why I have asked myself – and I believe that this question is of importance when teaching the fundamental principles of analysis and geometry – whether one could find a curve without tangent for which the geometrical appearance is in agreement with the fact in question. The curve which I found and which is the subject of this paper is defined by a geometrical construction, sufficiently simple, I believe, that anyone should be able to see [pressentir] through "naïve intuition" the impossibility of a determinate tangent. (von Koch, 1906, 146).

Von Koch's project must be seen against the background of the philosophical discussion among mathematicians on the demarcation between "visualizable" (or "intuitable") and "non-visualizable" curves. This discussion (see Volkert (1986)), to which Klein, du Bois Reymond, Köpke, Chr. Wiener and others contributed, should draw our attention to the fact that a detailed history of attitudes towards visualization in the twentieth century might reveal a more complex pattern than a simple and absolute predominance of a linguistic, non visual, notion of proof.

2. THE RETURN OF THE VISUAL AS A CHANGE IN MATHEMATICAL STYLE

But granting the predominance of a linguistic, non visual, notion of formal proof in mathematics – and examples such as Bourbaki make clear that this not a myth – let us now try to characterize the salient features of this 'return of the visual'.

One of the most important aspects is certainly the development of visualization techniques in **computer science** and its impact on mathematics. Here there has clearly been a two ways influence as mathematical techniques have helped shape techniques in computer science (including those leading to great progress in visualization techniques). Conversely, developments in visualization techniques developed by computer scientists have had important effects on mathematics. Computer graphics has allowed researchers to display information (say, analytic or numerical information) in ways that can be represented in the form of a graph, or a chart or in other forms but in any case in a form that allows for a quick visual grasp. Two areas are usually singled out as paradigmatic of the powerful role displayed by visualization in the mathematical arena. The first one is the area of chaos theory, and in particular fractal theory (see Evans (1991)). In "Visual theorems", Philip Davis emphasizes that "aspects of the figures can be read off (visual theorems) that cannot be concluded through non-computational mathematical

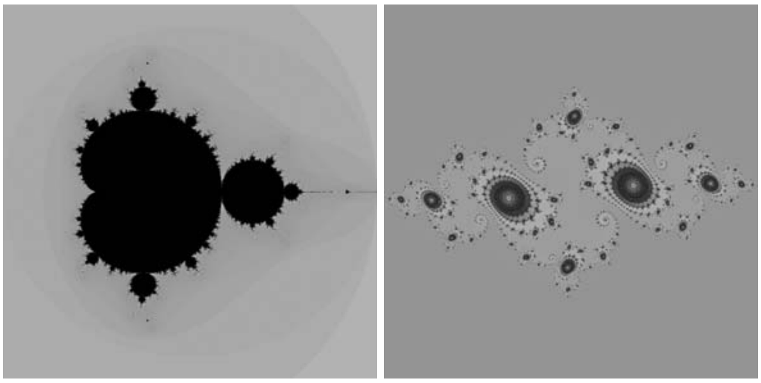

FIGURE 2. Mandelbrot and Julia sets.

devices" (1993, 339). For delightful examples of visual proofs see Roger B. Nelsen (1993, 2000).

Here one can point at the dramatic case of the relationship between the Mandelbrot set and all the Julia sets sitting inside it. It would have been impossible to recognize analytically, without the visual support offered by the computer, that the Julia sets are present inside the Mandelbrot sets. Moreover, the connectedness of the Mandelbrot set became apparent to Mandelbrot on the basis of its graphical appearance.

Another area where the benefits of computer graphics have been greatly exploited is differential geometry. The visual study of three-dimensional surfaces was pioneered in the late seventies by T. Banchoff and C. Strauss. Through the use of computer graphic animation they were able to construct surfaces and gain a better grasp of them by the application of transformations. However, the two most eventful results obtained in this way were the problem of everting the 2-sphere and the discovery of new minimal surfaces.[4] Palais aptly summarizes the situation:

> Two problems in mathematics have helped push the state of the art in mathematical visualization – namely, the problem of everting the 2-sphere and of constructing new, embedded, complete minimal surfaces, especially higher-genus examples. In the case of eversion, the goal was to illuminate a process so complex that very few people, even experts, could picture the full details mentally. In the case of minimal surfaces, the visualizations actually helped point the way to rigorous mathematical proofs. (Palais, 1999, 654).

In his account of the latter discovery David Hoffman says:

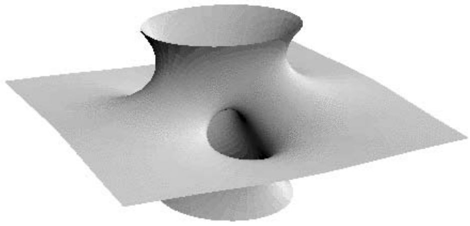

FIGURE 3. Costa's surface.

> In 1984, Bill Meeks and I established the existence of an infinite family of complete embedded minimal surfaces in R^3. For each $k > 0$, there exists an example which is homeomorphic to a surface of genus k from which three points have been removed. Figure [3] is a picture of genus-one example. The equations for this remarkable surface were established by Celsoe Costa in his thesis, but they were so complex that the underlying geometry was obscured. We used the computer to numerically approximate the surface and then construct an image of it. This gave us the clues to its essential properties, which we then established mathematically. (Hoffman, 1987, 8).

Hoffman emphasizes the importance of computer generated images as "part of the process of doing mathematics". However, in his paper he also emphasizes the importance of proving 'mathematically' the properties of the surface which can be 'seen' directly in the visualization:

> Also it [the surface] was highly symmetric. This turned out to be the key to getting a proof of embeddedness. Within a week, the way to prove embeddedness via symmetry was worked out. During that time we used computer graphics as a guide to "verify" certain conjectures about the geometry of the surface. We were able to go back and forth between the equations and the images. The pictures were extremely useful as a guide to the analysis. (Hoffman 1987, p.17)

We thus see that the 'return of the visual' has led to new mathematical discoveries, which, it might be argued, could not have been obtained without the application of computer generated images. Nonetheless, these 'images' are not taken at face value. The properties they display must then be verified 'mathematically'.[5]

The reaction against a purely symbolical conception of mathematics has also found its way in new presentations of certain mathematical subjects that emphasize the visual aspects of the discipline. Paradigmatic examples are Fomenko's 'Visual geometry and topology' (1994) and Needham's 'Visual complex analysis' (1997).

Both of them recognize the importance of the influence of computer science in the recent shift towards more visual methods but their call for a return to intuition and visualization runs deeper and it is rooted in an appreciation of the importance of visual intuition in areas such as geometry, topology, and complex analysis. Fomenko quotes Hilbert approvingly to the effect that notwithstanding the importance of analytical and abstract reasoning "visual perception [Anschauung] still plays the leading role in geometry". Fomenko however does not consider a visual presentation to be logically self-sufficient:

> Many modern fields of mathematics admit visual presentations which do not, of course, claim to be logically rigorous but, on the other hand, offer a prompt introduction into the subject matter. (Fomenko, 1994, preface p. vi).

And later:

> It happens rather frequently that the proof of one or another mathematical fact can at first be 'seen', and only after that (and following the visual idea) can we present a logically consistent formulation, which is sometimes a very difficult task requiring serious intellectual efforts. (Fomenko, 1994, preface p. vii)

Thus, Fomenko's emphasis is on the pedagogical and heuristic value of visual thinking and he does not seem to ascribe to results obtained by visual thinking a justificatory status comparable to that obtained by a 'logically consistent formulation'.

Needham is also very strongly critical of the tendency of modern mathematics to downplay the importance of visual arguments. In a 'parable' he compares the situation in contemporary mathematics to that of a society in which music can only be written and read but never be 'listened to or performed'. He says:

> In this parable, it was patently unfair and irrational to have a law forbidding would-be music students from experiencing and understanding the subject directly through 'sonic intuition'. But in our society of mathematicians we *have* such a law. It is not a written law, and those who flout it may yet prosper, but it says, *Mathematics must not be visualized*![6]

Just like Fomenko, Needham concedes that "many of the arguments [in the book] are not rigorous, at least as they stand" but that "an initial lack of rigor is a small price to pay if it allows the reader to see into this world more directly and pleasurably than would otherwise be possible" (p. xi).

In concluding this section then I would like to point out that many contemporary mathematicians are calling for a return to more visual approaches to mathematics. However, this return of the visual does not seem to upset the traditional criteria of rigor. In all the cases mentioned above all the authors remark on the cognitive importance of visual images in doing mathematics but also seem to recognize that images do not satisfy the criteria of rigor necessary to establish the results being investigated. In this sense this new trend towards visualization, while marking an important shift in style of research and mathematical education (on mathematics education see Zimmerman and Cunningham (1991)), does not seem to me to bring about a radically new position on the issue of the epistemic warrant which can be attributed to arguments which rely on visual steps. In any case, this problem is not addressed directly by any of the authors mentioned above.

At this point several problems could be raised. First, it would be interesting to know more about the cognitive visual roots of our mathematical reasoning and the exact role that mental imagery plays in our mathematical thinking. Second, a number of classical foundational and epistemological questions can still be raised about the warrant afforded by diagrammatic or visual reasoning. In the next section I will try to mention what seem to be the most promising directions of research in these areas at the moment.

3. NEW DIRECTIONS OF RESEARCH AND FOUNDATIONS OF MATHEMATICS

Despite the great revival of interest for visual imagery in **cognitive psychology** (Kosslyn, 1980, 1983; Shepard and Cooper, 1982; Denis, 1989) research in the specific field of mathematical visualization has still a long way to go. Some interesting results are emerging in the study of diagrammatic reasoning (Larkin and Simon, 1987; Glasgow et al., 1995) and problem solving (Kaufmann (1979); for a survey and extensive references see Antonietti et al. (1995)). However, the two most up to date treatments of how the brain does arithmetic (Butterworth, 1999; Deheane, 1997) contain very little on visualization in arithmetic. These investigations in cognitive psychology have influenced a number of researchers active in foundations of mathematics, who are interested in addressing the complex web of issues related to perception, imagery, diagrammatic reasoning and mathematical cognition.

Let me begin by mentioning the project “Géométrie et cognition” led by G. Longo, J. Petitot, and B. Teissier at the ENS in Paris. Their approach emphasizes the need to provide cognitive foundations for mathematics, in opposition to logical foundations à la Hilbert. Their research is strongly influenced by the dramatic developments in cognitive psychology, especially in the area of perception theory. And although mental imagery is not stressed in their ‘manifesto’ (where the emphasis is on perception)[7], it is obvious that their program calls for an account of the cognitive role of mental imagery in mathematics. This ‘cognitive’ approach to the foundations is less concerned with the traditional goals of logical foundation, as it had been pursued in the tradition of proof theory. Rather, it goes back to the tradition represented by Riemann, Poincaré, Helmholtz and Weyl. Moreover, they appeal to Husserl’s phenomenological analyses and in particular the work on genesis of concepts. For this tradition the foundational task is essentially to give an epistemological analysis of the constitutive role of the mind in the construction of mathematics and geometry in particular.

Of great interest is also the epistemological work on visualization carried out by Marcus Giaquinto.[8] One can read Giaquinto’s project as trying to account for the epistemological status of certain experiences of visualization which, he argues, are substantially different both from observation and from conceptual reasoning. There is obviously a “Kantian” flavor to the project. The thesis, as presented by Giaquinto, is that the epistemic function of visualization in mathematics can go beyond the merely heuristic one and be in fact a means of discovery. We are used to associate discovery with the heuristic context. But discovery is taken here in a technical sense according to which “one discovers a truth by coming to believe it independently in an epistemically acceptable way”. The independence criterion is meant to exclude cases in which one comes to believe a proposition just by being told. One of the conditions on the requirement of epistemic acceptability is that the way in which one comes to believe a proposition is reliable. Giaquinto then proposes a case of visualization (a simple geometrical fact about squares) for which he claims that through that process of visualization one could have arrived at a discovery (in the sense above, which does not entail priority) of the result. However, the justification provided by the visualization need not be a demonstrable justification, i.e. a justification that can be checked by intersubjective standards of proof. And nonetheless this is, he concludes, a legitimate way to come to know a mathematical proposition. The upshot of his investigations is the claim that whereas in elementary arithmetic and geometry it is possible to discover truths by means of visualization this is not the case in elementary real analysis, except perhaps in extremely restricted cases. It is important to point out here some important features of Giaquinto's approach. In traditional philosophy of mathematics the emphasis is mostly on major theories, such as arithmetic, analysis, or set theory. The main question that has been pursued is whether these theories are true and, if so, how do we know them to be true. Since deduction can preserve knowledge, usually the question becomes that of the epistemology of the axioms of such theories. Giaquinto asks the analogous question for the case of the individual and his or her mathematical beliefs. How do people know their initial (uninferred) mathematical beliefs? And more generally, how do they acquire their beliefs, whether initial or derived? His strategy is then to investigate how people actually acquire their beliefs, and this is where cognitive psychology comes in. Once we have isolated the cognitive mechanisms of belief acquisition then we can subject them to epistemological analysis and ask whether they are in fact knowledge-yielding. While it is beyond the scope of this paper to address the argumentative line defended by Giaquinto, let me just grant him the thesis and see how it fits within the spectrum of positions in foundations of mathematics. Obviously, his project dovetails quite well with the issues raised by those philosophers of mathematics who insist that foundations of mathematics should address issues concerning the epistemology of mathematics. Giaquinto's last writings on this issue in fact (see this volume) put forth an account of the interaction between perception, visual imaging, concepts and belief formation in the realm of elementary geometry. But, as mentioned before, it is not central to Giaquinto's claims that the types of visualizations he discusses would count as proofs in the traditional sense. He thus moves away from the traditional concerns in philosophy of mathematics in two ways. First of all, he shifts the focus from the community to the individual. Second, since justification will ultimately depend on some unjustified premises that we must hold to be true, the question becomes how do we know these ultimate premises to be true. And that, Giaquinto argues, cannot be done by giving another justification (which would involve a regress) but rather by an epistemic evaluation of the way we come to believe those premises. But this is a question that even those who focus on major mathematical theories will have to address, as the starting point of those theories would have to be accounted for epistemologically. And how could that be done, without going back to the mechanisms of belief acquisition of the individual? In this way, Giaquinto's work shows its relevance also for traditional programs in the philosophy of mathematics.

By contrast, the work carried out by Barwise and Etchemendy on visual arguments in logic and mathematics is motivated in great part by the proof-theoretic foundational tradition.[9] While Giaquinto was mainly concerned with discovery (in the technical sense we have pointed out), Barwise and Etchemendy focus on proof.

Barwise and Etchemendy begin by acknowledging the important heuristic role of visual representations but want to go further:

> We claim that visual forms of representation can be important, not just as heuristic and pedagogical tools, but as legitimate elements of mathematical proofs. As logicians, we recognize that this is a heretical claim, running counter to centuries of logical and mathematical tradition. This tradition finds its roots in the use of diagrams in geometry. The modern attitude is that diagrams are at best a heuristic in aid of finding a real, formal proof of a theorem in geometry, and at worst a breeding ground for fallacious inferences. (Barwise and Etchemendy, 1996, 3).

Their position challenges the "dogma" 'that all valid reasoning is (or can be) cast in the form of a sequence of sentences in some language'. To this effect they aim at developing an information-based theory of deduction rich enough to assess the validity of heterogeneous proofs that use multiple forms of representation (both diagrammatic and verbal). The point is that language is just one of the many forms in which information can be couched. Visual images, whether in the form of geometrical diagrams, maps, graphs or visual scenes of real-world situations are other forms. The goal becomes then that of developing formal systems of reasoning in which diagrammatic elements play a central role. It is important here to keep two different claims in mind. The first claim is that "not all valid reasoning is (or can be cast) in the form of a sequence of sentences from some language." The second claim is that it is possible to construct heterogeneous systems of logic, which unequivocally show that it is possible to reason rigorously with diagrammatic elements. This requires extending to these new systems the analogue of notions of soundness, completeness etc., which are the adequacy conditions for formal systems of deductive inference. And in turn this requires a framework that 'does not presuppose that the information is presented linguistically'. Work along these lines has been done by Barwise, Etchemendy (see 1996) and their students (see Allwein and Barwise (1996); Shin (1994), among others).

What conclusions can one draw from the work that has been achieved in this area?

A far-reaching claim made by Barwise and Etchemendy was that "not all valid reasoning is (or can be cast) in the form of a sequence of sentences from some language". If what is meant is that there are forms of valid reasoning (visual or diagrammatic reasonings) which cannot be expressed in linguistic form, then I claim that the positive developments mentioned above do nothing at all to prove the point.[10] Indeed, even setting up the question in such a way is problematic, for there is very little clarity on what criteria one can appeal to in order to distinguish linguistic systems from visual systems. These issues are at the center of much recent work (Stenning, 2000).

However, the logical precision of these diagrammatic systems allows one to investigate a number of claims that were made for or against the use of visual elements in proofs. For instance, people have often noticed the lack of expressive power of diagrammatic systems. The setting up of diagrammatic systems has given us a better insight into the problem. Consider for instance Venn's idea of representing all relationships between an arbitrary number of classes by means of closed curves. It was obvious to Venn that if one only uses circles, there is no way to go beyond 3 classes, that is the addition of a fourth circle to the diagram will not be able to represent all the possible combinations between 4 classes.

In the case of Euler's diagrams there are also limitations, due to Helly's theorem, which shows that there are consistent sets of set intersection statements that cannot be represented by any diagram of convex curves. In short, it is essential to study how the geometrical and topological features of the representation system affects its expressivity.

Another advantage of setting up formal systems of diagrammatic reasoning is that one can give a logical analysis of the often made claim that diagrammatic systems are intrinsically more efficient. A recent article by Lemon and Pratt (1997) develops a computational complexity approach to the study of diagrammatic representations.

I would like to conclude with a reflection on how this work affects traditional foundational concerns. One claim made by Barwise, Etchemendy, Shin and others is concerned with the foundational issue of reasoning with diagrammatic representations, i.e. that it is possible to reason rigorously with diagrammatic elements. Thus, visual systems are not inherently deceptive, or no more than linguistic systems might be. Here I think that the work done by Barwise, Etchemendy, Shin and others proves the point. What they did was to show that to the traditional model of linguistic rigor we can now add rigorous forms of inference with diagrammatic elements.

However, there are several philosophers of mathematics who are opposed to this traditional approach and are interested in visual reasoning as an essential factor in providing a more 'realistic' philosophy of mathematics, sensitive to its practice and its cognitive roots. I believe that many of them would find the work by Barwise and Etchemendy on diagrammatic reasoning insufficient at best and misguided at worst. The problem, for many people in this tradition, is that exclusive attention to the goal of justification is unacceptable. There are many other important epistemic goals, such as discovery (in Giaquinto's sense), explanation, understanding, genesis of concepts etc., that philosophy of mathematics should account for.

In any case, the work on diagrammatic reasoning accounts for a very minimal part of our employment of visual tools in our logical and mathematical experience. Should the practicing mathematician feel more comfortable using visual or diagrammatic tools in his or her work? I think the work on diagrammatic reasoning does not do much to allay possible worries of being misled by the visual tools in research contexts but it does show that the reasons for why such tools are problematic is not necessarily on account of some intrinsic feature of the visual medium. It is rather that one must always check that the visual medium does not introduce constraints of its own on the representation of the target area. And I doubt this is an issue that can be settled a priori rather than by a detailed case by case analysis of such uses. But after all, mathematicians have been doing just that for more than two thousand years.

ACKNOWLEDGEMENTS

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NOTES

[1]See (Pasch, 1882/1926, 44).

[2]For diagrammatic reasoning in Greek mathematics see Netz (1999). For visualization in non-Euclidean geometry see Reichenbach (1956). For debates on visualization in the arithmetization of analysis see Volkert (1986).

[3]One should recall here Hadamard's distinction between visual and symbolic approaches to mathematical thinking. Hadamard himself claimed to think visually, and following Poincaré characterized mathematicians as falling into two broad classes, the analysts and the geometers. As for Hilbert, quoted by Hadamard: "I have given a simplified proof of part (a) of Jordan's theorem. Of course, my proof is completely arithmetizable (otherwise it would be considered non-existent); but, investigating it, I never ceased thinking of the diagram (only thinking of a very twisted curve), and so do I when remembering it" (Hadamard, 1949, 103).

[4]The problem of the eversion of the two sphere is that of turning it inside out without tearing. For reasons of space I will not give a detailed explanation of what the two problems are. Palais (1999) provides a very readable account of the two results. I trust that the main methodological point will be clear even for those who are not conversant with differential geometry.

[5]Indeed one should not make the mistake of underestimating the complexity of producing 'persuasive' images. For instance in his article Hoffman describes how much of the mathematical community admitted difficulty in understanding the images they were producing and that this forced them to produce more realistic images. See Hoffman, 1987, p.18.

[6]"More likely than not, when one opens a random modern mathematics text on a random subject, one is confronted by abstract symbolic reasoning that is divorced from one's sensory experience of the world, despite the fact that the very phenomena one is studying were often discovered by appealing to geometric (and perhaps physical) intuition.

This reflects the fact that steadily over the last hundred of years the honour of visual reasoning in mathematics has been bismirched. Although the great mathematicians have always been oblivious to such fashions, it is only recently that the "mathematician in the street" has picked up the gauntlet on behalf of geometry. The present book openly challenges the current dominance of purely symbolic logical reasoning by using new, visually accessible arguments to explain the truths of elementary complex analysis." (Needham, 1997, vii).

[7]See http://www.di.ens.fr/users/longo/geocogni.html#anchor1640003 for the program and further references.

[8]The epistemological investigations by Giaquinto (1992, 1994) also take their start from cognitive psychology. In particular, Giaquinto was influenced by Kosslyn, who in his book "Ghosts in the Mind's Machine" comments on the relationship between knowledge and imagery by making the point that previous knowledge constrains the images we come up with. By contrast, Brown (1997) takes its start from the work by Barwise and Etchemendy.

[9]I should immediately point out that the narrative contrast between Giaquinto's work and Barwise and Etchemendy has to be taken with caution. Barwise and Etchemendy focus on diagrammatic reasoning, which is a form of visual reasoning, but have nothing to say about the phenomenology of visualization, which constitutes the main contribution by Giaquinto.

[10]A proper discussion of this topic would quickly lead into cognitive psychology and to the 'imagery debate'. See Tye (1991).

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M. GIAQUINTO

FROM SYMMETRY PERCEPTION TO BASIC GEOMETRY

INTRODUCTION

How do we acquire our basic geometric beliefs? Do those beliefs, acquired in the way that we normally acquire them, constitute knowledge? On the view that I propose, the starting point is visual perception of basic shapes: the geometrical concepts of basic shapes that we form first depend on the way we perceive those shapes. I hold that in having geometrical concepts for shapes, we have certain belief-forming dispositions; these dispositions can be triggered by experiences of seeing or visual imagining, and when they are, we acquire geometrical beliefs. I further claim that the beliefs thus acquired constitute knowledge, in fact synthetic *a priori* knowledge, provided that the belief-forming dispositions are reliable. This is the skeleton of my proposal.

In this article, I will try to illustrate this idea with respect to a very simple truth of Euclidean geometry, namely, that the parts of a square to each side of a diagonal are congruent. There are four parts: (1) Perceiving a figure as a square. (2) A geometrical concept for squares. (3) Getting the belief. (4) Is it knowledge?

1. PERCEIVING A FIGURE AS A SQUARE

Visual object recognition depends on parsing a scene into bordered segments. More specifically, functions required for object recognition – e.g. visual search, texture segregation, and motion perception – require as input a representation of the scene as a set of surfaces, and the construction of such a representation requires as input a representation of the scene in terms of bordered segments (Nakayama et al., 1995). In what follows I take the visual representation of bordered segments for granted, and use it as a basis for introducing some of the ingredients of 2-dimensional shape perception.

1.1. Orientation and reference systems

Perception of an object or figure can be radically affected by its orientation. A well known example first introduced and discussed by Mach (1959) is the square-diamond. A square with a base perceived as horizontal will be perceived as a square and not as a diamond; but a square perceived as standing vertically on one of its corners will be perceived as a diamond, not a square (Figure 1). Irvin Rock drew attention to other examples, such as the difficulty in recognising familiar faces in photographs presented upside down and failure to notice that a figure is the outline of one's country when it is presented at 90° from its familiar north-up orientation (Rock, 1973).

P. Mancosu et al. (eds.), Visualization, Explanation and Reasoning Styles in Mathematics, 31-55.

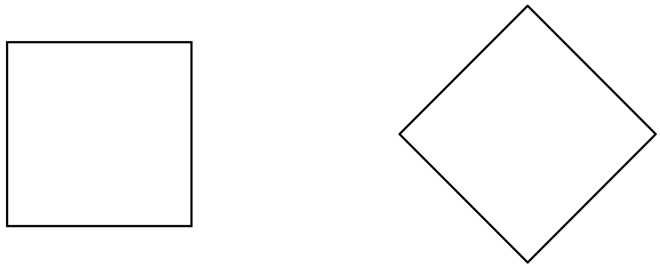

FIGURE 1.

Orientation is relative to a reference system. A *reference system* (RS) is a pair of orthogonal axes, one of which has an assigned 'up' direction.[1] A reference system can be based on features of the perceived object, on the perceiver's retina, head or torso, on the edges of a page (if the object is a diagram), or on the environment (horizon plus gravity). Rock stressed the importance of specifying which reference system is operative when making claims about the effects of orientation on perception· A change of reference system is liable to alter perceptual outcomes. Suppose you are looking at a symbol on a page from the side (Figure 2). Switch from a head-based to a page based reference system and what was perceived as a capital sigma (Σ) may come to be perceived as a capital em (M). Perceptual processing prefers some reference systems to others. View a square with its sides at 45° to floor and ceiling and, as mentioned earlier, it will appear as a diamond; tilt your head 45° so that the figure has sides that are parallel to the retinal axes and it will *still* be perceived as a diamond. This is because, in the absence of additional factors, the visual system prefers environmental axes over retinal axes.

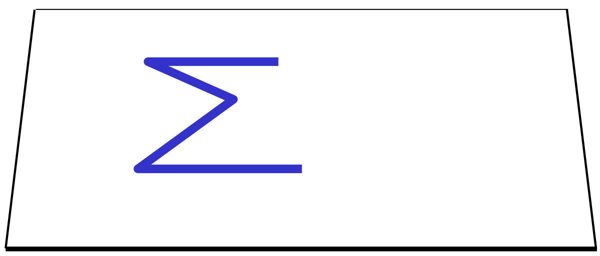

FIGURE 2.

By thinking of the content of a visual representation as a set of descriptions we can make sense of the dependency of perception on orientation. Descriptions in the description set (DS) of a visual representation use a selected reference system. When a square is perceived as a diamond the description set will include the information that the object is symmetrical about the vertical (up-down) axis with one vertex at its top, another at its bottom, and one vertex out to each side. That description will not be in the description set for perception of a figure as a square; that will include instead the information that the object is symmetrical about the vertical axis with one horizontal edge at its top, another at its bottom, and one vertical edge out to each side. The description sets are different, hence the perceptions have different contents. Similarly, the description set for Σ contains 'centred horizontal line at top', which is not in the description set for M. The description set for M contains 'vertical lines at each side', which is not in the description set for Σ.

A couple of warnings about description sets may be helpful. First, the descriptions in a description set are not the perceiver's commentary and they are not expressed in a natural language. They are simply feature representations encoded in a format that has neural realization. The use of the word 'description' is not intended to suggest that the format is sentential rather than pictorial. Secondly, perceivers may not have conscious access to the description sets of their perceptual representations. When we experience a representational change in viewing a constant figure (duck to rabbit; upright Σ to fallen M), there is a change of description sets. But we rarely know just what changes of description are involved.

When we see a figure in an unusual orientation, such as the letter M on its side, how do we recognize it as an M? To answer this we need to distinguish between mere perception and perceptual recognition. Perception involves generating a set of descriptions of what is perceived; recognition involves this and the additional step of finding a best match between the generated description set and a stored description set for the conventional appearance of the figure. In the case under consideration, the page-based and head-based axes coincide and have the same up and down directions, and that will be the preferred reference system, initially at least. The conventional top of the figure, however, will not be perceived as top, since relative to the preferred reference system it is off to one side rather than vertically above; and for the parallel reason its conventional bottom will not be perceived as bottom. Matching needs the conventional top and bottom of the figure to be top and bottom with respect to the up and down directions of the preferred reference system. This can be achieved by selecting a different reference system, one

FIGURE 3.

that assigns up and down directions to an axis that is horizontal with respect to the page and head.

1.2. Intrinsic axes and frame effects

To recap briefly, the visual system prefers an environmental reference system over an egocentric reference system, e.g. gravitational over retinal axes, when these do not coincide; and both may be overridden by consciously directed attention, even when they do coincide. In fact they may be overridden *without* consciously directed attention when the figure viewed has a strong intrinsic axis. For example, an isosceles triangle with large equal angles and a narrow third angle will be perceived as having the narrow vertex as its top and the short side opposite the narrow vertex as its base, even if the narrow vertex is way off pointing up with respect to environmental, egocentric and page-based axes. The bisector of the narrow vertex is the intrinsic axis to which the visual system is drawn, and so one naturally perceives the triangle in figure 3 as tilting and pointing in the 'North East' direction, while the accompanying figure, lacking a strong intrinsic axis, need not be seen that way.

When is a line through two points on the perimeter of a figure a strong intrinsic axis? Let the part of a line falling within the boundary of the figure be called its internal segment. One proposal is that if the internal segment of one such line is significantly longer than all the others, such as the internal segment of the major axis of an obvious ellipse, that line will be the figure's strong intrinsic axis (Marr and Nishihara, 1978). There is some evidence that length is an important factor in recovering the descriptions of a perceived shape (Humphreys, 1983). But the results of experiments on frame effects led Stephen Palmer (1990) to conclude that reflection symmetry outweighs length as a determinant of the intrinsic axis used by the visual system. (The only symmetries mentioned in this study will be reflection symmetries.) If a figure has more than one pair of orthogonal axes of symmetry, which pair of axes the visual system uses as reference axes depends on surrounding

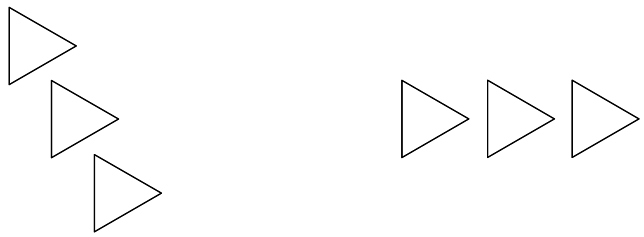

FIGURE 4.

features of the scene. Palmer and his colleagues showed that equilateral triangles can be perceived as pointing in one of three directions and the selected direction depends on contextual features (Palmer, 1980; Palmer and Bucher, 1981). Compare the central equilateral triangles in each of the three-triangle arrays in figure 4. Although all the triangles have the same orientation with respect to page and retina, the triangles in the left array are likely to be seen initially as pointing in the 11 o'clock direction while those in the right array are likely to be seen initially as pointing in the 3 o'clock direction. This can be explained in terms of the coincidence of axes of symmetry. In the left hand array the 11 o'clock symmetry axes coincide, whereas in the right hand array their 3 o'clock symmetry axes coincide.

1.3. Reflection symmetry and shape perception

Other frame effects can also be explained in terms of axes of reflection symmetry. Earlier I mentioned Mach's observation that a square with sides at 45° to the edges of the page (and to retinal axes) is seen not as a square but as a diamond, unlike a square whose sides are horizontal or vertical with respect to the page. But the Mach phenomenon can be offset by additional configurations, such as other squares (as in figure 5a) or a rectangular frame (as in figure 5b).[2] Although the central squares are in the diamond orientation with respect to page and retina one sees them as squares. Palmer explains this by the fact that the bisector of the diamond's upper right and lower left sides is the diamond's only symmetry axis that coincides in 5a with a symmetry axis of the accompanying diamonds and in 5b with a symmetry axis of the surrounding rectangle. One sees a square as a diamond rather than a square just when the visual system uses an axis through opposite *vertices* as the main up-and-down axis; one sees it as a square rather than a diamond just when the visual system uses an axis through opposite *sides* as the main up-and-down axis. When a symmetry axis of one figure coincides with a

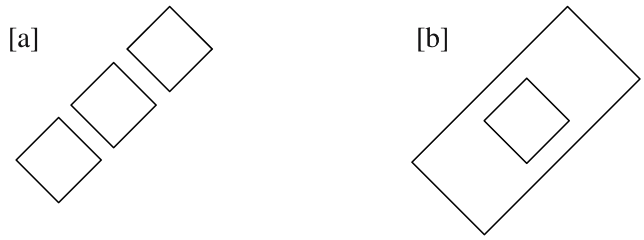

FIGURE 5.

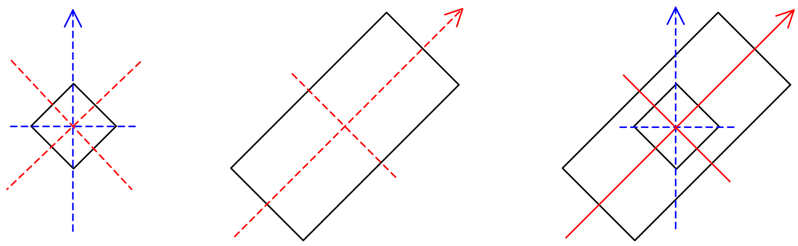

FIGURE 6.

symmetry axis of one or more surrounding figures, the visual system is more likely to use that axis as the main up-down axis for feature descriptions. It is as though there were an augmentation rule for the salience of a symmetry axis: the salience of a symmetry axis of a figure increases when the figure is accompanied by another figure symmetrical about the same axis. Figure 6, adapted from one of Palmer's (1985) figures, illustrates this for the configuration in 5b. Symmetry axes are shown for the diamond, the rectangle and then the two combined.

Before investigating description sets for these shapes, an apparent circularity in this account must be removed. To perceive a figure as having a certain reflection symmetry, the visual system must first select the relevant axis as an axis of possible reflection symmetry. But in the account given above, in order to select an axis for generating descriptions, the visual system must first determine the figure's reflection symmetries. This problem disappears if symmetry perception involves two processes: a fast but rough test of reflection symmetry in all orientations simultaneously; then, if one or more axes of symmetry are detected, a more precise evaluation of symmetry about one or more of these axes in turn.[3] The initial selection of reference

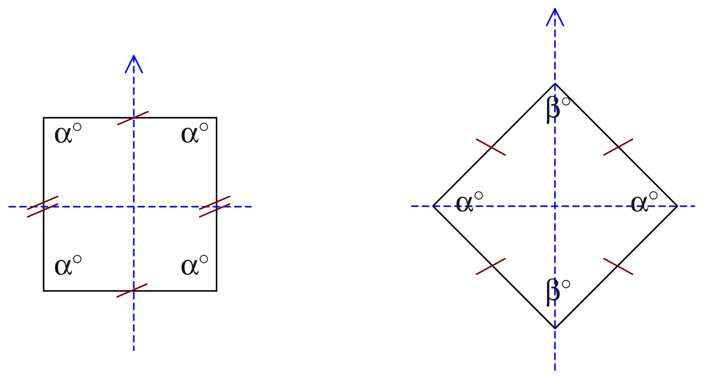

FIGURE 7.

system axes for generating descriptions depends only on the rough and rapid process of symmetry detection, while perceiving the figure as having a certain reflection symmetry (so that that symmetry is in the figure's description set) depends on the second more precise evaluation process.[4]

Once axes are selected, reflection symmetries may have a further effect on which feature descriptions are generated, hence, on which features are perceived. This can be illustrated by examining a square in the normal orientation and in the diamond orientation. Since the reflection symmetries about the selected axes are perceived, features entailed by those symmetries may also be perceived. Look first at the square in normal orientation (on the left in figure 7). It is perceived as symmetrical about its vertical and horizontal axes. But it would not look symmetrical about the vertical axis unless its upper angles looked equal and its lower angles looked equal. It would not look symmetrical about the horizontal unless the angles on the left looked equal and the angles on the right looked equal. So perceiving these symmetries entails perceiving every pair of adjacent angles as equal. Perceiving these symmetries simultaneously, one perceives all the angles as equal.

Compare this with the perception of angles of the diamond (on the right in figure 7). In that case perceiving the symmetries entails perceiving *opposite* angles as equal, not on perceiving adjacent angles as equal. If all pairs of adjacent angles are equal, all angles are equal; but all pairs of opposite angles may be equal without all angles being equal. If we pressed the left and right vertices closer together so that the angle α° is greater than the angle β° of the top and bottom vertices, the figure would still be perceived as a diamond. So perceiving a figure as a square entails that all its angles are perceived as equal, whereas perceiving it as a diamond does not.

How does perception of the symmetries about the vertical and horizontal axes relate to the perception of the *sides* of the figures? It is the reverse of the relation between perceiving the symmetries and the angles. When seeing the figure as a square, simultaneously perceiving the symmetries about the vertical and horizontal axes entails perceiving its opposite sides as equal, not on perceiving all its sides as equal. When seeing it as a diamond, simultaneously perceiving the symmetries entails all sides being perceived as equal.[5]

We can thus explain the Mach phenomenon in terms of the selection of a page-based reference system of vertical and horizontal axes for producing feature descriptions. Though the square and the equiangular diamond are congruent, their different orientations with respect to the reference system produces different feature descriptions, hence different perceptual contents.

This much explains the perceptual difference between a square and an equiangular diamond, but it does not explain the perceptual difference between a square and other rectangles (nor the perceptual difference between an equiangular diamond and other diamonds). Even when a figure is perceived as a rectangle but not as a square, its adjacent angles will look equal and its opposite sides will look equal. Clearly what distinguishes a square from all other rectangles is that all its sides are equal. The visual system can pick up this additional information by means of a secondary set of orthogonal axes, the axes at 45° to the primary pair of axes. Perceiving the symmetries of the square about these diagonal axes involves perceiving adjacent sides as equal, and that is enough to distinguish the square from other rectangles in perception. As there are now two sets of orthogonal axes involved, the visual system must discriminate between them. Only one of these sets can be used as the reference system for descriptions. The axis-pair of the reference system is primary. A secondary pair of axes can be singled out by reference to the primary pair. In this case the other axes might be described as angle-bisectors of the primary pair.

An alternative possibility is that the visual system uses co-ordinates based on the axes of the reference system with a Euclidean metric. Using this co-ordinate system, a measuring mechanism computes lengths, which serve as inputs to a system that encodes spatial properties based on size, as distinct from a system for coding non-metric spatial relations such as connected/apart, inside/outside, above/below.[6] Using the co-ordinate system we could detect equality of sides, and thereby distinguish squares from other rectangles. But a more economical way of achieving this end is by perceiving the symmetries about the diagonal axes, and so I will assume that this is how the visual system operates.

1.4. A description set for squares

The foregoing provides all the ingredients needed for a description set for squares. Let V and H be the vertical and horizontal axes of the reference system. Then to perceive a figure as a square it suffices that the visual system detects the following features.

Plane surface region, enclosed by straight edges:
edges parallel to H, one above and one below; edges parallel to V, one each side.
Symmetrical about V.
Symmetrical about H.
Symmetrical about each axis bisecting angles of V and H.

My suggestion is that this is the description set used by the human visual system for perception of squares.[7] This is not to say that these are the only features of squares that we perceive. But whenever the visual system detects these features, the figure will be perceived as a square. In short, these features are enough, though they are not all. The fact that this description set is so economical may help to explain why squares are so basic to our visual thinking. Thinking about squares requires that, in addition to our capacity to perceive a figure as a square, we have a concept for squares. How a concept for squares relates to square perception is the subject of the next section.

2. A GEOMETRICAL CONCEPT FOR SQUARES

A concept, as that term is used here,[8] is a constituent of a thought, and a thought is the content of a possible mental state which may be correct or incorrect and which has inferential relations with other such contents. Neither thoughts nor concepts are here taken to be linguistic entities. Though thoughts can usually be expressed by uttering a sentence and concepts can usually be expressed by a word or phrase in an uttered sentence, thoughts are not taken to be sentence meanings, and concepts are not taken to be lexical meanings.[9]

Why assume that thoughts have constituents? They are needed to account for inferential relations between thoughts. Consider the following inferences.

> Mice are smaller than cats. Cats are smaller than cows. Therefore mice are smaller than cows.

Tom was an uncle. Therefore Tom was a brother.

The validity of the first inference depends on a common constituent of all three thoughts, here signalled by the expression 'smaller than'. The validity of the second depends on the connection between the constituents expressed by 'uncle' and 'brother'. Concepts are typically constituents of thoughts on which some of their inferential relations depend. In fact we can characterise a concept in terms of these relations. To possess a concept one must be disposed to find certain inferences[10] cogent without supporting reasons. So we can in principle specify a concept in terms of these basic inferences.[11] For example, we can specify a concept for uncles thus:

> {**uncle**} is that concept C which one possesses if and only if one is disposed to find inferences of the following forms cogent without supporting reasons:
>
> x C y. Therefore x is a brother of a parent of y.
>
> x is a brother of a parent of y. Therefore x C y.

2.1. A perceptual concept for squares

Before proceeding to specify a *geometric* concept for squares, I will specify a *perceptual* concept for squares. This is because the initial geometric concept for squares that I am aiming at is a slight refinement of a perceptual concept for squares, and is most easily explained if the perceptual concept is introduced first. The actual specification of a perceptual concept will be quite complicated. But the root idea is that the perceptual concept for squares centres on a disposition to judge something square when it appears square and one does not suspect that circumstances are illusiogenic or one's vision is malfunctioning. This kind of account would be circular (hence fail to specify any concept) if it were not possible for something to appear square to a person without that person's deploying the perceptual concept for squares. As a square figure appears square just when its squareness is perceived, we can see that this is possible, by noting the distinction drawn earlier between merely perceiving the squareness of a figure and perceptually recognizing the figure as square. Recognizing a figure as square involves a perceptual experience of it which draws on an antecedently acquired category representation of squares: not only must visual processing generate the descriptions in the description set for squares, it must also make a best match between the set of generated descriptions and the previously stored description set which constitutes the category representation of squares. But for merely perceiving the squareness of a figure it will suffice if visual processing generates the descriptions in the description set for squares, without also matching the generated descriptions with those belonging to a stored description set for squares. Thus there is no pressure at all to hold that perceiving the squareness of a figure must involve deploying a concept for squares.[12]

To specify a perceptual concept for squares we use the features in the description set, but allow for imperfections, as we can recognize a figure as a square even if, for example, it is visibly not completely enclosed or its sides are visibly not perfectly straight. The degree of imperfection allowable is not something that I can specify; obviously the lines must be sufficiently straight and the figure sufficiently enclosed to generate the feature descriptions 'straight line' and 'closed figure', and so on. I will just use the modifier 'n/c' for 'nearly or completely' to deal with this. Here is one further point before giving the perceptual concept. We can apply perceptual concepts to things that we are not perceiving. To cater for this the perceptual concept will have two parts, for the cases in which one is thinking about an item one perceives, and an item one does not perceive, respectively.

The concept {**square**} is the concept C that one possesses if and only if both of the following hold:

(a) When an item *x* is represented in one's perceptual experience as a n/c plane figure n/c enclosed by n/c straight edges, one edge above H and n/c parallel to it, one below H and n/c parallel to it, and one to each side of V and n/c parallel to V, and as n/c symmetrical about V and n/c symmetrical about H, and as n/c symmetrical about each axis bisecting angles of V and H – when *x* is thus represented in perceptual experience and one trusts the experience, one believes without extra reasons that that item *x* has C. Conversely, when one trusts one's perceptual experience of an item *x*, one believes that *x* has C only if *x* is represented in the experience as a n/c plane figure n/c enclosed by n/c straight edges ... etc.

(b) Let 'Σ' name the shape that figures appear to have in the experiences described in clause (a). When an item *x* is unperceived one is disposed to find inferences of the following forms cogent without supporting reasons:

x has Σ. Therefore x has C.
x has C. Therefore x has Σ

Obviously what perceptual concept for squares one actually possesses depends on the description set actually used by the visual system in perceiving something as square. There are possibilities other than the description set given here that are consistent with the data about shape perception. Also, it is at least theoretically possible that different people have different perceptual concepts for squares. So it would be wrong or at least potentially misleading, given the present state of knowledge, to talk of *the* perceptual concept for squares. But there is no impropriety in talking of *the* perceptual concept {**square**}, since that is the concept individuated above.

2.2. A geometrical concept for squares

The perceptual concept {**square**} is a vague concept; that is, there may be things for which it is indeterminate whether they fall under the concept. This is because there is some indeterminacy in the extensions of perceptible properties in the description set square-perception, such as straightness and reflection symmetry. Among things which are clearly square, such as a handkerchief or the surface of a floor tile, we can sometimes see one as a better square than another: edges sharper, straighter or more nearly equal in length, for example, corners more exactly rectangular, halves more symmetrical[13] and so on. Sometimes we can see a square, one drawn by hand for instance, as one which could be improved on and we can imagine a change which would result in a better square. It can be part of the content of an experience of those having the concept {**square**} that one square is a better square than the other.

It can also be part of the content of experience that a square is perfect. Since there is a finite limit to the acuity of perceptual experience, there are lower limits on perceptible asymmetry and perceptible deviation from straightness. Asymmetry about an axis which is so slight that it falls below the limit will be imperceptible; similarly for non-straightness. So any figure veridically perceived as symmetrical about an axis α, if its asymmetry about α falls below the lower limit of perceptible asymmetry, will be perceived as maximally symmetrical about α; similarly for straightness. Hence there is a maximum degree to which a bounded plane surface region can be perceived as symmetrical about a given axis, and a maximum degree to which a border segment can be perceived as straight. When in experiencing something as square these maxima are reached, this fact may be encoded in the description set generated in the perceptual process. In that circumstance the perceived item will be experienced as having perfectly straight sides, and as perfectly symmetrical about vertical and horizontal axes. If in addition the same applies with respect to the other features in the description set for squares, the item will appear perfectly square. To be precise, let us say that an item *appears perfectly square* when it is represented in one's perceptual experience as a perfectly plane figure completely enclosed by perfectly straight edges, one edge above H and perfectly parallel to it, one below H and perfectly parallel to it, and one to each side of V and perfectly parallel to V, and as perfectly symmetrical about V and perfectly symmetrical about H, and as perfectly symmetrical about each axis perfectly bisecting angles of V and

H. Just as the root of the *perceptual* concept for squares centres on a disposition to judge something square just when it appears square and one trusts the experience, so the root idea of an initial *geometrical* concept of squares centres on a disposition to judge something square just when it appears *perfectly* square to one and one trusts the experience. The only difference is that the features that figure in the geometrical concept must be perfect exemplars of their kind. Hence where, in the specification of the perceptual concept, the figure or parts of it are required to be *nearly or completely* this or that (e.g. n/c straight edges, one edge above H and n/c parallel to it, one below H and n/c parallel to it, and one to each side of V and n/c parallel to V . . .), in the specification of the geometrical concept they must be *completely* this or that. This is the only difference between the perceptual concept and the initial geometrical concept; the specifications are just the same except that every occurrence of the qualifier 'nearly or completely' gets cut down to 'completely'.

2.3. *From concepts to belief-forming dispositions*

Concept possession may bring with it a belief-forming disposition. I will try to show this for the case of someone possessing the concept {**perfect square**}. I assume that one also possesses a concept for restricted universal quantification, as in 'All Fs' or 'Every F'. If one has that concept, whenever one finds cogent inferences of the form '*x* has F, so *x* has G', one is disposed to believe the proposition 'Every F has G'. Now suppose that having these concepts you perceive a particular surface region *x* as perfectly square. You can think of its apparent shape demonstratively, as *that* shape. Letting 'S' name that shape thought of demonstratively, your coming to believe of some item that it has S will result in your believing that it is perfectly square; and your coming to believe of some item that it is perfectly square will result in your believing that it has S.[14] This is due to your having the concept {**perfect square**}. As you also have a concept for restricted universal quantification, you will have the following disposition:

(PS) If you were to perceive a figure as perfectly square, you would believe of its apparent shape S that whatever has S is perfectly square, and that whatever is perfectly square has S.

If you have this disposition, merely seeing a figure as perfectly square will produce in you a pair of general beliefs. Although these beliefs are not logical trivialities, they are not empirical truths either, as epistemic rationality does not require that one has evidence for these beliefs: one does not need to inspect a sample of the domain, e.g. of the class of things having S, for rationally believing that whatever has S is perfectly square. This then illustrates a rational way of getting a belief as a result of a concept-generated disposition triggered by a visual experience. In a similar way we can acquire the geometrical belief that the parts of a square either side of a diagonal are congruent. I will now try to show this.

3. GETTING THE BELIEF

Suppose one has a concept for congruence, i.e. sameness of both shape and size. If a figure ***a*** appears to one symmetrical about a line *l* and one trusts the perceptual experience, one will believe that the parts of ***a*** either side of *l* are congruent. We can further say that if ***a*** appears to one symmetrical about *l*, one will believe that if ***a*** is as it appears (in shape), the parts of ***a*** either side of *l* are congruent. Such a belief is just about the particular figure ***a***; it contains no hidden generality. But given that ***a*** appears symmetrical about *l* one will have the stronger belief that if ***a*** *were* as it appears (in shape), the parts of ***a*** either side of *l would be* congruent. This belief does have some generality, as it covers a range of possible cases.[15] One would believe this because one realizes that it is only the apparent shape of ***a*** that is relevant: having the apparent shape of ***a*** suffices for the attributed property. So one has a yet more general belief, about *any* figure having the apparent shape of ***a***, that it has the attributed property. This is the level of generality that we require for geometrical truths, so let us focus on our disposition to form this belief. Of course, the attributed property in this case is not congruence of the parts of the figure either side of the line *l*, because the line *l* is just a line through ***a***. What we have in mind, for a figure *x* having the apparent shape of ***a***, is a line through *x* that would correspond to *l* (through ***a***) if ***a*** were as it appears. Correspondence is this relation: a line *k* through ***b*** *corresponds to* line *l* through ***a*** if and only if some similarity mapping of ***a*** onto ***b*** maps *l* onto *k*.[16] To put all this together, suppose one has geometrical concepts for correspondence, as well as for similarity and congruence. Then one will have the following belief-forming disposition.

(C) If one were to perceive a plane figure ***a*** as perfectly symmetrical about a line *l*, then (letting 'S' name the apparent shape of ***a***) one would believe without extra reasons that for any figure *x* having S and for any line *k* through *x* which would correspond to *l* through ***a*** if ***a*** had S, the parts of *x* either side of *k* are perfectly congruent.

If a figure appears perfectly square to one, it appears perfectly symmetrical about its diagonals. This is because perfect symmetry about a line that bisects the angle made by the vertical and horizontal axes of the reference system is a feature in the description set for perceiving perfect squareness, and a line that bisects those axes is a diagonal of the square. Given a concept for diagonals as well as the concepts that provide one with disposition (C), one will have a disposition that is a special case of (C):

> If one were to perceive a plane figure ***a*** as perfectly square, then (letting 'S' name the apparent shape of ***a***) one would believe without extra reasons that for any figure *x* having S and for any line *k* through *x* which would correspond to a diagonal of ***a*** if ***a*** had S, the parts of *x* either side of *k* are perfectly congruent.

Now recall the disposition one has a result of possessing the concept {**perfect square**}:

(PS) If you were to perceive a figure as perfectly square, you would believe of its apparent shape S that whatever has S is perfectly square, and that whatever is perfectly square has S.

Because of this, when one perceives ***a*** as perfectly square, one moves freely in thought between 'having S' and 'being perfectly square'. So in the statement of the disposition above, the special case of (C), we can eliminate talk of S. The consequent belief is this:

> For any perfect square *x* and for any line *k* through *x* which would correspond to a diagonal of ***a*** if ***a*** were perfectly square, the parts of *x* either side of *k* are perfectly congruent.

But given a concept for diagonals as well as the concept {**perfect square**}, one would be disposed to think of a line through perfect square *x* which would correspond to a diagonal of ***a*** if ***a*** were perfectly square as, simply, a diagonal of *x*. Thus one has a disposition that is a special case of (C) with this consequent belief:

> For any perfect square *x* and for any diagonal *k* of *x*, the parts of *x* either side of *k* are perfectly congruent.

Spelled out then, the disposition is this.

(CPS) If one were to perceive a plane figure ***a*** as perfectly square, one would believe without extra reasons that for any perfect square *x* and for any diagonal *k* of *x*, the parts of *x* either side of *k* are perfectly congruent.

The point here, the truly remarkable point, is that if the mind is equipped with the appropriate concepts, a visual experience of a particular figure can give rise to a general geometric belief. In short, having appropriate concepts enables one to 'see the general in particular.' One cannot have those concepts without having a disposition that can be triggered by a visual experience to form a general belief. In the example at hand this is the target belief that the parts of a square to each side of a diagonal are congruent.

The original question is how we acquire this belief. Of course, people may get this belief in different ways. Moreover, it is an empirical question whether anyone gets this belief in the way that I have described. What is suggested here is merely one possibility. In one respect it is a rather unlikely possibility. How often do we see something as *perfectly* square? A closely related possibility is one in which the triggering experience is of the kind described except that the figure is seen as a square but not a perfect square. In this case I suspect that one can acquire the target belief in the same way except that the route goes through visual imagination: perceiving the figure as a square causes one to imagine a perfect square.[17] This is possible if, as I believe, possession of the relevant concepts gives rise also to similar belief-forming dispositions activated by visualizing rather than visual perceiving.[18] There is not space to pursue this here. These suggestions are, as I said, mere possibilities, to be eliminated or modified in the light of future findings. But they are partial answers to the Kantian question 'How is it possible to have (basic) geometrical knowledge?' which respect the role of sensory experience without collapsing geometry into an empirical science.

4. IS IT KNOWLEDGE?

If one comes to believe in the way described above that the parts of a square to each side of a diagonal are congruent, is that belief knowledge? To be knowledge the belief must be true, it must have been acquired in a reliable way, and there must be no violation of epistemic rationality in the way it was acquired and maintained. In my view these three conditions suffice for knowledge. It is at least arguable that there is a further condition: the believer must have a justification for the belief. I will briefly address the question in the light of these four conditions, and then respond to a couple of objections. We can deal with the first condition quickly. The belief is about squares in space as it would be if it were as the mind represents it, that is, in Euclidean space. In Euclidean space the parts of a square either side of each diagonal are congruent. So the belief is true. This leaves the conditions of reliability, rationality, and justification.

4.1. Reliability

If one reaches the belief in the way described, the belief state results from the activation of the belief-forming disposition (CPS). So the question we have to answer is whether this belief-forming disposition is reliable. In standard cases, a belief-forming disposition is reliable in just this circumstance: if the antecedent condition were realized, the belief mentioned in the consequent would be true. In this case the disposition is somewhat trivially reliable, because the belief mentioned in the consequent (that the parts of a square either side of a diagonal are congruent) would be true whether or not the antecedent condition is realized. But (CPS) is just a special case of (C), one which one has because one has (C) together with the disposition (PS). The reliability of (C) is not at all trivial, as the belief mentioned in the consequent of (C) might easily be false if the antecedent condition were unrealized. Yet (C) is reliable, for it is clear by examining (C) that if its antecedent condition were realized the belief mentioned in the consequent would be true. In the same way the disposition (PS) is non-trivially reliable. So I do not think that there can be any serious doubt about the reliability of this way of getting the belief.

4.2. Rationality

To qualify as knowledge a belief state must satisfy certain rationality constraints. Suppose for instance that having acquired a true belief *b* in a reliable way you become aware of having other beliefs with as much justification as *b* which are inconsistent with *b*. In this circumstance believing *b* is epistemically irrational and so cannot count as knowledge. There are other rationality constraints. There is no good reason to think that it is impossible or even difficult to meet rationality constraints. The contrary thought arises when one imposes rationality constraints that are much too strict. An example is the view that consistency of one's total belief set is required for avoiding irrationality. This is too harsh because it overlooks the possibility of arriving at a number of jointly inconsistent beliefs, each with explicit justification, when the inconsistency is extremely difficult to detect. In that case the believer would be unlucky but not necessarily irrational. In the absence of any plausible argument to the contrary, I take it that it is possible, perhaps easy, to get a belief by activation of reliable belief-forming dispositions and keep it, without violations of rationality. Thus one may come to believe the target geometrical truth in a reliable way and keep it without irrationality. In such circumstances the belief has an epistemically valuable status. I hold that it is knowledge.

4.3. Implicit justification

Some people say that a belief is not knowledge unless the believer has a justification for the belief. This is too strong if it is required that the believer is able to express a justification, otherwise young children would not have knowledge. A more plausible version of this doctrine requires only implicit justification. But what that comes to is not clear. If it suffices that the person's beliefs can be marshalled so as to provide a justifying argument, the requirement can be met. Here is the justifying argument.

1. x is a perfect square. [Assumption]
2. ∴ For any y perceived as perfectly square, x is as y appears.
3. Anything perceived as perfectly square appears symmetric about its diagonals. [Description set for squares]
4. ∴ x is symmetrical about its diagonals.
5. ∴ The parts of x either side of a diagonal are congruent.
6. ∴ The parts of any perfect square either side of a diagonal are congruent. [Discharging the assumption]

If implicit justification involves something more, I would not accept that knowledge requires implicit justification. The kind of implicit justification that is available, on top of the satisfaction of the reliability and rationality conditions, is enough to make the attribution of knowledge safe.

4.4. First objection: no a priori knowledge

On my account a visual experience causes the belief, but does not play the role of reasons or grounds for the belief, as it is not necessary for the believer to take the experience to be veridical – it is enough that a perceived figure *appears* perfectly square. So the visual experience is used neither as evidence nor as a way of recalling past experiences for service as evidence. The visual experience serves merely to trigger certain belief-forming dispositions. So this way of acquiring the belief is *a priori*.

The first objection is simply that there is no *a priori* knowledge, as no belief whatever is immune from empirical overthrow. The argument is Quine's, but it starts from a point made by Duhem, that what constitutes evidence for or against a belief depends on what other beliefs we hold fixed (Quine, 1960; Duhem, 1914). When we find that our beliefs as a whole conflict with observations, we may reject seemingly non-empirical beliefs in order to bring the totality of our beliefs into line with our observations. An oft-cited example is the overthrow of the Euclidean Parallels Postulate. Hence the acceptability of a belief depends on its belonging to a totality of beliefs that fits well with our experience. Thus the acceptability of any belief depends on experience, and so cannot be an instance of *a priori* knowledge.

The most important point in reply to the Quinean objection is this. The way in which the belief about squares in my account is *a priori* relates to its genesis: no experience is used as evidence in *acquiring* the belief. This is consistent with the possibility of *losing* the belief empirically. So even if Quine were right that all beliefs are vulnerable to empirical overthrow, that would not show that beliefs acquired in the way described above could not be knowledge. Secondly, the argument for the claim that no belief whatever is immune from empirical overthrow is inconclusive. It is true that some seemingly non-empirical beliefs have been overthrown, such as the belief that the shortest distance between two physical points is a straight line. But there may be others that we cannot rationally reject in order to accommodate observations which conflict with the corpus of our beliefs.[19]

4.5. Second objection: no conceptual knowledge

The second objection, put by Paul Horwich (2000), is that there is no conceptual knowledge. The argument is that there may be nothing in reality answering to a concept (no reference or semantic value), in which case general thoughts that issue from the concept will not be true. So in order to know of a conceptual thought that it is true, one must know that the concept has a reference, and that knowledge must have an evidential basis independent of the concept. As an example, consider Priestley's concept of phlogiston. That involves a number of inference forms, e.g. 'x is combustible; $\therefore$ x contains *phlogiston*.' But nothing simultaneously satisfies all those inference forms. Although the belief that all combustible matter contains phlogiston issues from the concept, that could not be known without knowing that there really is some kind of substance answering to Priestley's concept of phlogiston. Exactly parallel remarks apply with respect to the belief that the parts of a square either side of a diagonal are congruent. Even though the belief issues from the concept {**perfect square**} and others, we could not know the belief to be true without knowing that there really is something answering to the concept {**perfect square**} and we cannot get that knowledge from merely having the concept. So this is a challenge to the reliability of the way in which the belief about squares was reached.

There are two complementary replies to this objection. First, I think that we do in fact know that something answers to the concept {**perfect square**}. This is because the visual experiences involved in acquiring the concept {**perfect square**} acquaint us with the property of being perfectly square. Not that we ever see things that are perfectly square: perhaps nothing perceptible is in any way geometrically perfect, as Plato maintained. Rather we become aware of the *possibility* of a figure's being perfectly square, as a result of the visual experiences that produce in us the concept.[20] I think that the same is true of the other concepts involved in the target belief, such as concepts for diagonal, the part-of relation, and for congruence. This is in stark contrast to the phlogiston case. In that case the existence of a kind of substance answering to the concept of phlogiston was merely postulated, and that postulation remained to be justified. So, even though the belief about perfect squares may not be *purely* conceptual, we have no reason to doubt that it is knowledge.

The second reply is this. The central claim of the objection is that in order to know a truth we must know that its constituent concepts each have a reference. (I take it that negative existential truths are not under consideration here.) While this is clear for concepts introduced by explicit definition or explicit postulation, it is not clear for other concepts. For example, at a fairly early stage in language competence one comes to know that for any statements S and T, if the statement of the form 'S and T ' is true, so is the statement S. This could not be true unless the concept for conjunction here expressed by 'and' had a reference or semantic value, which in this case is the truth function for conjunction. But we surely do not come to know that the concept has this or any other reference until much later, if at all, when we start thinking about semantics. The condition on knowledge that the objection is based on is too strong. It is reasonable only where a certain class of concepts is involved, and we do not have reason to think that basic geometrical concepts belong to this class. So, putting these two replies together, it is clear that the objection poses no threat to the knowledge claim made in this paper.

5. SUMMARY

How do we know basic geometrical truths? In answer to this I have presented one possibility for a belief about squares, in the hope that it would serve as a guide in similar cases. (1) The story started with an account of perceiving squareness in which visual detection of reflection symmetries is crucial. This is built into a stored category representation or description set for squares. (2) A perceptual concept for squares uses that description set, and a basic geometrical concept for squares is obtained by slight modification of the perceptual concept. (3) Possessing these concepts (and others) entails having certain belief-forming dispositions, which can be triggered by accessing the stored description set for squares either through seeing something as square or by visualizing a square. When a visual experience thus triggers the relevant belief-forming disposition, the experience does not have the role of evidence for the resulting belief. (4) The belief acquired this way is, or at least can be, knowledge. The mode of acquisition is reliable, and there need be no violation of epistemic rationality. Also, there is some sense in which the believer has an implicit justification for the belief. The only two serious objections that I know about can be met.

One final point. This manner of acquiring the belief is non-empirical, because the role of experience is not to provide evidence. At the same time, some visual experience is crucial in triggering the relevant belief-forming disposition; and it is clear that this way of reaching the belief involves no unpacking of definitions, conceptual analysis or logical deduction. Hence it must count as non-analytic. Given that 'non-analytic and non-empirical' translates as 'synthetic *a priori*', we have a vindication of Kant's claim that some knowledge is, or at least can be, synthetic *a priori*.

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NOTES

[1]If one axis is assigned up-down direction, surely the other is assigned left-right direction? Not necessarily. Reorienting figures by reflection about their vertical (up-down) axis has little effect on perceived shape (Rock, 1973). This could be because the horizontal axis is not assigned left-right direction – but then codes of features on one side of the vertical axis would have to bracketed together somehow, to separate them from codes of features on the other side, otherwise there would be left-right confusion. For further evidence that shape descriptions do not include left-right information see Hinton and Parsons (1981).

[2]See (Palmer, 1985). Rock (1990a) cites Kopferman as the source of the frame effect illustrated in figure 5b.

[3]If the axes of probable symmetry include vertical, horizontal, 45° diagonal axes and others, that is likely to be the order in which they are evaluated for symmetry; see (Goldmeier, 1972; Rock and Leaman, 1963; Palmer and Hemenway, 1978).

[4]This two-process model was proposed by Palmer and Hemenway (1978) to account for response-time data that conflict with the predictions of a model consisting of a single sequential symmetry evaluation procedure proposed by Corballis and Roldan (1975). A two-process model is also used by Bruce and Morgan (1975) to account for differences they found among small violations of symmetry, including the fact that some are detected much faster than others.

[5]This is an elaboration of the analysis in (Palmer, 1983). Evidence that these are the relevant features distinguishing the two shape perceptions comes from the fact that the figures we perceive as squares we view as special kinds of rectangle, not special kinds of rhombus, whereas squares we perceive as diamonds we view as special kinds of rhombus. Palmer cites Leyton: A unified theory of cognitive reference. Proceedings, 4th Annual Conference of the Cognitive Science Society, 1982.

[6]This is motivated by experimental results reported in (Kosslyn et al., 1989).

[7]How would the description set for perceiving a figure as an equilateral diamond differ from this? It will not contain the feature of sides parallel to the axes V and H of the reference system. A candidate description set for the equilateral diamond is this: Plane surface region, enclosed by straight edges, with vertices on H, one to each side of V, and vertices lying on V, one to each side of H; symmetrical about V; symmetrical about H; symmetrical about each axis bisecting angles of V and H.

[8]The term 'concept' is also used for word sense, explanatory theory, category representation, prototype (i.e. representation used in typicality judgements) and others. See the introduction of (Margolis and Laurence, 1999).

[9]In general, thoughts and concepts seem too fine to be identified with expression meanings. But I do not insist on this view, if only because the individuation of expression meanings is such a slippery, obscure and contentious matter.

[10]This is a slight oversimplification, if inferences are transitions between thoughts. In some cases one needs to consider transitions from content-bearing mental states that are not thoughts. My use of the word 'inference' here is intended to include such transitions.

[11]The idea is Peacocke's (1992). This view of concepts (thought constituents) is minimalist. It is consistent with the approach taken in this paper that minimalism misses out something essential to the nature of concepts. For example, one might hold that part of what constitutes possessing a perceptual concept for squares is that one has a symbol for the perceptual category of squares. Giuseppe Longo suggests that if the minimalist account is wrong, what I am talking about might better be called 'proto-concepts'.

[12]Nor is it required that one deploys a concept for any of the features in the description set for squares, such as straightness or reflection symmetry, though it is required that the visual system can detect and represent these features.

[13]See Palmer and Hemenway (1978) for evidence of our ability to make judgements of approximate reflection symmetry. For a definition of a variable symmetry magnitude see (Zabrodsky and Algom, 1996). Degree of symmetry (using this definition) was found to correlate fairly well with perceived figural goodness.

[14]The resulting beliefs may be tacit. That is, you may believe that whatever has S is perfectly square without thinking the thought, just as you have believed that none of your grandmothers' grandmothers were elephants, without thinking that thought before now.

[15]This would include not only the possible case in which $\boldsymbol{a}$ is as it appears but also possible cases in which $\boldsymbol{a}$ deviates from its appearance by an amount that would not normally be perceived.

[16]Let $\boldsymbol{a}$ and $\boldsymbol{b}$ be similar, i.e. figures with the same shape. Imagine $\boldsymbol{a}$ contracting or expanding uniformly until it forms a figure $\boldsymbol{a}'$ congruent with $\boldsymbol{b}$; then imagine $\boldsymbol{a}'$ moving so as to coincide with $\boldsymbol{b}$. A transformation of this kind, known as a similarity mapping, maps each line through $\boldsymbol{a}$ onto a line through $\boldsymbol{b}$. We count among similarity mappings those which involve zero expansion, contraction, rotation, or translation, including the mapping that involves no change at all.

[17]This would not appear to be a square separated from (and competing with) the seen square, but a representation whose activation is involved in recognizing the perceived figure as a square. See (Kosslyn, 1994, Ch. 5).

[18]This is because what is causally operative is the activation of the description set for e.g. perfect squares, whether by perceptually generating the descriptions or by exercising visual imagination so as to access a stored description set.

[19]In particular, it is not clear that the Parallels Postulate (PP) has or could be overthrown empirically. Perhaps the claim that PP is true of physical space has been overthrown, but taken as a claim about a certain kind of possible space PP has not been overthrown.

[20]The possibility involved is not physical realizability in actual space. I concede that this needs clarification, and that the claim needs support.

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NATURALISM, PICTURES, AND PLATONIC INTUITIONS

1. NATURALISM

The principal objection naturalists offer to platonism is epistemic. We have seen this over and over again in the writings of self-professed naturalists. They find platonic intuitions incredible. Many, of course, object to the supposed reality of abstract entities, existing as they do outside of space and time. But the real sticking point concerns our ability to perceive them. Platonists say we can; naturalists insist we can't. The debate, it seems safe to say, is on hold. It's been at a standstill for several years. However, the question at issue between Platonists and naturalists has suffered from a lack of development of platonistic epistemology. Naturalists typically (though not always) borrow from the well developed epistemology of the natural sciences. To perceive something, they point out, a mediating agent, such as a stream of photons, is needed. And, of course, there is nothing like this connecting us to the entities in Plato's heaven. There are no little "platons" emitted by perfect circles that enter the mind's eye. Contemporary Platonists have almost nothing to offer in the way of a detailed epistemology of abstract entities. And the original Platonist, namely Plato himself, conjectured a wholly implausible epistemology involving immortal souls that previously existed in this abstract realm, that came to know mathematical objects directly, but forgot what they knew in the act of being born, and that now in an embodied form are recollecting bits and pieces of what they forgot. We have to do better than this.

Contemporary Platonists cling to the idea of perception. They talk of "seeing," or "grasping," or "intuiting" abstract entities. It's often metaphorical, to be sure, but the idea is that we can have some sort of perception of the objects of our mathematical knowledge. One of the more vivid versions of this comes from a famous passage by G.H. Hardy.

> I have myself always thought of a mathematician as in the first instance an observer, a man who gazes at a distant range of mountains and notes down his observations. His object is simply to distinguish clearly and notify to others as many different peaks as he can. There are some peaks which he can distinguish easily, while others are less clear. He sees A sharply, while of B he can obtain only transitory glimpses. At last he makes out a ridge which leads from A, and following it to its end he discovers that it culminates in B. B is now fixed in his

P. Mancosu et al. (eds.), Visualization, Explanation and Reasoning Styles in Mathematics, 57-73.

> vision, and from this point he can proceed to further discoveries. In other cases perhaps he can distinguish a ridge which vanishes in the distance, and conjectures that it leads to a peak in the clouds or below the horizon. But when he sees a peak he believes that it is there simply because he sees it. If he wishes someone else to see it, he points to it, either directly or through the chain of summits which led him to recognize it himself. (1929, 18)

Naturalists react to views such as this with impatience, or amusement, or both. I don't. I take Hardy's account seriously. But there is one thing wrong. Hardy sees all mathematical evidence as ultimately some sort of perception. Eventually, according to him, with enough training and guidance, we can directly see that any given theorem is true. We simply perceive the objects in questions. This is surely wrong. And platonism needn't go this far. We need only commit ourselves to the perception of *some* mathematical objects and *some* mathematical facts. And these perceptions are evidential grounds for other mathematical objects and propositions that we don't see. The situation is similar to natural science. We don't see elementary particles, but we do see white streaks in cloud chambers. What we actually do see can be turned into evidence for theories about what we don't see. This brings us to Gödel's brand of Platonism.

Gödel likened the epistemology of mathematics to the epistemology of the natural sciences in two important regards. First, we have intuitions or mathematical perceptions that are the counterpart of sense perceptions of the physical world. Second, we evaluate (some) mathematical axioms on the basis of their consequences, especially the consequences that we can intuit, just as we evaluate theories in physics or biology on the basis of their empirical consequences.

On Gödel's view, mathematics is fallible for a number of reasons. We can have faulty intuitions, just as we can make mistakes in our sense perceptions. And false premises can have true consequences, so the testing of axioms by checking their consequences is not foolproof either. Many people dislike the idea of giving up certainty in mathematics; perhaps they expect axioms to be "self-evident" truths. Naturalists typically will not object to the test-the-axioms-by-their-consequences feature of Gödel's view. But physicalist-cum-nominalist-cum empiricist-minded naturalists will utterly oppose the idea of Platonic intuitions, fallible or not.

The plan of this paper is as follows: First, I'll give a brief statement of Platonism, or at least my version of it. It may differ from other versions floating around, but not by too much. Then I'll take up the idea of observation and of intuition. This is the main sticking point. I will try to develop the idea in a number of respects and, perhaps thereby, to make it a bit more palatable. A key feature will be the use of pictures as proofs. Next I'll discuss a particular version of naturalism, Penelope Maddy's. In order to challenge her view, I'll describe an interesting thought experiment that tries to refute the continuum hypothesis (CH). Finally, a negative moral for Maddy's naturalism and a positive moral for Platonic intuitions will be drawn.

2. PLATONISM

There are a few key points to mention. I take these ingredients to be more or less central to Platonism.

1. Mathematical objects are perfectly real and exist independently of us, and mathematical statements are objectively true (or false) and their truth-value is similarly independent from us.
2. Mathematical objects are outside of space and time. By contrast, the typical subject matter of natural science consists of physical objects located in space and time. Some commentators like to say that numbers "exist," but they don't "subsist." If this just means that they are not physical, but still perfectly real, then I am happy to agree. But if it means something else, then it's probably just confused nonsense.
3. Mathematical entities are abstract in one sense, but not in another. The term "abstract" has come to have two distinct meanings. The older sense pertains to universals and particulars. A universal, say redness, is abstracted from particular red apples, red socks, and so on; it is the one associated with the many. Numbers, by contrast, are not abstract in this sense, since each of the integers is a unique individual, a particular, not a universal. On the other hand, in more current usage "abstract" simply means outside space and time, not concrete, not physical. In this sense all mathematical objects are abstract.
4. We can intuit mathematical objects and grasp mathematical truths. Mathematical entities can be "seen" or "grasped" with "the mind's eye." The main idea is that we have a kind of access to the mathematical realm that is something like our perceptual access to the physical realm.
5. Mathematics is *a priori*, not empirical. Empirical knowledge is based (largely, if not exclusively) on sensory experience, that is, based on input from the usual physical senses: seeing, hearing, tasting, smelling, and touching. Seeing with the mind's eye is not included on this list. It is a kind of experience that is independent of the physical senses and to that extent, *a priori*.

6. Even though mathematics is *a priori*, it need not be certain. These are quite distinct concepts. The mind's eye is subject to illusions and the vicissitudes of concept formation just as the empirical senses are. Mathematical axioms are often conjectures, not self-evident truths, proposed to capture what is intuitively grasped. Conjecturing in mathematics is just as fallible as it is elsewhere.
7. Many methods are possible in mathematics. There is no limit to what might count as evidence, just as there is no limit in principle to how physics must be done. We might discover new ways of learning. By contrast, for formalist or constructivist accounts, the only source of evidence is, respectively, rule governed symbol manipulation or constructive proof. In principle, nothing else could count as evidence for a theorem according to those two views. Platonism is not similarly constrained.

3. GÖDEL'S PLATONISM

In what are perhaps the three most famous and most often quoted passages in all of Gödel's works, he asserts the key ingredients in Platonism: the ontology of realism and the epistemology of intuitions.

> Classes and concepts may, however, also be conceived as real objects... existing independently of our definitions and constructions. It seems to me that the assumption of such objects is quite as legitimate as the assumption of physical bodies and there is quite as much reason to believe in their existence. They are in the same sense necessary to obtain a satisfactory system of mathematics as physical bodies are necessary for a satisfactory theory of our sense perceptions... (Gödel, 1944/83, 456f)
>
> ... despite their remoteness from sense experience, we do have something like a perception also of the objects of set theory, as is seen from the fact that the axioms force themselves upon us as being true. I don't see any reason why we should have any less confidence in this kind of perception, i.e., in mathematical intuition, than in sense perception, which induces us to build up physical theories and to expect that future sense perceptions will agree with them and, moreover, to believe that a question not decidable now has meaning and may be decided in the future. The set-theoretical paradoxes are hardly more troublesome for mathematics than deceptions of the senses are for physics... [N]ew mathematical intuitions

> leading to a decision of such problems as Cantor's continuum hypothesis are perfectly possible... (Gödel, 1947/83, 484)
>
> ...even disregarding the intrinsic necessity of some new axiom, and even in case it has no intrinsic necessity at all, a probable decision about its truth is possible also in another way, namely, inductively by studying its "success." Success here means fruitfulness in consequences, in particular in "verifiable" consequences, i.e., consequences demonstrable without the new axiom, whose proofs with the help of the new axiom, however, are considerably simpler and easier to discover, and make it possible to contract into one proof many different proofs.... There might exist axioms so abundant in their verifiable consequences, shedding so much light upon a whole field, and yielding such powerful methods for solving problems ... that, no matter whether or not they are intrinsically necessary, they would have to be accepted at least in the same sense as any well-established physical theory. (Gödel, 1947/83, 477)

I take these passages to assert a number of important things, many overlapping the ingredients of Platonism that I listed above. These include: mathematical objects exist independently from us; we can perceive or intuit them; our perceptions or intuitions are fallible (similar to our fallible sense perception of physical objects); we conjecture mathematical theories or adopt axioms on the basis of intuitions (as physical theories are conjectured on the basis of sense perception); these theories typically go well beyond the intuitions themselves, but are tested by them (just as physical theories go beyond empirical observations but are tested by them); and in the future we might have striking new intuitions that could lead to new axioms that would settle some of today's outstanding questions. In a later part of this paper I will describe a mathematical thought experiment that generates a new intuition which in turn leads to a refutation of CH.

Beginning in the next section, I'll take up the idea of intuition or perception of abstract entities. But the notion plays some role here, so we need to have at least a minimal idea. Gödel took intuitions to be the counterparts of ordinary sense perception. Just as we can see some physical objects (trees, dogs, rocks, the moon), so we can intuit some mathematical entities. And just as we can see that grass is green and the moon is full, so we can intuit that some mathematical propositions are true. These perceptual facts will play a big role in deciding which propositions to accept or to reject when they cannot be directly evaluated perceptually.

Since Gödel invokes the analogy with the empirical sciences, it is natural to look there for information about the relation between our mathematical theories and intuitions. Gödel, himself, offered little in the way of details.

4. THE CONCEPT OF OBSERVABLE

It's surprising how much counts as perception within the natural sciences. Physicists, for example, regularly talk about "seeing the interior of the sun." How do they do this? The sun produces neutrinos which normally pass through regular matter. Because of this, neutrinos produced in the deep interior of the sun pass with ease to the outside, some in the direction of the earth. In deep, abandoned mine shafts large tanks filled with dry cleaning fluid will detect the odd neutrino on those very rare occasions when one is absorbed by a proton which subsequently decays. Out of this whole process a number of conclusions about the interior of the sun are drawn.

Is this really seeing the interior of the sun? Or is this such a stretch that it amounts to an outright abuse of the concept of seeing? It seems plausible to object that all we really see is a few streaks in a photo, caused by the products of the decaying proton. The rest is inference based on some rather sophisticated theory. But this rather conservative account may be unjustified. We are happy to claim we can see things with a magnifying glass or microscope that we couldn't otherwise see with the unaided eye. This goes for high-powered electron microscopes as well as for low-powered optical microscopes. It's hard to draw a line between the naked eye and any powerful instrument. Perhaps the apparatus for neutrino detection should also be taken as an instrument for seeing the interior of the sun—a new type of telescope.

There is quite a different sort of thing that we also happily call observable. Consider the sort of thing we often see in an article or textbook on high-energy physics, namely, a picture of some sub-atomic decay process. These pictures are often given to us twice over. One of them is a photo of an event in a bubble chamber. The second (usually right beside the first) is an artist's drawing of the same event. The difference is that all the messiness of the first is tidied up. There are just a few bare lines in the artist's version, everything else in the photo is eliminated as irrelevant, perhaps stemming from processes having nothing to do with the one we're interested in, or perhaps mere scratches produced in the process of photographing, and so on. There is certainly a difference between these two pictures, yet it seems fair to call both a representation of something observable.

There is a useful terminology for this. The original photo is of a *datum*, while the artist's drawing is of a *phenomenon*. (Bogen and Woodward

1988, Brown 1993) Interestingly, scientific theories usually try to explain phenomena, not data. Phenomena are doubtless constructed (in some sense) from data and occupy a middle ground between data and theory. One of the most interesting and important aspects of phenomena is that they seem to legitimize inductive inference from a single example. They are not alone in doing this. So-called natural kind inference has this pattern. If any sample of water is discovered to have the chemical structure H_2O, then we infer that all water has this structure. True, for safety's sake a few samples would typically be considered, just to make sure the test was done properly. By contrast, for many other properties (e.g., are all ravens black?), we would insist on a very large sample before cautiously drawing any conclusions. Not so in a natural kind inference where a single instance is in principle sufficient.

Chicken sexing provides us with yet another unusual sense of seeing. Expert chicken sexers are remarkable people. They can classify day old chicks into male and female with 98% accuracy, and they can do this at a rate of about 1000 per hour. The vast majority of us get it right about 50% of the time, which is to say we're utterly hopeless. The skill is considered economically important if you want to feed those chicks who will eventually become egg-layers, but not the others. (In an article on the epistemology of mathematics, it is best not to reflect on the fate of the males.)

How do chicken-sexers do it? No one could do it until the Japanese discovered a perceptual method of discrimination in the 1920s. This method was passed on to North Americans in the 1930s. Some of the initial practitioners have only just retired. Heimer Carlson of Petaluma, CA, for instance, spent 50 years classifying a total of 55 million day-old chicks. His expertise has been the subject of psychological study. (Biederman and Shiffrar, 1987)

The ability to correctly classify is so difficult that it takes years of training in order to achieve the rare expert level; this training largely consists of repeated trials. The difference between good sexers and poor ones consists for the most part in where they look and what distinctive features they look for, especially contrastive features. It seems that expert chicken sexers were not aware of the fact that they had learned the contrasting features, nor were they aware of the exact location of the distinguishing information. By telling novices where the relevant information was precisely located the novices became experts themselves at a much quicker rate.

For our purposes the crucial thing to note is that the experts had some sort of tacit understanding of where to look and what to look for. It may seem that chicken-sexing is similar to riding a bicycle. We may all know how to do it, but we can't say what it is that we know. These two different types of knowing are usually called "knowing how" and "knowing that."

Is chicken sexing just a case of knowing how, rather than knowing that? There are certainly similarities, but there is one important difference between classifying chicks and riding a bicycle. Knowing how to ride a bicycle is a non-propositional skill; it results in actually riding. Knowing how to classify chicks is also a non-propositional skill; it results in sorting. But it results in propositional knowledge, as well, namely, being able to truly say "This is a male."

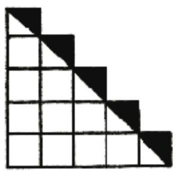
One might think that knowing how to ride a bicycle also results in propositional knowledge: "I am riding." Not so. This instance of knowing that does not come from knowing how, but from an empirical observation, a case of knowing that: I see myself riding. The how-that order is reversed in the two cases. In the bike example, the skill (riding) preceeds the knowledge (knowing that I am riding), but in the sexing example the knowledge (his knowing is a male) preceeds the skill (sorting).

Of course, there are lots of everyday examples such as seeing a cup on a table just in front of us. This is certainly a legitimate case of perception. I mention the other cases mainly to help prepare the case for mathematical perception. Intuition may seem a deviation from the ordinary sense of seeing. Perhaps it is, but so are a lot of other things, and it is not so great a deviation as to be dismissed.

5. PROOFS AND INTUITIONS

Consider the following theorem and the picture that attempts to prove it. It may take a few moments to see how the picture works, but it is certainly worth the effort.

Theorem: $1+2+3+\ldots+n = \frac{n^2}{2}+\frac{n}{2}$
Proof:



I wish to claim that the diagram is a perfectly good proof. One can see complete generality in the picture, even though it only illustrates the theorem for $n = 5$. The diagram does not implicitly suggest a "rigorous" verbal or symbolic proof. The regular proof of this theorem is by mathematical induction, but the diagram does not correspond to an inductive proof at all (where the key element is the passage from n to $n+1$). The simple moral I want to draw from this example is just this: We can in special cases correctly infer theories from pictures, that is, from visualizable situations. An intuition is at work and from this intuition we can grasp the truth of the theorem.

What is an intuition? A standard definition of intuitive knowledge runs as follows.

A knows p *intuitively* if and only if:

1. A knows that p
2. A's knowledge that p is immediate
3. A's knowledge is not an instance of the operation of any of the five senses. (Dancy, 1992, 222)

This is good for a start, but there are problems with this definition. For one thing, "knows" should be qualified to acknowledge the fallibility of intuitions. Perhaps we should be talking about intuitive beliefs instead of intuitive knowledge. Second, "immediate" should be qualified too. It does not mean temporally immediate, though typically the process of coming to know is fairly quick. Moreover, background knowledge and reflection may be involved. The crucial thing in calling it immediate is that p is not derived as the conclusion of an argument from other propositions.

Following Gödel, Platonists think of mathematical intuition as similar to the sense perception of physical objects. Indeed, we could imagine an analogous definition of sensory knowledge. It would be exactly the same as the definition of intuitive except for the final clause which would assert rather than deny that some of the five senses are involved.

If we return to the picture proof above, it seems a perfect candidate for intuitive knowledge. There is one objection that might be raised. It might be claimed that pictures give us sensory information and that is sufficient for the proof. After all, I could come to know that Alice has red hair just by looking at a colour photo of Alice. It is very doubtful, however, that something similar is happening in the number theory example. The most that one can acquire from the diagram by means of sense impressions, is a limited version of the proof, namely a proof that works in the special case of $n = 5$. Clearly, the picture provides a proof of very much more than that. It proves the theorem for every natural number, all infinitely many of them.

We might try, as Jon Barwise and his associates have tried, to take the picture to be not isomorphic but rather homomorphic to the structure described in the theorem. Barwise and Etchemendy remark that "a good diagram is isomorphic, or at least homomorphic, to the situation it represents…" (1991, 22) Hammer (1995) also adopts this account. The problem with this proposal is first, that the picture is obviously not isomorphic to the whole natural number structure, since there are infinitely many numbers, and second, that there are too many homomorphisms; the picture does not tell us which is the right one. And yet, we can seem to "grasp" it, nevertheless. So, I conclude that the diagram is not a representation in any strict sense, but rather something like a telescope that helps us to "see" into the Platonic realm. In short, it's a device for facilitating a mathematical intuition.

Let me take stock with a brief summery of what I've tried to establish so far. Mathematical intuitions are similar to empirical observations, immediate but fallible. Pictures and diagrams in mathematics are usually taken as mere heuristic devices, psychologically useful, but not genuine proofs. Particular examples, however, strongly suggest this is not so, that some pictures provide genuine proofs and are just as legitimate as traditional verbal/symbolic proofs. A mathematical diagram can be seen, but it does not work because it is literally observed. The observation and the intuition may be quite different things. Often this will be the case, since what is seen is a finite entity, while the intuition involves infinitely many things. This means the picture is more like a device for seeing something else, an implement for generating the appropriate intuition. The connection between sensory experience and mathematical observation is two-fold. In one sense, they are analogous—both are perceptions. Having an intuition is similar to having a sensory experience. They are connected in another sense: one sees a diagram (sense perception) that induces an intuition (mathematical perception) of something very different. This is what happens when a picture is not merely a heuristic aid, but an actual proof.

Now I will turn to a topic that is apparently quite different, Maddy's mathematical naturalism. In criticizing her view, I will make use of and even reinforce the idea of mathematical intuition. There are two issues to consider. First, does the Platonism described above succumb to Maddy's naturalism? Second, does the use of picture proofs lead to any problems for Maddy's naturalism?

6. MADDY'S NATURALISM

Penelope Maddy has changed her self-description from realist to naturalist. Her earlier realism has two main characteristics (Maddy, 1990). First, an ontological aspect: mathematical entities and mathematical facts exist independently from us. Second, an epistemic aspect: we can perceive sets, even though they are abstract entities, and this perception is compatible with naturalist accounts of the perception of physical objects. These philosophical claims lead her to make a methodological claim about mathematical practice. Mathematicians make decisions based on philosophical assumptions. Thus, set theorists who accepted a realist ontology tended to accept impredicative definitions and adopt so-called large cardinal axioms.

More recently, Maddy has adopted a view she calls naturalism. She actually has not rejected the two ingredients in her realism, but she has rejected the methodological outlook that she thought went along with the realism. Her new naturalism is the view that philosophy does not matter to mathematical practice. In other words, working mathematicians do not accept impredicative definitions or the axiom of choice because of their realist philosophical assumptions. Rather they do so because impredicative definitions and the axiom of choice *work*. It's a kind of internal pragmatism. Nothing else matters, not philosophy, not science, not theology, just the needs of mathematics itself.

Her argument is disarmingly brief: "Impredicative definitions and the Axiom of Choice are now respected tools in the practice of contemporary mathematics, while the philosophical issues remain subjects of ongoing controversy. The methodological decision seems to have been motivated, not by philosophical argumentation, but by consideration of what might be called ... mathematical fruitfulness..." (1998, 164) Hence, her conclusion: "Given that the methods are justified, that justification must not, after all, depend on the philosophy." (*ibid.* See also (Maddy, 1997, 191).)

There are two methodological practices that Maddy finds in the history of mathematics: maximizing and unifying. "If mathematics is to be allowed to expand freely... and if set theory is to play the hoped-for foundational role, then set theory should not impose any limitations of its own: the set theoretic arena in which mathematics is to be modelled should be as generous as possible...Thus, the goal of founding mathematics without encumbering it generates the methodological admonition to MAXIMIZE" (1997, 210f, her capitalization).

There are several points with which one could take issue. But there is only one that I want to discuss in this paper. She claims that the policy MAXIMIZE, rather than philosophical beliefs about ontology or epistemology, is what drives mathematics. I wish to counter this claim (in effect arguing that her older view was right) and to counter it in a way that appeals to the notion of intuition (as developed above) in a very fundamental and quite striking way. This will arise in the following remarkable mathematical thought experiment.

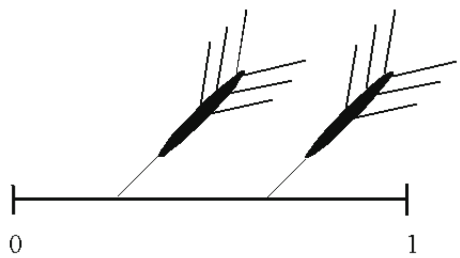
7. REFUTING THE CONTINUUM HYPOTHESIS

One of the more striking developments in recent mathematics is the use of probabilistic arguments. This has been especially true in combinatorial branches of mathematics such as graph theory, but the potential is much greater and could even be quite revolutionary. Given Maddy's attitude to means-ends relationships and especially her principle MAXIMIZE, she is likely to endorse probabilistic proofs and want to see room made for these methods in the foundations of mathematics. Amazingly, this may have consequences for the continuum hypothesis, CH, and perhaps could even rebound against her naturalism.

Christopher Freiling (1986) constructed the following "refutation" of CH. He calls his argument "philosophical," since he does not provide a proof or a counter-example in the normal mathematical way.

Imagine throwing darts at the real line, specifically at the interval [0,1]. Two darts are thrown and they are independent of one another. The point is to select two random numbers. As background we assume ZFC. If CH is true, then the points on the line can be well-ordered and will have length $\aleph_1$. If we pick a point in the well ordering then the set of earlier points will have a lower cardinality. Thus, for each $p \in [0,1]$, the set of all points $\{q \in [0,1] : q < p\}$ is countable. (Note that $<$ is the well ordering relation, not the usual *less than*.) Call this set S_p.

Suppose the first throw hits point p and the second hits q. Either $p < q$, or vice versa; we'll assume the first. Thus, $p \in S_q$. Note that S_q is a countable subset of points on the line. Since the two throws were independent, we can say the throw landing on q defines the set S_q "before" the throw that picks out p. The measure of any countable set is 0. So the probability of landing on a point in S_q is 0. While logically possible, this sort of thing is almost never the case. Yet it will happen every time there is a pair of darts thrown at the real line. Consequently, we should abandon CH, that is, the assumption that the number of points on the line is the first uncountable cardinal number.



If the cardinality of the continuum is $\aleph_2$ or greater, the argument as set out here would not work, since the set of points S_q earlier in the well ordering need not be countable, and so would not automatically lead to a zero probability of hitting a point in it. (Freiling actually goes on to show that there are infinitely many cardinal numbers, $\aleph_1, \aleph_2, \aleph_3, \ldots$, between $\aleph_0$ and $2^{\aleph_0}$.)

It is important to note that this argument cannot be formalized within standard mathematics. Many sketchy arguments that appeal to vague intuitions can be rigorously reconstructed. But this one cannot. If we try to recast it in purely mathematical terms we would violate established mathematical principles. CH is, after all, independent of the rest of standard mathematics.

Freiling's argument is contentious. But the mere possibility of its correctness (for all we know) is enough to make it an interesting example and one that is useful for my purposes. Any realistic example is likely to be contentious and I suspect that the majority of set theorists don't accept this refutation of CH. But some mathematicians do, including (Fields medallist) David Mumford who would like to reformulate set theory, in consequence. This is enough to make the example especially worth considering in connection with Maddy's naturalism.

Mumford would like to see CH tossed out and set theory recast as "stochastic set theory", as he calls it. The notion of a random variable needs to be included in the fundamentals of the revised theory and not be a notion defined, as it currently is in measure theory terms. Among other things, he would eliminate the power set axiom. "What mathematics really needs, for each set X, is not the huge set 2^X but the set of sequences $X^{\mathbf{N}}$ in X." (Mumford, 2000, 208) I won't pursue the details of this, but instead get right to the philosophical point that has a bearing on Maddy's views.

In the light of this example, we have two proposals, both of which could claim support from Maddy's methodological principle MAXIMIZE. First, we have standard set theory in search of additional axioms, guided by the desire not to limit in any way the notion of an arbitrary set. On this version of MAXIMIZE the standard axioms remain, the proposed axiom of constructability $V = L$ is rejected as too restrictive, and various large cardinal axioms are tentatively accepted.

Second, we have Mumford's programme. He can be seen as a maximiser, too. But his focus is on maximizing the range of legitimate proof techniques and, in particular, making room for a more fruitful notion of randomness. In enlarging the realm of mathematics for the sake of stochastic methods and taking random variables seriously in their own right, Mumford would reformulate set theory so as to pare down the universe of sets to a much smaller size. This version of MAXIMIZE is, I suspect, also a perfectly legitimate mathematical aim by Maddy's lights. Though it is not one she anticipated.

How are we to settle this dispute? Clearly, appeal to MAXIMIZE will not help, since both sides could cheerfully embrace it. Freiling called his argument "philosophical" and that seems exactly right (see Appendix). Why "philosophy"? Because, it involves beliefs about symmetry, randomness, and causal independence that go well beyond existing standard mathematics, and his approach will likely stand or fall with the correctness or incorrectness of those philosophical assumptions. Remember, Maddy's naturalism excludes not just science and philosophy, but everything non-mathematical from having mathematical influence. If Freiling is right about CH, then Mumford's programme to overhaul mathematics gets a big boost and so will his version of MAXIMIZE. Obviously, this will affect mathematical practice. In other words, philosophy has an effect on mathematical practice after all. Freiling's "philosophical" assumptions may be false, of course, but that is neither here nor there. His particular assumptions and the (arguable) legitimacy of pictures, diagrams, and thought experiments in mathematical reasoning are the kinds of considerations that matter, at least in principle. It is enough that one allows the *possibility* of intuitions based on visualization – diagrams or thought experiments – and that this possibility is open to philosophical debate. That is sufficient to undermine Maddy's brand of naturalism, since she denies any role at all for philosophy.

The final moral I wish to draw from the dart throwing example is to reinforce the initial part of this paper. There is some sort of mathematical perception which cannot be reduced to either physical perception or to disguised logical inference. This, I think, is clear from the example. Obviously, we have not refuted CH on the basis of accepted mathematical facts, since CH is independent of those facts. Could it be an empirical process? This seems very unlikely, since we cannot really pick out random real numbers with darts. The process of this thought experiment, though highly visual, is at bottom an intellectual one. Platonic intuitions *à la* Gödel play a crucial role. And pictures, diagrams, and thought experiments can generate them. Maddy and other naturalists might dispare, but Platonists should be cheered by all of this.

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APPENDIX: FREILING'S "PHILOSOPHICAL" REFUTATION OF CH

The refutation of CH that I gave above is based on Mumford's presentation. The original version by Freiling is different in some respects. His thought experiment assumes the following four "self-evident philosophical principles" (1986, 199):

1. Choosing reals at random is a physical reality, or at least an intuition mathematics should embrace to the extent possible.
2. A fixed Lebesgue measure zero set predictably will not be hit by a random dart.
3. If an accurate Yes-No prediction can always be made after a preliminary event takes place (e.g., the first dart is thrown) and, no matter what the outcome of that event, the prediction is always the same, then the prediction is also in some sense accurate before the preliminary event.
4. The real number line cannot tell the order of the darts.

To Freiling's four assumptions I would add one more: the line consists of pre-existing points. Aristotle, by contrast, thought that points could be constructed, say, by throwing darts, but those points do not already exist on the line. If Aristotle is right, then Freiling's argument will certainly not work; so the assumption of pre-existing points is crucial.

Freiling's argument runs as follows: We throw two darts, one after the other, at the real line $[0,1]$. There are a few obvious things we might note. For instance, the first dart will land on an irrational number with probability 1, because the set of rational numbers is countable and so has Lebesgue measure 0. It is not impossible to hit a rational number, but the probability is 0, nevertheless.

Let $f : \mathbf{R} \to \mathbf{R}_{\aleph_0}$ be a function that assigns a countable set of real numbers to each real; the number hit by the second dart will not be in the countable set assigned to the number hit by the first dart. The situation is symmetrical; the order of throwing is irrelevant. Thus, we can say that the number hit by the first dart will not be in the set assigned to the second. This leads to the following intuitive principle that I'll call Freiling's Symmetry Axiom:

$$FSA : (\forall f : \mathbf{R} \to \mathbf{R}_{\aleph_0})(\exists x)(\exists y)\ y \notin f(x)\ \&\ x \notin f(y)$$

Theorem (of ZFC): FSA $\Longleftrightarrow \neg$CH

Proof: ($\Rightarrow$): Assume FSA and let $<$ be a well ordering of $\mathbf{R}$. The existence of a well ordering follows from the axiom of choice which we have assumed. We will further assume CH which implies that the length of the well ordering is $\aleph_1$. Our aim is to get a contradiction. Now let $f(x) = \{y : y \leq x\}$. Thus,

$f : \mathbf{R} \to \mathbf{R}_{\aleph_0}$. The way cardinal numbers are defined implies that we are always bumped down a cardinality when picking a set of earlier points in a well ordering. Moreover, a well ordering is total, so if some particular $y \notin \{y : y \leq x\}$, then $x > y$. Consequently, by FSA, $(\exists x)(\exists y)\ x > y\ \&\ y > x$, which is a contradiction. Hence, $\neg$CH.

For our purposes the refutation of CH is sufficient, but I will include the rest of the proof of equivalence for those who are interested to see that $\neg$CH implies FSA.

($\Leftarrow$): Assume that CH is false, i.e., $2^{\aleph_0} > \aleph_1$. Let $x_1, x_2, x_3, \ldots$ be an $\aleph_1$-sequence of distinct real numbers and let $f : \mathbf{R} \to \mathbf{R}_{\aleph_0}$. Now consider the set $A = \{x : (\exists \alpha < \aleph_1)\ x \in f(x_\alpha)\}$, which is the $\aleph_1$-union of countable sets. Thus, the cardinality of A is $\aleph_1$. Since, by assumption, $2^{\aleph_0} > \aleph_1$, $\exists y \notin A$. Thus, $(\forall \alpha < \aleph_1)\ y \notin f(x)$. Since $f(y)$ is countable, we have $(\exists \alpha \in \aleph_1)\ x_\alpha \notin f(y)$. Therefore, $y \notin f(x)\ \&\ x \notin f(y)$.

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M. GIAQUINTO

MATHEMATICAL ACTIVITY

Philosophy of mathematics has mainly focussed attention on bodies of mathematical theory: How do we know that ZF is consistent? Are the theorems of Euclidean Geometry true? What are the objects of Number Theory? And so on. In recent years, by contrast, there has been a growing interest in mathematical practice among philosophers—Jamie Tappenden gives a concise and insightful overview (Tappenden, 2001). However, this interest is still rather narrowly focussed. When philosophers of mathematics are asked to consider mathematical activity, as opposed to bodies of established mathematics, they tend to think of the research activity of professional mathematicians, typically, proving theorems.

What other activities might there be? It is the aim of this paper to lay out a preliminary map of mathematical activities, in order to highlight some relatively neglected philosophical aspects of mathematics. An initial broad-stroke list with associated goals might be as follows.

• Discovery	knowledge
• Explanation	understanding
• Justification	relative certainty
• Application	practical benefits

For each of these there are three different kinds of activity. For a discovery there is the primary activity involved in *making* it; but there is also the activity of *presenting* it, by means of talks, demonstrations, journal articles, or books; and there is the activity of *taking in* the presentation by audience or readers.

$$\text{Activities relating to Discovery}\begin{cases}\text{Making a discovery}\\ \text{Presenting a discovery}\\ \text{Taking in a discovery}\end{cases}$$

The trio of making, presenting, and taking in obtains also for other kinds of endeavour on the list. The makers are primarily research mathematicians, pure and applied, though not exclusively. Physicists and in an earlier age, amateur mathematicians, play a prominent part. The presenters, by contrast, include not only mathematicians but also teachers. The takers-in include not only mathematicians and teachers, but also apprentices, students, and schoolchildren. So mathematical activity thus broadly conceived is something that most of us indulge in at some time. Philosophy of mathematics, then, could engage a much wider audience, if it considered all mathematical activity. Moreover, there could be practical benefits. Philosophical studies

P. Mancosu et al. (eds.), Visualization, Explanation and Reasoning Styles in Mathematics, 75-87.

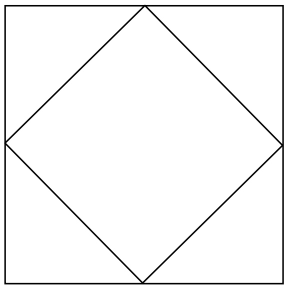

FIGURE 1.

of mathematical activity may well prove fruitful for teaching and learning mathematics, especially in combination with cognitive and historical studies.

The list given above is not supposed to be exhaustive. But proof is so central to mathematics that one might think that proving should figure on a list of mathematical activities. Yet it is not listed here. Why not? The main reason is that proving comes under justifying. Proving a theorem is not the only kind of justifying, as I aim to show a little later, and so it should not replace justification on the list. Furthermore, some cases of proving are also cases of discovering or explaining. This has been pointed out by Karine Chemla and is illustrated in some of her research on Liu Hui's commentary on *The Nine Chapters on Mathematical Procedures* in (Chemla, 1997) and this volume. In what follows I will try to substantiate the claim that in mathematics discovering, explaining, and justifying are distinct and do not collapse into proving.

1. DISCOVERY

Here is an intuitive way of coming to believe a Euclidean theorem, which, I will argue, is a way of discovering it but not a way of proving it. Imagine a square. Each of its four sides has a midpoint. Now visualize the square whose corner-points coincide with these four midpoints. If you visualize the original square with a horizontal base, the new square should seem to be tilted, standing on one of its corners, 'like a diamond' some people say. (Figure 1). Clearly, the original square is bigger than the tilted square contained within it. How much bigger?

By means of visual imagination plus some simple reasoning one can find the answer very quickly. I came across this example in (Kosslyn, 1983). By visualizing the corner triangles folding over, with creases along the sides of the tilted square, one can come to view the corner triangles folded flat so as to cover the tilted inner square exactly, without any gap or overlap. (If you are in doubt, imagine the original square with lines running between mid-points of *opposite* sides, dividing the square into its quadrants. The sides of the tilted inner square should seem to be diagonals of the quadrants.) So it becomes apparent that the total area of the corner triangles equals the area of the inner square. At the same time it is clear that the area of the original square equals that of the inner square plus the total area of the corner triangles; so the area of the original square equals twice the area of the inner square.

You may have known this already; you may have acquired this belief by having followed a proof of it from certain other beliefs, or by being told, or in some other way. But your experience should confirm that a person *could* have acquired this belief in the way suggested. Now taking the proposition believed to be about squares in the Euclidean plane, I hold that this way of reaching the belief is reliable, hence delivers knowledge. I have found that it is not at all a trivial task to substantiate this claim of reliability; I ask you to take on trust that it can be done. Given that this route to the theorem is reliable, it is a way in which someone who did not already believe it could discover it.

Yet it is not a way of proving it. This is because, if we get this belief this way, our confidence that the corner triangles can be arranged to cover the inner square without gap or overlap is produced by a means that does not constitute a proof of it. The activity of visualizing triggers some dispositions of which we are not conscious, producing immediate belief, without any steps of reasoning. Not all belief-producing dispositions that can be triggered by visual experience are reliable. This is notoriously the case in geometry (even more so in analysis). This fact, coupled with the fact that we very easily acquire geometrical beliefs in this visual way, made it necessary to develop a means of checking the correctness of beliefs thus acquired, that is, to develop ways of justifying or proving them. In the case at hand it is not at all difficult to see how such a proof might go. The core of one proof considers the figure that results from dividing the original square into its quadrants and proves that each outer triangle is congruent to the inner triangle in the same quadrant. So the visual way of reaching the theorem illustrates the possibility of discovery without proof.

2. EXPLANATION

The example just given shows that there may be discovery without proof. Similarly, proof and explanation can come apart: there may be a proof that

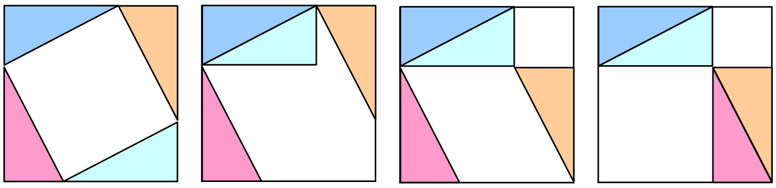

FIGURE 2.

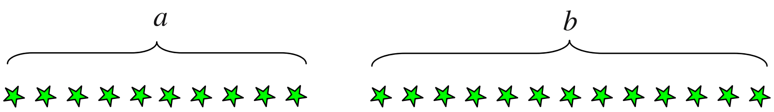

FIGURE 3.

does not explain its conclusion and there may be an explanation that does not prove the fact explained. It is not straightforward to establish this, as it is difficult to say what constitutes explanation in mathematics, and I am not going to attempt that here. This is the subject of recent important work by Paolo Mancosu, for example (Mancosu, 2000) and this volume. One role for explanation is to help make a theorem that one already knows more intuitive. Often this can be done for a theorem in analysis by means of geometric illustration. A very simple example for Pythagoras' theorem is the sequence of stills in figure 2.

For another example, consider the commutativity of addition on the finite cardinal numbers. A proof of this from the Dedekind-Peano axioms is prolix and unobvious, involving nested induction. Such a proof *establishes that* addition is commutative, but does not *explain why*. Yet our understanding of counting gives us a ready explanation. Correctly counting the set of stars (figure 3) from left to right gives $a + b$ while correctly counting the same set from right to left gives $b + a$. As a set has exactly one cardinal number, the number $a + b$ is the same number as $b + a$. This argument can be readily generalised to any set divided into subsets A and B. To count the set it does not matter which subset is counted first. The ease with which the commutativity of addition is understood is attested by the confidence children have in it before they can grasp a proof of it, and before they are reliable enough at simple additions to induce the principle from noticed instances of it.

However, the two cases just presented instantiate only a rather weak, subjective kind of explanation. This is explaining in the sense of making a fact more intuitively compelling to a person than it was before. Mancosu and others suggest that there is a stronger kind of explanation in mathematics, one that has an objective basis. In contrast to the sequence of stills presented earlier for making Pythagoras' theorem more intuitive, Mancosu (2001) cites an argument that draws the theorem out of two fundamental facts: (1) the areas of similar plane figures (with at least one straight side) are to each other as the squares of their corresponding sides; (2) any right angled triangle t is composed of two right angled triangles similar to t, whose hypotenuses are sides of t. The argument stays moored to intuition, but delivers the theorem in a way that shows it as a special case of the more general truth that for any kind K of plane figures such that all Ks are similar, the K on the hypotenuse of a right angled triangle is equal in area to the sum of the Ks on the other two sides. It will not be disputed that the argument given by Mancosu has a much stronger claim to be an explanation of Pythagoras' theorem than the sequence of stills presented earlier. But at the moment no one has a clear and satisfactory account of the criteria for mathematical explanation of the objective kind.

Besides the explanation of theorems, there are explanations of the aptness of definitions. For an example, consider Euler's formula:

(1) $$e^{i\theta} = \cos\theta + i\sin\theta.$$

This is often introduced as a definition for extending the exponential function to complex numbers.[1] Here, I hope you will feel, something does need explaining. Euler's formula is so useful, so much falls out of it or becomes easier by means of it that the question 'Why?' arises quite naturally. It is not the case that any other definition would have served as well; there must be something beyond our conventions that makes the definition perfectly apt.

This can be explained by presenting the geometric significance of the formula. Consider the point on the unit circle at angle θ (anticlockwise from the unit vector on the x-axis), as in figure 4. That point has co-ordinates $\langle \cos\theta, \sin\theta \rangle$. So it represents the complex number $\cos\theta + i\sin\theta$. Thinking of this as the vector from the origin to the point $\langle \cos\theta, \sin\theta \rangle$, Euler's formula tells us that $e^{i\theta}$ is that vector. If we expand (or contract) the x and y co-ordinates of that vector by real magnitude r to $r\cos\theta$ and $r\sin\theta$, it is clear that the corresponding vector must also expand or contract by a factor of r. This gives an immediate geometrical significance to the following trivial consequence of Euler's formula:

$$re^{i\theta} = r\cos\theta + ri\sin\theta.$$

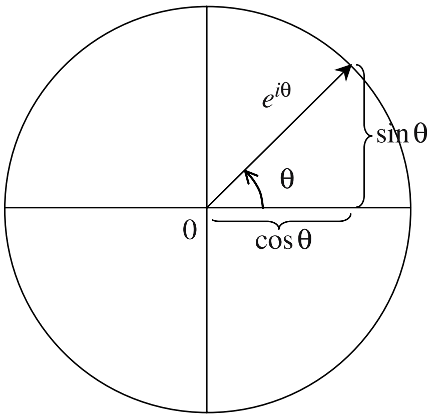

FIGURE 4.

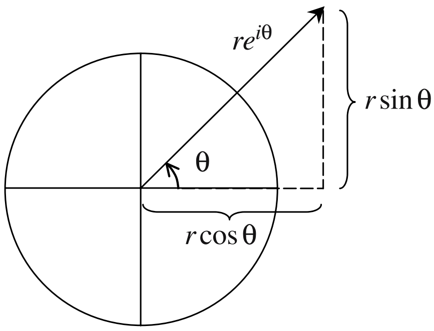

FIGURE 5.

It tell us that $re^{i\theta}$ is the vector with length r at angle θ (figure 5).

Thus we have a notation for vectors which makes explicit its determining geometric properties, its length r and its angle θ, properties hidden by the pairs-of-reals notation. This is what explains the aptness of Euler's formula. For confirmation, recall the puzzlement one feels when first introduced to vector multiplication in terms of pairs-of-reals. Given that $i^2 = -1$, it is clear that

$$(x+iy)(u+iv) = (xu-yv)+i(xv+yu).$$

But why does the term (on the right) denote the vector whose length is the *product* of the lengths of the multiplied vectors and whose angle is the *sum* of the angles of the multiplied vectors? Given the law for multiplication by adding exponents, the answer is immediate using the Euler notation for vectors:

$$re^{i\theta}se^{i\eta} = rse^{i(\theta+\eta)}.$$

3. JUSTIFICATION

The foregoing, I hope, will have convinced you that utility or aptness of Euler's formula as a definition of the exponential function on the complex domain can be explained in terms of its geometrical significance. The utility of Euler's definition provides pragmatic justification for adopting it. But is there not also some intrinsic justification of Euler's definition? One cannot prove a definition, so you may think that the answer must be negative. Despite this, I think that there is an intrinsic justification.

There is a delightful argument, called the Moving Particle Argument, that I have taken from (Needham, 1997), which provides what is needed. The argument is less rigorous but more explanatory than alternatives using power series expansions of e. Here is the argument. The starting assumptions are that the exponential function is its own derivative and that its value at 0 is 1. Then by the chain rule it follows that for real constant k, the derivative of e^{kt} with respect to t is ke^{kt}. We want this to hold for the complex constant i in place of k. So we should require our definition to permit the following:

$$\frac{d}{dt}e^{it} = ie^{it}. \tag{2}$$

Now imagine a particle moving along a curve in the complex plane. Let its position at time t be the complex number denoted parametrically as $Z(t)$. Let its velocity at t be the complex number $V(t)$. Thought of as a vector, the length of $V(t)$ represents the particle's instantaneous speed at t and the direction of $V(t)$ represents the particle's instantaneous direction at t (tangent to the trajectory in the direction of motion). Finally, let M denote the change in the particle's position between t and $t + \delta$. Figure 6 illustrates this.

Then change of position Z with respect to time gives velocity V:

$$\frac{d}{dt}Z(t) = \lim_{\delta\to 0}\frac{Z(t+\delta)-Z(t)}{\delta} = \lim_{\delta\to 0}\frac{M}{\delta} = V(t).$$

We use this to find the trajectory when $Z(t) = e^{it}$. According to formula (2), the derivative of $Z(t)$ with respect to t is ie^{it}, or $iZ(t)$. Noting that multiplication of a vector by i is anti-clockwise rotation through a right angle, this yields:

$$\text{velocity } V = iZ = \text{position, rotated through } \pi/2.$$

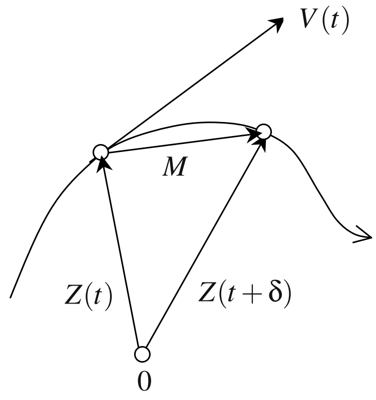

FIGURE 6.

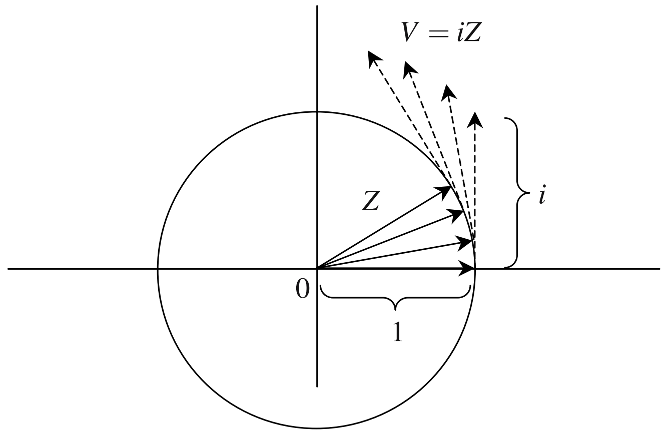

FIGURE 7.

The initial position of the particle is $Z(0) = e^0 = 1$. So its initial velocity is i, which means that at the starting point it is moving vertically upward with unit speed. See figure 7. A moment later the particle will have moved a smidgen in this direction, and its new velocity $V(\delta)$ will be at right angles to its new position vector $Z(\delta)$; a moment after that it will have moved a smidgen in the direction of $V(\delta)$ and its velocity $V(2\delta)$ will then be at right angles to its position vector $Z(2\delta)$; and so on. Thus the trajectory forms a regular polygon whose sides, in the limit, have infinitesimal length – in other words, a circle, as illustrated in figure 7.

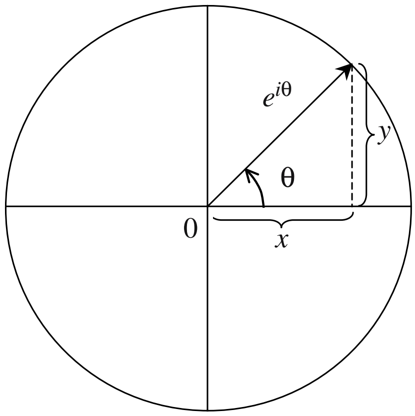

FIGURE 8.

As $|Z(t)| = 1$ for all t, that is, throughout the motion, the particle's speed $|V(t)| = 1$ throughout. Hence after time $t = \theta$ the particle will have moved a distance θ round the unit circle, and so the angle of $Z(\theta) = e^{i\theta}$ will be θ. That gives us the familiar picture in figure 8.

Now let $e^{i\theta} = x + iy$. As the length of $e^{i\theta}$ is 1, $x = \cos(\theta)$ and $y = \sin(\theta)$. Hence $x + iy = \cos(\theta) + i\sin(\theta)$, which gives us Euler's formula (1).

The Moving Particle Argument shows that we should accept Euler's formula, given the small requirement that a certain uniformity in the behaviour of the exponential function for real inputs carries over for complex inputs, namely formula (2). Moreover, the argument shows this in a way that makes it intuitively clear, by presenting the geometrical interpretation of each stage pictorially, rather than leaving it hidden in strings of symbols. The argument, however, is not a proof. Perhaps it can be transformed into rigorous argument without losing too much of its intuitive character. The resulting argument would be a proof of the conditional 'If (2) then (1)'; but the point of marshalling the argument is to justify adoption of (1) as a definition, and that is an activity distinct from proving a theorem. This kind of justification is rare in textbooks, but it is not that rare in practice. In good textbooks justifications of definitions are not uncommon. Another example is the justification or 'motivation' for defining the natural logarithm function as an integral given in (Apostol, 1967):

$$\ln(x) = \int_1^x t^{-1} dt.$$

Yet other kinds of justification distinct from proving a theorem arise in connection with axioms. At a first approximation these fall into two classes. Sometimes we have a structure or class of structures in mind, and a set of axioms is proposed for a theory of the intended structure(s). Then the criteria of justification would be the cogency of the axioms as truths about the structure(s), their joint comprehensiveness, their individual independence, and, in the case of a single structure, their categoricity. Axiom systems for Euclidean geometry are suitable for this kind of justification. The other kind of justification is sought when one has an axiom system with some intended subject matter but not a clear intended model. Then justification would include finding a model that incorporates the intended subject matter, or that is reasonably faithful to the original intentions. The history of set theory provides an example. We did not have a full justification of Zermelo's axioms until we had the idea of a universe of sets as a cumulative hierarchy.[2]

4. REFINING AND EXTENDING THE LIST OF ACTIVITIES

Reflecting on the kinds of justification mentioned here gives reason to look back at the initial list of activities. Here is a kind of puzzle. Justifying axioms might seem to be a self-defeating exercise, because if the justification is not circular its premisses are shown to be the real axioms, i.e. starting points, while the 'axioms' justified are really just theorems. Something similar can be argued with regard to justifications of definitions. Will not a succesful argument in justification of a 'definition' show that it is really a theorem? The simple answer is that a successful justification of an axiom or axiom system or definition does not so much prove its truth as warrant our adopting it. Since the locution 'justifying' as applied to a statement usually means establishing its truth, it might be better to talk of *motivating* axioms and definitions, as some authors already do. In any case it is clear that justification falls into two different kinds: proving theorems and motivating definitions or axioms. Whereas the goal of proof would be to achieve some degree of certainty about the truth of a theorem, the goals of motivating a definition or axiom system would be to achieve some degree of assurance about the wisdom of adopting that definition or axiom system.

To motivate a definition or axiom system one must already have the definition or axioms. The activity of formulating definitions or axioms is no trivial matter in mathematics. One only has to think of the struggle to make rigorous differential calculus. Algebra provides a current example. There are several definitions of weak *n*-category on offer that are not obviously equivalent. Leinster (2002) discusses ten of these definitions. Homing in on the intrinsically important ones will take time. This case is rather different from the calculus. We have concepts of category and bicategory, and definitions thereof. The task then is to find a suitable way of generalising to n-category for $n > 2$ (and perhaps to α-category for transfinite α) in the absence of a prior concept of weak n-category. But in the case of calculus there were prior perception-based concepts of continuity and convergence. So *formulation* is an important kind of activity in mathematics, with its own subkinds. I suggest then that the list of activities that we started with should be expanded to include formulation, with the goals of precision, explicitness and rigour.

Related to the kinds of formulation just mentioned is the invention of symbol systems and associated algorithms for problem solving. The prime case is the invention of the place system of numerals and the associated algorithms for multidigit addition and multiplication. Another case is matrix algebra. Should these come under formulation? Or do they belong under a separate heading?

Of no less importance is the invention of types of diagrammatic representations, and their conventional links with symbolic notations. Here the prime cases are the number line and the Cartesian co-ordinate system. The importance of the link established by these representations between number and space, and between algebra and geometry, can hardly be overestimated. More recently the use of arrow diagrams helps to anchor our grasp of quite abstract levels of algebra to spatial representations. I suggest that the construction of symbol systems and diagram systems deserves to be listed as a separate kind of activity, construction of systems of representation, which I will call 'representation' for short.

Revising the list to incorporate these points gives the following.

- Discovery
- Explanation $\begin{cases} \text{Subjective} \\ \text{Objective} \end{cases}$
- Formulation
- Application
- Justification $\begin{cases} \text{Proving theorems} \\ \text{Motivation definitions / axioms} \end{cases}$
- Representation $\begin{cases} \text{Symbol systems} \\ \text{Diagram systems} \end{cases}$

It is clear that mathematical activity is rich, varied and complex, so that any preliminary account such as this one is bound to need overhaul, if not outright replacement. The value of this is only that it may serve as a springboard for more thorough philosophical study of mathematical activity.

5. CONCLUDING REMARKS

In drawing to a close I would like to change direction a little and raise some questions for further investigation. The mathematical activities I have discussed have associated goals. But often mathematical activity has intrinsic rewards for the active participant, rewards that may differ from the primary goals. I have in mind the pleasure of new understanding, and aesthetic pleasure. For example, we might notice some surprising equivalences between propositions in two quite distinct mathematical domains; then we discover that both domains are instances of a structure or kind of structure, and that the equivalences are to be explained by the shared structure. This can be rewarding, and if we have some intuitive grasp of the structure there can be an aesthetic reward accompanying the intellectual reward. I am struck by how often gains in understanding involve the use of visually presented spatial representations, diagrams or mental imagery. Is the aesthetic reward in such cases due to the aesthetic properties of the visuo-spatial representations? Or can the mathematical entities themselves, the abstract structures, bear aesthetic properties? Mathematical explanations often involve visuo-spatial representations. To what extent, in such cases, does understanding a mathematical fact, as opposed to merely knowing it, depend on visuo-spatial cognition? And when explanations are visuo-spatial in character, can there really be distinct presentations of the same explanation? Is there really a difference between a presentation of an explanation and the explanation itself?

A more refined account of mathematical activities would raise many more questions like these, whose investigation would restore to the philosophy of mathematics its ancient depth and succulence. In this paper I have merely attempted to indicate the wealth and variety of mathematical activities and to show that, despite our training, mathematical activity does not reduce to applying algorithms and proving theorems.[3]

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NOTES

[1]Given the law for multiplying by adding exponents, Euler's formula is trivially equivalent to the equation $e^{x+i\theta} = e^x(\cos\theta + i\sin\theta)$. Sometimes this equation is given as the definition.

[2]This is not to say that the conception of a cumulative hierarchy on its own suffices to justify all the axioms. Cardinality considerations are needed as well.

[3]I would like to thank Paolo Mancosu for helpful comments on a draft of this paper.

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PART 2

MATHEMATICAL EXPLANATION AND PROOF STYLES

JENS HØYRUP

TERTIUM NON DATUR: ON REASONING STYLES IN EARLY MATHEMATICS[0]

Árpád Szabó in memoriam

1. TWO CONVENIENT SCAPEGOATS

Some philosophers of mathematics hold that real proof is quite recent and that, for instance, Euclid's arguments for the correctness of his theorems and constructions do not count as "proofs" (those contributing to the present volume are less dogmatic!). The rest of the world (in as far as it knows at all about the topic) sees things differently. Contemporary mathematicians may find Euclid's proofs insufficient or shaky, but they agree with their predecessors that Euclid's strings of arguments from the properties of the objects involved do constitute proofs.[1] According to this view, Greek theoretical geometry is thus based on proofs. Does that mean that mathematical proof was invented by the ancient Greeks (and, by tacit but rampant corollary, that it is thus yet another "proof" of "Western" superiority)?

Some writers on mathematics and its history have indeed claimed proof to be a Greek invention (without necessarily deducing from that the corollary that "our" saturation of selected spots of the world with napalm, cluster bombs and depleted uranium is morally justified). In (1972, 3, 14), Morris Kline wrote the following lines:

> Mathematics as an organized, independent, and reasoned discipline did not exist before the classical Greeks of the period from 600 to 300 B.C. entered upon the scene. There were, however, prior civilizations in which the beginnings or rudiments of mathematics were created.
>
> [...]
>
> The question arises as to what extent the Babylonians employed mathematical proof. They did solve by correct systematic procedures rather complicated equations involving unknowns. However, they gave verbal instructions only on the steps to be made and offered no justification of the steps. Almost surely, the arithmetic and algebraic processes and the geometrical rules were the end result of physical evidence, trial and error, and insight.

Such blunt statements (as well as the less blunt but similar attitudes of many fellow writers) have called forth objections from other quarters.[2] As an example one may quote George Gheverghese Joseph's statement (1991, 89*f*)

P. Mancosu et al. (eds.), Visualization, Explanation and Reasoning Styles in Mathematics, 91-121.

that if "the Greek dependence on Egypt and Babylonia is now recognized, the myth of the 'Greek miracle' will no longer be sustainable".[3] Unfortunately for Joseph's intended undermining of "one of the central planks of the Eurocentric view of history of progress" (1991, 90), the whole discussion of Egyptian and Babylonian mathematics is nothing but support for Kline's view.[4] Admittedly, Richard Gillings (1972, 233) is quoted to the effect that a

> nonsymbolic argument or proof *can* be quite rigorous when given for a particular value of the variable; the conditions for rigor are that the particular value should be *typical*, and that a further generalization to *any* value should be *immediate*

– but Joseph does not show that (nor discuss in which sense) the various *rules* applied to particular cases he quotes from Egyptian and Babylonian material can really be read as paradigmatic (or "potentially general") "argument or proof" in Gillings's sense.

In the following sections of the paper I shall show that much of Old Babylonian mathematics *was* indeed reasoned in this sense; characterize the type of reasoning involved; confront it with Euclidean reasoning about analogous cases; use this to characterize the approach of Greek theoretical geometry as embodied by Euclid's *Elements*; and briefly discuss a different type of Greek mathematical reasoning. In the end I shall widen the perspective toward other mathematical cultures.

2. OLD BABYLONIAN GEOMETRIC PROTO-ALGEBRA

Kline as well as Joseph speak about "Babylonian mathematics" as if this entity remained the same as long as the Babylonian culture lasted; so did until very recently almost everybody who dealt with the topic without being a specialist of exactly this historical field. At closer inspection, however, there are important differences between the mathematics of the Old Babylonian and the Seleucid periods (c. 1900–1600 BCE and c. 300–100 BCE, respectively). The large majority of texts comes from the Old Babylonian period, on which I shall concentrate at first.

The Old Babylonian mathematical corpus consists of three parts: tables, tablets for rough numerical work, and problem texts. Only the third group is relevant for the present discussion – actually only the "procedure" texts, texts which prescribe how to solve the problem stated in the beginning.

A large part of the problem texts have been understood since they were first interpreted in the 1930s to be of "algebraic" character.[5] Taken at their words, most of them deal with the measurable sides and areas of rectangles and squares, but these were taken to serve as mere dummies for unknown numbers and their products. Correspondingly, the operations that were performed were supposed to be arithmetical additions, subtractions, multiplications, etc. In this reading, the procedure descriptions look like mere prescriptions of numerical algorithms, with no indication of the way these have been found. A historian like Otto Neugebauer, who knew the corpus well, was fully aware that the procedures could not have been found without genuine mathematical reasoning, and presupposed that the texts had gone together with a system of oral instruction explaining the reasons for the steps; those general historians who knew only one or two simple examples in translation often believed that they had been found by trial and error (Kline, as we see, combines the two ideas).

Only a thorough investigation of the structure of the terminology and of the discursive organization of the texts reveals that the texts have to be taken at their geometrical words.[6] The problems are indeed (in a loose sense) homomorphic with those of numerical equation algebra, but many of the operations are geometric, not arithmetical.

As a first example we may look at the text YBC 6967,[7] which contains a single problem dealing with two numbers *igûm* and *igibûm* belonging together in the table of reciprocals, "the reciprocal and its reciprocal". This problem thus illustrates another respect in which the technique is similar to modern equation algebra: a functionally abstract "basic representation" (with us abstract numbers, with the Babylonians measured or measurable segments and areas) is used to represent magnitudes belonging to other ontological domains but involved in relations that are structurally similar to those characterizing the basic representation.

The text goes as follows in literal translation:

Obv.

1. [The *igib*]*ûm* over the *igûm*, 7 it goes beyond
2. [*igûm*] and *igibûm* what?
3. Yo[u], 7 which the *igibûm*
4. over the *igûm* goes beyond
5. to two break:[8] 3°30′;
6. 3°30′ together with 3°30′
7. make hold:[9] 12°15′.
8. To 12°15′ which comes up for you
9. [1` the surf]ace append: 1`12°15′.
10. [The equalside[10] of 1`]12°15′ what? 8°30′.
11. [8°30′ and] 8°30′, its counterpart,[11] lay down.

Rev.

1. 3°30′, the made-hold,

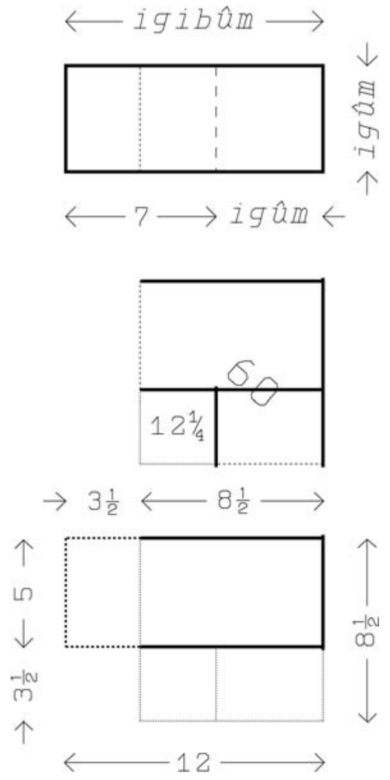


FIGURE 1. The representation of the *igûm-igibûm* problem of YBC 6967.

2. from one tear out,
3. to one append.
4. The first is 12, the second is 5.
5. 12 is the *igibûm*, 5 is the *igûm*.

What goes on may be followed in the diagram of Figure 1. We should expect the product of the two numbers to be 1, but it is actually meant to be 60 (whether due to the floating-point character of the number system or to the origin of the table of reciprocals as a tabulation of aliquot parts of 60 is uncertain). The two numbers are thus represented by the sides of a rectangle with area 1` (as obvious, e.g., from the reference to 1` in obv. 9 as a "surface". Since we are told that the *igibûm* exceeds the *igûm* by 7, the length of the rectangle exceeds the width by 7. This excess (with appurtenant section of the rectangle) is bisected, and the outer part moved around so as to contain together with the inner part a square □($3^1/_2$), whose area will be $12^1/_4$. When the original rectangle (transformed into a gnomon) is joined to this, a square with area $60+12^1/_4 = 72^1/_4$ is produced. The "equalside" of this area is $8^1/_2$, and so is its "counterpart". When that part of the rectangle which was "made hold" is restored to its original position, we get the original length, the *igibûm*, which will thus be $8^1/_2+3^1/_2 = 12$. But before we can restore it, we must remove it from the place where it was put; this removal produces the *igûm*, which must therefore be $8^1/_2–3^1/_2 = 5$.

As we see, no attempt is made to discuss *why* or *under which conditions* the operations performed are legitimate and lead to the correct result. On the other hand it is intuitively obvious, once we are familiar with the properties of rectangles, that everything *is* correct. In this sense the prescription is, as formulated by Karine Chemla ((1991), (1996), and elsewhere) regarding Chinese mathematics, algorithm and proof in one.

The clay tablet contains no drawing; a few others do, but only as support for the statement, never as a supplement to the prescription. For this reason we cannot know the precise character of the diagrams that supported the reasoning – they may have been drawn in sand strewn on a brick floor, on a wall, or in any other medium that has not been conserved; we do not even known to which extent trained calculators would make actual drawings, and to which extent they would rely on mental geometry. We may be confident, however, that drawings were made use of at some stage of the instruction – mental geometry builds on previous experience with material geometry, just as mental addition of multi-digit numbers presupposes previous exposure to pen-and-paper algorithms for almost all of us; we may also be fairly confident that the diagrams in question were structure diagrams and not made carefully to scale – field plans, at least, had this character (see Figure 2, a plan from the 21st century BCE). As we see, only the right angles (those angles which are essential for the determination of areas) are rendered correctly; in general, the Babylonians seem not to have regarded angles as quantifiable magnitudes – expressed in a pun, an angle which was not "right" was simply considered "wrong".

The notion of a "naive" proof integrated in the algorithm may astonish us, but should not do so. How, indeed, will we normally treat the corresponding problem in symbolic algebra if we merely need to solve it? More or less in the following steps:

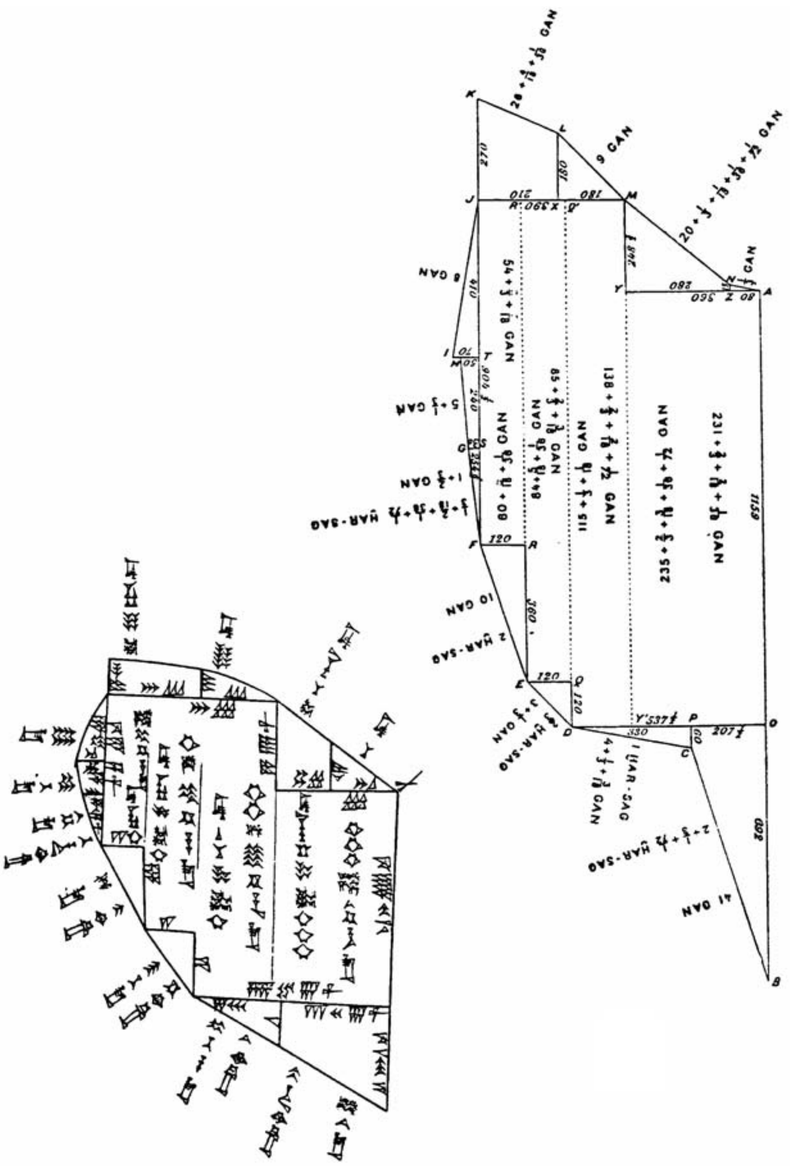


FIGURE 2. Field plan as drawn on the tablet (left) and in true proportions (right). From (Thureau-Dangin, 1897, 13,15).

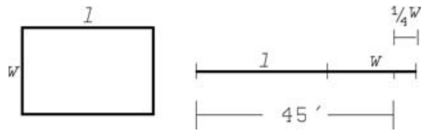


FIGURE 3. The situation of TMS XVI #1.

$$
\begin{array}{lrcl}
(3) & x-y & = & 7 \qquad xy = 60 \\
(4) & \frac{x-y}{2} & = & 3\tfrac{1}{2} \\
(5) & (\frac{x-y}{2})^2 & = & 12\tfrac{1}{4} \\
(6) & (\frac{x-y}{2})^2 + xy & = & 12\tfrac{1}{4} + 60 \;=\; 72\tfrac{1}{4} \\
(7) & (\frac{x+y}{2})^2 & = & 72\tfrac{1}{4} \\
(8) & \frac{x+y}{2} & = & \sqrt{72\tfrac{1}{4}} \;= 8\tfrac{1}{2} \\
(9) & x & = & \frac{x+y}{2} + \frac{x-y}{2} \;=\; 8\tfrac{1}{2} + 3\tfrac{1}{2} \;=\; 12 \\
(10) & y & = & \frac{x+y}{2} - \frac{x-y}{2} \;=\; 8\tfrac{1}{2} - 3\tfrac{1}{2} \;=\; 5
\end{array}
$$

We would obviously be able to justify every step if asked by somebody who did not follow the idea – but we would hardly justify the step from (3) to (4) with exact reference to the appropriate Euclidean axiom (or corresponding arithmetical theorem or axiom). Just as the Babylonian calculator, we thus proceed *naively*; so did any equation algebra until the advent of the Modern era. And just as that of the Babylonian calculator, our approach is *analytic:* we take the existence of the solution for granted, manipulate it as if it were known, and stop when we have disentangled the unknowns from the complex relationships in which they were involved.

Whereas the geometrical diagrams on which the reasoning was made have not survived, a few texts have transmitted the kind of explanations which must normally have been given orally. All are from Susa, a peripheral area (which may be the reason that explanations which elsewhere were transmitted within a stable oral tradition had to be put into writing). One –

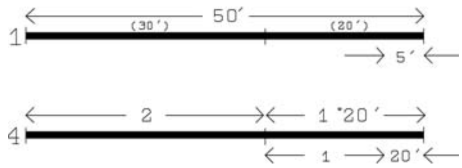


FIGURE 4. The transformations of TMS XVI #1.

TMS XVI – explains the transformations of two linear equations.[12] The first transformation runs as follows in translation:[13]

1. [The 4th of the width, from] the length and the width to tear out, 45´. You, 45´
2. [to 4 raise[14], 3 you] see. 3, what is that? 4 and 1 posit,[15]
3. [50´ and] 5´, to tear out, ⌜posit⌝. 5´ to 4 raise, 1 width. 20´ to 4 raise,
4. 1°20´ you ⟨see⟩, 4 widths. 30´ to 4 raise, 2 you ⟨see⟩, 4 lengths. 20´, 1 width, to tear out,
5. from 1°20´, 4 widths, tear out, 1 you see. 2, the lengths, and 1, 3 widths, accumulate, 3 you see.
6. Igi 4 de[ta]ch,[16] 15´ you see. 15´ to 2, lengths, raise, [3]0´ you ⟨see⟩, 30´ the length.
7. 15´ to 1 raise, [1]5´ the contribution of the width. 30´ and 15´ hold.
8. Since "The 4th of the width, to tear out", it is said to you, from 4, 1 tear out, 3 you see.
9. Igi 4 de⟨tach⟩, 15´ you see, 15´ to 3 raise, 45´ you ⟨see⟩, 45' as much as (there is) of [widths].
10. 1 as much as (there is) of lengths posit. 20, the true width take, 20 to 1´ raise, 20´ you see.[17]
11. 20´ to 45´ raise, 15´ you see. 15´ from $30_{15'}$[18] [tear out],
12. 30´ you see, 30´ the length.

The equation deals with the length (ℓ) and the width (w) of a rectangle – see Figure 3; in the actual case, however, this concrete meaning is relatively unimportant. In line 1, we are indeed told (in symbolic translation) that

$$(\ell+w)-{}^{1}\!/\!_{4}w=45'.$$

At first we are instructed to multiply the right-hand side by 4, from which 3 results. In line 2, the meaning of this number is asked for; the explanation given in lines 2–5 can be confronted with Figure 4, which may correspond

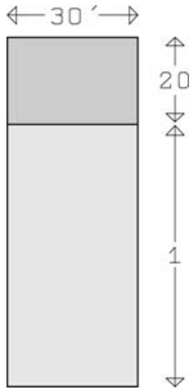


FIGURE 5. The configuration described in TMS IX #1.

more or less closely to something the author had in mind, and which is anyhow useful for us. As we observe, no problem is solved, the explanations presuppose (and the student is thus supposed to know) that the length is 30´ and the width 20´, their sum 50´ and the fourth of the width 5´.

In line 6, the equation is multiplied by $\frac{1}{4}$, from which follows both the "contribution of the width", that is, the value of the member $(1-\frac{1}{4})w$, and the *coefficients* ("as much as there is") of length and width. All in all, the explanations thus aim at giving concrete meaning to the outcome of the multiplication and to the original equation, not "proving" anything to be correct – no statement is involved which could be true or false except the claim that $4\times45 = 3$ – but making everything transparent, thus facilitating "naive" understanding of the correctness of procedures.

Two other didactical expositions are found in the text TMS IX #1 and #2.[19] Both deal with geometry of the kind that was used to represent the *igûm* and *igibûm* in YBC 6967. They run as follows:

#1

1. The surface and 1 length accumulated, 4[0´. ¿30, the length,? 20´ the width.]
2. As 1 length to 10´ ⌈the surface, has been appended,]
3. or 1 (as) base to 20´, [the width, has been appended,]
4. or 1°20´ [¿is posited?] to the width which 40´ together ⌈with the length ¿holds?]
5. or 1°20´ toge⟨ther⟩ with 30´ the length hol[ds], 40´ (is) [its] name.
6. Since so, to 20´ the width, which is said to you,

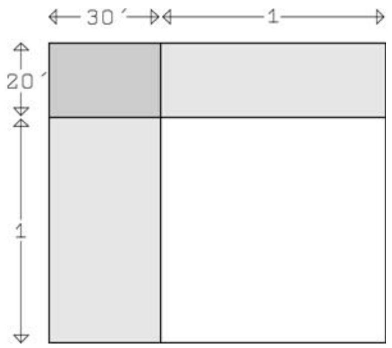


FIGURE 6. The configuration of TMS IX #2.

7. 1 is appended: 1°20´ you see. Out from here
8. you ask. 40´ the surface, 1°20´ the width, the length what?
9. [30´ the length. T]hus the procedure.

#2

10. [Surface, length, and width accu]mulated, 1. By the Akkadian (method).
11. [1 to the length append.] 1 to the width append. Since 1 to the length is appended,
12. [1 to the width is app]ended, 1 and 1 make hold, 1 you see.
13. [1 to the accumulation of length,] width and surface append, 2 you see.
14. [To 20´ the width, 1 appe]nd, 1°20´. To 30´ the length, 1 append, 1°30´.
15. [¿Since? a surf]ace, that of 1°20´ the width, that of 1°30´ the length,
16. [¿the length together with? the wi]dth, are made hold, what is its name?
17. 2 the surface.
18. Thus the Akkadian (method).

In #1, as we see, we are told that the arithmetical sum of the length and the area of a rectangle is $A+\ell = 40'$; once again, the explanation of what goes on presupposes the student to know that the length is 30´ and the width 20´. The text then explains how this is to be given a concretely meaningful interpretation. The trick is to replace the length ℓ by a rectangle $\sqsubset\!\!\sqsupset(1,\ell)$, which corresponds to joining an extra "base 1" to the width, as shown in Figure 5 (the orientation of which follows from the designation of the extension as a "base"). The resulting total "width" is $1°20'$; since the total area is $40'$, this is seen to correspond to the length $30'$, as it should.

In #2, we are told instead the arithmetical sum of the length, the width and the area, $A+\ell+w=1$. Once again, the dimensions are presupposed to be known, $\ell=30'$, $w=20'$, as can be seen in line 14. This time we are told to add $\sqsubset\!\sqsupset(1,1)=1$ to the sum $A+\ell+w$; the result is then shown to be the area of a new rectangle with length $L=1+30'=1°30'$, width $W=1+20'=1°20'$ – cf. Figure 6.[20] This section of the text is said to explain the "Akkadian method"; since the trick that distinguishes #2 from #1 is the joining of a quadratic complement to a (pseudo)gnomon, the "Akkadian method" is likely to be exactly this trick, basic for the solution of all mixed second-degree problems. Once again, the exposition serves to make clear why and how the methods works.

#3 of the tablet, the last problem and a problem in the proper sense (omitted from the translation), combines the equation of #2, $A+\ell+w=1$, with an equation of the same type as the one explained in TMS XVI though more abstruse – namely

$$\frac{1}{17}\big(3\ell+4w\big)+w=30'.$$

This is reduced, now without didactical explanation, to

$$3\ell+21w=8°30'$$

after which the corresponding equation for "the length and width of the surface 2" (L and W) is derived,

$$3L+21W=32°30'$$

Since $\sqsubset\!\sqsupset(L,W)=2$, $\sqsubset\!\sqsupset(3L,21W)$ is found to be $2\cdot3\cdot21=2`6$ (i.e., 126), and in the end the resulting rectangle problem for $\Lambda=3L$, $\Omega=21W$,

$$\Lambda+\Omega=32°30', \qquad \sqsubset\!\sqsupset(\Lambda,\Omega)=2`6$$

(the additive analogue of the problem solved in YBC 6967) is solved, and first L and W, next ℓ and w are found. No didactical explanation of how to solve the rectangle problem is extant, but we may safely assume that such an explanation was at hand and that its style was similar to what we know from TMS XVI and TMS IX #1–2.

Before we leave the Old Babylonian period it should be pointed out that certain aspects of the procedure descriptions reflect the presence of "critique", that is, the question about the reasons for and the limits of the validity of the procedure; this question is the antithesis of the "naive" approach. One instance is the precedence of "tearing-out" over "appending" in YBC 6967, rev. 2–3, the other the explicit introduction of the "base 1" in TMS IX #1.

That these features of the text are "critical" only becomes visible when the historical development of Old Babylonian "algebra" is understood, which requires another structural analysis of the corpus, this time associating the distribution of synonyms and characteristic phrases with orthography and what (little) is known about the archaeological provenience of tablets (most, indeed, have been bought by museums on the antiquity market), and correlation of the problems found in the Old Babylonian corpus with those found in a number of other historical contexts (Seleucid and other Late Babylonian problem texts, ancient Greek theoretical, Neopythagorean and practitioners' mathematics, Arabic algebra and agrimensorial texts, Jaina and Italian *abbaco* mathematics). I shall not attempt to reduce the necessary complex arguments to what can be contained in a few paragraphs[21] but only sum up the relevant results.

In the later third and incipient second millennium BCE, a restricted number of geometrical riddles circulated in a lay (that is, non-scribal, non-schooled) and probably Akkadian-speaking[22] environment of surveyors/practical geometers. A number of these were to be solved by means of the kind of naive cut-and-paste geometry which we have encountered in YBC 6967 and by application of the trick of quadratic completion (thus for good reason designated the "Akkadian method"; the trick seems to have been discovered at some moment before c. 1900 BCE, and probably after c. 2200 BCE): to find the side of a square from the sum of the side or "all four sides" and the area, or from the difference one or the other way around; to find the sides of a rectangle from the area and the diagonal or from the area together with the sum of or difference between the sides (with a few variants); problems dealing with two concentric squares (with given sum of/difference between the sides and the areas) were apparently solved by means of standard diagrams.

In the nineteenth century BCE, these problems were adopted into the Old Babylonian scribe school, where they gave rise to the development of the so-called "algebra" (which is much more refined than can be seen from the above examples: solving mixed third-degree problems by means of factorization – reducing biquadratic problems and even a bi-biquadratic problem stepwise – inverting the role of unknowns and coefficients – etc.). As it turns out, those text groups which are closest to the lay tradition do not respect the "norm of concreteness" according to which "tearing-out" must precede "appending" of the same entity but use the elliptic phrase "append and tear out"; some early texts, moreover, follow the habit of many non-Mesopotamian lay surveying traditions and operate with a notion of "broad lines", that is, with the idea that a line carries an inherent standard width.[23] For this reason, they are able to "append" sides to areas, which indeed they do.

The school environment, however, appears to have found it difficult to accept the conflation of linear and planar extension, and therefore formulated the inhomogeneous sums as "accumulations" (namely, of the measuring numbers), devising moreover a variety of designations for the standard width which transforms a side into a rectangular area.[24] Some schools also seem to have found it absurd to "append" something which is not yet at hand, and therefore introduced the "norm of concreteness". If "critique" is understood as investigations of *why* and *under which conditions* our usual naive ways and conventional wisdom hold good,[25] then these are full-blown examples.

The chronological dissection of the Old Babylonian corpus allows a final observation of importance for our topic.[26] All above examples were formulated around paradigmatic cases, yet in agreement with Gillings's criteria for when an argument from a paradigmatic case can be considered rigorous – cf. p. 2. This is no accident: almost all Old Babylonian mathematical texts that present us with explicit or implicit arguments have this character. There are, however, exceptions, and a few texts do indeed formulate rules in general terms. These rules may build on insight and argument, and can hardly have been invented without the intervention of some kind of mathematical insight; the rules themselves, however, only prescribe steps to be performed, and contain no trace of an argument.[27] Interestingly, all such attempts at general formulation belong in the earliest texts. The way such rules turn up in later sources suggest that they were a borrowing from the lay tradition, within which they may indeed have been very useful.[28] Within the school, however, they were soon eliminated, being both ambiguous when not supported by an example and pedagogically useless (probably because they were deprived of argument). The absence of abstract general rules is thus, like the compliance with the norm of concreteness, no consequence of a primitive mind unable to free itself from concrete thought; to the contrary, both have resulted from deliberate pedagogical or philosophical choice.

3. EUCLIDEAN GEOMETRY

Figure 1 is quite similar to the diagram of *Elements* II.6 – see Figure 7. Since the underlying mathematical structures are also analogous (to the extent a problem can be analogous with a justification of the way it is solved), it seems obvious to look closer at this Euclidean proposition.

In Thomas Heath's faithful translation (1926, I, 385) it states the following:

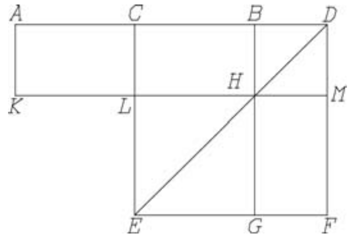


FIGURE 7. The diagram of *Elements* II.6.

> If a straight line be bisected and a straight line be added to it in a straight line, the rectangle contained by the whole with the added straight line and the added straight line together with the square on the half is equal to the square on the straight line made up of the half and the added straight line.

Next follows what Antiquity would apparently see as a particular example with indubitable paradigmatic value[29] but which Kline (and most modern readers) have come to regard as actually and not only potentially general: [30]

> For let ⟨any⟩ straight line *AB* be bisected at the point *C*, and let ⟨any⟩ straight line *BD* be added to it in a straight line; I say that the rectangle contained by *AD*, *DB* together with the square on *CB* is equal to the square on *CD*.

The proof starts by constructing the latter square (*CEFD*) and drawing the diagonal *DE*. Next through *B* the line *BHG* is drawn parallel to *CE* or *DF* (*H* being the point where the line cuts *DE*) and through *H* the line *KM* parallel to *AB* or *EF*. Finally, through *A* the line *AK* is drawn parallel to *CE* or *DF*.

Now the diagram is ready, and with reference to the way the construction was made ▭*AL* is shown to equal ▭*HF*. Adding ▭*CM* to both, the gnomon *CDFGHL* is seen to equal ▭*AM*. Further addition of □*LG* shows that ▭*AM* together with □*LG* equals □*CF*, as stated in the theorem.

The second part of the proof follows the pattern of the cut-and-paste procedure of YBC 6967 precisely. The important difference is the presence of the first part. Thanks to this, things are not just “seen”, they are as firmly established as required by the norms of Greek geometry – we do not move areas around and glue them together, we *prove* that one area (▭*AL*) is equal to another (▭*HF*). Even the fact that the gnomon *CDFGHL* together with □*LG* is identical with □*CF*, though not argued in detail, could be proved rigorously by repeated use of proposition II.1.

The first part of the proof of proposition II.6 can thus be seen as a *critique* which consolidates the well-known. Other propositions and proofs from the sequence *Elements* II.1–10 invite to make similar observations and interpretations. To this we may add that the riddles of the surveyors' tradition were doubtlessly known in classical Antiquity – as we shall see below, the riddle of "the four sides and the area" turns up in the pseudo-Heronian *Geometrica*. The whole sequence repeats matters that were familiar in the surveyors' tradition at least since the earliest second millennium BCE; many of the propositions, moreover, are never used explicitly later on in the work, which supports the interpretation that their critical consolidation was an aim in itself. Finally, all are proved independently, although a derivation of one from the other would often have been easy (actually, II.5 and II.6 are equivalent, and so are II.9 and II.10); what needs to be consolidated is thus not only the customary knowledge contained in the propositions but also the traditional naive-geometric *argument*.[31]

Greek theoretical geometry as a whole was evidently much more than a consolidation of the well-known; in as far as its ideals of what constitutes a *proof* are concerned, however, book II of the *Elements* may be regarded as representative. In aiming at critique of the already familiar it is certainly no first in the history of mathematics – as we have seen, something similar was made in the Old Babylonian scribe school, and it is part of the dynamics of any institutionalized teaching of mathematics at levels where appeals to the reasoning of the students are required.[32] In the Old Babylonian school, however, the role of critique had been peripheral and accidental; in Greek theoretical geometry it was, if not *the* very centre then at least an essential gauge.[33]

4. STATIONS ON THE ROAD

In the Old Babylonian mathematical texts we find names for particular lines (lengths, widths, various transversals, etc.); but we find no term for linear extension in general. Nor is any term for an angle (or a right angle) to be found. This does not mean that surveyors could not speak about lines unless they were already defined as the length or width of a field, the length or height of a wall, a carrying distance, etc., nor that they were unable to refer to the corner of a building; but *tubqum* ("corner") was not used as a technical term in mathematics. In general, it is doubtful whether the terminology of Old Babylonian mathematics can at all be characterized as "technical". Instead, as concluded in (Høyrup, 2002, 302),

> it is rather a very standardized use of everyday language to describe an extra-linguistic – computational and naive-geometrical – practice which was always *more* standardized than the linguistic description. The linguistic description was thereby analogous to our heuristic explanations in standardized ordinary language of what goes on in those symbolic formulae which with us constitute the level of real technical operation.

An early step in the unfolding of Greek theoretical critique was the establishment of definitions. Irrespective of Aristotle's claim that Socrates "was the first to concentrate upon definition",[34] discussions of semantic delimitations go back as far in Greek (proto-)philosophy as we can follow it – a very early example is Hesiod's pointing out in *Works and Days* (ed., trans. Mazon, 1960, 86) that the word "strife" (ἔρις) corresponds to two very different things (namely peaceful competition and cruel war). The definition of number as a "multitude composed of units"[35] is likely to go back at least to the fifth century BCE, and many other definitions were known to, and discussed by, Plato and Aristotle. Of particular interest are the definitions of the various classes of (rectilinear) angles (trans. Heath 1926, I, 181):

> 10. When a straight line set up on a straight line makes the adjacent angles equal to one another, each of the equal angles is *right* [. . .].
> 11. An *obtuse angle* is an angle greater than a right angle.
> 12. An *acute angle* is an angle less than a right angle.

These were known to Aristotle, who refers to them in *Metaphysics* M 1084 ^{b}7. But they may have been a relatively fresh invention in his days, since Plato's Socrates speaks in *Republic* VI, 510C (trans. Shorey 1930, 1935, II, 111) of the three kinds of angles as things of which geometers "do not deign to render any further account to themselves or others, taking it for granted that they are obvious to everybody".[36]

A clear notion of a right angle is evidently essential for making proofs like that of *Elements* II.6. In Aristotle's times the above definition was apparently supposed to be sufficient. This follows from what can be derived from Aristotle's writings about the status of the Euclidean postulates. On the whole, he does not seem to have heard of them (McKirahan, 1992, 133–137), which would suggest that their need had not yet been felt. Only the second postulate appears to have been known to Aristotle in a formulation close to what we find in the *Elements* – *Physics* III, 207^{b}29–31 (trans. Hardie & Gaye, 1930) explains that mathematicians "do not need the infinite and do not use it. They postulate only that the finite straight line may be produced as far as they wish".

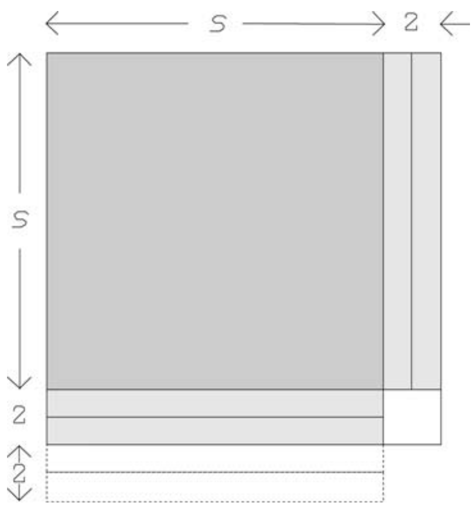


FIGURE 8. The procedure described in *Geometrica* 24.3.

This implies that no need had as yet been discovered around the mid-fourth century for postulate 4, "that all right angles are equal to one another", and thus, since this principle is essential for a large number of proofs of the equality of figures, that it was tacitly believed to be inherent in the definition. In Euclid's time, on the other hand, it was recognized that this was not the case. Although critique may have been just as compulsory for Greek geometers of the early fourth century as for their third-century successors, the level at which critique was actually performed was raised in the historical process – which of course cannot astonish if we recognize that mathematical rigour is a human product in process, never absolute and never finished once and for all.

5. OTHER GREEKS

The community of "theoreticians" (however that was delimited) was not the only community of the classical world to deal with mathematics. On one hand, the social need for mathematical practitioners was certainly not lower than it had been in the older Egyptian and Mesopotamian civilizations (nor probably significantly higher); on the other, the diffuse area encompassing Neopythagoreanism, Hermeticism, Gnosticism and Neoplatonism was also fond of mathematical metaphors and astounding mathematical insights.[37] In sources stemming from either community, instances or traces of mathematical reasoning can be located. In both cases what we find is naive, not critical. I shall present one example from each.

The first, belonging to the practitioners' tradition, comes from the pseudo-Heronian *Geometrica*.[38] It is a Greek version of the riddle of "all fours sides and the area":

> A square surface having the area together with the perimeter of 896 feet. To get separated the area and the perimeter. I do like this: In general [καθολικῶς, i.e., independently of the parameter 896 – JH], place outside [ἐκτίθημι] the 4 units, whose half becomes 2 feet. Putting this on top of itself becomes 4. Putting together just this with the 896 becomes 900, whose squaring side becomes 30 feet. I have taken away underneath [ὑφαιρέω] the half, 2 feet are left. The remainder becomes 28 feet. So the area is 784 feet, and let the perimeter be 112 feet. Putting together just all this becomes 896 feet. Let the area with the perimeter be that much, 896 feet.[39]

The procedure that is described is shown in Figure 8 (the manuscript only contains a drawing of a square with inscribed value for the side and the area; apparently, the geometry is meant to be either mental or performed independently by the reader[40]). As we see, the procedure is identical with what we have seen in the text YBC 6967, apart from those details that follow from the fact that we are dealing with a square and not with a rectangle. The style is certainly reasoned: "I have taken away underneath the half, 2 feet are left. The remainder [when these too are removed] becomes 28 feet"; but it is fully naive. The text also points out which numbers belong to the type *in general* (square area and perimeter) and do not depend on the particular parameters of the example, safeguarding thus potential generality; this is currently done in the various *Geometrica*-components and also in kindred medieval treatises, and already in one text from Old Babylonian Susa.

The various Neopythagorean writings are less generous when it comes to revealing the reasoning behind the mathematical facts they relate – maybe because astounding mathematical facts, once we understand their grounds, tend to be less astounding and therefore less serviceable for the display of wisdom beyond ordinary human reason. Sometimes, however, reasons shine through. One interesting case is found in Iamblichos's commentary to Nicomachos's *Introduction*:[41] namely the observation that 10×10 laid out as a square and counted "in horse-race" (see Figure 9) reveals that

$$10\times10=(1+2+\ldots+9)+10+(9+\ldots+2+1)$$

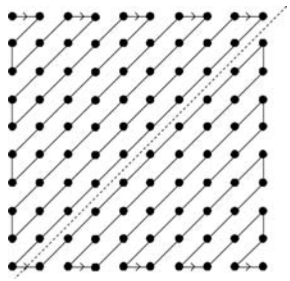


FIGURE 9. 10×10 arranged as a "race-course".

whence

$$10 \times 10 + 10 = 2T_{10},$$

T_n being the triangular number of order n. This argument will have been common Pythagorean or Neopythagorean lore, if we are to believe Iamblichos's exposition, though hardly a discovery made within this environment.[42] In any case, the naive type of reasoning will not have been left behind when the Pythagorean scientologists took over from existing mathematics that which they managed to understand (which could be neither the theory of *Elements* X, Apollonian *Conics*, Archimedean infinitesimal methods, nor "Heron's" formula for the triangular area).

6. PROPORTIONALITY – REASONING AND ITS ELIMINATION

Does this mean that mathematics is always in some way reasoned, either naively or critically? In some sense *yes*, simply because we are unlikely to count as "mathematics" activities which are wholly devoid of understanding, however much they have to do with countable items or take place in geometrical space. But mathematics need not always *be taught*, nor to *be exercised* as a reasoned practice. When learning to drive a car you probably received a number of instructions and explanations, about changing gears, about braking and aquaplaning, etc. But woe to your passengers if you use your conscious mental reserves too intensively on thinking about these matters when you move in the traffic.

A mathematician behaves no different. Most of the transformations of symbolic expressions are performed automatically, leaving energy for conscious reflection on the more intricate and still unfamiliar aspects of the problem that is treated; the activity of the mathematician thus remains reasoned, if only at a higher level.

But the routine activity of the mathematical practitioner may be different in character. Remaining in the pre-Modern epoch, we may illustrate this through a look at the way simple linear problems were dealt with.

A typical late medieval rule for solving such problems can be found in Jacopo da Firenze's *Tractatus algorismi* from 1307.[43] It runs as follows:

> If some computation should be given to us in which three things were proposed, then we should always multiply the thing that we want to know against that which is not similar, and divide in the other thing, that is, in the other that remains.

After this follows a sequence of examples, beginning with this:

> I want to give you an example to the said rule, and I want to say thus, VII *tornesi* are worth VIIII *parigini*.[44] Say me, how much will 20 *tornesi* be worth? Do thus, the thing that you want to know is that which 20 *tornesi* will be worth. And the not similar (thing) is that which VII *tornesi* are worth, that is, they are worth 9 *parigini*. And therefore we should multiply 9 *parigini* times 20, they make 180 *parigini*, and divide in 7, which is the third thing. Divide 180, from which results 25 and $\frac{5}{7}$. And 25 *parigini* and $\frac{5}{7}$ will 20 *tornesi* be worth. And thus the similar computations are done.

This is the rule of three, and may be customary. But try to explain why it works without using paper and symbolic manipulations to somebody who is not too well trained in mathematics![45] The reason for the difficulty is of course that the intermediate result 9 *parigini* $\times$ 20 *tornesi* has no concrete interpretation.

Babylonian, Egyptian and ancient Greek calculators would have proceeded differently. Their normal procedure would have been to divide first (by whatever method they would use for division) 9 *parigini* by 7 *tornesi*. The result has an obvious concrete interpretation, the value of 1 *torneso* in *parigini*. Next, this could be multiplied by 20 in order to find the value of 20 *tornesi*.

Why was this easy and didactically efficient procedure given up? The key is inherent in the remark "by whatever method . . . ". Division is difficult, and often leads to rounding (either for reasons of principle, namely if you have to multiply by a non-exact reciprocal, or because it may lead to a very unhandy string of aliquot parts). Subsequent multiplication will lead to multiplication of the rounding error, quite apart from the practical difficulty of multiplying an inconveniently composite numerical expression. Better therefore postpone the division and make it the last step.

Why, then, was it not given up before?[46] Once again, the explanation is straightforward and of a practical nature. It was set forth by Christian Wolff alias Doktor Pangloss in his *Mathematisches Lexikon* (1716, 867):

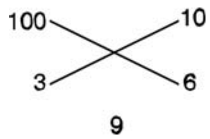
> It is true that performing mathematics can be learned without reasoning mathematics; but then one remains blind in all affairs, achieves nothing with suitable precision and in the best way, at times it may occur that one does not find one's way at all. Not to mention that it is easy to forget what one has learned, and that that which one has forgotten is not so easily retrieved, because everything depends only on memory

– in other words, only procedures that are performed so often that you run no risk of forgetting them (like changing gears in a car) can be safely taught as mere skills. Probably the scribes of Near Eastern Antiquity did not perform the kind of proportional operations we are speaking of so often that the appeal to their understanding could be given up safely.

More complex linear problems were often solved by means of the so-called "double false position", which is even more opaque. The intelligible alternative to this rule can be illustrated by another quotation from Jacopo (fol. 22r):

> I have new *fiorini* and old *fiorini*. And the *old fiorino* is worth *soldi* 35, and the new *fiorino* is worth *soldi* 37. And I have changed 100 *fiorini* new and old together, and I have got for them *libre* 178. I want to know how many new *fiorini* and how many old *fiorini* I had. Do thus, posit the case that all were of one of these rates, that is, all 100 of whatever rate you want. And let us say that they are all 100 old *fiorini*. And know how much they are worth for *soldi* 35 each, they are worth 175. Now say thus, from 175 until 178 there is *libre* 3, which are *soldi* 60. Now divide *soldi* 60 in the price difference which there is from one *fiorino* to the other, that is, from 35 *soldi* until 37, which is 2. Divide 60 in 2, 30 results. And 30 *fiorini* shall we say have been of the opposite (sort) of those {...} which we said were all old. And therefore we shall say that these 30 have been new, and the rest until 100, which is 70, have been old. And thus I say that they were.

This is easily understood (once you know that 1 *libra* is worth 20 *soldi*) – and precisely the same method (starting only from a fifty-fifty assumption) is used in the Old Babylonian problem VAT 8389 #1 (ed. Neugebauer, 1935–1937, I, 317*f*, III, 58). If the double false position had been applied, the procedure had been much less comprehensible. One false assumption might be that all were old, in which case they would have been worth 3500 *soldi* = 175 *libre* – three less than I really get. The other false assumption might be that only 10 were old[47] and 90 hence new; in this case, I would have got 184 *libre*. The whole thing might be inserted in a graphical scheme



in which you were the to perform a cross-multiplication, add and divide by the sum of the two errors as written at bottom,[48] finding the real number of old *fiorini* to be $\frac{100\times6+10\times3}{9} = 70$.

The principle can be explained as a linear interpolation; the real origin may be the alligation rule. But the texts never give any explanation, they simply set it forth as a rule to be followed. The obvious danger is that it may happen to be applied to non-linear situations, and that the reckoner would have no possibility to know that this was wrong.[49]

The moral is that Doktor Pangloss was right as soon as we get beyond the most routine applications of mathematics. A fundament in reason *is* an advantage not only in mathematical theory (where it belongs to the definition and is thus no mere advantage) but also in every application that goes beyond complete routine. It is therefore to be expected that mathematics teaching in any mathematical culture which went beyond mere routine (on its own conditions for what could constitute routine) did include appeals to reason – whether naive or critical, and whether in Greek style (or that dubious reading of the Greek style in which we project ourselves) is a different matter. If we cannot find traces of this reasoning in extant sources we may safely conclude that this is due, *either* to failing understanding of the sources on our part, *or* to the insufficiency of extant sources as mirrors of educational practice. *Tertium non datur*.

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NOTES

[0]The arguments of the following paper are largely distilled from a variety of topics I have worked on over the years; in the interest of relative brevity I have been forced to leave out almost all of the factual background for the conclusions I have drawn on earlier occasions. To an unpleasant extent, the bibliography is therefore dominated by my own publications; further references to sources and to works of other scholars are found in these.

[1]Such "internal" arguments must of course be distinguished from other types of arguments. To say that something is true because it is stated by Euclid or in the Bible constitutes no mathematical proof; nor do deductions from metaphorical connotations of the terms involved or from metaphysical postulates – for instance, Cusanus's postulate that the maximal and the minimal coincides in combination with the observation that area measurement divides complex areas into triangles, from which follows that God must be triangular, that is, Trinity.

[2]In some sense these anti-Eurocentric objections have often been paradoxical, their aim being to show that "non-Western" cultures had the same kind of (meta-)mathematics as the Greeks: implicitly, the ideals of (what we find in) Greek mathematics are accepted.

As I shall argue in the end, certain mathematical cultures (not ethnic but professional cultures) have had the attitude that under particular circumstances some mathematics should *not* be reasoned, and have had it for a good reason.

[3]Elsewhere Joseph (1991, 125–129) goes into direct though imprecise polemic with Kline.

[4]It is immaterial for the present purpose that it is often awfully mistaken in details (terribly wrong datings, freely invented "translations", confusion of modern interpretation and ancient text, similar confusion between algorithm and theoretical algebra – see (Høyrup, 1992)) and thus allows opponents of the author's general aim to conclude that no good arguments can be found in favour of the existence of non-Greek, not Greek-derived mathematics. In the view of anybody who shares the aim, this is of course the most serious shortcoming of the book.

[5]The history of these interpretations is described in (Høyrup, 1996a).

[6]The first thorough exposition of this analysis is (Høyrup, 1990); equally thorough but probably more reader-friendly is (Høyrup, 2002).

Part of the outcome of the structural analysis (and one of the reasons that the arithmetical interpretation breaks down) is the sharp distinction between two different additive operations (not merely synonyms for the same operation), between two different subtractive operations, two different halves, and no less than four different "multiplications". Since we shall encounter the additions below, they may serve as example. One of them I shall translate "appending", the other "accumulation". The former stands for a concrete joining to a magnitude which conserves its identity (in the same sense as addition of the interest conserves the identity of my bank account – interest on a loan *is* indeed called "the appended" in Babylonian); the other may be used about the purely arithmetical addition of the measuring numbers of ontologically different magnitudes – e.g., of lengths and areas, of areas and volumes, or of men, days, and bricks carried by the men in question during the days in question.

[7]Based on the transliteration in (Neugebauer and Sachs, 1945, 129); as everywhere where no translator is indicated the English translation is mine. The numbers are expressed in a sexagesimal place value system (that is, a system with base 60), in which ´, ˝, ... indicate decreasing and `, ˵, ... increasing sexagesimal order of magnitude (and ° when needed "order zero"); 30´ is thus $30 \cdot 60^{-1} = 1/2$, $15´ = 15 \cdot 60^{-1} = 1/4$. These indications of absolute order of magnitude are not present in the original – the number notation of the mathematical texts (obviously not that of accounting and practical surveying!) is a floating-point system.

Words in [ ] are damaged on the tablet and reconstructed from parallel passages; words in ( ) are added for comprehensibility.

[8]"Breaking" is a bisection that produces a "necessary half", a half that could not have been chosen differently – e.g., that half of the base of a triangle that serves in area calculation. On the other hand, if a problem states that a square area and a half of the side are accumulated, the other, "accidental" half occurs – it might just as well have been a third.

[9]"Making a and b hold" stands for the construction of the rectangle contained by the sides a and b – henceforth $\square(a,b)$.

[10]The "equalside of A" (in the terms of other texts, that which "is equal along A") is the side of A when this area is laid out as a square; numerically it corresponds to the square root of A.

[11]The "counterpart" of an "equalside" is the side with which it has a corner in common.

[12]The use of the term "equation" is no anachronism. The equations of a modern engineer or economist state that the measure of some composite magnitude equals a certain number, or that the measure of one magnitude equals that of another; exactly the same is done in the Babylonian texts.

[13]Based on the hand copy and transliteration in (Bruins and Rutten, 1961, 91f, pl. 25), with corrections from (von Soden, 1964). Cf. revised edition of the full tablet in (Høyrup, 1990, 299–302). The translation in the original edition should be used with caution, and the commentary is best disregarded completely.

[14]"Raising" designates the determination of a concrete magnitude by means of a multiplication, and presupposes a consideration of proportionality. Originally the metaphor referred to the determination of a prismatic volume with height h, obtained by "raising" the base from its virtual height of 1 cubit (presupposed by the metrology, which measured volumes in area units) to the real height.

[15]"Positing" appears to mean "taking note of" materially, at times on a counting board, at times by writing a length along a line as in Figure 2.

[16]igi n designates the reciprocal of n. For numbers where this was possible, division by n was performed as a raising to igi n (in administrative calculation it was always possible, since all technically relevant coefficients were rounded to numbers that possessed a convenient igi).

Finding igi n was spoken of as "detaching" it; the idea was probably that one part was detached from a bundle of n parts of unity.

[17]This step may refer to a distinction between a "real" field with dimensions 30 and 20 (180 m $\times$ 120 m, since the tacitly presupposed length unit was the "rod" equal to c. 6 m) and a "model field" 30´ $\times$ 20´, i.e., 3 m $\times$ 2 m, certainly more easily drawn in the school yard; since the text does not indicate absolute order of magnitude this must remain a hypothesis.

[18]This renders the non-standard way () in which "45" is written in this place in the tablet.

[19]Based on the transliteration and hand copy in (Bruins and Rutten, 1961, 63*f*, pl. 17), with corrections from (von Soden, 1964). Cf. revised edition in (Høyrup, 1990, 320–323). Even in this case, the translation and the commentary in the original edition ask for benign neglect.

[20]This presence of several "lengths" and "widths" shows why the exposition needs to presuppose that the measures of the configuration are known: these measuring numbers serve as identifying tags, and are needed for this purpose in the absence of letter or similar symbols.

Even many genuine problem texts refer to the value of certain entities before they are found. This may give the impression that the problems are overdetermined and their authors hence mathematically incompetent. That, however, is a mistaken reading: the information which is made use of never exceeds what is necessary; this constitutes the set of "given numbers", which is always kept strictly apart from those numbers which are "merely known" and used as identifiers.

[21]The structural analysis of the corpus is described in (Høyrup, 2000b), and (with some extensions and minor revisions) in (Høyrup, 2002, 317–361). The place of Old Babylonian "algebra" in the network of mathematical cultures was first investigated in (Høyrup, 1996b); a more thorough exposition is (Høyrup, 2001). Information on the latter topic is also given in (Høyrup, 2002, 362–417, *passim*).

[22]The hegemonic and scribe school language of the third millennium was Sumerian. However, the presence of Akkadian (later split into a Babylonian and an Assyrian dialect) is attested already before 2500 BCE, gradually rising to become the dominant language in the early second millennium. With extremely few exceptions the language of the Old Babylonian mathematical texts is Akkadian, though the writing often makes heavy use of Sumerian word signs (as English writing may make use of the medieval word sign for Latin *videlicet*, rendered as *viz* yet presupposing a pronunciation "namely").

[23]As does cloth today, when we buy "three yards of curtain material". The notion of the "broad line" and its appearance in a number of practical geometries is examined in (Høyrup, 1995).

[24]One of these designations is the "base" of TMS IX #1; but at least two alternatives are attested in the corpus.

[25]"Untersuchung der Möglichkeit und Grenzen derselben", as expressed in Kant's *Critik der Urtheilskraft* (B III (Kant, 1956–1964, Werke V, 237)).

[26]See (Høyrup, 2002, 344, 383, and *passim*).

[27]Nor should they, this is not the nature or purpose of a rule – our multiplication table contains no hint of the role of associativity and distributivity of the operations involved.

[28]The general rule is an adequate tool for an oral tradition, being more easily remembered mechanically and transmitted faithfully than the full paradigmatic example; explanations and examples can then be improvised once the master knows what is meant by a possibly ambiguous rule. A parallel is offered by the relation between fixed formulae and relatively free use of these by the singer in oral epic poetry, see (Lord, 1960, 99–102 and *passim*).

[29]Apart from the use of the habitual format rule–example and the precise wording, this interpretation is supported, for instance, by Aristotle's analogous reference to geometric arguing which is correct if only we avoid including in the premises we draw on the particular characteristics of the drawing made on the ground (*Metaphysics* 9, 1078^a19–20). See also the detailed discussions in (Mueller, 1981, 11–14) and (Netz, 1999, 247–258 and *passim*).

[30]Trans. Heath (1926, I, 385), with minor corrections in ⟨ ⟩.

[31]Being necessarily ignorant of the whole prehistory, Heath (1926, I, 377) formulated this as follows:

> What then was Euclid's intention, first in inserting some propositions not immediately required, and secondly in making the proofs of the first ten practically independent of each other? Surely the object was to show the power of the *method* of geometrical algebra as much as to arrive at results.

[32]This topic is dealt with in (Høyrup, 1985), and, more crudely but more precisely in aim and with broader historical scope, in (Høyrup, 1980).

[33]In the introduction to the *Method*, Archimedes argues that "we should give no small part of the credit to Democritus who was the first to make the assertion [that the cone is the third part of the cylinder, and the pyramid of the prism] though he did not prove it" (trans. Heath, 1912, 13). The rhetoric of the argument implies that the opposite attitude prevailed; rhetoric may distort things but becomes ineffective if the recipient knows that it is fully off the point – which we may therefore suppose that it was *not*, the recipient (Eratosthenes) being as conversant as anyone with both the mathematics and the norms that governed it at his times.

[34]*Metaphysics* A, 987^b3, trans. (Tredennick, 1933, 1935, I, 43). The Greek term is ὁρισμός, related to the Euclidean term ὅρος, the former meaning something like "delimitation"/"marking out by boundaries", the latter "limit"/"boundary".

[35]Itself an outcome of critique, which remained fateful for more than 2000 years and encumbered the theoretical justification of *algebra* in the early Modern era, since this attempt to make unambiguous and stable sense of the notion of a number excluded not only 1 (a fact which Euclid forgets when defining a "part" in *Elements* VII, immediately after he has repeated the habitual definition of a number!) but also fractions.

[36]The passage may also mean, however, that they allow no further discussion *beyond* the definitions they have given, in which case the definitions will obviously have been older.

[37](Cuomo, 2000) is a pioneering investigation of the situation and interplay of these groups in late Antiquity, in particular as reflected in Pappos's *Collection*.

[38]*Geometrica* 24:3, ed. (Heiberg, 1912, 418), photographic reproduction of the manuscript (Bruins, 1964, I, 53). As with the Babylonian texts, my translation is meant to be pedantically literal. Actually, we should speak of "Heiberg's" rather than of any pseudo-Hero's *Geometrica*. Heiberg produced the bulk of the conglomerate from two ancient treatises which were already composite and cannot be traced back to a common source (as told quite explicitly by Heiberg, but in Latin and in a different volume of the Heronian *Opera omnia* (Heiberg, 1914, xxiii–xxiv), for which reasons the fact has generally gone unnoticed). These two treatises are represented, respectively, by Heiberg's mss A+C and mss S+V. Chapters 22 and 24, however, are independent treatises (24 another conglomerate) which happen to be contained in the same codex as *Geometrica*/S but at a distance. See (Høyrup, 1997, 77).

[39]Heiberg does not grasp the geometrical procedure that is described, for which reason his commentaries are misguided, imputing the faulty understanding on the ancient copyist.

[40]This is also the case in the *Liber mensurationum*, an Arabic treatise building on the surveyors' tradition (known from Gherardo da Cremona's Latin translation, ed. (Busard, 1968)): the sequence of problems about squares starts by a drawn square, that of rectangle problems with a rectangle, etc. Only a few fourteenth- and fifteenth-century Latin and Italian descendants of the tradition contain drawings illustrating the whole procedure.

[41]Ed. (Pistelli, 1975, 75^{25-27}), cf. (Heath, 1921, 113*f*).

[42]Firstly, in belongs squarely within the style of *psēphos* arithmetic that can be presupposed to be at the basis of the "doctrine of odd and even"; this was generally familiar at too early a moment to be Pythagorean – Epicharmos Fragment B 2 ((Diels, 1951, I, 196; earlier than c. 475 BCE)) refers to the representation of an odd number ("or, for that matter, an even number") by a collection of *psēphoi* as something trivially familiar. Secondly, the ensuing formula for the triangular number,

$$T_n = \frac{n^2+n}{2}$$

belongs no less squarely within a cluster of summation formulae shared between Seleucid and Egyptian Demotic sources which betray no Greek influence in any other respect (Høyrup, 2000a). Together with the whole technique of *psēphos*-based reasoning it is thus almost certainly a borrowing from Near Eastern practical mathematicians.

[43]MS. VAT Lat. 4826, fol. 17r. I translate from my own transcription of the manuscript (1999).

[44]The *parigino* and the *torneso* are coins, minted in Paris and Tours, respectively.

[45]Or observe how even the mathematically well-trained person grasps a piece of paper and starts writing down symbols when confronted with the verbal rule!

[46]In fact it was – but in India, where the characteristic terms of the rule of three can be traced back to c. 400 BCE (Sarma, 2002), and in China, where it is introduced in chapter 2 of the *Nine Chapters on Arithmetic* from the first century CE, trans. (Chemla & Guo, 2004, 225*ff*). Medieval Islamic mathematicians (and probably practical reckoners) borrowed it from India.

[47]The Indians might have chosen that none were old, since they operated with both zero and negative numbers; but this simple choice was not accessible around the Mediterranean.

[48]Presupposing that one error is an excess, the other a deficit.

[49]I am referring here to Mediterranean texts. Even though Arabic writers ascribe the rule to India, the simple form is not found in extant Indian sources. But what may be a correct iterated use in a non-linear situation turns up in a Sanskrit text from the fifteenth century (Plofker, 1996; 2002); – if so, Indian reckoners knew what they were doing when applying the rule, and why.

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KARINE CHEMLA

THE INTERPLAY BETWEEN PROOF AND ALGORITHM IN 3RD CENTURY CHINA: THE OPERATION AS PRESCRIPTION OF COMPUTATION AND THE OPERATION AS ARGUMENT[0]

In the 1960s, historians of mathematics in China drew the attention of the international scholarly community to the fact that a Chinese text dating from the 3^{rd} century, in fact, contained mathematical proofs. Both their emphasis on the phenomenon and, in some respect, their way of analyzing it bore witness to the importance Western scholars attach to such facts.

As is well known, history of mathematics has regularly been used as a battlefield where nations, even civilizations, were competing and producing the evidence of their value. In this context, mathematical proof has played a dramatic part. As a last resort for some, it represents that by which the Western contribution to mathematics is deemed to be the most decisive. In some of my colleagues' opinion, it would be that which proves that only the West developed a speculative approach to mathematics.

It seems to me useful to recall this context, since it deeply influenced the way in which the proofs written by Liu Hui, our 3^{rd} century author, have regularly been analyzed. In the first place, they were compared with Greek geometrical texts of antiquity, or measured by a yardstick inspired by Aristotle's *Analytics*. This approach led to two opposite kinds of statement. Some scholars rejected the idea that these could be considered as proofs, since they did not emulate the axiomatico-deductive model: the fact that Liu Hui did not single out any axiom or definition ruined, for them, the contention that he proved anything. In opposition to the latter, others tried to elaborate ways in which one could consider some statements in Liu Hui's text as axioms, definitions and the like.

Despite the fact that they obtain opposite results, it seems to me important to stress that these two types of statement share the same basis. They all agree in taking a given practice of proof as an *a priori* norm, and they measure Chinese texts by this yardstick. I have argued elsewhere (Chemla, 1997) why I thought the procedure was questionable. Indeed, should history of proof be the history of proofs that mathematicians "should" have written, whatever the meaning of this "should" may be? In any case, if the question has to be: "Are there Greek proofs in Chinese texts?", we need not do research to guess that the answer will be: no! However, is this the question that should be asked? I do not think so. This has been the trick which blurred

P. Mancosu et al. (eds.), Visualization, Explanation and Reasoning Styles in Mathematics, 123-145.

discussion on the history of proof – a trick which, incidentally, had the property of making what some call the "Western superiority in these matters" indisputable.

History of proof, I would argue, has very much been dominated by certain *a priori* ideas, normative ideas especially, concerning what a proof should look like, how it should be constructed and why it should be carried out. Liu Hui's text shows how a mathematician in a given tradition dealt with the problem of establishing why a given mathematical statement is correct, in a way that differs from what we can find in Greek geometrical texts of antiquity. This raises a couple of questions that seem to me worth addressing. Why did Liu Hui get interested in this problem? What were his motivations for it? What was he expecting from the answer? Which procedures did he think were adequate to this end? Since the way in which he tackles the problem would seem satisfactory to a mathematician today, I do not hesitate to call what Liu Hui wrote a proof. And, rather than worrying whether this title is rightly deserved, I choose to describe this practice for itself and pay attention to the way in which it inserts itself into the heart of mathematical activity, in a given historical context.

My thesis, which I shall not be able to substantiate here,[1] is that Liu Hui's proofs attest to a practice of mathematical proof as carried out in ancient China, which is sophisticated and which was produced through a process of elaboration. This practice differs from what the known Greek texts of antiquity attest to, and, apparently, it developed independently from them historically. In other words, we may well have here the testimony of another origin for mathematical proof.

Let me make clear that, in putting forward such a thesis, my intention is by no means to enter the battlefield on China's side and with new weapons. I believe that such ideological questions prevent us from thinking about mathematics. However, we'd better be aware of them, rather than let them surreptitiously creep into our assumptions. What is at stake here lies at another level.

A detour through China could help us analyze our categories, in this case, that of proof, in a critical way. These texts may attest to the elaboration of functions for a proof other than establishing the truth of a statement. Examining these Chinese texts can thereby provide us with tools for inquiring into some of the contemporary functions imparted to proving. This, in turn, raises questions relating to how the contemporary practices of proof were historically shaped.

To sum up, we may expect from our inquiry into such Chinese sources to understand better the nature of the activity of proving in mathematics as well as the processes through which the related cultural practices took shape in history. With these questions in mind, in what follows, I shall concentrate on how Liu Hui dealt with the measure of a circle. Examining how he handled a proof will put us in a position from which to analyze what is proved and how it was proved[2].

1. ELEMENTS OF CONTEXT

Before proceeding to dealing with our questions, we need to sketch the context within which Liu Hui operated. In fact, our third-century author, whose proofs we are to analyze, is a commentator. It is hence for the sake of exegesis that the first known mathematical proofs were composed in ancient China. The book that brought about such developments had been composed around the beginning of the Common Era and was entitled *The nine chapters on mathematical procedures*[3] – a title that, in what follows, I abbreviate into *The nine chapters*. This book, which carried out a compilation of mathematical knowledge available at the time, was to become the Canon *par excellence* for mathematics. Most of the mathematicians who worked in China up until the beginning of the 14^{th} century and whose writings came down to us demonstrate a knowledge of it, or refer to it.

Roughly three centuries after its completion, Liu Hui commented on *The nine chapters*, and its commentary was to be selected by the tradition to be transmitted, together with the Canon. The simultaneous use of the two texts appears to have become so systematic that, today, there is no surviving edition of *The nine chapters* that does not contain Liu Hui's commentary. The formation of such writings, composed of a Canon and commentaries selected by the written tradition, is typical of Chinese history, where most of the disciplines experienced prominence being bestowed on texts of this kind. It is thus within the framework of a commentary that Liu Hui was led to deal with the measure of the circle.

Here is, more precisely, the local context within which he inserts the development we are interested in. Problem 31 of chapter 1 of *The nine chapters* reads as follows[4]:

"SUPPOSE ONE HAS A CIRCULAR FIELD, WITH A CIRCUMFERENCE OF 30 *BU* , AND A DIAMETER OF 10 *BU*. ONE ASKS HOW LARGE THE FIELD IS.

"ANSWER: 75 *BU*.

(...[5])

"PROCEDURE: HALF OF THE CIRCUMFERENCE AND HALF OF THE DIAMETER BEING MULTIPLIED ONE BY THE OTHER, ONE OBTAINS THE *BU* OF THE PRODUCT (*JI*)."

It is, hence, in reaction to this piece of text that Liu Hui produces what I call a proof. Two remarks should be made on this excerpt. First, *The nine chapters* yield a procedure to compute the area of the circle that is exact. If we denote by A, the area of the circle, C, the circumference, and D, the diameter, the procedure can be represented as follows:

$$A = \frac{C}{2} \cdot \frac{D}{2}$$

Secondly, however, the terms of the problem supply both the diameter and the circumference as if they were independent of each other and both were needed. The ratio between them is that of 1 to 3. Yet, immediately afterwards, the Canon offers two other procedures, each of which uses only one of the data, and none of which is exact:[6]

"ANOTHER PROCEDURE: THE DIAMETER BEING MULTIPLIED BY ITSELF, MULTIPLY BY 3 AND DIVIDE BY 4."

"ANOTHER PROCEDURE: THE CIRCUMFERENCE BEING MULTIPLIED BY ITSELF, DIVIDE BY 12."

We shall analyze, in what follows, how Liu Hui comments on this set of problems and procedures. Let us, for the moment, stress that the passage of *The nine chapters* quoted gives a faithful idea of how the Canon is composed. It is constituted of problems, answers and algorithms, i.e., as it appears, lists of operations that rely on the data provided by the terms of the problems to yield the unknown sought. We can, however, question this reading, as will become clear below.

In echo with the composition of the Canon, Liu Hui's proofs systematically tackle how to establish the correctness of algorithms. Thus we are taken to a world different from, e.g., Euclid's *Elements*, where proofs mainly aimed at establishing the truth of propositions. Let us enter into it.

2. SKETCH OF THE PROOF

It will be useful, for developing our analysis, to start by outlining the proof Liu Hui presents for establishing the correctness of the first algorithm mentioned above.

The opening remarks of his commentary are devoted to making clear that the ratio between the circumference and the diameter, as 3 to 1, i.e., that between the two data provided by the terms of the problems devoted to computing the area of the circle, in fact holds true for the regular hexagon inscribed in the circle. The introduction of the figure of the hexagon initiates a development that Liu Hui concludes by the following statement:

" Therefore/This is why (*gu*), when one multiplies the half-circumference by the half-diameter, that makes the area (*mi*) of the circle."

In other words, depending on how one interprets the first word of the final statement, the commentator himself conceives of his development as *establishing the correctness* of the algorithm, or *bringing to light the reason why* the algorithm stated by the Canon yields the correct answer. Whichever interpretation one prefers, it remains true that, in Liu Hui's eyes, his commentary on the algorithm relates to establishing its correctness. This constitutes an additional reason why it seems to me adequate to refer to it as a "proof". This statement shows that it is not only from our perspective that the commentary may contain proofs. It appears to have been one of its function in the actors' own perspective. Therefore we must analyze how Liu Hui argues to reach such a conclusion. Let us follow the course of this reasoning.

In a first step, relying on the figure of the regular hexagon inscribed in the circle, Liu Hui brings to light that an exact relationship links the diameter, the circumference of this polygon and the area of the regular 12-gon inscribed in the circle. This relationship can be represented as follows:

the half-diameter multiplied by the half-circumference of the hexagon

=

Area of the 12-gon

This relationship can be easily grasped in Figure 1. The commentator appears to conceive of the hexagon, as well as the n-gons introduced in what follows, as a collection of quarters of polygons cut in sectors of the circle and assembled around its center[7]. Consider, on figure 1.a, one quarter composing the hexagon, OBD, cut along the radius OC, which goes through A, the middle of BD. If one introduces the two corresponding quarters of the 12-gon, as reproduced on figure 1.b, figure 1.c makes clear that multiplying AB by OC yields the area of these two quarters. Multiplying this by 6 yields the relationship sought-for. In Liu Hui's terms, cutting the quarter OBD along OC yields two quarters of a regular 12-gon inscribed in the circle.

The repetition of the operation (see figure 2) leads to a similar relationship between the 12-gon and the 24-gon, as follows:

the half-diameter multiplied by the half-circumference of the 12-gon

=

Area of the 24-gon

At this point, Liu Hui has introduced two elements that will prove fundamental in his reasoning: a sequence of n-gons, produced through cutting quarters of polygons within the body of the circle; and a relationship linking

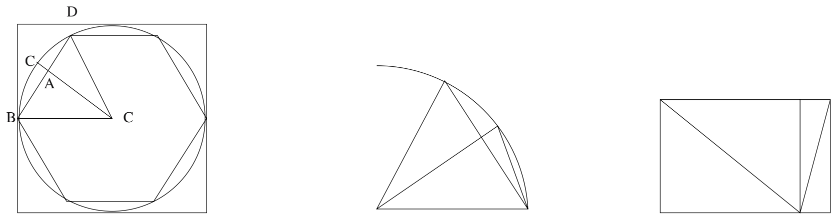

Fig. 1.a Fig. 1.b Fig. 1.c

FIGURE 1.

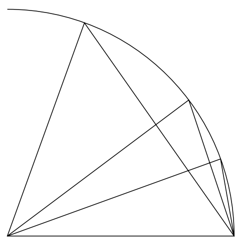
FIGURE 2.

the circumference of the n-gon, the area of the $2n$-gon and the half-diameter of the circle.

In what follows, he turns to considering these two elements separately, dealing first with the evolution of the relationship between the circle and the polygons generated by the successive cuts. He hence goes on:

"The finer one cuts, the smaller is that which is lost."

The quarters of the polygons yielded by the sequence of cuts, Liu Hui notices, are finer and finer. He then re-introduces the circle, through considering the evolution of the relation of the successive polygons to the circle. With respect to the unknown to be determined, i.e., the area of the circle, the area of the polygons formed, Liu Hui states, gets increasingly closer[8]. Having thus made explicit the relation of the figures first introduced to the problem considered, the commentator goes on:

"One cuts it and re-cuts it until one attains (*zhi*) what cannot be cut. Then its body (*ti*) makes but one (*he*) with the circumference of the circle, and there is nothing lost."

The statement consists in two parts, each corresponding to a member of the previous sentence.

The first part takes up again the cut introduced and prescribes to repeat it until "attaining what cannot be cut". What the commentator means exactly by this cannot be completely elucidated. Does he think of an actual infinity of steps? Or, does he prescribe to carry out the operation until the moment when our senses give us the quarters of the polygon as impossible to cut further? Or else, does he believe that magnitudes are composed of finite elementary constituents that can be reached after a given number of cuts? I have argued elsewhere in favor of the first of these interpretations, without being able to find evidence that would decisively rule out the other possibilities[9]. The main point, however, is that, according to Liu Hui's own terms, there is a moment when what cannot be cut any longer is *attained*. The term is not used by accident: the character expressing this nuance, *zhi,* occurs in two other contexts in which we would put into play an infinite number of steps[10].

Whichever interpretation one adopts, we find ourselves confronted with a direct reasoning, unlike the indirect arguments encountered in Euclid's or Archimedes' treatments of similar problems[11]. This feature evokes pre-Eudoxian fragments like Antiphon's, a point to which we shall come back.

Whatever the manner in which this attainment is realized, the second part of the sentence quoted formulates its consequences. First, the body of that which is produced is said to coincide with the circle, by virtue of the coincidence of their circumferences. Note that it is hence held to be different from the circle, but the contours "make but one". Secondly, from this, Liu Hui moves on to stating that the areas do not differ. The process would have thus yielded a polygon – the following sentences make clear that this is how the commentator conceives of the figure produced as a result of the process – the circumference and, hence, the area of which match those of the circle.

In what follows, Liu Hui offers an argument to establish this last statement. The first step consists in introducing a magnitude that will constitute the pivotal element in the reasoning: the so-called "diameter remaining". This expression refers to the part of the diameter that goes outside the n-gon, beyond the mid-point of one of its sides, which offers a kind of measure of the distance between the circumferences of the n-gon and the circle. On figure 1, it is measured by AC for the hexagon. The second step then introduces the sequence of rectangles, whose dimensions are respectively a side of an n-gon and the diameter remaining (see Figure 3). Their areas exceed that of the circular segments that represent the differences between the successive n-gons and the circle[12]. In other terms, the rectangles constitute an upper bound for "that which is lost". The point that will prove crucial is that their areas are expressed with respect to the so-called "diameter remaining".

Liu Hui has described the situation in general. In a third step, he focuses on the body produced at the point when one cannot cut the quarters any longer – this is where he still refers to it as "a polygon". Applying to

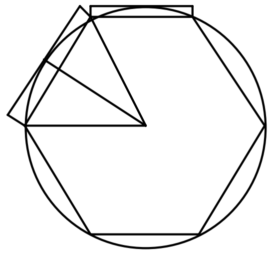

FIGURE 3.

it the previous device, he notices that the matching of the circumferences implies that the diameter remaining has vanished. As a consequence, the areas exceeding the difference between the areas of the n-gon and the circle, which depend on the diameter remaining, also vanish. Hence the equality of the areas of the "last" polygon and the circle.

This constitutes an essential feature in the structure of the reasoning, and one that distinguishes Liu Hui's argument from the pre-Eudoxian fragments evoked earlier, provided that we may judge them on the basis of the remaining evidence[13].

Liu Hui has thus exploited the evolution of the relation of the n-gons to the circle, by bringing to light a polygon whose body coincides with that of the circle and which shares the same area. In a final section of this part of his commentary, he turns to considering the transformation of the algorithm linking the circumference of the n-gon, the area of the $2n$-gon and the half-diameter of the circle, which he has introduced so far with respect to the hexagon and the dodecagon. His next point consists in highlighting the basic reason that grounds the correctness of this relationship for any n-gon. The central operation of multiplying a side of an n-gon by the half-diameter, he states, introduces two quarters of the $2n$-gon[14] and yields each of them twice. This amounts to stating that what figure 1 shows holds true with full generality.

Stating the relationship for the polygon yielded at the end of the process provides the algorithm that *The nine chapters* offered for computing the area of the circle. The circumference of the polygon fuses into the circumference of the circle. Multiplied by the half-diameter, it yields the area of the polygon, equal to that of the circle. Hence the conclusion, which completes Liu Hui's proof of the correctness of the algorithm provided by the Canon for the area of the circle.

3. FIRST REMARKS ON THE PROOF

Let us highlight some features of the proof just sketched.

It is interesting, in the first place, to notice that the proof proceeds through exact relationships, holding for a sequence of polygons. The final part of the commentary stands in contrast to this, since Liu Hui makes use of inequalities – the areas of the sets of rectangles constitute upper bounds of the areas left over, in which he is interested, and this is what makes the argument work. However, this last feature occurs seldom in Liu Hui's proofs, and this distinguishes them from, say, the arguments in Euclid's *Elements*, which mainly bring inequalities into play.

A second feature is worth mentioning. The proof actually embeds the circle in the set of all inscribed regular polygons, and it brings to light a common algorithm computing the areas of all these figures. The proof thus connects various realities, and it is through this extension that the reason for the correctness of the algorithm examined appears. This correlation between establishing the correctness of an algorithm and bringing to light more general operations underlying it is not an accident. On the contrary, it constitutes a characteristic feature of Liu Hui's proofs throughout his commentary. We can observe here how it manifests itself within the context of geometry[15]. In fact, this feature can be correlated with more general statements on mathematics made by the commentator[16]. In brief, he conceives of his commentary on a procedure as bringing to light its "source" (*yuan*), which, for him, appears to constitute the level at which it can be extended (*shi*) to deal with other categories (*lei*) of problems. Hence the connection between proving and bringing to light more general operations.

If we go back to the case of the circle and to the way in which the correctness of the algorithm is established, we notice that, once the algorithm has been described for the hexagon, then for the dodecagon, the proof considers the evolution of its terms – the circumference of the n-gon and the area of the $2n$-gon – and of the relationship between them, through considering the transformations of the underlying polygons. It thus brings to light *how* the algorithm for the circle is in continuity with those for the polygons. The proof shows by way of which variation the circle can be embedded in the set of all inscribed regular polygons.

A third point should be stressed. As already mentioned, the proof prescribes carrying out an operation until reaching the point at which it cannot be performed any longer. Such is the case also in the similar reasoning by which Liu Hui establishes the correctness of the algorithm given for computing the volume of the pyramid. The presence, in both cases, of this stage in which the decrease of the remainder is assessed – which we emphasized above – underlines the character of necessity that, in Liu Hui's view, it probably bore. The comparison of both reasonings shows that we are confronted here, not with an argument *ad hoc*, but with a stable mode of reasoning. This evokes the way in which the method of exhaustion manifests itself in Greek geometrical texts of antiquity[17]. As a matter of fact, the mathematical ideas used by Liu Hui and Euclid in the case of, both, the circle and the pyramid, are the same. The stable differences in their way of bringing these ideas into play in their proofs are all the more interesting and seem to refer to a stable difference in the practice of proof. However, I would not like to dwell here on such questions of comparison, all too frequently addressed. Nor am I going to ask whether Liu Hui's commentary outlined above offers a "real" proof, since, as I suggested, here lies the trap ready to open under the feet of the historian. Let us, instead go deeper in our analysis of the Chinese text.

4. THE OPERATION AS RELATION OF TRANSFORMATION

To this end, the first question I suggest to raise is very simple: what has Liu Hui proved? The answer seems to be very simple. We find it in Liu Hui's conclusion:

"Therefore/This is why (*gu*), when one multiplies the half-circumference by the half-diameter, that makes the area (*mi*) of the circle."

But, immediately after, Liu Hui adds a remark concerning the terms of "circumference" and "diameter" entering this statement:

"Here, by circumference and diameter, we designate the quantities that *attain (zhi*) what is so, what the *lü*'s of 3 for the circumference and 1 for the diameter are not." (My emphasis)

We need to sketch the meaning of the concept of *lü*, which appears in the statement, before commenting on it. Introduced in *The nine chapters* within the context of the rule of three, *lü* designates numbers that are defined only relatively to each other. For example, the numbers expressing a ratio between entities are referred to as *lü*. This is the case in the sentence quoted above. However, the extension of the concept goes beyond this case[18].

This implies that, for Liu Hui, the algorithm, the correctness of which was just established, bears on quantities different from those to be found in the terms of the problem of the Canon after which the algorithm is stated.

This remark extends even further. Elsewhere, Liu Hui speaks of the ratio between the diameter and the circumference of the circle as not possibly exactly expressible[19]. If the terms of the algorithm proved to be correct are "the quantities that *attain* what is so", they cannot be simultaneously expressed by actual values. As a consequence, we discover that Liu Hui has proved the correctness of an algorithm that, in his view, can lead to no computation. This algorithm expresses a relationship of transformation, or of production, between magnitudes, but cannot receive actual values for all its terms. This entails that, here, *the "algorithm" must be distinguished from the "prescription of a computation*". This conclusion incidentally highlights why the stand according to which mathematics in ancient China was only practical is indefensible.

If we now look up again at the Canon, we see that, there, the algorithm means, perhaps not only, but *also* computations, since an answer is provided for the problem. The procedure is what yields the value of the area of the circle, on the basis of the two data given for the circumference and the diameter in the terms of the problem. This aspect of the algorithm has not yet been addressed by the commentator, and Liu Hui will consider it in a second part of his commentary on the measure of the circle.

Before turning to this other aspect of his commentary, let us draw some general conclusions from what was just observed.

Such a case leads us to distinguishing the algorithm as producing a magnitude, expressed in terms of the situation of a given problem – in our case, the area of a circle –, from the algorithm as producing a value. We are to distinguish the algorithm as expressing a relation from the algorithm as prescribing a computation. In other terms, we are to dissociate the algorithm viewed from a *semantical* point of view and the algorithm considered from a *numerical* perspective. The same conclusions could derive from examining other parts of Liu Hui's commentary, the difference being that the field with respect to which the interpretation of the result of the operations is expressed is not always geometrical[20].

The two aspects of an algorithm can run in parallel. But there are cases when a discrepancy appears between the two, as is the case here. This situation results in having Liu Hui comment on the algorithm in two sections. He first deals with the algorithm semantically, establishing the correctness of the relation of transformation. It is only in a second section that he comments on the algorithm as pure computation, relating to the context of an actual problem, such as what can be found in the Canon.

What was first proved was thus the relation of transformation. But how was it proved?

If we look again at the proof, we discover retrospectively that it makes use of algorithms, as relations of transformation too, with no computations.

In fact, some of these algorithms are exact geometrically, semantically. It is as such that they are involved in the proof. But they can lead to no computation that would be exact from a numerical point of view: this is the case for the computation of the circumference of the sequence of polygons. If we start from a circle with 2 *chi* of diameter, as is suggested at the beginning of the commentary, attempts to yield the numerical values of the sides of the *n*-gons would soon lead us to introduce kinds of quantities that go beyond those considered by mathematicians in ancient China[21]. But, in fact, nothing is done in this part of the text to turn them into actual computations.

For other operations that are introduced for the sake of the proof, the point is in the value of the *algorithm as argument*, not in the computation it would prescribe. This aspect is clear with respect to the so-called "diameter remaining". It plays a crucial role in linking, through algorithms, the convergence of the areas to that of the circumferences of the *n*-gons. But its actual value does not matter. The important point is that an algorithm expresses how the convergence of areas towards the area of the circle depends on the nature of the evolution of the "diameter remaining", while the circumference of the *n*-gons approaches that of the circle. This algorithm constitutes what I would call an "operation-argument".

This analysis hence reveals a whole world of such algorithms, as relations of transformation, independently from algorithms as computations. The proof examined shows how they are articulated to one another, producing one another.

So much for now. Let us at this point turn to the second part of Liu Hui's commentary, in which he tackles the algorithm from a numerical point of view. And let us consider how it also reveals another characteristic of the practice of proof as carried out in ancient China.

5. THE ESSENTIAL LINK BETWEEN PROOF AND ALGORITHM

The point I want to make on the basis of the second part of Liu Hui's commentary consists in showing that, as his practice of proof highlights, proofs are not closed onto themselves. They do not only constitute an aim in themselves, as would be the case if they were understood as merely establishing the correctness of algorithms. On the contrary, they can also serve, for instance, as the basis for elaborating new algorithms. With this purpose in mind, let us follow how Liu Hui deals with the situation numerically[22].

To this end, the commentator puts forward an algorithm, the aim of which is to yield more precise values for the relationship between the circumference and the diameter. At each step, this algorithm makes the meaning of the computations explicit. Therefore, in the end, it is clear that the values produced are approximations, for what they are approximations and which kind of approximation they represent.

In fact, this algorithm appears to be derived from the proof outlined in the first part. Let us observe *how* this algorithm precisely relies on the operations of the previous demonstration.

The "procedure of the right-angled triangle (*gougushu*)", which, in the realm of algorithms, corresponds to the so-called Pythagorean theorem, is the object of the ninth of *The nine chapters*. Liu Hui brings it into play for transforming the previous geometrical argument into an algorithm.

Let us first sum up the main points of the text, before stressing other features of the practice of proof in ancient China.

The algorithm that Liu Hui now starts describing relies on an iteration. With respect to figure 1.a, we can summarize the procedure to be repeated as follows: in the right-angled triangle OAB, the hypotenuse OB is the half-diameter, and the base AB is half the side of the n-gon. Applying the "procedure of the right-angled triangle (*gougushu*)" yields the height OA. Furthermore, in the right-angled triangle ABC, the base is the difference between the half-diameter and OA, the height is half the side of the n-gon. Applying the "procedure of the right-angled triangle (*gougushu*)" yields CB, the corresponding hypotenuse, which turns out to be the side of the $2n$-gon. Here is how Liu Hui formulates the first application of this sub-procedure, to be thereafter iterated:

"Procedure consisting in cutting the hexagon in order to make a dodecagon:

Set up the diameter of the circle, 2 *chi*. Divide it by 2, that makes 1 *chi* and gives the side of the hexagon that is in the circle. Take half of the diameter, 1 *chi*, as hypotenuse, half of the side, 5 *cun*[23], as base, and look for the corresponding height. The square of the base, 25 *cun*, being subtracted from the square of the hypotenuse, there remains 75 *cun*. Extract the square root, descending to the *miao*, to the *hu*, then retrograde the divisor one more time[24], in order to find a digit from the decimal part (of the root). One takes as numerator the digit from the decimal part that has no name, and one takes 10 as denominator. By simplifying that makes two-fifths of *hu*. Consequently, one obtains 8 *cun* 6 *fen* 6 *li* 2 *miao* 5 and three-fifths *hu* for the height. Subtract this from the half-diameter, 1 *cun* 3 *fen* 3 *li* 9 *hao* 7 *miao* 4 and three-fifths *hu* remains, that one calls small base. Half of the polygon side then is called once again small height. Look for the corresponding hypotenuse. Its square is 267949193445 *hu*, the remaining fraction being abandoned. Extract the square root, that gives a side of the dodecagon."

This subprocedure is repeated, and, in the course of the first iterations, there is no computation carried out for determining either the area or the circumference of the successive n-gons. It is when he reaches the 48-gon that Liu Hui first actually computes the side of the 48-gon. Moreover, he uses the relation brought up during the previous demonstration to deduce from it the area of the 96-gon, obtaining 313 and 584/625 *cun*.

Again in the next iteration of the subprocedure, Liu Hui computes similarly the side of the 96-gon, and then the area of the 192-gon, obtaining 314 and 64/625 *cun*.

Having obtained values that are smaller than the area of the circle[25], Liu Hui turns to computing an upper bound for the value of the area of the circle, by bringing into play the same rectangles as those used for the proof (see Figure 3). The areas of the set of rectangles covering the segments of circle left over by the regular 96-gon inscribed in the circle is, however, computed with a new insight. The difference between the area of the 96-gon and that of the 192-gon is doubled, which yields the value sought-for. It is then added to the area of the 96-gon, providing the value of 314 and 169/625 *cun* as upper bound for the area of the circle. Since the lower and upper bounds found share the same integral part of 314 *cun*, it is kept to represent the *lü* of the area of the circle, with respect to the *lü* 400 for the square of the diameter. Thereby, new approximate values are offered for the area and circumference of the circle, in a way that clearly indicates their nature.

This sketch of the algorithm enables us to examine further some interesting features of the text. Notice, first, that the same figures as previously, based on the hexagon, are considered, and that the same central ideas are used. However, they are brought into play in different ways. The most striking example of this difference relates to the computation of the areas of the exceeding rectangles. When their areas were considered within the proof, they were computed so as to highlight their dependency with respect to the diameter remaining. However, when the operation-argument becomes the operation-computation, these areas are computed in another way. Yet the value of the diameter remaining (AC) is determined at each stage. This difference between the two contexts sheds more light on the essential part played by the circumference in the first part of the commentary we examined. It also highlights the process through which the proof is transformed into an algorithm.

Furthermore, with the example of this algorithm, we are in a position to observe another modality of the relation linking the proof and the algorithm. As already alluded to, the algorithm proceeds along parallel lines. It prescribes computations that are to be numerically performed. In addition, the *meaning* of the result is always made explicit in terms of the geometry of the situation. Look at, for instance, the concluding proposition of the passage quoted above: "..., that gives a side of the dodecagon".

More generally, the various algorithms corresponding to the Pythagorean theorem produce both a meaning and a result. In the latter example, one of these algorithms determines the interpretation of what is yielded as "hypotenuse", which is exact. It also produces a numerical value, which approximates that of the hypotenuse with an accuracy that is explicitly provided.

All in all, the algorithm aims at yielding actual numerical values. It can, in this sense, be compared to the algorithm as used by *The nine chapters* to determine a value for the area. However, at the same time, the algorithm shapes a semantical interpretation for each result, and is to be compared, in this sense, to the proof examined above, in the first part of the commentary.

The fact that *this algorithm also has this argumentative component* here can easily be deduced from the fact that some of the computations are stated *only* for the sake of the reasoning, but are not executed. This is for instance the case for the last computation of the paragraph quoted above, which gives a side of the dodecagon. The computation is prescribed, but not carried out. Instead, the square of the result is kept, since it is that which will be used at the beginning of the next sub-procedure, where the square of the half-side is needed.

This leads to another range of remark. In fact, one can prove that the argumentative function of the algorithm has prominence over the computational dimension. The first application of the subprocedure theoretically yielded a side of the dodecagon. This side was to be halved, and its half squared, to start the next application of the sub-procedure. Instead, the square is directly divided by 4. Rewriting the sequence of operations "searching the square root, halving and squaring" as "dividing by 4" is absolutely correct, at the algebraic level. This is also correct from a numerical point of view, if the result of the square root is given as "square root of N", when needed. This is, according to Liu Hui, the reason why quadratic irrationals were introduced in *The nine chapters*: he relates them to the requirement that squaring the result of a square root extraction should restore the number with which one started[26]. However, here, the results are given in an approximate way, and rounded off. This implies that applying the sequence of operations "searching the square root, halving and squaring" might *not* yield the same numerical result as "dividing by 4". Here, we have a point where the algorithm provided for shaping the interpretation of the result and proving the correctness of the computation diverges from the algorithm used for computing. However, they are stated in parallel. At the level of pure operations, extracting the square root and squaring, as relations of transformation, are useful for making sense of the flow of computation, but, in fact, they cancel each other. They do not at the level of the operations as carried out by Liu

Hui here. This remark reveals that the proof requires an algorithm that gives the meaning of the values computed. This algorithm is rewritten, at the level of pure operations, to yield the algorithm actually used for the computations. But we have clear evidence, here, that it is the level of the proof that guides the numerical execution of the algorithm.

In his commentary on the introduction of quadratic irrationals, Liu Hui compared them to fractions in that they allow cancelling the sequence of two inverse operations. This implies that the results of the two types of algorithm, that for proof and that for computation, remain identical. This property is put into play in a kind of "algebraic proof in an algorithmic context", as I call it[27]. It involves taking algorithms as lists of operations and rewriting them as such, without prescribing computations. This relates to what we just saw, regarding rewriting the algorithm. However, this also evokes the operations as practiced in the first part of the commentary, where Liu Hui addresses proving the correctness of the algorithm.

However, quadratic irrationals are not introduced in relation to the circle here. This results in having, right from the outset, a divergence between operations as relations of transformation and operations as prescriptions for computation.

If we go back to the algorithm analyzed, it is interesting to note that, in parallel to the fact that we had pure operations in the proof, we now discover argumentative operations in the algorithm. This reveals that this algorithm prescribes computations to produce values, *at the same time* as it produces the reasons for its correctness. *Proof is not to be expected to be always a text distinct from the text of what is proved.* Here *algorithm and proof have merged into a unique text*.

This text fulfils this double function simultaneously by making use of the double face of an operation, carrying it out both semantically and numerically.

This double face of an operation is reflected in two other features of Liu Hui's commentary. First, it corresponds, as we saw above, to the split of the commentary in two parts here. Secondly, it can be correlated to a specificity in the set of mathematical concepts to be found in the commentaries. In fact, the commentators make use of two concepts of area. *Ji* refers to the area as the number produced by the computation, which can be linked to the operation as numerical prescription. In contrast to it, *mi*, which is to be found only in the commentaries and not in *The nine chapters*, refers to the area as the spatial extension corresponding to the multiplication between two magnitudes. This concept may relate to the face of the operation as relation of transformation.

In the same vein, it is important to stress the part played by the problems for making possible the interpretation of the successive steps of an algorithm[28]. If we go back to the example mentioned above, asserting that a sub-procedure of the algorithm described by Liu Hui yields the hypotenuse of a right-angled triangle requires having identified that the terms to which it is applied are the base and the height of such a triangle. On this basis, one can recognize the problem, for the solution of which an algorithm has been shown to correctly yield the hypotenuse. As for any problem contained in the Canon, not only do we have the situation to which the subprocedure can be applied, but we also have numerical values for each of the term. Applying the algorithm yields both a value and a meaning for the number obtained, which are exactly the two tracks along which Liu Hui's text develops. In this way, problems are building blocks to write down a proof, in that they offer a field of interpretation with which to make explicit the meaning of the result of an operation.

One can interpret along the same lines the way in which, in the first part of the commentary, the meaning of the multiplication of the circumference of the n-gon by the half-diameter of the circle is brought to light. Liu Hui introduces a figure, that of the $2n$-gon, the area of which corresponds to the result obtained. It is the situation bringing together the circumference of the n-gon, the area of the $2n$-gon and the half-diameter of the circle that is rich enough to yield the interpretation of the operation. Interestingly enough, Liu Hui uses the same term *yi* to designate the meaning of an operation in both cases: when it is expressed in terms of the situation described in the terms of a problem and when its explicitation is made possible thanks to the introduction of visual auxiliaries[29].

6. CONCLUSION

At this point, let me gather the various threads that were followed, while attempting to describe this practice of proof for itself and observing how it was embedded in mathematical activity taken as a whole.

We stressed the fact that Liu Hui's commentaries bore on algorithms. Sometimes, they establish the validity of a relation of transformation, as in our first case. Sometimes, they establish that a value obtained is indeed the one sought-for, or in which ways it can stand for it, as in our second case.

In any case, Liu Hui's proofs present stable modes of reasoning.

In contrast to what we would expect if we took for granted that the sole aim for proving is to convince of the truth of a statement, we saw that proofs can serve as a basis for the production of new algorithms.

In fact, we met with two examples of this fact.

First, the algorithm constituting the second part of the commentary analyzed was produced on the basis of the proof delivered in the first part, and we observed modalities of the transformation of proof into algorithm.

Secondly, the proof of the relation yielding the area of the circle proceeded through an extension of the algorithm to be proved to a whole set of figures, thereby bringing to light the reason for its correctness as well as the connection of the circle to these other figures.

This suggests that mathematicians had other reasons to get interested in the question of knowing whether a statement was correct, besides establishing its correctness.

Obsessed as we have been by this latter function of a proof, haven't we failed to describe the general part played by proof in mathematics and its various articulations to other moments of mathematical activity? This failure seems to me to have had a lasting impact on the history of proof.

In any case, Chinese mathematicians like Liu Hui might have been interested in the correctness of algorithms for the mathematical productivity of the question or for the understanding it provides of that which has been proved.

Neither with respect to their nature, nor even with respect to the texts that give expression to them, did we observe a clear-cut opposition between what was proved and what proved it. The reason behind this is that proofs are constituted of operations – equalities, as we stress, and not inequalities. And, in order to describe the relationship between algorithm and proof in a more precise way, we were led to oppose operation-argument to operation-computation, with respect to the form of the operation. In another perspective, we opposed operation-relation of transformation to operation-prescription of computation, as regarded their nature.

On such a basis, the enunciation of an algorithm and the writing of a proof could interact in various ways with each other, a conclusion also supported by the analysis of other parts of Liu Hui's commentary[30]. The proofs thus open onto the production of new algorithms, whereas the algorithms can go along with a proof.

These Chinese authors experienced it: there is no antagonism between computation and reasoning. This remark sounds obvious to us, whose proofs proceed so often through computation, in contrast to what Euclid did. How did that happen? What are the consequences for the activity of proving? Liu Hui's text incites me to raise these questions. Perhaps it can help us answer them both conceptually and historically. Perhaps history of proof, too, will display a non-linear pattern.

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APPENDIX

"Commentary: half of the circumference makes the length and half of the diameter makes the width; consequently the width and the length being multiplied one by the other, this makes the *bu* of the product (*ji*).

"Let us suppose that the diameter of the circle is 2 *chi*; the values (*shu*) of a side of the hexagon inscribed in the circle and half of the circle's diameter are equal. Corresponding to the *lü* of diameter 1, the *lü* of the polygon's circumference is 3.

"Once again, relying upon the drawing, multiplying the half-diameter for a segment by half a side of the hexagon, that makes two pieces (*er*) of it and, multiplying this by six, one obtains the area (*mi*) of the dodecagon.

"If once again one cuts it, multiplying the half-diameter for a segment by a side of the dodecagon, that makes four pieces (*si*) of it and, multiplying this by 6, then one obtains the area (*mi*) of the 24-gon.

"The finer one cuts, the smaller is that which is lost.

"One cuts it and re-cuts it until one attains (*zhi*) what cannot be cut.

"Then its body (*ti*) makes but one (*he*) with the circumference of the circle, and there is nothing lost.

"If to the exterior of the sides of the polygon, there is still some diameter remaining, when one multiplies the remaining diameter by the sides, then the area (*mi*) extends to the exterior of the circular segments.

"In case this polygon attains a degree of fineness[31] such that its body (*ti*) coincides with the circle, then there is no diameter remaining to the exterior. If there is no diameter remaining to the exterior, then the area does not extend outside.

"When, with one side, one multiplies the half-diameter, this amounts to cutting the quarter of the polygon (*gu*) and each piece is obtained twice.

"Therefore/This is why (*gu*), when one multiplies the half-circumference by the half-diameter, that makes the area (*mi*) of the circle."

NOTES

[0]This paper was completed, while I was spending a week at the Fondation des Treilles, Tourtour. It is my pleasure to thank this institution for its hospitality. I am grateful to John McCleary for his help in the process of polishing the English.

[1]See (Chemla and Guo Shuchun, n.d.), especially chapter A.

[2]In the appendix, I give the translation of my critical edition of the proof by which Liu Hui establishes the correctness of an algorithm yielding the area of the circle. In Chemla (1996), I have discussed this passage of Liu Hui's writings extensively, especially regarding the philological problems that it raises. Here, I concentrate only on discussing questions relating to proof. The reader interested in the argumentation for establishing the critical edition, on which my analysis here is based, is referred to this former publication. There, he or she can find a more detailed discussion of the various received versions. This passage of Liu Hui's commentary has been previously discussed by several scholars, among whom: (Chen Liang-ts'o, 1986), (Guo Shuchun, 1983), (Lam Lay Yong and Ang Tian-Se, 1986), (Liu Dun, 1985), (Volkov, 1994).

[3](Chemla and Guo Shuchun, n.d.) provides a critical edition and a French translation of this book and the earliest extant commentaries. I am glad to acknowledge my debt towards Professor Guo Shuchun, with whom, since 1984, I have discussed each character of this text. This book also contains a glossary discussing the mathematical and philosophical terms of both *The nine chapters* and the commentaries, which I established. I shall refer below to it, as *Glossary*.

[4]I use capital letters for the text of the Canon, in opposition to lower case letters for the commentary.

[5]Here we skip the statement of a second problem, similar to the first one.

[6](Chemla, 1996) offers an interpretation of these facts. I refer the reader to it.

[7]The 7^{th} century commentator concretely describes how to produce the figure of the 6-gon by assembling 6 triangles around the center of the circle, see (Chemla and Guo Shuchun, n.d.). This is in agreement with the fact that the geometrical figures to which the commentators refer seem to have been material objects, the spatial extension of which appears to be their foremost feature. See (Chemla, 2001) and see *gu* "quarter of a polygon" in the *Glossary*.

[8]Interpreting in this way Liu Hui's statement is in agreement with several features underlined above: the polygon consists in a set of quarters; cutting the quarters of the n-gon yields the $2n$-gon; the first element attached to a geometrical figure in ancient China is its area.

[9]See (Chemla, 1996). (Volkov, 1994) offers a completely different interpretation, based on a numerical interpretation of the whole passage. In my view, the way in which he suggests to link the two parts of the commentary (for the second part, see the end of this paper) requires further examination.

[10]See the commentary on the area of the circular segment, after problem 36 of chapter 1, and the commentary of the volume of the pyramid, after problem 15 of chapter 5.

[11](Chemla, 1992, 1996) touch the comparison between these reasonings from different angles.

[12]The term *mi* used to designate their areas conveys both the idea of geometrical extension and measure, see *Glossary*. Multiplying the two lengths to produce such an area is an usual way of introducing a rectangle, through its length and width. One can find another such example at the beginning of the passage translated.

[13](Chemla, 1992) shows that this step characterizes all such reasonings by Liu Hui, and (Chemla, 1996) compares them to pre-Eudoxian fragments in a more detailed way.

[14]This sentence justifies our interpretation that "cutting a quarter of a n-gon" is meant to refer to the production of two pieces of the $2n$-gon.

[15](Chemla, 1992) discusses other manifestations of this feature in geometry. (Chemla, 1997) shows its relevance for interpreting the proofs in other cases, and discusses the correlations in terms of textual characteristics of Liu Hui's commentary.

[16]I have a paper in preparation on this topic, " Une conception du fondement des mathématiques chez les commentateurs chinois (1[er] au 13[e] siècle) des *Neuf chapitres sur les procédures mathématiques* ", presented at the Conference "Fondements des mathématiques", Nancy, September 2002. An abstract can be found in http://www.univnancy2.fr/ACERHP/colloques/symp02/PreliminaryProgram.htm

[17]This is the main point made in (Chemla, 1992).

[18]See (Li Jimin, 1982), (Guo Shuchun, 1984) and my *Glossary*, entry *lü*.

[19]See the discussion of this passage in (Chemla and Keller, 2002).

[20]Chapter A, in (Chemla and Guo Shuchun, n.d.), summarizes the argument that problems in ancient China offered fields of interpretation for the operations of the algorithm following them. More on this below.

[21](Chemla and Keller, 2002) discuss the quadratic irrationals introduced in ancient China and India, but, if we wanted to carry out the computations exactly, the iteration would soon break this framework and require the introduction of more complex quantities. In the second part of his commentary, devoted to computations, Liu Hui deals rather with decimal approximations of the quantities. See below.

[22](Chemla, 1996) deals with this part of the commentary in a more detailed way. I restrict myself here only to the points relating to the analysis of specific features of the practice of proof.

[23]10 *cun* = 1 *chi*. Other units appearing below in the text form a decimal sequence.

[24]This is a reference to the algorithm for extracting square roots as described in *The nine chapters*. The root is yielded digit by digit. Here, when one reaches the last unit available, the algorithm is carried out a last time, yielding a digit that is taken as numerator corresponding to the denominator 10. Concerning this aspect of the algorithm, see the corresponding introduction in (Chemla and Guo Shuchun, n.d.).

[25]In fact, the values are only smaller than the circle in the interpretation provided. They are interpreted to represent the areas of inscribed n-gons and their circumferences. As regards the actual values, Liu Hui seems to lose the control of the approximation. (Volkov, 1994) examines the conduct of the computation with great care.

[26]On this point, see (Chemla, 1997/98).

[27]For details about this, see (Chemla, 1997/98).

[28]This point is developed in (Chemla, 1997a) and (Chemla, 2002).

[29]See Chapter A, in (Chemla and Guo Shuchun, n.d.) and *yi* "meaning", in the *Glossary.* In a forthcoming paper, I shall address the way in which visual auxiliaries are used for determining the "meaning" of some algorithms.

[30]See for example (Chemla, 1997) and (Chemla, 1997/98).

[31]I.e., in case the quarters of the n-gon are the finest possible, those obtained at the point when one reaches "what cannot be cut".

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JAMIE TAPPENDEN

PROOF STYLE AND UNDERSTANDING IN MATHEMATICS I: VISUALIZATION, UNIFICATION AND AXIOM CHOICE[0]

To the memory of Heda Segvic

1. INTRODUCTION – A “NEW RIDDLE” OF DEDUCTION

Mathematical investigation, when done well, can confer understanding. This bare observation shouldn’t be controversial; where obstacles appear is rather in the effort to engage this observation with epistemology. The complexity of the issue of course precludes addressing it *tout court* in one paper, and I’ll just be laying some early foundations here. To this end I’ll narrow the field in two ways. First, I’ll address a specific account of explanation and understanding that applies naturally to mathematical reasoning: the view proposed by Philip Kitcher and Michael Friedman of explanation or understanding as involving the *unification* of theories that had antecedently appeared heterogeneous. For the second narrowing, I’ll take up one specific feature (among many) of theories and their basic concepts that is sometimes taken to make the theories and concepts preferred: in some fields, for some problems, what is counted as understanding a problem may involve finding a way to represent the problem so that it (or some aspect of it) can be *visualized*. The final section develops a case study which exemplifies the way that this consideration – the potential for visualizability – can rationally inform decisions as to what the proper framework and axioms should be.

The discussion of unification (in sections 3 and 4) leads to a mathematical analogue of Goodman’s problem of identifying a principled basis for distinguishing grue and green. Just as there is a philosophical issue about how we arrive at the predicates we should use when making empirical predictions, so too there is an issue about what properties best support many kinds of mathematical reasoning that are especially valuable to us. The issue becomes pressing via an examination of some physical and mathematical cases that make it seem unlikely that treatments of unification can be as straightforward as the philosophical literature has hoped. Though unification accounts have a grain of truth (since a phenomenon (or cluster of phenomena) called “unification” *is* in fact important in many cases) we are far from an analysis of what “unification” is. In particular, the degree of unification cannot be usefully taken to turn upon simple syntactic criteria such as counting axioms or argument patterns. I’ll argue that existing unification – based accounts need to be supplemented by an account of qualitative distinctions between

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homogeneous and heterogeneous theories, between "natural" and "artificial" predicates. I'll argue further that in both mathematical and broader scientific practice, rational distinctions between more and less natural properties are made systematically. The considerations brought to bear to rationalize these distinctions are typically more complex and varied than philosophical accounts tend to recognize. But though the principles we rely on to distinguish "natural" from "artificial" categories are varied and case-specific, they are no less rational for all that.[1] I'll emphasize one particular consideration among those that are important in practice: the "natural formulation" of a problem is typically expected to be *fruitful* (to generate further discoveries, "make things easier" and so on).[2] The later parts of the paper take up a specific case: one among the considerations that is sometimes adduced in practice as supporting the fruitfulness of a formulation is that it allows crucial problems or objects to be *visualized*.

The role of visual reasoning, as represented by Artin's *Geometric Algebra*, is complicated in some cases where mathematical axioms and basic categories are chosen. This section is meant also to serve a particular dialectical function. Considerations like "fruitfulness" and especially "visualizability" are sometimes classed as "merely subjective" or "merely psychological" or "merely pragmatic", and therefore not of genuine epistemological significance. This dismissive position can gain an initial force because, of course, some cases where diagrams are preferred, or where "fruitful" techniques are recommended, *are* epistemologically uninteresting. One point the concluding case study is meant to illustrate is that the most interesting cases are simply too systematic and far-reaching to be shrugged off in this way. "Visualizability" is a more intricate and more methodologically interesting theoretical property than it seems on a cursory analysis. A similar point holds for the more general case of "fruitfulness": on examination it turns out to be so extensively and systematically embedded in our theoretical practices that to dismiss it on the grounds that it is "psychological" / "subjective" / "pragmatic" is tatamount to a global skepticism of the "why should our best theories be true?" variety. (Since this is everyone's problem, it is not specifically a problem for the student of visualization or fruitfulness.)

The first section is devoted to spelling out why I think these questions are urgent for the student of methodology and why I am approaching it in just the way I am. First I'll note a key historical motivation that will otherwise serve as an unarticulated background point of reference: the mathematical revolution initiated by Riemann in the nineteenth century. The rest of the section discusses some of the issues the Riemann's revolution in mathematical style raises, to bring out how systematically embedded in mathematical practice these methodological issues are.

2. UNDERSTANDING AND EXPLANATION IN MATHEMATICAL METHODOLOGY: THE TARGET

2.1. Riemann vs Weierstrass: a classic opposition

Toward the end of the nineteenth century, developing into the twentieth, and in some ways continuing to the present, a division emerged in the proof methods in complex analysis that raises compelling questions for mathematical methodology. One approach, whose driving force was Weierstrass, was broadly computational in its outlook: it aimed at finding explicit representations of functions and explicit algorithms to compute their values. The other, initiated by Riemann, has appropriately been called "conceptual": it aimed to describe functions in terms of general properties, and to prove indirect function existence results that need not be tied to explicit representations.[3] The ramifications of this split were (and remain) far-reaching. In the words of the only philosopher to have discussed this development in print: "Mathematics underwent, in the nineteenth century, a transformation so profound that it is not too much to call it a second birth of the subject – its first having occurred among the ancient Greeks..." (Stein (1988)) There were differences in what proof techniques were counted as acceptable, in what connections to physical applications were available, in which generalizations were natural, and even in what definitions were accepted for the basic objects of study.[4] The opposition persisted through several other changes in the mathematical scene and in some respects continues to this day.[5]

This work defined the mathematics of the late nineteenth century and onward. It was so central to the development of contemporary mathematics that it is a challenge to the philosopher, as an analyst of mathematical method, to give an account of what was at stake.[6] This prompts a second challenge: once it is granted that these developments are of philosophical importance, it requires work to find a philosophical niche to put them in.

The historical details and philosophical overtones in the development of complex analysis are too complicated to be addressed in a single paper; this paper is one installment in a divide-and-conquer strategy of slicing the problem into parts. This installment will deal with the questions induced by the following observations: A) among the points of separation between the Riemann and Weierstrass approaches are a cluster of considerations that could reasonably be described as constituting different ways of *understanding* the subject–matter of complex analysis.[7] B) Among the motives for the

Riemann style is that it proved, and has continued to prove, *fruitful* both in facilitating the solution of given problems and in unearthing new important problems. C) The fact that Riemann surfaces (the basic context for analysis in the Riemann style) allow complex functions to be easily *visualized* was, and remains, a contributor to the fruitfulness of the Riemann approach.[8] To keep the discussion relatively simple and independent of specialized background, I'll set the motivating case of complex analysis aside, and address the phenomenon of styles of understanding proving fruitful, with special reference to visualization as a contribution to that fruitfulness in connection with more elementary and tractable examples.[9]

2.2. Understanding as an objective guiding mathematical investigation

This methodological analysis is given a special urgency by the observation – widely accepted by those familiar with the events – that in Riemann's work an entirely new style of mathematical thinking appeared, one that (in different subspecies corresponding to different interpretations of the style) has come to be part of the core of contemporary mathematics. In this connection it will be useful to glance at a discussion of some distinctive characteristics of twentieth century mathematics, as they were perceived by Hermann Weyl (who, of course, was right in the middle of this research).

> We are not very pleased when we are forced to accept a mathematical truth by virtue of a complicated chain of formal conclusions and computations, which we traverse blindly, link by link, feeling our way by touch. We want first an overview of the aim and of the road; we want to understand the *idea* of the proof, the deeper context.
>
> ...Minkowski contrasted the minimum principle that Germans tend to name for Dirichlet... with the true Dirichlet principle: to conquer problems with a minimum of blind computation and a maximum of insightful thoughts. It was Dirichlet, said Minkowski, who ushered in the new era in the history of mathematics. (Weyl, 1995, 453)[10]

In the essay Weyl contrasts the topological style for approaching Riemann's results – which he takes to be an orthodox descendant of the original Riemann function theory – with algebraic approaches to the subject.[11] (Both approaches are seen as possessing different, contrasting strengths.) Weyl echoes some of the themes noted above, singling out fruitfulness as a criterion marking the "natural" generalizations:

> What is the secret of such an understanding of mathematical matters, what does it consist in? Recently, there have been attempts in the philosophy of science to contrast understanding, the art of interpretation as the basis of the humanities, with scientific explanation, and the words intuition and understanding have been invested in this philosophy with a certain mystical halo, and intrinsic depth and immediacy. In mathematics, we prefer to look at things somewhat more soberly.... I can single out, from the many characteristics of the process of understanding, one that is of decisive importance. One separates in a natural way the different aspects of a subject of mathematical investigation, makes each accessible through its own relatively narrow and easily surveyable group of assumptions, and returns to the complex whole by combining the appropriately specialized partial results. This last synthetic step is purely mechanical. The great art is in the first, analytic, step of appropriate separation and generalization. The mathematics of the last few decades has revelled in generalizations and formalizations. *But to think that mathematics pursues generality for the sake of generality is to misunderstand the sound truth that a natural generalization simplifies by reducing the number of assumptions and by thus letting us understand certain aspects of a disarranged whole.* Then it is subjective and dogmatic arbitrariness to speak of the true ground, the true source of an issue. *Perhaps the only criterion of the naturalness of a severance and an associated generalization is their fruitfulness.* (Weyl (1995) p. 454–455 emphasis mine)

Among the many things in these remarks is: Weyl associates understanding with the unification of “aspects of a disarranged whole”. I’ll revisit this in connection with theories of explanation later. In this section I’ll set that point aside, and concentrate on the idea of understanding and the indicated connections to assessments of “naturalness” and “fruitfulness”. It is of course a truism that in mathematical practice we seek understanding, not just logically cogent argument. A characteristic case is described by Michael Atiyah in these words:

> I remember one theorem that I proved, and yet I really couldn’t see why it was true. It worried me for years and years... I kept worrying about it, and five or six years later I understood why it had to be true. Then I got an entirely different proof...

> Using quite different techniques, it was quite clear why it had to be true. (Atiyah, 1984, 305)

A proof or proof sketch can give cogent grounds for believing a claim, but it might fail nonetheless to provide the sort of illumination we can hope for in mathematical investigation. It is not unusual, nor is it unreasonable, to be dissatisfied with a proof that doesn't convey understanding and to seek another argument that does. Sometimes one proof may be counted superior to a second even though both proofs are carried out within the same theoretical context (same definitions, primitive concepts, formal or informal axiomatic formulations, etc.) In other cases, notably those I want to consider here, the advantages of one argument over another appear to derive partly from the definitions and/or axioms in terms of which they are framed.[12]

One among the many reasons accepted in practice for preferring one formulation over another is that one way of framing and addressing a topic can be more fruitful than another. When a formulation (set of definitions or axioms, etc.) is found to be fruitful, this fact often is taken as evidence that the formulation in question is the "natural" one, that the problem or subject is properly understood when it is set up this way.[13] Moreover, it is generally a necessary condition on a principle or definition proposed as natural that it support interesting new proofs.[14] Though it is difficult to lay out with draftsman's precision what "fruitfulness" is, the effort to devise fruitful ways of setting up problems and topics is part of what constitutes mathematical activity and makes it valuable to us.[15]

Whether or not a framework is "fruitful" is something that must be borne out by the facts, in the long run. This was a test passed by the Riemann approach to complex analysis: as devices for visually representing complex functions Riemann surfaces were indeed helpful, but they would have been relegated to the marginal shelf labeled "mere handy tricks" had they not consistently, systematically, and in unexpected ways continued to facilitate understanding and discovery. When they continued to bear fruit in novel and unexpected ways they were stably accepted as the proper context in which to study the functions of interest to complex analysis, as "not merely a device for visualizing the many-valuedness of analytic functions, but rather an indispensable essential component of the theory; not a supplement, more or less artificially distilled from the functions, but their native land, the only soil in which the functions grow and thrive." as Weyl put it elsewhere.[16]

That is: if it is judged that some framework or theoretical context is "the right one" because it is judged to confer understanding in a way that facilitates the solution to important problems, the judgement is *revisable*. Subsequent investigation can reveal that the framework provided just the illusion of understanding, if its advantages prove short lived or one – dimensional while another framework is more flexibly fruitful in the long run.[17] This observation helps address a worry that is especially favored by Wittgenstein enthusiasts: the worry that an assessment of "being right" will be empty if there is no independent criterion of success – if there is no difference between "being right" and "seeming to be right". That might seem to be a problem here if we concentrate solely on the psychological sense of "naturalness" that can be generated by placing a problem in its "natural" context/finding the "natural" formulation. But there is more to being the "proper" context/formulation than just generating that feeling: innovation and proof of important results must be facilitated *in practice*, over the long haul.

What is "natural" about a "natural" formulation typically must be *learned*: it will not be obvious on a simple inspection of the formulation.[18] The natural formulation need not be the one that is most initially attractive to the untutored. This runs counter to a suggestion that has been advanced, and which has some initial plausibility, that to understand a problem or concept is to reduce it to terms that are already familiar.[19] In many of the most interesting mathematical examples (Riemann surfaces in complex analysis, projective geometry for the study of (*inter alia*) conic sections, scheme theory in algebraic geometry, and much else...) what is counted as "understanding" is achieved by reformulating familiar problems in terms which are initially strikingly unfamiliar.

Another related point – one that will be crucial in section 3 and 4, and will be explored in connection with an example in section 5 – is that these judgements of "naturalness" and the like are *reasoned*. It is not just some brute aesthetic response or sudden, irrational "aha!" reaction that brings about the judgement that – for example – "the scheme is the more natural setting for many geometric arguments" (Eisenbud and Harris, 1992, 5) or that the Artin framework considered in section 5 is "the proper setting for many problems in linear algebra." (Hughes and Piper, 1973, 285).[20] Quite the contrary: elaborate reasons can be and are given for and against these choices. One job facing the methodologist of mathematics is to better understand the variety of reasons that can be given, and how such reasons inform mathematical practice.

This factual observation should be beyond controversy: reasoned judgements about the "proper" way to represent and prove a theorem inform mathematical practice. I have found that more contention is generated by the disciplinary classification of the study of these judgements and the principles informing them: is this *philosophy*, or something else, like cognitive psychology? It is hard to frame this worry precisely, since it inherits all the unclarity of what, precisely, philosophy is. Much of the rest of this paper will tacitly address the issue, but in addition I should say a few words directly.

The point of contention is the classification of advantages of theories that appear to depend on psychological facts about the people who use the theories. There seems to be a relatively widespread view that a property of a theory must be "objective" to be the proper object of a philosopher's attention.[21] (I should note explicitly that I am not endorsing this view, but only reporting that it is widespread enough that it needs to be confronted.) The relevant sense of "objective" is hard to pin down precisely, but one aspect of the idea seems to be that for an advantage to be "objective" in this sense it must be an advantage for every possible inquirer. "Subjective" features of theories – it is maintained – are someone else's problem.[22]

This sort of objection might seem to be an obstacle here. No doubt extraterrestrial beings with different wiring in their brains might differ from us on what formulations of problems are "most natural", and would discover that different ways of setting up problems would be most fruitful, and better at facilitating discovery or "making things easier". And there are *some* cases where mathematicians display nearly uniform preferences that do indeed seem to be of interest only to the student of psychology and not the student of method. The use of "z" as the variable of choice for complex variables is so entrenched it can be distracting if a different letter is chosen, but that is hardly a deep and interesting point of method. Using yellow paper rather than white may reduce fatigue and hence foster creativity, but that is not the methodologist's concern.

But while acknowledging that *some* cases should be shunted into the "not philosophically interesting" scrap yard, it would be a mistake to ignore them all. The situation is similar to the (more widely recognized) one that faces us when evaluating the role of judgements of "simplicity" in general philosophy of science. The assessments of simplicity or fruitfulness we make would no doubt be different if our brains were wired differently, and this would affect the mathematics and science that we produced, but still the judgements we actually make are too systematically embedded in our actual practices to be simply shrugged off in studies of either scientific or mathematical method.[23] The advantages and shortcomings of the Riemann approach to complex analysis as opposed to the Weierstrass approach is just one among many concrete examples that illustrate and anchor the point.[24] The reasons that can be cited for the preferences are so far-reaching and systematic that to set the issue aside as philosophically insignificant would be to abandon altogether the hope of a philosophically satisfying account of mathematical method. Given the thoroughness with which mathematical method is intertwined with physics, this would be tantamount to conceding defeat to skepticism of the global "Why should we think our best theories are true?" variety. Here and in the coming papers of this series I'll spell out why we need not make such a drastic sacrifice: if not a refutation of demon skepticism, we can at least get a better sense of what makes our best mathematical theories deserve the status of "best".

2.3. Visualization as factor in mathematical reasoning

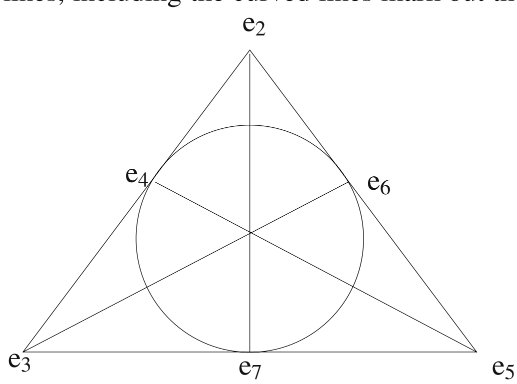
> In my opinion, a well-balanced introduction to topology should stress its intuitive geometric aspect, while admitting the legitimate interest that analysts and algebraists have in the subject... I have followed the historical development where practicable, since it clearly shows the influence of geometric thought at all stages. This is *not* to claim that topology received its main impetus from geometric recreations like the seven bridges; rather it resulted from the *visualization* of problems from other parts of mathematics – complex analysis (Riemann), mechanics (Poincaré) and group theory (Dehn). (Stillwell (1993) vii emphasis in original)

One among the many reasons that can be (and sometimes is) cited for formulating a subject in one way rather than another is noted in the quoted remarks: the ability to visualize can be among the factors that shape a subdomain of mathematical practice. To avoid unnecessary controversy it is important to see that the importance of visualization in mathematical reasoning can be explored without making any assumptions about the nature of visualization itself. We don't have to answer the question of just what visualization is, or take a position in the now classic debate among cognitive scientists and philosophers of mind about how to make sense of talk of mental imagery.[25] We don't need to address what it could mean to literally "form a picture" of an infinite dimensional figure or a Klein bottle. All that will be needed here is the observation that we have enough of a naïve grip on the notion of visualization or pictorial representation to acknowledge that whatever such representation may ultimately amount to, in mathematical practice there are cases where people try to "visualize" (whatever we may be doing when we do that), that such visualization often is helpful, and that this often informs mathematical practice (more often in some fields than in others).[26] (This should not be philosophically contentious: it is just a factual observation about the methods and theoretical preferences of many working mathematicians and amateurs.)

As mentioned above, this aspect of mathematical activity has tended to be ignored by philosophers, in part because of a sense that the phenomenon is accidental, or "pragmatic" or "subjective" or "psychological" in ways that make it philosophically uninteresting. This response is reinforced by the simple fact that a large number of examples in which visual representation in diagrams assists reasoning and problem-solving, really are uninteresting to the student of methodology. It has been well-known to memory system designers from medieval times that it can be easier to remember information if one can visualize or draw some table of pictures to list the information.[27] In such cases, the fact that visualization is involved might not be particularly interesting for the present purposes. Cases like this are ubiquitous in mathematics too. As an example, consider these two ways of representing the multiplication table for octonions.[28] Here is the multiplication table (with $e_1 = 1$):

	e_2	e_3	e_4	e_5	e_6	e_7	e_8
e_2	-1	e_4	$-e_3$	e_6	$-e_5$	$-e_8$	e_7
e_3	$-e_4$	-1	e_2	e_7	e_8	$-e_5$	$-e_6$
e_4	e_3	$-e_2$	-1	e_8	$-e_7$	e_6	$-e_5$
e_5	$-e_6$	$-e_7$	$-e_8$	-1	e_2	e_3	e_4
e_6	e_5	$-e_8$	e_7	$-e_2$	-1	$-e_4$	e_3
e_7	e_8	e_5	$-e_6$	$-e_3$	e_4	-1	$-e_2$
e_8	$-e_7$	e_6	e_5	$-e_4$	$-e_3$	e_2	-1

This is a lot to absorb. To assist the memory there is a standard picture. The connecting lines, including the curved lines mark out the products.[29]



Unquestionably it is helpful to picture the multiplication operation of the octonions this way. However, the visual representation in a diagram might plausibly be said to function *solely* as a memory aid. That is, it might be that the sole advantage gained from the visual representation in this case is that most people just happen to find information easier to recall if it is encoded into pictures of this kind than if it isn't. It isn't an implausible suggestion that it is an accidental feature of our cognitive makeup that memory can be thus facilitated by visualization. If so, there might not be any philosophically interesting consequences to the fact that people generally have a strong preference for the visual arrangement over the tabular one.[30]

I think that is the right thing to say in this case, and in many others like it.[31] The advantage gained by the diagram of the octonion table is one-off; it has no interesting systematic connections to anything else. Most significantly, the availability of the diagram doesn't correspond to anything deep about the basic concepts or proof methods used in the study of the algebraic structure represented. The point here will be that this case is a misleadingly simple exemplar. The function of visualization is much more intricate and systematic in cases like the Riemann approach to complex analysis. This, of course, is not so much an answer as a gesture in the direction of an answer: if the Riemann approach is different, we should be able to spell out how. This paper is a first step toward meeting that challenge.

One point that is worth noting is that the advantages of the diagrammatic representation of the octonions table seem to be *essentially* visual: the diagram helps because visual perception is an especially vivid mode of cognition, and the visual arrangement of the table allows it to be easily "taken in at a glance". The cases that interest us here are more complicated: though Riemann surfaces (for instance) admit of visual representation in a particularly straightforward way, the fruitfulness of this theoretical context for complex analysis persists even if diagrams are eschewed altogether. One might put the situation this way: there is a mode of organizing the subject which is especially natural, and which *happens* to connect to visualization in a direct way, but the mode of organization is theoretically valuable even without the essentially perceptual features like the vividness and immediacy of representation in diagrams. That is, the connection to vision is an intriguing and useful bonus, but the issues raised by the Riemann – Weierstrass opposition are of interest independently.

The distinction between the contexts of discovery and justification might also be suggested to apply here, since theoretical formulations count as "fruitful" at least partly because of how they facilitate the discovery of proofs.[32] It could be suggested – in line with the usual appeals to this distinction – that fruitfulness is only of interest in the "context of discovery" and that only what matters to the context of justification is relevant to epistemology, so once again these issues are someone else's concern. This issue is complicated and I can only indicate the shape of the answer here, deferring a more extensive discussion for elsewhere.[33] In brief, this is a case where the context of discovery/context of justification distinction blurs. The preferred, fruitful formulation is *not* generally dispensable in the "context of justification" because there might not *be* any proofs available that do not use the fruitful formulation.[34] [35]

It will help us sharpen the issues to look for a philosophical niche served up by treatments of explanation and understanding in the natural sciences, since these have been extensively addressed. It will be especially helpful to consider the treatments of explanation and understanding as bound up with theoretical unification. I'll argue that these accounts are correct only with revisions and qualifications, but following out a presentation of the view and some of its obstacles will be a useful springboard. This will be the goal of the next two sections.

3. UNDERSTANDING, UNIFICATION AND EXPLANATION – FRIEDMAN

Efforts to explain and understand also guide investigation in the natural sciences, and mathematical theories and methods are integral parts of these investigations. This is a truism. Even if a sharp and principled divide between "applied mathematics" and "pure mathematics" can be marked out, it is unlikely that it would be so stark as to exclude unifying methodological themes. So it makes sense to try to compare explanation and understanding in science and mathematics.

Of course, there is a crucial *disanalogy*. The propositions to be explained in mathematics are not contingent, and so we can't appeal to the plausible and widely accepted suggestion that the explanation of an event provides part of the causal history of that event. If the plausible suggestion is right, then whatever is involved in mathematical explanation and understanding must differ from what is involved in at least *some* cases of scientific explanation and understanding.[36] This is not a problem here: I *grant* that the words "understanding" and "explanation" are probably not used unambiguously to pick out a single uniform phenomenon, and I agree that many things that are properly called explanations appeal essentially to causal ancestry. However, mindful of the fact that some explanations in physics and mathematics *do* seem to be governed by the same principles, I'll count it as an advantage of an account that it supports a uniform treatment of some mathematical and some physical explanations.

A promising candidate to support a uniform treatment of some pure mathematical cases and some non-mathematical ones is the treatment of explanation as unification as proposed in the seventies by Michael Friedman and Philip Kitcher. In this section I'll pass through Friedman's now classic account in his (1974). Friedman embraces the suggestion that in at least some important cases, to provide an explanation of an event is to provide the resources to understand that event, and motivates his account accordingly. A further reason for considering Friedman's treatment here is that he states explicitly a requirement that we noted earlier: accounts of understanding must be *objective*. Scrutiny of the way that Friedman understands this condition will help clarify what is at issue.

This discussion aims to shore up these two points: a) The unifications that are regarded as valuable in practice involve some kind of *qualitative* advance, by producing a theory that is comparatively homogeneous to subsume two or more theories that had appeared heterogeneous. Mere reduction in the *number* of axioms or basic principles is typically not at issue. B) The conception of "objectivity" that motivates Friedman's account has to be relaxed in order to be true to the kinds of example that make unification accounts plausible. I'll suggest that Friedman's desiderata are actually better served by effecting something of a rapprochement with the view of Stephen Toulmin, which he examines and rejects.

3.1. Friedman: understanding as "objective"

The core of the Friedman approach is the compelling observation we've already noted, that often it is an important advance to bring about a unification of theories or hypotheses that had previously seemed disparate, by crafting a single theory or hypothesis that subsumes both. There are famous cases in which such unification appears to have been a clear advance: two prominent examples are Newton's unification of terrestrial and celestial mechanics and the unification first by Maxwell, and then within relativity theory, of electric, magnetic and optical phenomena. The prospect of a super theory encompassing quantum mechanics and gravitation is recognized as a motivating objective of current physical research. Looking back to the Riemann – Weierstrass contrast with which we began this study, we find different variations on this motif marking out Riemann's style. Riemann's approach to complex function theory admits of a variety of points of view in part because he effected a unification of complex function theory with the theory of complex curves and surfaces. Also, on a smaller scale, the "proper" choice of unifying definitions and basic principles was regarded by Riemann's contemporaries as one of the hallmarks of his work. A particularly clear expression appears in these words of Casorati:

> We believe that it is truly admirable with what assimilating power [Riemann] knew how to gather and establish in some

> compact, simple, and general theory, together with his own, all of the other studies that had an important relation to them. Of particular importance were Cauchy's many studies, spread over numerous publications, which were conducted with differing purposes and often wrapped in a heterogeneous variety of terms and special notations. ... In paricular it is especially worth observing how Riemann always sets up his own conventions and definitions in such a way that every theorem can be stated as true without exception, or how many formulas and theorems ordinarily thought to be different from each other can be united in a single formula or theorem. (Casorati (1868, 140 note 2); quoted in (Bottazini, 1986, 218)).

Though it is open to dispute whether such advances are reasonably described as "explanations", it is clear that they are successes, so I'll treat it as uncontroversial that unification (whatever it may amount to in the final analysis) does represent an objective implicit in scientific practice.[37] What will be at issue is the question of how to analyze these acknowledged successes.

Friedman argues that there is an irreducible advantage to reducing the *number* of facts that have to be accepted as "brute facts".[38] Friedman faces the elementary problem that we can reduce the number of axioms in any theory generated by a finite collection of axioms $A_1 \ldots A_n$ just by taking as a single axiom the conjunction $A_1\&\ldots\&A_n$. But of course this wouldn't be a theoretical advance. Friedman refines the idea of reducing basic premises by introducing a characterization of premises as "independently acceptable". A successful unification, on his account reduces the number of *independently acceptable* premises.

Friedman's treatment doesn't seem to hang together unless a purely *a priori* delineation of "dependent" and "independently acceptable" can be worked out. Friedman's efforts to craft a technical definition of "independently acceptable" foundered on straightforward counterexamples, the initial efforts at patching the theory foundered on further counterexamples, and nothing much has been done to revive this part of the account in the last couple of decades.[39] In light of this I'll regard Friedman's theory in this specific form as untenable. But it will be useful for diagnostic purposes to set aside this problem: let us say for the sake of argument that we have managed to fix which propositions are acceptable independently of which others. Even so, Friedman faces the problem of distinguishing the *valuable* syntheses that can plausibly be called "explanations" or "advances in understanding" from those that cannot.

By unifying the theories of electricity and magnetism, Maxwell brought together two theories that had previously seemed to govern different *kinds* of phenomena. But there is no reason to expect that the new, more encompassing framework of electromagnetism had fewer axioms. Say that in some canonical axiomatisation, Maxwell's electromagnetism had twelve axioms and the two theories it replaced had just four each. So what? Would we cease to regard Maxwell's electromagnetism as a successful unification if we discovered, upon actually doing the count, that it had more axioms than the total axioms of the theories it replaced?[40] Similarly, if relativity theory and quantum mechanics were to be brought together into a single homogeneous theory, we would hardly reject the unified theory, or regard it as a regressive, non-explanatory step, if it had an independent axiomatic basis with more sentences than the theories it subsumed. In short, reducing the number of axioms need not increase understanding and increasing the number of axioms need not detract from understanding.[41] We need to focus not on the number of axioms but rather on what makes a given theory more homogeneous than just the conjunction of the two that it subsumes.[42]

Mathematical examples are worth adding, to be consistent with the theme that the relevant issues arise in both mathematics and natural science. In a classic paper (1882), Richard Dedekind and Heinrich Weber produced a unified theory of algebraic functions of one variable and algebraic numbers.[43] It would be hard to overstate the importance of this work; much of the subsequent development of German algebraic geometry in the first part of the twentieth century can be traced back to this source (in both content and style).[44] Much of the jolt of this work comes from the unification of a paradigmatically arithmetical subject (algebraic numbers) with another (one-variable algebraic functions) that (in the Riemannian tradition Dedekind occupied) had been seen as geometric. Key insights were (as we would now put it) that both algebraic numbers and algebraic functions can form a field, and that the elements of these fields can be analyzed in terms of ideals, and many of the crucial properties of both can be seen to depend just on this.[45] (In identifying the concepts of field and ideal as crucial, we can now recognize that Dedekind and Weber were backing winners. As the subsequent history has borne out, few concepts can rival these for fruitfulness within mathematics.) The uniform treatment of two apparently disparate subjects (achieved in this case by general argument patterns exploiting a "truly central" idea) gives a provisional credibility to the idea that the concepts involved, within the theoretical framework as a whole, really laid out what is going on. (The provisional credibility was, of course, ratified by subsequent research.) Once again, this advantage is completely disconnected with any issue of how many axioms might be involved in the unifying theory.[46]

This is borne out by the account of Dedekind's algebraic work in the Weyl (1995) essay mentioned above. Weyl does say, in one quote given above that "a natural generalization simplifies by reducing the number of assumptions and by thus letting us understand certain aspects of a disarranged whole". But Weyl's subsequent discussion reveals that these remarks should not be understood in terms of a reduction of axioms. In the detailed analyses of Dedekind - style approaches to the theory of algebraic functions of one variable that form the main body of Weyl's essay, the idea of "reduction" in play is *conceptual*, in the sense that propositions incorporating a variety of apparently disparate concepts can be reduced to claims incorporating just one fruitful key idea: Dedekind's concept of ideal, that "runs through all of algebra and arithmetic like Ariadne's thread" (1995, p.649). So far as Weyl's analysis is concerned, a reduction of two propositions containing a range of heterogeneous concepts to more than two propositions framed solely in terms of ideals would be a unification contributing to understanding. Ideal theory is a natural framework for these problems, according to implicit and sometimes explicitly articulated criteria of "naturalness" that inform mathematical practice. We need to address the extra qualitative condition – that the theory doesn't just effect a bare, gerrymandered unification but that it does so in the right terms – before we can understand what gives the unification value and leads mathematicians to regard it as an advance in understanding.

An example from geometry will be especially helpful in connection with the discussion of section 5. A breakthrough in the understanding of general geometries, and the algebraic structures corresponding to geometries, was provided by Hilbert's axiomatization in Hilbert (1899) (refined in subsequent editions). This set out a single framework within which the range of geometries studied up to that point could be deductively developed and studied. However, this framework was remarkable in part for the *number* of axioms it contained. Far from reducing the number of axioms, Hilbert actually went out of his way to increase it: he aimed to isolate precisely what axioms a particular theorem depended on. To this end it was important to have available the axioms to support fine distinctions of deductive strength.[47] Consequently, even though the framework as a whole unified the study of diverse geometries, this was not unification according to Friedman's analysis; indeed, on Friedman's analysis it would be a regressive step.

The other mathematicians who set the tone at the turn of the century seemed also to be indifferent to number of axioms, though they were alert to differences between frameworks. Thus in his otherwise glowing review of Hilbert's foundations, Poincaré, was silent on the increased number of axioms, but he indicated one felt lack: unlike Lie's foundations, Hilbert did not set the idea of transformation in an important place. (Poincaré, 1903) For his own part, Hilbert complained of Lie's foundations that in Hilbert's view Lie's axioms were insufficiently "elementary". The points of comparison and contrast were solely qualitative; which of the frameworks had the fewest axioms didn't come into the debate at all.

I'll turn to the broader standpoint that informs Friedman's discussion. As noted above, Friedman suggests that his is the only proposal among those he considers to be both *objective* and to capture something that might fairly be called a notion of understanding.[48] Friedman appears to take "objective" to mean something in the ballpark of "ascertainable *a priori*", though it is not clear what positive account of objectivity he has in mind. However, it is clear what he thinks *isn't* objective in his sense: the view proposed in Stephen Toulmin's *Foresight and Understanding*, which Friedman calls (uncharitably, as Friedman acknowledges) "the intellectual fashion view." I think this rejection is a mistake. The spirit of Friedman's view seems to be salvageable only if something like Toulmin's position is grafted on, and it is unclear why we should take this to involve any sacrifice in objectivity.

We'll need a closer look. On Toulmin's telling, what counts as "understanding" is conditioned by a broader "ideal of natural order". The function of these ideals that is interesting for our purposes is that they tell us when something is in need of explanation and when it may stand unexplained. Similarly, it colors our view of what properties are "natural" and which "artificial". These ideals can change from one era to another. So for example, action-at-a-distance explanations stood for a time as absurd, then became acceptable for awhile, and then became unacceptable again. Friedman objects to incorporating this into an account of understanding because it represents understanding as "not objective" – as depending on capricious or subjective factors. But this is not obviously just: it depends on *what kind of reasons*, if any, are given to support the ideal. If we take something as basic, we need not be mute about *why* we take it as basic. Nor must the reasons we offer be bad reasons just because other people in different conditions might offer different reasons, supporting different conclusions. Quite the opposite: especially if the different conditions include different available data, as generally they do, we should *expect* different answers. We don't want to say that it is just fashion that leads us to change our minds when we *learn more*. Our judgements as to what is basic and what requires explanation, and so on, are not independent of what is known about the world, and what is appreciated about method, at a given time. It does not impeach this observation that there will generally be no uniform algorithm for going from a theoretical situation and range of evidence to some fixed range of ideals and natural choices of basic axioms and properties. The best arguments may be highly case – specific. But they need be no less rational for all that: there is no way to tell in advance before we consider what the specific reasons are.

The argument of Friedman's paper has an instructive instability on this point. Friedman acknowledges explicitly that by Toulmin's account the choice of a particular "ideal of understanding" can typically be defended with reasons. This (appropriate) concession is in tension with both Friedman's core objection and the connotation of the label "intellectual fashion view" that ideals of intelligibility are arrived at arbitrarily or as caused by individual or group psychology rather than adopted as rationally justified. To be sure, Toulmin invites this uncharitable reading, as at times he appears to both assert and deny that reasons can be given for a preferred explanatory framework. For example, he writes:

> Those who build up their sciences around a principle of regularity or ideal of natural order come to accept it as self-explanatory. Just because (on their view) it specifies the way in which things behave of their own nature, if left to themselves, they cease to ask further questions about it. It becomes the starting point for explaining other things. Yet the correctness of a particular explanatory ideal (as we shall see) can never be self-evident, and has to be demonstrated as we go along. (Toulmin, 1961, 41-42)

The statement that an ideal "can never be self-evident, and has to be demonstrated as we go along" sits uncomfortably with the suggestion that this basis cannot be explained further:

There must always be some point in a scientist's explanations where he comes to a stop: beyond this point, if he is pressed to explain further the fundamental basis of his explanation, he can say only that he has reached rock-bottom. (Toulmin, 1961, 42)

I think it is the first of these that is truer to the facts: even when something is taken basic or fundamental, this does not preclude the possibility of arguments for and against *regarding it as* basic or fundamental, nor does it preclude arguments for and against its truth.[49] And in many of the interesting cases (such as Toulmin's example of magnetism in the nineteenth century) the changes in the categories taken as central and the principles taken as basic could be, and were, provided with a detailed rationale. Again, this is exemplified by the vicissitudes of German action-at-a-distance explanations, in their course from rejected to accepted to rejected again over the course of a century.[50] This was a dispute over what to regard as the basic shape of an acceptable theory, but the proponents and opponents of action-at-a-distance were hardly reduced to inarticulate grunts. Elaborate reasons were given on both sides, involving appeal to reflections on general methodology and appeals to the advantages of the known available theories, as well as the range of known facts. It is hardly caprice or brute fashion if people come to prefer a different framework when they have gathered more data and learned more, or when they have formulated attractive theories that had previously eluded them.

Toulmin's (unnecessary and in my opinion regrettable) suggestion that the basic concepts and propositions are the bedrock at which the spade of argument is turned is (rightly) a sticking point for Friedman. It will be useful to quote and comment on a long stretch of Friedman's text to clarify what is at issue:

> There are many cases in the history of science where what seems explanatory to one scientist is a mere computational device for another; and there are cases where what is regarded as intelligible changes with tradition. However, it seems to me that it would be desirable, if at all possible, to isolate a common, objective sense of explanation which remains constant throughout the history of science; a sense of "scientific understanding" on which the theories of Newton, Maxwell, Einstein and Bohr all produce scientific understanding. It would be desirable to find a concept of explanation according to which what counts as an explanation does not depend on what phenomena one finds particularly natural or self-explanatory. In fact, although there may be good reasons for picking one "ideal of natural order" over another, I cannot see any reason but prejudice for regarding some phenomena as somehow more natural, intelligible, or self-explanatory than others. All phenomena... are equally in need of explanation, though it is impossible, of course, that they all be explained at once.
>
> Therefore, although the 'intellectual fashion' account may ultimately be the best that we can do, I don't see how it can give us what we are after: an objective and rational sense of 'understanding' according to which scientific explanations give us understanding of the world. (Friedman, 1974, 13)

There is much in these words I agree with. I am of one mind with Friedman in resisting the suggestion that "some phenomena should be seen as natural, intelligible or self-explanatory", if this is to mean that the status of "natural" cannot be defended by good reasons. It was, I think, a misstep for Toulmin to cast the issue in these ways. But we do, in practice, see some categories and principles as "more natural, intelligible or self-explanatory" in the sense that they are methodologically basic: they are taken as the most reasonable principles to appeal to in addressing problems of a given kind, or the most natural categories to employ in connection with a given subject. That concepts and principles have this status is not inexplicable: part of the practice of mathematics and science – part of what makes it valuable to us – is that it incorporates reasons why the basic principles chosen are the good ones. There can be general changes of mind, because more facts are learned, or the theoretical situation comes to be better appreciated, or because new problems are confronted that prompt a reappraisal of accepted techniques, or just because people working in the field come up with some new ideas. (Such changes in what is taken as natural and basic can, of course, also be the result of irrational caprice, but they need not be.) If "natural" is understood as here, there need be no loss of "objectivity", and no appeal to caprice or irrationality, in "a concept of explanation according to which what counts as an explanation does [depend] on what phenomena one finds particularly natural or self-explanatory." Section 5 will deal with such a case, in which choices are made of axioms and basic categories for good reasons.

These observations do not conflict with Friedman's stated objective "to isolate a common, objective sense of explanation which remains constant throughout the history of science; a sense of "scientific understanding" on which the theories of Newton, Maxwell, Einstein and Bohr all produce scientific understanding." Indeed, what I've written above even strengthens the case for a (modified and weakened) version of the core theses that understanding is an objective of mathematical and natural scientific investigation and that in some interesting cases something reasonably described as "unification" is an important contributor to increased understanding. The quarrel is only with the details of the analysis of "unification": it isn't a reduction in the number of axioms, but something more complex. The bottom line is that we can't expect an account of the sort Friedman desires without concrete information about the reasons offered and accepted in scientific and mathematical practice for choosing what is to count as the "natural" or "reasonable" or "proper" primitive concepts and axiomatic formulations. If some of the features of theories that make them good examples of "unification" are more qualitative and (so to speak) "softer", that doesn't make them any less real, important or objective.

To nail down the point, let's reconsider the concepts of "field" and "ideal". Dedekind and Weber (1882) helped bring out what has subsequently become patent: these concepts are mathematically central. By contrast, during roughly the same period, Frege developed the concept of "quantitative domain", which is similar to the concept of field in some respects, and which hasn't caught on at all.[51] The definitions alone don't make it obvious which of these would turn out to be the best choice of a framing concept: this has to be discovered by seeing what they can do. That field theory became central and the concept of quantitative domain sank below the waves was not because of academic politics or mob psychology or the whims of capricious fashion. Any working mathematician today who was introduced to Frege's definition could give a cogent rationale for the preference for the concept of "field" over that of "quantitative domain" as a framing concept.

The explanation will be intricate, though. It is not just that the concept of "field" is used to prove *more* theorems, since both concepts can be used for the same number of theorems (i.e. infinitely many). It is rather that field theory is needed for a striking number of "important" or "interesting" or "central" theorems. That a theorem deserves such an honorific designation can in turn be justified in terms of some combination of the connection to other mathematical subjects or physical applications, the importance of further theorems, qualitative observations about what is unexpected or surprising, and the like.

Other features of the concept can be invoked beyond just the results that it supports. For example, it can be pointed out that the concept of field is naturally refined into further fruitful subcases (zero vs. nonzero characteristic, finite and infinite fields). Once again, the preferences and distinctions that lead us to count one framework as more natural than another need not rest on brute, inarticulate preferences or transient fashion.

In sum, to make progress in clarifying what is at issue when theories are successfully unified, we need to learn more about qualitative features of theories: what makes a framework, and the categories in it, "natural" and "homogeneous" or whatever. Of course, there is only so far that a discussion of whether or not a set of reasons count as "objective" in the absence of the reasons themselves. This will of course depend on the specific case at issue. It will be the goal of section 5 to illustrate how involved these sorts of reasons can be in a case that is worked out in some detail. First I'll look more carefully at a specific touchstone: the problem of excluding gerrymandered predicates. This will provide the occasion to take up the potential for visualization, as a feature of theories that occasionally (though not always) contributes to an assessment of a category or framework as natural.

4. KITCHER: PATTERNS OF ARGUMENT

The account in Kitcher (1976, 1982, 1989) shares with Friedman the emphasis on unification as an animating objective in scientific inquiry. An important point of agreement between Kitcher's discussion and mine is in his appreciation of the affinities between some mathematical explanations and some natural scientific ones. He therefore sets this objective: an account that will accurately represent explanations in both domains. (1989 p. 482) Because of the above-noted technical problems in demarcating what is "independently acceptable", Kitcher sets aside the suggestion that reduction in the number of "independently acceptable" premises is the criterion for successful unification. Rather, the goal of unification is taken to be a reduction in the number of argument patterns. The background stance is that explanation involves seeing connections among phenomena, with the degree of unification depending on how economically this is achieved:

> Understanding the phenomena is not simply a matter of reducing the "fundamental incomprehensibilities" but of seeing connections, common patterns, in what initially appeared to be different situations... *Science advances our understanding of nature by showing us how to derive descriptions of many phenomena, using the same patterns of derivation again and again, and in demonstrating this, it teaches us how to reduce the number of types of facts we have to accept as ultimate (or brute).* (Kitcher (1989) p. 432 emphasis his)

Kitcher's view begins with a set K, "the set of statements accepted by the scientific community", with a set of arguments deriving some members of K from other members of K a *systematization* of K. Argument patterns are represented as sequences of schematic sentences, with a restricted class (the "filling instructions") of acceptable substitutions into the schematic places.[52] (So for example, an argument pattern might have one place restricted to chemical substances, another to real numbers for arguments relating substances and atomic weights.) An explanation, for Kitcher, is an argument in the best systematization – which Kitcher designates E(K). Already – even before we consider the criteria of goodness of systematizations – an interesting stance on explanation is marked out. According to this view, whether or not an argument counts as an explanation is a global matter, depending on the overall structure of the theoretical framework. Explanations are arguments belonging to some class which has theoretical virtues as a class. This leaves open a range of different possibilities, depending on what criteria of "bestness" for systematizations are proposed.

For Kitcher, the best systematization of K is the one that "best satisfies" the two constraints of "minimizing the number of patterns of derivation employed and maximizing the number of conclusions generated." (p. 432) There is, of course, an important point of agreement here; the goal of "maximizing the number of conclusions generated" is close to the goal of identifying fruitful formulations discussed above. Also as noted above, to make sense of comparing two sets of infinitely many conclusions, and to get closer to actual practice, we will have to introduce some refinements, such as placing special weight on "important" "interesting" or "deep" conclusions. But this point will not be at issue here. Unification is thus taken to have this goal: providing single argument schemes that apply to a variety of special cases, with a special premium placed on keeping down the number of schemata.

As in the case of axioms, the suggested emphasis on keeping the *numbers* down isn't a good fit with actual practice. Quite the opposite, it is reasonable and common to seek many different arguments for a single result, each argument exemplifying different principles and exploiting different techniques, and giving a different theoretical diagnosis. There is no shortage of examples; the search for novel proofs of already established results is a standard practice. In the more profound cases (the prime number theorem, say, or the Riemann-Roch theorem) entirely different subfields are induced by different proofs of one result.[53] This reflects the fact that successfully identifying unifying generalities is assessed not by counting the total number of patterns but rather by the quality of the patterns themselves: Are they the *right* ones (are they deep or fruitful or revealing or whatever?) Again, it is a challenge to clarify what these qualitative desiderata amount to, but we have to tackle them before we can count ourselves as having clarified the goal of unification in mathematics and science.

This section will address the following points. The first subsection will take up the suggestion that finding general patterns allows a reduction in the number of facts taken as brute; I'll argue to the contrary that the general patterns don't supersede the particular ones. In the second subsection, I'll argue that unification according to Kitcher's pattern does occur in important cases, but it is not an unconditional goal. Additional constraints – for example that the predicates employed in reasoning are not "gerrymandered" – come into play as well. To lay the groundwork for the final section I'll consider one case – graphic statics – which is especially favorable to Kitcher and in which visualizability is one of the contributing factors to the assessment of the naturalness of the formulation.

4.1. Unification: General patterns and Brute Facts

The identification of the general patterns of argument doesn't reduce the number of "brute facts" because the general cases typically don't supersede the special cases they generalize. The interaction of the special and general cases is complicated. Kitcher emphasizes the economy of thought that is gained by identifying these general patterns of reasoning, and he should, but it is well to realize that generality of this sort is sometimes valuable for the dual advantage that different special cases may have specific advantages, and the ability to shift back and forth gives problem-solving advantages. Furthermore, when you have a single pattern of argument unifying two domains, the pattern might be useful <u>for different reasons</u> in each: it might generalize in different directions or admit different fruitful modifications in different instances. That is, a unified general theory can be valuable in part because it allows the systematic exploitation of residual differences.

A mathematical example – the duality of variety and ideal in algebraic geometry – helps bring out this point.[54] The example rests on the "dictionary" connecting ideals in simple algebra and varieties in elementary algebraic geometry. It is useful to note how the process of working out this duality is described in an intermediate – level undergraduate textbook:

> In this chapter, we will explore the correspondence between ideals and varieties.... [The *Nullstellensatz*][55] will allow us to construct a "dictionary" between geometry and algebra, whereby any statement about varieties can be translated into a statement about ideals (and conversely). We will pursue this theme in ##3 and 4, where we will define a number of algebraic operations on ideals and study their geometric analogues.... In ##5 and 6 we will study [additional] more important algebraic and geometric concepts... notably the possibility of decomposing a variety into a union of simpler varieties and the corresponding algebraic notion of writing an ideal as an intersection of simpler ideals. (Cox et al., 1992, 168)

Although the study of the "ideal" – "variety" duality (in contrast to the duality in projective geometry that we'll consider in a few pages) is not directly framed in terms of linguistic schemata, it still stands as a striking supporting example for Kitcher's picture of mathematical practice as pursuing understanding and explanation by seeking out general argument patterns. General arguments can be transformed into arguments in geometry or arguments in algebra by systematic substitutions into general schemes. The apparent qualitative difference between the subjects of algebra and (algebraic)

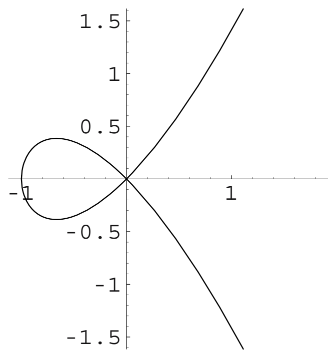


FIGURE 1. $y^2 - x^2 - x^3 = 0$.

geometry makes the discovered unity all the more compelling. However, the two subjects remain importantly different. It can be a subtle question whether a problem is more naturally addressed in one context or the other, and the ability to shift between the formulations is itself exploited as a problem – solving strategy.[56]

Other, less involved examples are easy to come by. Theories of integration in the plane are indifferent to what the underlying coordinates of the plane happen to be, but sometimes a careful choice of a specific set of coordinates can transform an integral from nasty to nice.[57] Another family of simple examples appears in the birational geometry of the plane (the study of properties of figures that are invariant under birational transformations).[58] Identifying curves that are birationally equivalent turns out to yield an interesting and useful theory, since certain key properties, such as the genus of a curve, are invariant under birational transformations. But the resulting generality does not mean that in studying these curves we should become indifferent to the specific details. Consider for example the resolution of singularities.[59] Say that in the plane we have the curve $(y^2 - x^2 - x^3) = 0$ (figure 1) which crosses the y-axis twice at a single point. (We count the origin *twice* because it is approached in two different ways by tangents.)

It is a bit of an irritation that the intersections coincide like this, and so it is helpful to exploit the fact that by a quadratic transformation – an especially

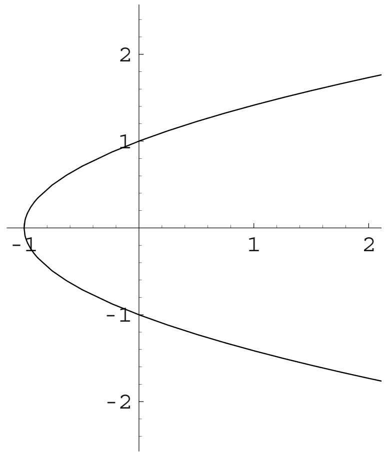


FIGURE 2. $y^2 - x - 1 = 0$.

simple birational transformation – the original curve can be mapped into the parabola $y^2 - x - 1 = 0$ crossing the y axis in two distinct places (figure 2).

So, as far as the properties of birational geometry are concerned, nothing is lost if the more convenient representative stands as a proxy for the less convenient one. This was a simple example; the gain in order and simplicity is of course even greater when the zeros occur in clumps from cloverleaf patterns and such. When we know we can "blow up" singularities in this way, the fact that we have a general pattern of argument doesn't lead us just to focus on the general pattern to the exclusion of specific details. The general pattern also affords us a way of squeezing more information and efficiency out of a good choice of special cases.[60]

4.2. Kitcher Unification in Practice: Projective Duality and the Gerrymandering Challenge

As we noted above, the goal of "minimizing the number of derivations" faces a problem analogous to that faced by the candidate goal of "minimizing the number of premises" in Friedman's treatment: the quantitative restriction, to reflect actual practice, needs qualitative reinforcement. The derivations have to be "the right kind", the unifying framework has to be "homogeneous" and its basic categories "natural". The point also arises in connection with Kitcher's account in two ways – one historical, one philosophical. One is an analogue of the problem Friedman faced in defining "independently acceptable": some "unifications" (like logical conjunction) unify in an artificial way that doesn't advance understanding. Another concerns the history of some developments in mathematics and engineering that support Kitcher's account, but only part way. In the examples in question (general projective geometry, and its particular application to engineering techniques for analyzing the strength of components of physical structures) we find an a striking example of a mathematical theory being used for the explanation of physical events, in a way that reflects Kitcher's theory of explanation to a striking degree. The availability of dual patterns of argument is explicitly marked out as a theoretical virtue. But also, in this example, the advantages of patterns of argument that can be exploited in multiple ways is not treated as an unconditioned objective: it is also constrained by the assessed "naturalness" of the basic categories, and the fruitfulness of the framework as a whole. I'll take up the second point after addressing the first.

The first problem is that unification will be too easy to achieve unless we can rule out "gerrymandered" properties as potential substitutions into argument schemes. If there is no constraint on what can count as a property then using a device well-known since Goodman's (1955) it is mere sport to come up with a theoretical unification of any two claims. Say we have two facts we want to explain/understand:

a) A ball of uranium under conditions of extreme temperature never attains a radius of ten metres.
b) Actors playing alongside chimps never win Oscars.

Here is an easy recipe to unify these theses. Let's define:

Pxy	iff	x is a ball of uranium in state y or x is an actor playing opposite y
Qx	iff	x is a state of extreme temperature conditions or x is a chimp
Rx	iff	x has a radius of 10 metres or x wins an Oscar.

Then, from the general proposition $(x)(y)(Pxy \mathbin{\&} Qy \supset \sim Rx)$, and some extra specifications (No actor is a ball of uranium…) we can derive both of our specific theses. It isn't difficult to set things up so that the derivations will be instances of a single argument scheme. This will give us a unified theory of critical mass and academy awards. We even get some unexpected, novel verifiable predictions (though not very interesting ones) like "No ball of uranium in a state of extreme temperature wins an Oscar". But I expect that it will be agreed on all sides that we haven't managed to explain or improve our understanding of either of the claims that we began with. In this case the artificiality of the properties defined is evident, and their disjunctive character is written into the syntax of the definition. But of course we can't assume that spoiler predicates will always come with such a syntactic advertisement.[61] On what basis are we to distinguish the properties that support valuable unification from those that give us rubbish that isn't worth the effort? Kitcher is aware of this problem, and explicitly addresses a variation on it, but his response only makes our problem more urgent:

> We need some requirements on pattern individuation that will enable us to block the gerrymandering of patterns by disjoining, conjoining, tacking on vacuous premises, and so forth. The strategy sketched in the last paragraph attempts to disguise two patterns as one, and it does so by making distinctions that we take to be artificial and by ignoring similarities we take to be real. Thus the obvious way to meet the challenge is to demand that the predicates occurring in the schematic sentences [and playing other critical roles] all be projectable predicates of the language in which K is formulated. (Kitcher, 1989, 482)

Unfortunately, this answer loses one of the advantages of Kitcher's account that was most attractive to us here: the prospect of a unified treatment of mathematical and physical explanations. To the extent that we have any grip on the idea of projectibility at all, it has only been specified with reference to empirical predictions, and it remains to be seen how we should extend the idea to mathematical contexts.[62] So Kitcher's account of unification is incomplete: we need to supplement it with an account of how the range of acceptable substitutions is delineated in practice. This, of course, gets us back to our main theme, of how in practice we ascertain the methods of organization we will take as preferred and "natural".

It will give us a foothold if we turn to mathematical cases that support Kitcher's analysis in an interesting way, though only up to a point. It is true that uniformity of the kind he indicates has been sought, often quite self-consciously in the history of mathematics, science and even engineering. But there is always a bottom line: if the uniform patterns don't make things easier, if they don't support further discoveries, if they don't provide satisfying diagnoses, in short if they aren't fruitful, then they are set aside. A particularly illuminating example of this is embodied in the principle of duality in projective geometry and graphic statics in the nineteenth century and into the twentieth.

In the mid-nineteenth century, the development of projective geometry is strikingly close to the pattern Kitcher describes.[63] After extending the

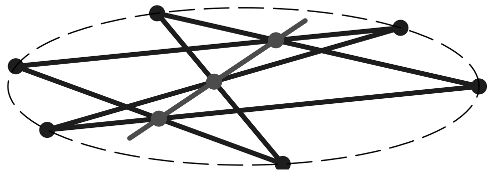


FIGURE 3. Pascal's Theorem.

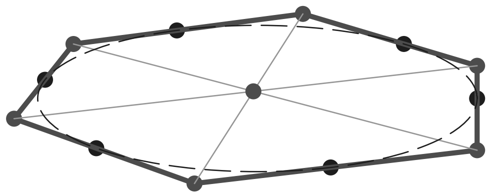


FIGURE 4. Brianchon's Theorem.

Euclidean plane with points at infinity, reciprocal relations reveal themselves in the theorems of the extended system. It is possible to pair up vocabulary ("point" – "line"; "passes through" – "lies on" etc.; with induced pairings like "circumscribes"-"inscribes" etc.) so that given any theorem in projective geometry, the result of uniformly substituting each expression for its partner is also a theorem. This can yield quite striking pairs, as in the canonical examples of the Pascal and Brianchon theorems (see Figure 3 and 4):

Pascal's Theorem: Given a hexagon inscribed in a conic section, the points at which corresponding sides intersect all lie on a single line.

Brianchon's Theorem: Given a hexagon circumscribed about a conic section, the lines on which corresponding vertices rest all pass through a single point.[64]

This fact induces a quite general duplication of reasoning, as the substitutions also transform proofs into proofs, so that a single schematic argument delivers two proofs in one. This feature of general geometric reasoning became a fundamental aspect of the discipline in the late nineteenth century, to the extent that the standard convention in elementary textbooks and advanced research monographs alike was to write arguments in parallel columns to display the dual arguments. This was an especially compelling example because the observed logical affinities among arguments were seen as more than just remarkable but idle epiphenomena. Duality came to be a cornerstone of general methodology for many pure and applied geometers in the mid-nineteenth century and subsequently. Producing theories that would issue in such dualistic patterns was seen by some geometers working in the area as a goal guiding the formulation of mathematical theories.[65]

Projective geometry and cognate fields, structured in this way in conformity to duality principles, are striking exemplifications of Kitcher's account.[66] But even in this highly favorable case, the issue is more complicated. The self-conscious focus on producing general schemata is not an unconditional goal. Once again it is important not only that the properties unify but that they are otherwise the "natural" or "right" ones.

A useful illustration here is the application of projective geometry to structural design, in the so-called "graphic statics" developed in the late nineteenth century by Maxwell and Culmann and developed further by Cremona. I will consider just the aspects of this rich history that are necessary to the issues we're addressing here. Fine details are available in secondary literature.[67]

Graphic statics was a theoretical formulation of techniques for analyzing engineering problems of structural reliability and strength of materials. Projective geometry is taken as the basic framework in the most ambitious and systematic formulation, presented in Culmann's *Die Graphische Statik*.[68] The problems involved a range of forces and pressures on hypothetical structures. One crucial early breakthrough from Maxwell (1864) was a technique for analyzing systems of forces in terms of *reciprocal diagrams*.[69] These worked by exploiting dualities to effect simplifications in the representations of forces acting on a structure. We won't need any further details here, except the key observation that this reciprocity allows complex stress diagrams to be reconfigured into diagrams that are easier to analyze, and which often display explicitly the desired information about stresses. Here is a relatively simple illustration:[70] (See figure 5.)

Say that the figure on the right represents a bridge in equilibrium with downward forces W_1, W_2 and W_3 and upward reactions A and B. The lengths of the lines represent the magnitude of the forces and the direction of the arrows mirror the direction of the forces. The stress diagram is the closed figure on the left. It will suffice for our purposes here to consider just one application to illustrate the technique. We can obtain the force on the particular support marked s by measuring the corresponding line s' on the stress diagram. One thing we can learn even without measuring is that the line t'

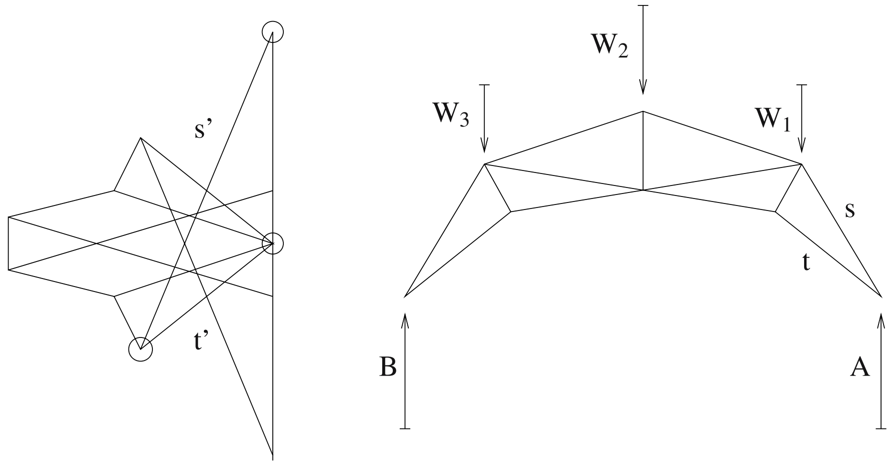


FIGURE 5.

corresponding to t in the original diagram is far shorter than s', reflecting the fact that the stress on s is much greater than that on t. (s' and t' are the lines between the circled points.) So if s and t are made of the same material and have no flaws, and the structure collapses because of s buckling, we can read off of the diagram a (defeasible) answer to the question "Why did s buckle rather than t?".

This example is satisfying from Kitcher's perspective not just because of the global role of duality in shaping the framework.[71] Note also that the preferred theoretical formulation doesn't distinguish between physical and mathematical situations. The account of (for instance) the stability of a configuration is the same whether we are concerned with an abstract vector sum or the stability of an actual bridge. It is of course an empirical question what frameworks are adequate representations of given physical situations, but solely mathematical/geometric criteria came into play in choosing which among the many equivalent frameworks is to be preferred as the representation of decomposition into component forces.

This yields a compelling example in which the theoretical virtues that led to the choice of a mathematical framework (and that consequently inform the ideas of understanding and explanation that the framework induces) influence the explanation of physical events as well. The fact that a rooftop can hold ten inches of dry snow, or that a cantilever bridge collapses, will be explained not just by appeal to familiar physical properties (the weight of the snow, the thickness of the roof, the weight of the girders, the temperature,...) but also by properties of the structure that are filtered through the theoretical framework (the distribution of the load, the shear stresses at critical points. . .). What counts as an explanation here is shaped by global aspects of the theory, rather than merely by a picture of individual events with simple causal dependencies. So for example, the most renowned cantilever bridge failure of the time – the Quebec bridge collapse of 1907 – was set in motion when a single overstressed girder buckled.[72] The explanation of why that girder buckled of course required a grasp of not only the overall downward force and the strength of the materials but also an account of how the cantilever frame distributed the downward force through the structure as a whole, so as to indicate why precisely *that* girder was the one to go.[73]

For the last two decades of the nineteenth century and well into the twentieth, this was the dominant approach for studying the strength and stability of engineering structures.[74] Subsequently it was dislodged from its dominant position for a battery of reasons, among them that engineers came to confront problems of greater complexity than the graphic approach could easily address, and (more recently) because computers became more central to engineering practice.[75] However, our concern here is to address *why* graphic statics held sway over its analytical rivals during the time it *did* hold sway. Among the advantages that were noted, two are of special interest here: a) the theoretical formulation borrowed the fruitfulness of the general projective geometry that informed the treatments of Culmann and Cremona.[76] b) the visual representations in diagrams systematically conveyed the information in particularly vivid and effective ways; among the cited advantages were that the visual arguments make mistakes easier to catch,[77] that the visual presentation is easier to learn and teach without extensive mathematical training[78], and most importantly the graphic approach has that mysterious but crucial theoretical advantage: it just makes things easier.[79] Here too, the latter preference cannot be just shunted off into an incidental "context of discovery" since it shapes the terms in which justifications are given. The graphic framework was not set aside after it did the work of spurring creativity.

This brings us to a juncture similar to the one we reached above in the discussion of Friedman's account. It turns out to be true that dualities of the sort Kitcher isolates in his account of understanding have been taken to be contributors to the fruitfulness of mathematical formulations of problems. But even in the favorable case we are looking at, there is more going on. The value of the unifying account is not given *merely* by the fact that there are a variety of shared patterns (though in this case the sharing of argument patterns is important). The status afforded to graphic statics as the preferred way to address structural problems (during the period when it was so regarded) didn't depend exclusively on how well the dualities of the background projective framework effected unification in Kitcher's sense. In addition – as the gerrymandering problem would lead us to expect – some additional constraints on the formulation have to be in place for the unified patterns of argument to be seen as worthwhile.

Any case like this will be complicated, with many factors involved, but we do have one foothold in the case of graphic statics since *one* of the principles taken to govern the theoretical formulation is stated unambiguously: it was taken to be a selling point that the representations of forces are *visualizable*. The interaction between visual representation and conceptual organization can be intricate. In particular, a survey of the textbooks of the time gives an interesting glimpse at a fact that will occupy us in the next section: some textbooks which were explicitly directed at laying out the valuable features of graphic statics didn't contain a single diagram.[80] The graphic framework remains valuable even if we do not directly exploit diagrams or vision *at all*. One reason for this is that rules for vector addition of forces is built into the principles for manipulating and interpreting the diagrams. At the time, the abstract versions of the ideas of vector space and vector sum were still imperfectly worked out and not well understood. Naturally the theoretical value of studying forces as vectors subject to rules of vector composition and decomposition goes well beyond the value that derives from the fact that they can be represented in diagrams. That is: some of the value of the visual presentation derives from features of the organization of information that are shared by the diagrammatic presentation and some analytic presentations. For these advantages the visual presentation – the fact that we can *see it* in the way we do – is incidental. This is true more generally of the projective framework that forms the background of the Culmann – Cremona treatment of graphic statics. Even in the abstract analytic presentation given by homogeneous coordinates, where diagrams or other visual representations need not be used, patterns of "geometric argument" are often judged to be especially acute.[81]

Let's review the state of play before moving on. We began with the challenge to clarify what was philosophically and methodologically at issue in the nineteenth century revolution in mathematical thought initiated by Riemann. We claimed a foothold in section 2 with the observation that one type of success is recognized in both mathematical and scientific reasoning, and counted as a contribution to understanding: Unifying apparently disparate phenomena within a single homogeneous framework. What was at issue was clarifying what the valuable unifications should be taken to be. The two candidate analyses we considered turned out to be at best incomplete, needing supplementation by an account of what reasons were given for basic principles and axioms and how preferred ("non-gerrymandered") categories were arrived at. This requires some appeal to an idea of "natural" categories and principles, but contrary to what was argued by Friedman, we do not have to set this aside as "non-objective" if the choice of candidate axioms and principles is based on good reasons. (This shifts the question of the objectivity of the "natural" categories to the question of what the supporting reasons are.) In classical projective geometry, especially in its application in graphic statics, we found an example exemplifying, on a smaller scale, one principle informing the hard case (Riemann's complex analysis) that we set out to approach. In some cases, a contributor to an assessment of the "naturalness" of a framework and its basic categories is that the arguments and analyses of the framework can be visualized. Finally, we noted that some of the advantages of the visualizable frameworks we considered persisted even when they were formulated in non – diagrammatic terms, as systems of abstract analytic geometry or vector addition. This helps narrow our search: we need to get a better sense of how this sort of indirect connection to vision can inform our choice of theoretical frameworks.

5. ARTIN AND AXIOM CHOICE: "VISUAL REASONING" WITHOUT VISION

Implicit in sections 3 and 4 was this answer to the problem of identifying "gerrymandered" predicates: it may well be that there is no general *a priori* principle that will divide categories into natural and artificial. But the absence of a general *a priori* answer doesn't indicate that everything is caprice: in particular cases, good reasons can be given for the choice of one framework as preferred. We also considered one basis that is cited in at least some cases: a framework can be preferred if it has a desired kind of connection to visual representation.

A relatively tangible example of the choice of a framework is given by the choice of axioms for a mathematical theory, which motivates the case study of this section: the choice of axioms and basic concepts within Artin's *Geometric Algebra* (Artin, 1957). Before engaging the details we need some ground – clearing concerning the use of the word "axiom". Contrary to what the expression may have meant in the past, in mathematical practice today "axioms" are not "self – evident truths neither needing nor admitting proof." Most of the axioms we'll see here are not self-evident, nor are they treated as unprovable.[82] What makes them the right candidates for axioms is that a good case can be made that they divide up the topic in the proper way.

Artin's volume has a polemical aspect. He is striving to revive a "geometric" style of presentation, as he notes in his preface. (This is not just a casual remark: the preferences it indicates are followed out consistently throughout the volume.)[83]

> Many parts of classical geometry have developed into great independent theories. Linear Algebra, topology, differential and algebraic geometry are the indispensable tools of the mathematician of our time. It is frequently desirable to devise a course of geometric nature which is distinct from these great lines of thought... (Artin, 1957, vi).

The specific orientation this stance involves is indicated later when Artin indicates how an algebraic result should be restructured. Artin is discussing the isomorphism connecting the ring of homomorphisms of an n-dimensional vector space (over a field K) into itself and the ring of nxn matrices (with entries from K). This isomorphism introduces two different modes of presentation, a fact upon which he comments as follows:

> Mathematical education is still suffering from the enthusiasms which the discovery of this isomorphism has aroused. The result has been that geometry was eliminated and replaced by computations. Instead of intuitive maps of a space preserving addition and multiplication by scalars (these maps have an immediate geometric meaning), matrices have been introduced. From the innumerable absurdities – from a pedagogical point of view-let me point out one example and contrast it with the direct description.
>
> Matrix method: A product of a matrix A and a vector X (which is then an n-tuple of numbers) is defined; it is also a vector. Now the poor student has to swallow the following definition:
>
> A vector X is called an eigen vector if a number λ exists such that $AX = \lambda X$.
>
> Going through the formalism, the characteristic equation, one then ends up with theorems like: If a matrix A has n distinct eigen values, then a matrix D can be found such that DAD^{-1} is a diagonal matrix.
>
> The student will of course learn all this since he will fail the course if he does not.
>
> Instead one should argue like this: Given a linear transformation f of the space V into itself. Does there exist a line which is kept fixed by f? In order to include the eigen value 0 one should then modify the question by asking whether a

> line is mapped *into* itself. This means of course for a vector spanning a line that
> f(X) = nX.
>
> Having thus motivated the problem, the matrix A describing f will appear only for a moment for the actual computation of n. It should disappear again. Then one proves all the customary theorems without speaking of matrices and asks the question: Suppose we can find a basis of V which consists of eigen vectors; what does this imply for the geometric description of f? Well, the space is stretched in the various directions of the basis by factors which are the eigen values. Only then does one ask what this means for a description of F by means of a matrix in terms of this basis. We have obviously the diagonal form. . . .
>
> It is my experience that proofs involving matrices can be shortened by 50% if one throws the matrices out. (Artin, 1957, 13-14).

There is much to comment on here. First, a basic observation: The structures of matrices and of homomorphisms *are* isomorphic but the differences between the structures are not, in this case, dismissed. Consider the opening question Artin floats (sticking to just two dimensions, for simplicity). Given a linear transformation of the plane with two independent eigenvectors is there a way to change the basis of the plane so that the transformation relative to that basis is representable as a diagonal matrix? There are two different ways to arrive at an answer. Artin's preferred approach sets up a visualizable situation and – only when needed – appeals to an algebraic representation of it. The second deals throughout with the computations that can be performed in the algebraic representation.

My informal canvassing has turned up the expected result that almost everyone is of one mind with Artin that the first of these approaches is preferable.[84] As noted earlier, this is echoed in print. In one example Hughes and Piper (1973) speak of Artin's framework as "the proper setting for many problems in linear algebra." (p. 285) There are many reasons for this. First of all, as a pedagogical observation most people find his preferred approach much easier to grasp on first exposure, as Artin observes. There are also gains in the most elementary kinds of economy like proof length, if Artin's observation that proof length can often be shortened "by 50%" is correct, as I will take it to be. These are important advantages of Artin's perspective that need to be taken seriously. But there are deeper, more systematic advantages as well, which will come out as the picture unfolds in more detail.

However, the point cannot be that pictures are an essential part of "geometric" presentation, since Artin has hardly any![85] [86] There are only 6 diagrams in a book of over 200 pages (all but one of these are meant to clarify the axioms to be considered in a moment.)[87] Rather, we find that a "geometric language" (p.26) which facilitates certain "intuitive pictures" (p.26) and visual handwaving is developed and fleshed out with axioms, but the power of the framework lies principally in its systematic theoretical fecundity.

The core concept of the approach is that of a *transformation* (or *symmetry*): the idea of moving a point from one position to another, thereby tracing a line. (This perspective is actually much closer to some "philosophical" analyses of space and intuition than it might appear to those unfamiliar with the tradition, but since this point will take us too far afield, I'll leave it to be developed in other work.[88]) Two axioms are set down to ensure the basic structure of parallelism: I) given two distinct points there is a unique line connecting them II) Given a point P and a line l, there is a unique line parallel to l passing through P. Also there is an axiom that states that there are three distinct non-collinear points. Given this, as Artin puts it "We can hope for a 'good' geometry only if the geometry has enough symmetries." (Artin, 1957, 58). Hence the remaining axioms posit the existence of symmetries, where these are *dilatations*: mappings σ such that given a line l, the image $\sigma(\ l\ )$ is parallel to l.[89] A sub-class of the dilatations is distinguished: a *translation* leaves no point fixed. (That is: τ is a translation if there is no point P such that $\tau(P) = P$). The only dilatation leaving more than one point fixed is the identity, which leaves everything where it is, so every non-degenerate dilatation that isn't a translation or the identity leaves exactly one point fixed. (Among the reasons to regard these as reasonable choices as basic ideas are algebraic: the dilatations form a group with the translations as a normal subgroup.)

Thus we have the general framework: there are points and lines, and symmetries mapping lines into parallel lines. The axioms will take the form of statements as to the existence and properties of symmetries. Given what we are looking for, these are the choices that suggest themselves right away:

<u>Desargues Axiom 1:</u> Given any two points P and Q there is a translation P such that $\tau(P) = Q$.

<u>Desargues Axiom 2:</u> For any points P, Q and R, there is a dilatation that holds R fixed and such that $\sigma(P) = Q$.[90]

Just by inspection we can say that these axioms certainly *seem* natural, in this framework. A deeper point, which comes out when we consider the reasons for regarding the axiom as a good axiom candidate, is that these axioms *ought to* seem natural: it is a strength of the framework that these axioms come out as natural – seeming as they do. First, though, it should be noted that the framework itself – representing the subject in terms of transformations of objects – has a compelling rationale of its own. It would require a separate paper – a long one – to even begin to develop the manifold ways that it has proven to be valuable to formulate a subject in terms of transformations and invariants. From physical theories of space and time to classifications of general geometries in the Klein program, to Galois theory and the theory of Lie groups, and untold other areas, pure and applied, this framework has shown itself to be a good one to choose, and the Artin framework of geometric algebra inherits these bona fides.

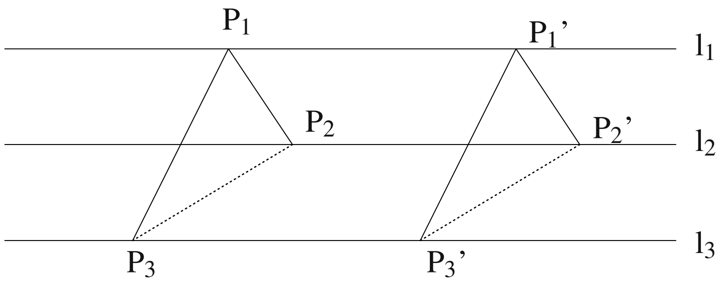
This point is worth emphasizing in connection with efforts to cut through the gerrymandering challenge by emphasizing elementary syntactic features of predicates – that they are "disjunctive" for example – as reasons to exclude them. This example illustrates a fairly general moral: whether or not something admits of a simple expression is going to depend upon global features of the framework it is studied in. In this particular case, the broader framework of studying geometries in terms of symmetries makes the Desargues axioms simple and immediate; the fact that we should treat especially seriously things that look simple in this particular framework is not justified by any *a priori* argument employing purely philosophical or linguistic criteria or appeals to principles of basic metaphysics. The justification of the framework has to appeal to the details of the subject – matter, including our amassed experience with frameworks of this type.

I'll return to the axioms themselves. For orientation it will be helpful to consider their classical forms:

(Classical) Desargues axiom 1:

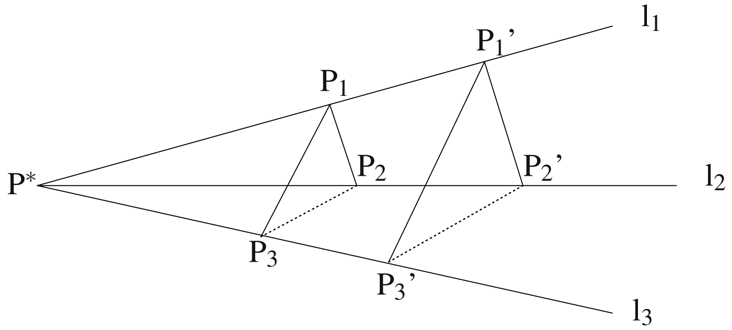
If l_1, l_2 and l_3 are parallel lines in the (affine) plane and P_1, P_1', P_2, P_2', and P_3, P_3' be points on l_1, l_2 and l_3 respectively. Say that the line P_1P_2 is parallel to P_1'P_2' and P_1P_3 is parallel to P_1'P_3'. Then P_2P_3 is parallel to P_2'P_3'.

Diagram:



(Classical) Desargues axiom 2:

If l_1, l_2 and l_3 are lines in the (affine) plane intersecting in a point P* and P_1, P_1', P_2, P_2', and P_3, P_3' be points on l_1, l_2 and l_3 respectively. Say that the line P_1P_2 is parallel to P_1'P_2' and P_1P_3 is parallel to P_1'P_3'. Then P_2P_3 is parallel to P_2'P_3'.



There are canonical reasons why these are good axioms.[91] A simple point is that the first Desargues axiom is equivalent to the uniqueness of a vector sum.[92] A more intricate consideration derives from the structure of familiar school analytic geometry. It is possible to assign coordinates to any collection of objects and introduce functions on those coordinates, so long as we are not too picky about what properties the coordinates and functions themselves have. If we assign coordinates in a general way to the objects of our geometry, the first Desargues property is equivalent to the statement that we can introduce operations of plus and times on the coordinates so that the equations of lines will be the familiar linear equations from school: $ax + b = y$.[93] A further, distinct consideration arises from the relations between planes and space: (relative to a reasonable axiomatisation of the geometry of space) the second Desargues axiom is equivalent to the thesis that the plane can be embedded in three dimensional space. The second Desargues axiom corresponds to further algebraic conditions on the addition and multiplication (the distributive law for the multiplication and addition operations on the coordinates) to form a skew field. (i.e. a field minus commutativity of multiplication)[94] These reasons for regarding the Desargues axioms as distinguished draw on the fact that they are "robustly" central, in that when we reformulate our theory in other terms, with quite different structures and motivations, the Desargues axioms in their new forms remain rationally defensible as natural axiom candidates.

A further consideration that tells in favor of the Desargues theorems as axiom choices is the interest and richness of the divides they mark. Geometries in which the Desargues theorem *fails* have proven in practice to be a class of uncommon interest, sustaining extensive, interesting programs of research. On the positive side, the theorem itself is of considerable intrinsic interest, both for the consequences it supports and for the depth and intricacy the theorem reveals under more detailed study.[95] There is more that can be said to support the claim that the Desargues theorem really does deserve to be granted a distinguished status as axiom, but what has been said so far will suffice to illustrate the key point: Artin's choice of transformations as a basic category and of Desargues' Axioms as basic principles can be rationally defended by appeal to a range of different considerations. Even in this context, where empirical predictions are not directly in the offing, the distinction between natural and artificial/gerrymandered properties can be objectively made out.

In connection with the issues we have been concerned with, here is where we have arrived:

i) The theory developed by Artin does have a deep and important connection to visual reasoning but
ii) as a means of organization of the subject – matter it has value independent of the connection to vision and furthermore
iii) The basic details of the framework – its fundamental concepts and its axioms – admit of extensive justifications. That something is "basic" or an "axiom" is not bedrock at which the spade of explanation and argument is turned. Some of the reasons for shaping the framework as it is shaped may seem to be immediate brute responses or appeals to the brevity and simplicity of the expressions used (for example: the Desargues axioms in their symmetry forms just "look natural" in this context, and the statements don't invoke "disjunctive" properties or other funny looking constructions) while others reach far afield even to applications (for example: the "rightness" of the framework taking transformations as basic extends to physics).

Of course, these judgements are defeasible. We might decide, when we come to learn more about the subject, that this framework is not the right setting for the problems addressed within it. But of course, the fact that in a different epistemic situation we would call different frameworks and categories "natural" for different reasons doesn't impeach the reasons we actually have, in the epistemic situation we are actually in, for our actual decisions as to what we regard as natural and what we don't. Much of the reasoning that goes into this decision is "quasi – empirical": among the information that the decision about what is a natural formulation or a good axiom choice draws on is information about what is fruitful, about what works and what doesn't. This makes it especially unlikely that a purely philosophical criterion of "gerrymanderedness" will suffice to exclude artificial, Goodman style properties. Our decisions about how to formulate the mathematical theories that we apply don't rest on *a priori* philosophical bedrock, and it appears unlikely that any *a priori* "rational reconstruction" could reproduce our best mathematical practice on abstract philosophical or logical grounds alone.

6. SUMMARY – THE "NEW RIDDLE OF DEDUCTION"

The paper began with two related questions. What philosophical niche can we find for a discussion of what was at stake in Riemann's revolution in mathematical method? What significance for general methodology should we grant to the role of visual representation as a mode of organization coloring some mathematical reasoning? We've arrived at a kind of mathematical analogue to Goodman's problem, but without the direct connections to causation and empirical prediction that are often taken to ground answers in the more familiar gruesome cases. To find a place for unification as a scientific and mathematical success, as it is treated in practice, we need to clarify certain qualitative features of theories and the properties they deal with. Which classes and theories are homogeneous and which are heterogeneous? Which classifications and properties are natural and which artificial? We need to be clear about what sorts of considerations are brought to bear, in deciding what formulations are the right ones to use. The conclusion suggested here, especially as exemplified in the case of *Geometric Algebra,* is that these distinctions are, in practice, made out in a way that is rationally justifiable, but also that they appeal to details of mathematical and scientific practice that are more involved and case-specific than philosophical accounts of explanation as unification have appreciated. This suggests that we reorient our conception of the methodology of mathematics in a "bottom up" direction: we can't hope to understand what mathematics contributes to our overall view of the world by shuffling philosophical abstractions alone; we need to get our hands dirty with the details of mathematics as it is done. To invert a famous Kripkean slogan, in this case there is also no philosophical substitute for mathematics.

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NOTES

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[1]This negative point – that there is no principled general delineation of the "essentially general" predicates – is argued in Railton (1993). This paper may be seen as a follow – up: if we accept that there is no general *a priori* account that will segregate the "essentially general" from the "gerrymandered" predicates, our attention naturally turns to working out the details in specific examples, with an eye to identifying defeasible heuristics and shared patterns that may be displayed in a range of cases. We can learn a great deal so long as we don't obsessively cling to an unrealistic picture of how simple a "philosophical" account of uniformity is allowed to be.

[2]I'm not, of course, suggesting that this idea of "fruitfulness" is clear or sharply defined, or even that it is a single uniform phenomenon, but only that it is a consideration that is in fact is appealed to in practice (under a variety of names). Explaining more clearly what "fruitfulness" amounts to is of course one of the jobs that has to be done.)

[3]The label "conceptual" is adopted for Riemann's innovative style in Laugwitz (1999) among others. The broader change in mathematical style that emerged in Göttingen in the mid-nineteenth century is explored with special reference to Dirichlet in the superb philosophical essay Stein (1988).

[4]The core details of the split in the approach to complex analysis have been well-explored by historians in recent years. An illuminating presentation of the Riemann stance is in Laugwitz (1999). On Weierstrass, Laugwitz (1992) is a helpful counterpoint. Good presentations of both sides of the split are Bottazini (1994) and Neuenschwander (1980) and (1981). Currently Jeremy Gray and Umberto Bottazini are carrying out joint work which promises to shed further light on the situation. I explore some of the philosophical ramifications of this split, and its historical connections to the development of the foundations of mathematics, in a manuscript in progress.

[5]Contemporary texts in complex analysis tend to be unreflectively Riemannian in outlook. One historically sensitive text that is self-consciously Riemannian is Remmert's textbook of function theory. (Remmert (1991) and (1998)). Textbooks that are avowedly Weierstrassian in general outlook are harder to find, but they do exist: Abhyankar (1964) is one example. The fact that this division of styles has been robust enough to persist this long reinforces the point that more than merely transient "spurs to discovery" are at issue.

[6]By contrast, the historical details of the events constituting this contrast have been reasonably well – explored in recent years, and current research promises to push our historical understanding even deeper. In my own work I am exploring some of the philosophical overtones of the mathematical developments (with special reference to Frege) (see Tappenden (2001c)). The Frege connection appears because Frege was trained in the Riemann tradition (then a minority stance) and continued to work in that vein in his subsequent teaching and research. This can be seen to have colored his methodology in several respects, such as his stance on the "Caesar problem", his definition of magnitude, and his regular criticism of Weierstrass.

[7]I am sure that only the loosest family resemblance unites all the things that we call "understanding". I am certainly not setting out to provide anything like an *analysis* of "understanding" in the sense of a set of necessary and sufficient conditions such that all and only persons who satisfy those conditions understand something. However, even in the absence of an analysis of the notion of understanding, it is possible to isolate aspects of what we commonly associate with the idea, and work out their significance for epistemology and logic.

[8]I do *not* mean to suggest that the visualizability of Riemann surfaces is the sole advantage, or the most important one. It just happens to be the one feature of Riemann's approach that I am addressing here. In fact, it is one aspect of the revolutionary character of Riemann's research that there have been, historically, so many different ways of cashing out what is important in it. Just to consider the point at issue here: some Riemann students – notably Dedekind – strove to purge Riemann's results of their visual character, while others (Felix Klein, the Italian tradition of algebraic geometry, much contemporary theory of functions of one complex variable) embraced the visual character and strove to exploit it. (For an especially forceful discussion of the importance of visual intuition to this mathematical tradition see Segre (1904) (especially p. 454–455)).

[9]So as not to leave the impression that these issues pertain solely to the nineteenth century, it is worth mentioning another example exemplifying the themes discussed here: the concept of scheme in algebraic geometry. Though scheme theory is an extremely compelling example in the current connection, and I will consequently refer to it from time to time, it is also complicated enough that I will have to defer a sustained treatment for some later part of the project, when Colin McLarty's work on Grothendieck is ready to circulate. (Why do today what someone else is going to do tomorrow!) However, to reinforce the connection of the issues discussed here to contemporary mathematics as well as that of the nineteenth century, I'll carry on a running commentary of scattered remarks about schemes in the footnotes.

Among the many reasons why scheme theory is especially interesting here is that one of its acknowledged virtues is that it supports a unification of number theory and algebraic geometry. It is an interesting question of methodology whether this theoretical unification is analogous to the benchmark unifications in physics, like Maxwell's or Newton's or in any important way different. In this connection it is worth noting that Grothendieck himself reportedly had equally grand hopes for the theory of "motives", envisioning a potential unification of Galois theory and topology. (cf. Cartier (2001, 405)).

[10]For smooth exposition I'm (inessentially) fudging some distinctions between Weyl's essay and my project, but it is worth a footnote to avoid leaving a misimpression. In my studies of the mid-century Göttingen revolution in mathematical methodology I have emphasized Riemann and his successors. Weyl (like Stein (1988)) emphasizes Dirichlet, Riemann's predecessor as professor at Göttingen; the reference to the Dirichlet principle could indeed be taken as a bit of a jab at Riemann's lack of contemporary rigor. For the issues I am most concerned with, Riemann is a better representative, and he is more important as a figure in Frege's intellectual environment. (Or at least he can be more easily documented to be a significant figure in Frege's Jena context.) But both figures represent, in different ways, the "conceptual" reorientation: Dirichlet was far more rigorous at the level of detail in argument, while Riemann's contributions to the stylistic innovations were more profound (full of what Ahlfors calls Riemann's "cryptic messages to the future.") though less rigorous. The work of both exemplified, in different ways, the style that in retrospect was a critical revolution laying the support for the twentieth century.

That it isn't distorting to take Weyl's words about Dirichlet's mathematics as remarks about Riemann's is borne out by the subsequent discussion in Weyl's essay: most of the mathematics he uses as his illustrations of contemporary work traces back to Riemann rather than Dirichlet.

[11]I speak about "algebraic approaches" in the plural here because Weyl runs together here what I count as two very different traditions and styles with the label "algebraic": a computational tradition of Kronecker and a distinctive "structural" algebraic approach exemplified by Dedekind. There is no reason, in the present essay, to refine Weyl's classification further, but I don't want to leave a misimpression.

[12]A useful illustration of this point is the article Harris (1992). There the development of algebraic geometry in the twentieth century is framed by the observation

that "progress in algebraic geometry is measured more by its definitions than its theorems." (Harris, 1992, 99)

[13] I'm not suggesting fruitfulness is the only relevant consideration that bears on whether a formulation will be taken as "natural". One point of section 5 is to illustrate just how intricate the reasons for such judgements of "naturalness" can be.

[14] These remarks from Zariski give a typical statement of the mathematician's pragmatism in this regard:

> There is no doubt that the introduction of the concept of "schemes" due to Grothendieck was a sound and inevitable generalization of the older concept of "variety" and that this generalization has introduced a new dimension into the conceptual content of algebraic geometry. What is more important is that this generalization has met with what seems to me to be the true test of any generalization, that is, its effectiveness in solving, or throwing new light on, old problems by generalizing the terms of the problem (for example: the Riemann-Roch theorem for varieties of any dimension).... (Zariski, 1978, xvii)

[15] I do not know of any systematic studies of fruitfulness as a guiding criterion in mathematics or elsewhere, but the basic observation has some antecedents. Frege makes some fragmentary but rich remarks in *Grundlagen* which tie his logic and his account of "extending knowledge" to what he calls "the truly fruitful concepts". (I develop this observation about Frege in my (1995).) Thomas Kuhn observes that a crucial guide in practice to theory choice in the natural sciences is that the theory be "fruitful of new research findings: it should, that is, disclose new phenomena or previously unnoted relationships among those already known." (1977, 322) I don't know anywhere that Kuhn, or anyone else, expands on this bare observation. In the paper cited, Kuhn does not expand on his observation beyond the footnote remark: "The last criterion [listed], fruitfulness, deserves more emphasis than it has yet received." (How true.)

[16] (Weyl, 1955, VII) My attention was originally drawn to this passage by (Wilson, 1992, 111). It should be noted in connection with these words that in context Weyl is not endorsing them unequivocally. Rather he is describing an attitude he expressed when, as a young man he wrote Weyl (1913), which the older Weyl spoke of as revealing a certain youthful naiveté. "Even more than the text, the enthusiastic preface betrayed the youth of the author." (p.VII)

[17] Once again, the history detailed in Harris (1992) is a useful illustration. Throughout the twentieth century there was a sequence of better and better candidates for the natural context for algebraic geometry. The reasons for one candidate succeeding another was never merely that the preferred candidate "seemed right" but that it in fact facilitated the solution to key problems.

[18] One simple example is the use of homogeneous coordinates/projective space ("the unifier" in the words of Clemens (1980, 5) in the study of curves, especially

over the complex numbers ("the great unifier" – (Clemens, 1980, 7)). This is accepted as the right context for a range of problems, and it does indeed bring out forcefully many properties of (for example) conic sections. But it requires work to see this; it is not obvious at first sight. People who have had a standard North American mathematical education find ordinary Cartesian coordinates over the real plane so natural as to be almost inescapable; to come to see complex projective space as the natural context requires re-education.

[19]See for example the venerable Bridgeman ((1938, 37 and *passim*) and (Campbell, 1919/1957, 113 and *passim*)). I am grateful to John Norton for pressing me on the "explanation/understanding as reduction to the familiar" line. I do think that in an important range of cases we count something as an explanation, or as contributing to understanding, when it effects a reduction to the familiar, but the cases I am looking at aren't like that.

[20]It will be useful to give another example of broad motivating remarks concerning the concept of scheme. Though nothing can substitute for an analysis of the use of the concept in practice, some examples of what those mathematicians who use the concept say about its importance may serve as a temporary buffer until such an analysis is worked out. Here is how David Mumford puts it in a retrospective introduction (in 1988) to the publication of his by then already classic 1960's *samizdat* introduction to schemes in algebraic geometry. This contains a (long) book-length defense of the thesis that scheme theory provides the "natural language" of algebraic geometry. The reasons on which the defense rests include the ability of the framework to cleanly express results in a variety of different other conceptual frameworks, its connection to "geometric intuition", and its ability to support new and very exciting results":

> It may be of some interest to recall how hard it was for algebraic geometers, even knowing the phenomena of the field very well, to find a satisfactory language in which to communicate to each other. At the time these notes were written, the field was just emerging from a twenty-year period in which every researcher used his own definitions and terminology, in which the "foundations" of the subject had been described in at least half a dozen different "mathematical languages". Classical style researchers wrote in the informal geometric style of the Italian school, Weil had introduced the concept of specialization and made this the cornerstone of his language, and Zariski developed a hybrid of algebra and geometry... But here was a general realization that not all the key phenomena could be clearly expressed and a frustration at sacrificing the suggestive geometric terminology of the previous generation.
>
> Then Grothendieck came along ... [with] the new terminology of schemes as well as with a huge production of new and very exciting results. These notes attempted to show something that was still very controversial at that time: that schemes really were the most

> natural language for algebraic geometry and that you did not need to sacrifice geometric intuition when you spoke "scheme". (Mumford (1988) p. V – VI)

The attitude that "scheme" was a thematically proper generalization and that the test of this was effectiveness in problem solving was echoed even by members of the old guard, as indicated by the quote from Zariski given in footnote 14.

[21]One illustration, especially pertinent here, appears in Steiner (1978) when he dismisses the suggestion that something counts as a mathematical explanation only if it can be visualized, on the grounds that such a condition would make mathematical explanation "subjective". (p. 139) I think he is right to regard the proposed criterion as inadequate, but here I'll be concerned also to spell out some ways that visualization in mathematical practice is more intricate and systematic than it might seem at first view. (This is in accord with another remark of Steiner's, which is that any "satisfactory theory of mathematical explanation must show why [the "explaining is making visual" thesis] is plausible." (p. 139) I am indebted to Bertrand Guilliou here.

[22]Sometimes "pragmatic" is also used as a pejorative with the connotation "on to the 'not philosophically interesting' scrap heap with this one". So for example in his interesting account of the contributions of asymptotic explanations to our understanding of physical systems, Batterman (2002, 44) uses 'pragmatic' to frame a point with affinities to the one I am making in the text.

[23]One refinement is needed here. I'm not suggesting that no cases admit of analysis in "objective" terms. In some cases the advantages of a particular formulation can be analyzed in terms which are indisputably independent of psychological peculiarities of human reasoners. A paradigm of this sort of work is the analysis in Pratt and Lemon (1997). There certain advantages of diagrammatic reasoning are analyzed in the tangible terms of the computational complexity of algorithms. This work is extremely revealing and interesting, and I look forward to learning from further research of Pratt, Lemon and their collaborators. The attitude motivating the current work is not incompatible with that work, but rather complementary, studying some aspects of the choice of theoretical frameworks (especially in connection with the potential for visualization) that don't obviously admit an analysis in tangible complexity – theoretic terms.

(Clearly there are interesting cases where an analysis in terms of computational complexity is not going to help us much, even when what we gain are advantages in facilitated *practical* computation. One simple example is the use of homogeneous coordinates in computer modeling. The advantages of homogeneous coordinates over Cartesian coordinates are, I gather, well-established in practice, despite the (non-robust) complication of an extra parameter and the initial unfamiliarity of the framework (for most students). However, it is unlikely that computational complexity theory will support an analysis of the advantages of homogeneous coordinates, since the transition from homogeneous to Cartesian coordinates and back can be accomplished by operations that are insignificant from a complexity-theoretic point of view. And indeed, when advantages are explicitly noted for homogeneous coordinates in visual modeling they are qualitative advantages rather than complexity theoretic ones. (For example: the existence of dualities or the simplification of theoretically important expressions. See Reisenfeld (1981) and Stolfi (1991) especially ch. 1.)

[24]Another is the formulation of contemporary algebraic geometry in terms of the concept of scheme. See for example Eisenbud and Harris (1992), or the extended version Eisenbud and Harris (2000), which is largely devoted to explaining why "The scheme is… a more natural setting for many geometric arguments." (1992, 5); (2000, 8) In formulations of arguments in algebraic geometry in terms of schemes rather than antecedents like Weil's concept of "specialization", the concept of scheme is taken by some algebraic geometers to represent an advance because it is counted as "geometric". Another reason it is counted as an advance is that it supports a *unified* theory of key parts of algebraic geometry and number theory. The default assumption is surely that this unifying function is no more to be dismissed by the student of method as a "psychological" phenomenon than the unifying function of Maxwell's electromagnetic theories or relativity should be so dismissed.

Some functions of schemes are more complicated, and whether or not they are appropriate concepts depends on what questions are being addressed. In particular, the concept of scheme initially arises as an effort to extend a basic duality that occurs between restricted classes of rings, which appear in algebra, and varieties (loci of zeros of polynomials) that appear in algebraic geometry. This is a simpler version of a basic correspondence ("dictionary" in the words of (Cox et al., 1992, 168)) between ideals in simple algebraic settings and varieties in elementary algebraic geometry. This gives rise to a circumstance where two frameworks (algebraic and geometric) that are – in some important sense – equivalent are also – in another equally important sense as different as chalk and cheese. The philosophy of mathematics has emphasized the first sense, according to which the frameworks are the same if they are deductively equivalent, over the second. One could see the point of this paper as arguing that the sense in which the equivalent frameworks are crucially different also needs to be clarified before we can take ourselves to have made sense of the principles informing successful mathematical practice.

[25]The classic papers in this debate are assembled in Block (1981).

[26]The point here is not that the use of visual representation is uniform among mathematicians: it isn't. The point is rather that the preference is sufficiently widespread to make a mark on mathematical method.

[27]This has long been known. (See for example Yates (1966) for some history.) More recently it has been well-studied by cognitive psychologists. For some early research into the mnemonic advantages of imagery, see Bower (1972).

[28]Here is a quick explanation of what these are. There are familiar extensions of the real numbers gained by adding additional square roots of 1. The complex numbers are obtained by adding i and closing under $+$ and $\times$. The quaternions are obtained by adding three new roots of –1: i, j, k. For this extension to be adequately specified we need to say more than just that $i^2 = j^2 = k^2 = -1$. The results of

multiplying the new elements must be stated too: $ij = -ji = k$ for example, and further details need to be made explicit. The octonions are the numbers obtained with eight roots of -1.

[29]So for example the curve from e_4 to e_7 extends to the product $e_6 = (e_4 \cdot e_7)$. The sign of the product depends on the direction of travel: counter-clockwise products are negative, clockwise are positive.

[30]To avoid misunderstanding, I should make explicit that my discussion of this example is meant only to draw a contrast. Hence the discussion gives away several points for dialectical reasons. For example, I don't mean to grant more than provisional credence to the idea that some advantages are "solely" mnemonic. The facts about how memory interacts with reasoning are quite involved. Nor indeed do I want to assert that the preference for diagrammatic representations would be devoid of philosophically interesting consequences even in cases where the preference turned out to have purely mnemonic advantage. My point only that there is an at least *prima facie* plausible case to be made for these suggestions. In presenting this material, that *prima facie* plausibility has had a sufficiently strong pull for sufficiently many people that it is worthwhile to make explicit that the case for the philosophical importance of visualization in the cases I am studying here can be made out even if these points are granted.

In fact, I think the question of how to separate the methodologically interesting from "accidental" uses of visual representation is complicated. Even cases that might seem to use vision in a philosophically uninteresting way, such as when pictures are used as memory aids, can be surprisingly involved. I concede that there are some uninteresting cases, but this doesn't mean that I want to say that all cases that might appear uninteresting in this way really *are* uninteresting in this way. In cases where visual memory aids are well–developed and systematic, as in the elaborate medieval memory systems studied in Yates (1966) and Rossi (2000) it is surprisingly hard to make out sharp boundaries between visual coding as an accidental concomitant of artificial memory techniques and visual coding that facilitates memory in virtue of being embedded in an broader system of reasoning. The intricacy of the interweaving of systems of thought and systems of visual representation that was involved in the medieval arts of memory is especially emphasized throughout the uncommonly illuminating studies Carruthers (1990, 1998). Gaukroger (1995) (p. 160-164 and *passim*) points out that this perceived connection between visual imagery and thought informs Descartes' *Regulae* in striking ways. Conversations with Terri Palmer, Ian Hacking and Reviel Netz have helped me here.

Moreover, lest my frame of reference and choice of examples (stressing axiom choice and de-emphasizing actual diagrams, pictures and mental images) leave a misimpression, I should emphasize that I think that the study of the details of concrete visual representation (as in diagrams, etc.) and the manipulation of actual diagrams is extremely interesting to the philosopher of mathematics. By approaching the topic of visualization and geometry as I am, I am in no way meaning to slight those who have approached the topic of reasoning with actual diagrams. Quite the opposite, I regard the recent richness of work on the reasoning with diagrams and its connection to mathematical reasoning as of the greatest interest. (This work has been advanced from different perspectives and with reference to diverse problems by Barwise and Etchemendy (and their students), Oliver Lemon, Ian Pratt (and others in the Manchester group studying visual reasoning) Marcus Giaquinto, Jim Brown, Robert Lindsay, and others. Also of genuine interest in this connection is the work of Achille Varzi and Roberto Casati ((1994) etc.) on the logical structure of theories of spatial structure. Readers interested in such work can choose a beginning among the papers in Allwein and Barwise (1996), or Glasgow et al. (1995) and follow out the references. Also helpful for jump-off points from additional perspectives is Pratt and Lemon (1997). An unusual and stimulating investigation of overlapping themes by two mathematicians is in Carbone and Semmes (2000). Also worth mentioning in this connection is Hartshorne's (2000) masterful reexamination of Euclid's elements.

[31]Several books by Tufte on the visual arrangement of information (see, for instance, his (1983) and (1997)) are good collections of examples. What we find here are visual representations (tables, graphs, maps) that are clearly the most effective and forceful ways to present the information they present. In these cases, the function of visual representation appears to be important solely from a "pragmatic" point of view – in the sense of "pragmatic" that seems to connote 'not deserving philosophical attention' according to some philosophers' usage. If there is philosophical interest in such examples, it will be of a different kind from what we're exploring here.

[32]This is especially pressing in the case of the contrast of Riemann and Weierstrass since just this comparison of the two was made long before Reichenbach introduced the distinction into general methodology: "The method of Riemann is above all a method of discovery; the method of Weierstrass one of proof." (Poincaré, 1898, 7)

[33]I treat this point further in Tappenden (2001b).

[34]This was, for example, true of the concept of scheme. When Grothendieck introduced it, one clear testament to its fruitfulness was that it opened the way to a proof of the Weil conjectures. But to apply the context of discovery/context of justification distinction makes no sense here. Not only did Grothendieck (and subsequently Deligne) prove the Weil conjectures using his newly introduced scheme theory, but he provided what still remain as the *only* proofs available despite extensive attempts in some circles to find proofs that avoid the Grothendieck machinery. (Here I am indebted to correspondence with Colin McLarty and a conversation with Karen Smith.)

[35]This is, of course, an example of a widespread phenomenon in studies of confirmation: the familiar debates presuppose some language or framework remaining fixed. When this can't be taken for granted, many further assumptions break down. Examples of this phenomenon are revealingly treated in Earman (1992) chapter 5 and chapter 8.4.

[36]There is one alternative that is worth attention, but I will have to leave it for another place. In a paper (Kim, 1994) that touches on some of the issues addressed

here, Jaegwon Kim suggests that the causal component of causal explanations might be derivative from a prior idea of *dependence*. If the idea of metaphysical dependence can be made out, it could in principle be extended to mathematical explanations as well, to provide a unified treatment. Michael Strevens has developed an account with this shape in unpublished work.

The idea that mathematical explanation turns on an idea of logical or metaphysical priority over others was a feature of Bolzano's account of mathematical explanation as Paolo Mancosu has pointed out in recent work. (Mancosu, 1999)

[37]The thesis of Morrison (2000), which is the best general descriptive treatment of scientific unification I know, is that unification is often an objective of scientific inquiry but it has little if anything to do with explanation. I agree with much of this, but my final position is a bit more concessive to the idea of unification as understanding: sometimes (but not always) we count ourselves as having understood or explained some phenomena because we have set them in a unified framework, though generally unification alone is not enough unless the framework has other attractive features. (I'll add in this connection that I'm completely in agreement with this upshot of Morrison's perceptive treatment: theory unification is far more complicated in practice than it often is taken to be in the literature.)

[38]I should note that the positions Friedman takes in this early work need not be preserved in his more recent writings. Indeed, his most recent work on scientific theories and the "relativized a priori" has obvious affinities with the "rapprochement between (early) Friedman and Toulmin" that I suggest is necessary.

[39]The most effective display of counter – examples is in Kitcher (1976). The whole controversy is given a retrospective postmortem in (Salmon, 1989, 94 – 101).

[40]To simplify the discussion I am assuming that the theories we are dealing with are given to us already rendered into axiomatic form. In practice, of course, this can't always be assumed. (For example, continuum mechanics was studied for many years before Noll provided an axiomatization, as Clifford Truesdell often pointed out. (Truesdell, 1984, 137 and *passim*)) But since I am just using the assumption to simplify the formulation of this negative point against Friedman, the assumption is harmless here. Clearly if we are dealing with an unaxiomatized theory, the "reduction in the number of basic principles" account is even harder to defend.

[41]A variation on this point has already been made effectively by Humphries (1993), who pointed out the disconnect between number of axioms and understanding with respect to various axiomatizations of propositional logic.

[42]Readers familiar with the scholarly literature on William Whewell will recognize echoes of the ideas here in Whewell's notion of "consilience". Though there are no specific points at which this essay was informed by this literature, I do owe a general debt to the papers (Butts, 1973) and Morrison (1997).

[43]Algebraic numbers are real number solutions to polynomials $x^n + a_1 x^{n-1} + \ldots + a_n$ where the coefficients a_i are rational. Algebraic functions result when the a_i are themselves one-variable functions. Useful treatments of this material in the secondary literature are W. Geyer (1981) and throughout Dugac (1976). (Dieudonné (1985) is a good, though brief English – language discussion of the content of

Dedekind and Weber (1882). A clear (mid-twentieth century presentation) of general versions of results of Dedekind and Weber (1882), in a broadly Dedekindian style, plus a nod to other styles of proving the same results, is in (van der Waerden, 1991, Ch. 19). Another textbook presentation of the theory of algebraic functions of one variable that is avowedly in the spirit of Dedekind and Weber (1882) is Chevalley (1951).

[44]In retrospect, Dedekind and Weber (1882) appears as one of a handful of papers of the nineteenth century that inaugurated distinctive styles marking the twentieth century in mathematics. Dieudonné (1985) lauds the paper for originality and importance, and counts it as second only to Riemann's work in its "introduction of a series of notions which have become fundamental in the modern era."

[45]A field is a collection of objects with two associative, commutative operations defined on the whole collection. Relative to one of the operations (addition) there is an identity element 0 (one for which a + 0 = 0 + a = a) and every element a has an inverse a^{-1} such that $a + a^{-1} = 0$. Relative to the other operation (multiplication) there is an identity and inverses for the collection consisting of every element but the additive identity 0. Distributive laws hold.

To convey what an ideal amounts to, I'll define a special case (though the more general definition exploits the concept of "ring", which is weaker than "field"). A field I contained in another field F is an ideal, if given any a in I and any b in F the product ab is in I.

For the precise definition see any university level text, such as Jacobson (1974).

[46]Again we find this emphasis on the "qualitative" unification in contemporary discussions in algebraic geometry. This is not just true of the schemes and motives discussed in earlier footnotes; another instance – not at all exceptional – is the discussion in Smith et al. (2001) of the pre-Grothendieck work by Weil and Zariski as distinguished by how it brought out "deep connections between previously separate areas of mathematics, such as number theory and the theory of Riemann surfaces" (p.2) Once again, it is hard to see that these connections would lose any value, or be any less unifying, if they turned out not to reduce the number of brute facts in Friedman's sense.

[47]A historical aside: this is a point over which Frege and Hilbert simply stood at cross–purposes. Frege held as Friedman does (mistakenly, I think) that there is an intrinsic advantage to be gleaned from reducing the number of axioms, and indeed he held that the value of an explanation was directly proportional to the reduction. (1979, 36) A hint of this difference shows up in Frege's reaction to his first viewing of Hilbert's foundations. Frege states that he (Frege) believed he could have made do with fewer primitives. (cf. (Frege, 1980, 35))

[48]For example, Friedman argues that the well-known deductive – nomological account Hempel proposes falls afoul of the second requirement – it fails to connect explanation with something plausibly called "understanding" – though Friedman counts it as appropriately objective.

[49]I leave aside the expression "self-evident", as it has epistemological resonance that I want to shed. A proposition can be a reasonable choice as an axiom without being obvious, and a category can be a reasonable choice as natural without it appearing natural on first encounter. We'll see some examples in section 5.

[50]An accessible discussion of the basic points of this historical evolution, with glimpses at more recent showings of the action-at-a-distance view like the Feynman – Wheeler incarnation, is in (Hesse, 1961, ch. VII and VIII and p. 279-289). The discussion throughout Darrigol (2000) is illuminating on the give – and – take between action-at-a-distance accounts and rivals. A. Assis (1994) develops one of the nineteenth century theories in considerable detail, from a contemporary perspective, with an illuminating systematic comparison between theories in the Gauss – Weber style and the Maxwell – inspired theories that dominate today.

[51]See (Frege, 1980, 57) and *passim*. Also (Frege, 1903, 160) and *passim*.

[52]There are additional details in Kitcher's subtle analysis, but they will not be relevant to the points I'll be making here. For the full account of Kitcher's presentation of "patterns of argument" see his (1989, 432-435)

[53]More on the first example: the Erdös–Selberg ("elementary") proofs of the prime number theorem contrast with the ("analytic") proofs exploiting the Riemann zeta function following the path blazed by Hadamard and De Vallée-Poussin. The former have the advantage that they use only "elementary" techniques, while the latter, though presupposing much more analytic machinery, seem to be widely held to better "go to the heart of the matter." (Even setting aside such a suggestion as potentially too loaded it is clear that the analytic proofs are shorter, far less intricate and more easily understood.) For a textbook presentation of both styles of proof (presupposing only high school mathematics) see (Apostol, 1976, ch. 4 and ch. 13).

The many proofs of the Riemann-Roch theorem serve up a more complicated story, which I hope to discuss in further work. The early history is illuminatingly discussed in Gray (1998).

[54]It is worth pointing out as well that this is a twentieth-century example, which raises issues that remain alive in current work. Indeed, preserving this duality in a general setting is one of the more elementary functions of the concept of (affine) scheme. I mention this to reinforce the point that the issues raised by the development of projective geometry in the nineteenth century are not confined to some distant time, irrelevant to mathematics as it is currently practiced.

[55]You don't have to know what the *Nullstellensatz* is. I retained reference to it in the quote just as a benchmark for discussions in future work. (The point is that where the *Nullstellensatz* doesn't generally hold (in finite fields, for example), a new concept is needed to retain the algebra – geometry correspondence. This is part of the work that the concept of scheme does for us.)

[56]A similar point noted in a different connection by Batterman (2000, 233), who complains that the unification account "fails to respect the individuality of problems" (a neat phrase he attributes to Mark Wilson). I don't see this as an objection to

the unification account as much as it is a further subtlety that the account should incorporate. Unification is an important goal in scientific practice, but a multi-faceted one.

[57]Anyone who reflects on the miraculous solution of the integral $\int_{-\infty}^{\infty} e^{-x2}dx$ effected by recasting the question into polar coordinates will know what I mean.

[58]Birational transformations are 1-1 transformations that can be represented as fractions in which both the numerator and denominator are polynomials; in the plane these are also called Cremona transformations. An old-fashioned, concrete presentation of the topic is in Coolidge (1959). A presentation in more contemporary terms is in (Smith et al., 2000) see especially ch. 7.

[59]I'm grateful to Karen Smith for help with this example.

[60]Another example is the use of "reciprocal diagrams" to be considered in a few pages. Here too we have a device which both creates a dual pattern of reasoning that is interesting both because it isolates a significant general pattern and also because it allows the exploitation of shifts from special case to special case.

Naturally we don't see this interaction between general case and special just in mathematics: it shows itself whenever one physical realization of a general theory is used as a tangible model for another.

[61]There are very simple examples of properties that appear disjunctive in one context but which are revealed not to be disjunctive in the "natural setting". Whether or not a property is "disjunctive" can depend upon ontology – on what objects there are in the domain. The simplest example is perhaps the idea of "intersection" in the projective plane. In the Euclidean plane, arguments typically have annoying special cases that arise when two lines are parallel. By expanding the plane with "points at infinity" where parallel lines intersect, the artificial predicate "intersecting or parallel" becomes simply "intersecting", thus eliminating the special cases. (This motive for introducing points at infinity is discussed in many introductory level discussions; see for example (Courant and Robbins, 1941, 180ff).)

[62]One point is worth noting in passing here, though it is sufficiently complicated that I'll have to set it aside here; I will be developing it in future work. There is this much of an anchor to the idea of "projectibility" in mathematics, in that a judgement to the effect that a definition or principle is fruitful incorporates a prediction that results of desired kinds will in fact be produced in the future by those who adopt the definition or principle as part of their working repertoire. The connections between these previsions of future discoveries and judgements of plausibility in mathematics are quite involved, and bear some affinities to versions of the problems of old evidence that are familiar in the study of Bayesian methodology. Both seem to arise from a common root, in which assessments of likelihood depend crucially on expectations that the empirical event of the discovery of a necessary truth occurs.

[63](I will concentrate just on the plane for simplicity – similar patterns emerge in higher dimensions.)

[64]Some expressions – conic section, hexagon – are self-dual. In these theorems, "hexagon" is understood more broadly than we learned in school.

[65]Notably Chasles, in an influential essay of (1837) who called it a "general principle of science". For a statement of the general methodological significance of duality in the English literature, J. Booth (1873) is an especially unrestrained example. See especially p. xi - xiv

[66]Of course, it is important for the plausibility of Kitcher's account that we should find at least *some* cases where theories are consciously designed in a way that conforms to his general picture. If Kitcher is right that unification as he characterizes it is a governing objective of scientific and mathematical practice, we should expect that sometimes the pursuit of the goal would be self – conscious. So it is reassuring that in the case of nineteenth-century duality, the pattern Kitcher presents *was* self-consciously pursued.

Another case in which a variation on Kitcher's picture was a self-consciously adopted methodological guide was in the early nineteenth century debate over the adoption of the Leibnizian notation in Great Britain. Babbage, in his essay "On the Influence of Signs in Mathematical Reasoning" (1827) spells out examples of how a careful choice of notation can unify a proof that consists of several distinct arguments in Newton's *Arithmetica Universalis* into one single pattern. I discuss this in more detail in Tappenden (2001b).

[67]A good short overview of the subject is Scholz (1994). For further details, see Benvenuto (1991), Timoshenko (1953) and Charlton (1982). The role of duality considerations in the development of graphic statics is especially well brought out in Scholz (1989). Dubois (1877) is an English – language textbook of the time which gives a good glimpse into the subject and the attitudes toward it. Also helpful is Graham (1887) which contains extended contrasts and comparisons of analytic and graphic methods.

[68]Culmann (1865); the projective background is made more explicit and systematic in the second edition (1875).

[69]These are sometimes called textitCremona diagrams because Luigi Cremona popularized the technique in his widely used textbooks Cremona (1872) and (1874). (English translations in (Cremona, 1890).)

[70]This particular example is taken from Ziwet (1904) p. 226; I have chosen this example both because it is a good illustration of the point and also for a somewhat sentimental reason. The long – dead Ziwet has been a great help in my current projects (in ways that it would take too long to explain) so I'm happy to grasp the opportunity to cite him in some way. But similar examples are analysed in sources that are easier to obtain today: so for example there are several examples like this one worked out with characteristic clarity in the Schaum's outline on statics and strength of materials. (Jackson and Wirtz, 1983, 117 - 135).

[71]It should be noted, though, that graphic statics is not a perfect illustration in one respect: there is only an indirect connection between the dualistic patterns that best exemplify Kitcher's picture of explanation and the applications to engineering. (This is, of course, a price that has to be paid for choosing actual examples: the real world rarely serves up events that are as clean as the thought experiments that can be crafted at will in the thought laboratory.) On the abstract side, in general

projective geometry, the parallel patterns of argument were, and were taken to be, an important feature of the theoretical structure. In its applied form in graphic statics, dual diagrams played a central role, but these dualities didn't translate into a line-by-line parallelism of proofs and arguments, as in the abstract case. (I have only found one engineering textbook – Crotti (1888) – where the formal duality of argument is spelled out explicitly and represented in the "dual columns" format of projective geometry textbooks. (According to Charlton (1982, 155)) Crotti's text was unique in this regard.)) What we have in graphic statics is a case in which a framework explicitly informed by Kitcher – type patterns of multiple argument is applied to concrete problems, thereby coloring what counts as explanations of these concrete problems. This is good enough for the present purposes, though the example would of course be cleaner if the dualities of argument that shape the abstract mathematical investigations figured more prominently in the engineering applications.

[72]On the Quebec bridge collapse see (Ferguson, 1992, 172-178). An illuminating glimpse into the patterns of explanation characteristic of turn-of-the-century engineering can be found in the pages of the professional weekly *Engineering News* during the months after the disaster, where candidate reasons for the collapse are dissected and discussed at length.

[73]I am indebted here to Nancy Cartwright (1983, 56-67) who advances the similar point that against the background of realism about forces, patterns of vector addition and decomposition may involve reference to theoretical fictions. My point here is different – I am setting aside any questions of ontology – but Cartwright's discussion was helpful in nudging my thoughts at a crucial juncture.

[74]An illustration of its importance is that graphical statics was taken to deserve a massive (90 page) chapter to all to itself in the Physics volume of Klein's *Encyklopädie der Mathematischen Wissenschaften*. See Hennenberg (1903).

[75]Though it might be noted that a residual nostalgia for the older techniques persists. In his discussion of graphic statics, Ferguson remarks: "Even though digital computers are making graphical methods seem both old-fashioned and insufferably slow, a few younger engineers, along with the old fogeys, are beginning to understand that speed has sometimes been bought at the cost of understanding." (Ferguson, 1992, 152)

In this connection it is worth noting further that some of the old results of graphic statics have recently been revived and generalized. (See for example (Crapo and Whiteley (1982)) and Whiteley (1985). On this work I am grateful to Walter Whiteley for email correspondence and to Branko Grünbaum for sending me a copy of his unpublished lectures Grünbaum (1976).) Here too the interest of the results does not depend exclusively upon the visualizable character of the represented structures, though the visual flavor of the work is still important.

[76]See for example (Dubois, 1877, iv) (Cremona, 1890, 121,123-4,131-137) (Culmann, 1875, vii–xv etc.)

[77]So for example, the engineering professor Rankine remarks that an advantage of the graphical methods compared to analytical methods that they make mistakes

much easier to catch. ((Rankine, 1869, 411); quoted with tacit agreement in (Cremona, 1890, 133)). The point was reiterated in these terms in more recent days by a contemporary engineer defending the virtues of the old ways: "When nearly all engineers carried out structural analysis using ... graphic statics [and similar methods]... the advantages of visually monitoring one's calculations (Does it look right? Are the numerical answers reasonable?) were built into the graphical mathematics they used." (Ferguson, 1992, 152)

[78]As one textbook of graphic statics puts it, with a quaint Victorian flair: "... the power conferred by the graphical method is to a large extent at the disposal of those who have had but little mathematical training. The writer once had occasion to explain a practical application of the triangle of forces to a class of working men, who seemed at once to grasp and appreciate it." (Clarke, 1888, v)

[79]A characteristic opinion is expressed in Rankine's discussion of reciprocal figures:

> When compared with algebraic methods, the simplicity and rapidity of execution of the graphic method is very striking... If this is the case when the loads are uniform or symmetrical, the advantage is much more strikingly in favour of the graphic method when the loads are not symmetrical, and when they are inclined... or as in such cases as the framed arch and suspension bridge. In fine, the diagram once drawn acts as a sort of graphic formula for the strain on every part of the bridge or roof, and it is a formula which can hardly be misapplied. ((Rankine, 1869, 441); part of this passage is quoted with tacit agreement in (Cremona, 1890, 133))

[80]One example is Dubois (1877).

[81]This is an oft-repeated theme in the literature on analytic projective geometry; I'll mention just two examples that illustrate the point. Referring in particular to the nineteenth century analytic geometer Plücker, Felix Klein commends his style of argument in these terms:

> In Plücker's geometry, the bare combination of equations is translated into geometrical terms, and the analytic operations are led back through the geometric. Computation is avoided as much as possible, but by doing this, a mobility heightened to the point of virtuosity, of inner intuition, of the geometric interpretation of given analytic equations, is cultivated and extensively applied. (Klein, 1926/79, 110)

Bear in mind that Klein is here discussing someone who bucked the trend of the then – dominant synthetic geometry in favor of streamlined analytic methods. The praise is not for the use of diagrams but rather for a certain way of organizing the analytic methods so as to gain an elegant means of addressing the subject. This is reinforced by the Klein's subsequent illustration of his remarks with "an example of Plücker's way of thinking" (p. 110). He presents a device ("abridged notation") Plücker used to systematically manipulate and transform analytic equations so as to better fix on the crucial parts of the underlying geometric situation. Even without any connection to vision, the symbolic technique was, and remains a valuable (if now somewhat old-fashioned) tool.

A more recent commentator on essentially the same phenomenon of Plücker's style of analytic argument is Dieudonné:

> One of the attractions of [nineteenth century] complex projective geometry is its relative independence from algebra and the formal independence of its results, in contrast to the massiveness of most of the coordinate calculations of the preceding century.... Möbius, Plücker, and Cayley give projective geometry a solid base by the use of homogeneous coordinates accompanied by a harmonious choice of indexing notation that maintains a symmetry and a clarity in the calculations so that they closely follow the geometric argument. (Dieudonné, 1985, 9)

Here again an advantage of the "geometric" framework is taken to include an elegant way of formulating the subject matter, which happens to have an important tie to visual representation but which is valuable independently of it.

[82]A particularly charming illustration of this point appears in Coxeter's textbook *The Real Projective Plane* where Desargues' theorem is adopted as an axiom of projective geometry. After providing one proof of Desargues' theorem, Coxeter remarks "Since we will eventually take Desargues' theorem as an axiom, it seems worthwhile to give an alternative proof." (1992, 7) and he proceeds to give it.

[83]It should be noted that Artin's use of "geometric" is somewhat idiosyncratic. Artin was one of the greatest forces propelling the abstract turn of twentieth century algebra, and even when working in a self-consciously "geometric" vein, his tastes tilt to the algebraic. This makes his treatment especially useful for present purposes: "geometry" for Artin turns out to have an exceedingly indirect connection to *vision*.

This peculiarity of Artin's attitude hasn't gone unnoticed. A noted algebraic geometer told me in conversation that in his opinion Artin's text was "not really geometric" (except in the sense emerging from the Klein program of characterizing geometries with groups of transformations). We also find a variant of this opinion in a review of Artin (1957):

> Most of this book is devoted to the study of algebraic structures arising from various geometries. The approach is algebraic rather than geometric...
>
> ...
>
> In Chapter II [the focus of this article], affine geometry is introduced axiomatically and then coordinatized. Even here the approach is algebraic. (Jans, 1957, 604)

[84]Not everyone, though. The approach to matrices in Edwards (1996) is motivated by an explicit preference for computations that is apparently as strong as Artin's animus.

[85]Another advantage, mentioned in some textbooks that adopt the Artin approach, is that it allows a smooth introduction of coordinates, in contrast to the "messy" algebraic approach of (for example) Hall (1943). See (Hartshorne, 1967, 101) and (in nearly identical language) Kadison and Kromann (1996) (p. 105–106) Here too there is nothing specifically "visual" about this advantage.

[86]In a treatment of overlapping material, with some ideological affinities to Artin's book (Dieudonné, 1969) the absence of diagrams is principled. After emphasizing the importance of material which can be represented in visual intuition (p.12), Dieudonné continues: "I have taken the liberty of omitting all diagrams from the text, if only to show that they are unnecessary" (p.13) Though Dieudonné doesn't present himself as resurrecting a geometric presentation as Artin does, the approach is nonetheless "geometric" in Artin's sense to the extent that I) the concept of mapping rather than computation with coordinates is explicitly marked out as basic and systematically developed. (p.13-14 and *passim*) II) the core intuitions are spun out from a consideration of intuitive maps on linear varieties. (see especially chapter III) So we can draw the same conclusion: that this material admits of representation in diagrams is valuable, but it doesn't exhaust the value of this particular framework for organizing information.

[87]Though I should note that the point about the absence of pictures is slightly softened by Artin's instruction to the reader to draw pictures while reading. (Artin, 1957, 52) But even with this qualification it is clear from Artin's discussion that he sees the role of pictures as secondary in his "geometric" presentation.

[88]This historical point should be flagged, though it would represent too much of a digression to work it out here. Artin's approach to rendering the idea of geometric intuition rigorous has a distinguished pedigree: many debates in the mathematics of the nineteenth century are illuminated if this is recognized. In particular, as Michael Friedman points out in a superb article, (Friedman, 2000) Helmholz interpreted Kantian "intuition" in terms of transformations of space, setting aside the idea of "construction in intuition". (Friedman credits Robert DiSalle for key observations in this connection.) This forged a bridge with the new geometry that was then emerging.

The Friedman-DiSalle insight that the Kantian idea of intuition was undergoing a metamorphosis among scientifically informed students of geometry is of broad significance for our understanding of mathematicians' talk of intuition at the time. In particular, the insight allows one to flesh out some gnomic remarks Frege makes in *Grundlagen* about intuition and geometrical knowledge, and fit them into other features of his mathematical environment. I develop this point further in Tappenden (2001a).

[89]This version of the definition leaves aside a degenerate case that will be of no interest here.

[90]The second axiom implies the first.

[91]It deserves mention, but I won't expand on this point here, that classic versions of the Desargues theorem have historically been quite important in applied geometry, especially in the development of the theory of linear perspective in painting and

architecture. A good general source is Field (1997). On the geometry and theory of perspective of the historical Desargues see Field and Gray (1987). On Desargues' theorem and the historical development of perspective, Field (1987) and (1988) are detailed and helpful.

[92]This point is discussed in (Blumenthal, 1961, 81–84).

[93]Artin notes this fact on p. 51. It is discussed more extensively in this symmetry-based context by (Kadison and Kromann, 1996, 116–120).

[94]The additional constraint that the multiplication operation be commutative (i.e. that the coordinates are a field) corresponds to a further geometric axiom with a classical pedigree: the Pappus theorem. The Pappus theorem is another illustration of the themes of this section, but I will leave discussion of it for another time.

[95]An interesting discussion of the Desargues theorem from this point of view is in (Rota, 1997, 140–146) especially p. 141 on the "zen ideal" combined with many applications and 145 on the "horizon of possibilities" the Desargues theorem opens up. Rota's account of reasons for regarding the Desargues theorem as central draws on a venerable analysis of Desargues' theorem in connection with the underlying combinatorial situation (the "Desargues configuration") detailed at length in Baker (1929). This, incidentally, gives yet another point of view from which the Desargues axioms turn out to represent a natural carving point: it also corresponds to deep and rich facts in finite combinatorics. I won't be exploring this perspective further here: for those who are interested a contemporary introduction to this point of view, in which the Desargues configuration shows itself prominently, is Batten (1997).

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J. HAFNER AND P. MANCOSU

THE VARIETIES OF MATHEMATICAL EXPLANATION[0]

1. BACK TO THE FACTS THEMSELVES

When William James was faced with the task of writing in an encompassing way on religion he emphasized the variety of phenomena that fell under the topic and warned against the dangers of oversimplification:

> Most books on the philosophy of religion try to begin with a precise definition of what its essence consists of. Some of these would-be definitions may possibly come before us in later portions of this course, and I shall not be pedantic enough to enumerate any of them to you now. Meanwhile the very fact that they are so many and so different from one another is enough to prove that the word "religion" cannot stand for any principle or essence, but is rather a collective name. The theorizing mind tends always to the oversimplification of its materials. [...] Let us not fall immediately into a one-sided view of our subject, but let us rather admit freely at the outset that we may very likely find no one essence, but many characters which may alternately be very important to religion. (William James, The varieties of religious experience, 1902, p. 31)

If we substitute 'explanation' for 'religion' in the above quote the result captures our point of view about the philosophy of explanation. Contemporary work in scientific explanation has pursued to a great extent the project of a single unified account of the nature of explanation. Unfortunately the drive towards unification has also ignored an important number of phenomena. In particular, many theories of scientific explanation do not address mathematical explanation, either because they rule mathematical explanations out of court from the outset or because they hold that their account of explanation automatically takes care of mathematical explanation. Most of the time, mathematical explanation is simply not mentioned. This is a symptom, following James, of the dangers of the theorizing mind and like him we propose to begin "by addressing ourselves directly to the concrete facts". Of course, it is not our intention to downplay the importance of the work that has been pursued in the area of scientific explanation and which has yielded many remarkable insights. We do not even pass judgment on whether a more careful analysis of concrete scientific case studies of explanatory activity in the empirical sciences might have been beneficial for the subject as a whole.

P. Mancosu et al. (eds.), Visualization, Explanation and Reasoning Styles in Mathematics, 215-250.

However, our topic demands a different approach. Indeed, in the case of mathematical explanation we cannot rely, as people do in the natural sciences, on well-entrenched intuitions concerning paradigmatic examples of explanations.

In this paper we will begin with some general methodological remarks about mathematical explanations. We will then point out that attention to mathematical practice reveals the presence of a great variety of mathematical explanations. This realization affects two important aspects of the discussion of the nature of mathematical explanation. First of all, most of the traditional debates (see Mancosu 1999, 2000, 2001) have focused on the opposition between explanatory and non-explanatory proofs. However, there are mathematical explanations that do not come in the form of proofs and this has in fact been recognized by several scholars. Second, the variety of mathematical explanations challenges the current philosophical accounts of mathematical explanation, i.e. those of Kitcher and Steiner. As detailed discussion of case studies is necessary to see the limitations of such accounts, in the second part of the paper we restrict our focus to Steiner's theory and to the discussion of an example of an explanatory proof which, we claim, Steiner's theory cannot account for.[1]

2. MATHEMATICAL EXPLANATION OR EXPLANATION IN MATHEMATICS?

In the above we have been using freely the expression "mathematical explanation". The use was intentionally ambiguous and we should now clarify the source of the ambiguity. "Mathematical explanation" could mean a) explanations as they are given in mathematics; or, b) explanations that make use of mathematics. The two definitions characterize different classes. In the first case we intend to refer to explanatory practices that take place within the realm of mathematics itself. In the second case, this would include, among other things, mathematical explanations of physical facts which clearly do not belong to the first class.

The second kind of explanation is part of a large problem area concerning mathematical applications. Shapiro recently remarked that "a scientific 'explanation' of a physical event often amounts to no more than a mathematical description of it." His favorite example is given in the form of an anecdote:

> The story relies on the unreliable memory of more than one person, but the situation is typical. A friend once told me

> that during an experiment in a physics lab he noticed a phenomenon that puzzled him. The class was looking at an oscilloscope and a funny shape kept forming at the end of the screen. Although it had nothing to do with the lesson that day, my friend asked for an explanation. The lab instructor wrote something on the board (probably a differential equation) and said that the funny shape occurs because a function solving the equation has a zero at a particular value. My friend told me that he became even more puzzled that the occurrence of a zero in a function should count as an explanation of a physical event, but he did not feel up to pursuing the issue further at the time. (Shapiro 2000, p. 34)

Shapiro's friend had all the rights to be puzzled. After all, it could be claimed that the explanation why the equation in question has a zero at a particular value rests on the physical situation and not vice versa. Of course, the equation has its zeros independently of any physical reality and thus the last remark makes sense only under the assumption that the equation "represents" the physical reality. But this only points to the fact that without a general account of how mathematics hooks on to reality the role of mathematical explanations in physics is bound to remain mysterious:

> Clearly, a mathematical structure, description, model, or theory cannot serve as an explanation of a non-mathematical event without some account of the relationship between mathematics per se and scientific reality. Lacking such an account, how can mathematical/scientific explanations succeed in removing any obscurity - especially if new, more troubling obscurities are introduced? (Shapiro 2000, p. 35; cf. p. 217)

This is a daunting problem indeed but fortunately we will not have to discuss it here, as our major aim is to investigate the first sense of mathematical explanation. Even with this restriction in place, things are far from easy. "Explanation" is a notoriously ambiguous word and this ambiguity shows up in mathematics just as much as in ordinary parlance. We can explain the rules of a certain calculus, the meaning of a symbol, how to carry out a construction, how to fix or set up a proof. These are all "instructions" on how to master the tools of the trade. There are however deeper uses of "explanation" in mathematics which call for an account of the mathematical facts themselves, the reason why. The distinction we just drew between "instructions" and deeper senses of "explanation" should be no more puzzling than the equivalent one in physics. While doing physics we might ask for an explanation of a certain notation or of how to describe a certain phenomenon by means of a new formalism. These uses of explanation are of a different category from that involved in explaining, for instance, why salt dissolves in water.

3. THE SEARCH FOR EXPLANATION WITHIN MATHEMATICS

In addition to "explanation" mathematicians and philosophers use a cluster of expressions to refer to this phenomenon. Here is an illustrative sample of expressions we found in the mathematical and philosophical literature in which the search for explanations is sometimes characterized as a search for:

(a) "the deep reasons"
(b) "an understanding of the essence"
(c) "a better understanding"
(d) "a satisfying reason"
(e) "the reason why"
(f) "the true reason"
(g) "an account of the fact"
(h) "the causes of"

Of course, we are not claiming that the above expressions have the same intension. However, we maintain that the cluster of notions we indicated is not accidentally related.

That mathematicians seek explanations in their ordinary practice and cherish different types of explanations is for us, after working on this topic for so long, so obvious as to require almost no proof. However, some of the philosophical literature on the topic has denied that there are mathematical explanations and thus it will be useful here to provide some examples of "explanatory" talk in mathematical practice.

First of all, the search for explanations is often the drive towards mathematical research. What motivates mathematicians to look for explanations? It is the old desire to know the reason why. This desire might be awakened by different factors, a sample of which is given by the following illustrative examples.

1. A number of mathematical phenomena are perceived as too complicated. A desire to bring order in the "realm of facts" will drive the mathematician to look for an explanation or a deeper explanation of what is going on.

Example 1. In the article "On the Kummer solutions of the hypergeometric equation" Reese T. Prosser describes his aim as follows:

> One of the oldest, and still one of the most interesting applications of group theory arises in the study of the transformations of an ordinary differential equation. If we know that a given differential equation admits a group of transformations, then we know that the solution set must admit that same group of transformations, and we can deduce properties of all the solutions from the properties of any one of them. A case in point is offered by the celebrated hypergeometric equation whose solutions include many of the most interesting special functions of mathematical physics [...] In 1836 Kummer published a set of six distinct solutions of the hypergeometric equation. [...] A glance at the list of these solutions reveals a rather complicated set of relationships which pleads for some simple explanation. We show here that the Kummer solutions are related by a finite group of transformations which serve to explain their relationships and to exemplify the use of transformation groups in the study of differential equations. (p. 535)

2. Sometimes it is a desire of explaining "resemblances", mysterious or remarkable coincidences, as well as striking or deep analogies.

Example 2a. In "Eine Verbindung zwischen den arithmetischen Eigenschaften verallgemeinerter Bernoullizahlen", Kurt Girstmair writes:

> Let $m \geq 1$, $n \geq 2$ be integers. There are two kinds of generalized Bernoulli numbers which occur in the arithmetic of Abelian number fields: on the one hand Leopoldt's numbers [...], on the other hand, the cotangent numbers [...] For both kind of numbers theorems of the v. Staudt-Clausen type exist, which describe their (ideal) denominators. These theorems resemble each other in several respects, a fact that has not been explained so far. One aim of this paper is to supply this explanation. (p. 47)

Example 2b. In the article "On the Betti numbers of the moduli space of stable bundles of rank two on a curve" Bifet, Ghione and Letizia say:

> The aim of this paper is to begin exploring a new algebra-geometric approach to the study of the geometry of the moduli space of stable bundles on a curve X over a field k. This approach establishes a bridge between the arithmetic approach of G. Harder and M.S. Narasimhan and the gauge

> group approach of M.F. Atiyah and R.H. Bott. In particular, it might help explain some of the mysterious analogies observed by Atiyah and Bott. (p. 92)

3. Very often the mathematical fact to be explained is understood from a certain point of view but one looks for alternative explanations. When mathematicians speak about explanations they often modify the phrase by specifying the nature of the explanation: analytical, algebraic, group-theoretical, combinatorial, categorical, geometric, function-theoretic, measure-theoretic, number-theoretic, probabilistic, cohomological, representation-theoretic, topological etc. In some cases several of these goals are pursued at once.

Example 3. Iku Nakamura in "On the equations $x^p + y^q + z^r - xyz = 0$" writes:

> We know two strange dualities - the duality of fourteen exceptional unimodular singularities and the duality of fourteen hyperbolic unimodular singularities. The first purpose of this article is to recall and compare them. The second is to give explanations for the second duality from various viewpoints. [...] In section 5 we give a number-theoretic explanation for the duality. We see that the duality is essentially the relationship between a complete module and its dual in a real quadratic field. In section 6 we provide a geometric explanation for the duality by means of general theory of surfaces of class VII_0. In section 7 we give a lattice-theoretic explanation for the duality. (pp. 281f)

4. However, most of the time explanations are provided for mathematical facts independently of whether a particular point of view is emphasized. While sometimes these facts might be "striking" or "curious" in many cases the explanation is sought whether or not the fact in question might be striking.

Example 4a. Kubo and Vakil in "On Conway's recursive sequence" say:

> The recurrence $a(n) = a(a(n-1)) + a(n-a(n-1))$, $a(1) = a(2) = 1$ defines an integer sequence, publicized by Conway and Mallows, with amazing combinatorial properties that cry out for explanation. We take a step towards unraveling this mystery by showing that $a(n)$ can (and should) be viewed as a simple 'compression' operation on finite sets. This gives a combinatorial characterization of $a(n)$ from which one can read off most of its properties. (p. 225)

Example 4b. Leyendekkers and others in "Analysis of Diophantine properties using modular rings with four and six classes" write:

> A modular ring Z_A is described, and used together with a modular ring Z_6 and the Pythagorean triple grid, described earlier, to analyze various diophantine properties and explain why the area of a Pythagorean triangle can never be a square.

4. SOME METHODOLOGICAL COMMENTS ON THE GENERAL PROJECT

It should be obvious from the above that mathematicians seek explanations. But what form do these explanations take? It is here that two possibilities emerge. One can follow two alternative approaches: top-down or bottom-up. In the former approach one starts with a general model of explanation (perhaps because of its success in the natural sciences) and then tries to see how well it accounts for the practice. In the latter approach one begins by avoiding, as much as possible, any commitment to a particular theoretical/conceptual framework. We favor the second approach for the following reasons. As a rule contemporary accounts of explanation have been developed within the philosophy of natural science without addressing the specificity of mathematical explanations. Hence the conceptual resources of those accounts involving, e.g. the notions of causal connections or laws of nature seem inappropriate for capturing explanations in mathematics. Furthermore, even if some more abstract features of those accounts, e.g. construing the general form of explanations as answers to why-questions could perhaps be adopted for a theory of mathematical explanations[2] proceeding in this way would mean forcing the evidence from mathematical practice into a predefined mould, thereby narrowing the perspective from the outset and probably leading to distortions. The same holds for the few philosophical accounts of mathematical explanation found in the literature (Kitcher, Steiner). Making either theoretical unification or deformability (in Steiner's particular sense) the hallmark of mathematical explanations amounts to the imposition of a defining characteristic feature on what ought to be counted as "explanation" in mathematics.[3] Proofs, theories, methods etc. which do not satisfy that definition are then disregarded or discounted – regardless whether they are indeed taken to be explanatory by working mathematicians!

Thus, in our mind, a fruitful approach would consist in giving a taxonomy of recurrent types of mathematical explanation[4] and then trying to see whether these patterns are heterogeneous or can be subsumed under a general account. We maintain that mathematical explanations are heterogeneous. However, neither giving the taxonomy nor arguing for the previous claim is what we have set for ourselves in this paper. Rather, we would like to provide a single case study of how to use mathematical explanations as found in mathematical practice to test theories of mathematical explanation. This can be seen, as it were, as a case study of how to show that the variety of mathematical explanations cannot be easily reduced to a single model. In what follows we will thus look at Steiner's theory of explanation and discuss a counterexample to his theory.

5. MARK STEINER ON MATHEMATICAL EXPLANATION

In developing his own account of explanatory proofs in mathematics Mark Steiner discusses – and rejects – a number of initially plausible criteria for explanation, i.e. the (greater degree of) abstractness or generality of a proof, its visualizability, and its genetic aspect which would give rise to the *discovery* of the result. In contrast, Steiner takes up the idea "that to explain the behavior of an entity, one deduces the behavior from the essence or nature of the entity" (Steiner 1978, p. 143). In order to avoid the notorious difficulties in defining the concepts of *essence* and *essential* (or *necessary) property*, which, moreover, don't seem to be useful in mathematical contexts anyway since all mathematical truths are usually regarded as necessary, Steiner introduces the concept of *characterizing property*. By this he means "a property unique to a given entity or structure within a *family* or domain of such entities or structures" (*Ibid.*), where the notion of "family" is taken as undefined. Hence what distinguishes an explanatory proof from a non-explanatory one is that only the former involves such a characterizing property. In Steiner's words: "an explanatory proof makes reference to a characterizing property of an entity or structure mentioned in the theorem, such that from the proof it is evident that the result depends on the property" (*Ibid.*). Furthermore, an explanatory proof is *generalizable* in the following sense. Varying the relevant feature (and hence a certain characterizing property) in such a proof gives rise to an array of corresponding theorems, which are proved – and explained – by an array of "deformations" of the original proof. Thus Steiner arrives at two criteria for explanatory proofs, i.e. dependence on a *characterizing property* and *generalizability* through varying of that property (Steiner 1978, pp. 144, 147).

The following proof of the irrationality of $\sqrt{2}$ given by Steiner illustrates the two criteria.[5] Relying on the fact that each number has a unique prime power expansion (the Fundamental Theorem of Arithmetic) we can argue thus.

Assume that $2 = (\frac{a}{b})^2$, i.e. $a^2 = 2b^2$. The prime 2 has to appear with an even exponent in the prime power expansion of a^2. And since the same holds for the prime power expansion of b^2, the exponent of 2 in the expansion of $2b^2$ must be odd. Because of the uniqueness of prime power expansions it follows that $a^2 \neq 2b^2$ contradicting our assumption.

This proof is explanatory according to Steiner, because it uses – as a characterizing property of numbers – their prime power expansion. Also, the proof is generalizable to numbers different from 2, i.e. one can establish along the same lines the theorem that for any n, $\sqrt{n}$ is either a natural number or irrational. And generalizing further one can get the analogous result for the p^{th} root in place of the square root of n.

Steiner's account has been criticized by Resnik and Kushner. They doubt the existence of explanatory proofs in general, denying an objective distinction between explanatory and non-explanatory proofs. But more concretely they also challenge Steiner's account by proposing counterexamples, i.e. a proof that meets his criteria but is not accepted as explanatory by Steiner himself. And on the other hand Resnik and Kushner claim there are proofs, namely a certain proof of the intermediate value theorem and Henkin's proof of the completeness of first-order logic, which seem to qualify as explanatory but apparently fail to meet Steiner's criteria. However, one may ask how well these instances really work as counterexamples. To begin with, what justifications are put forward by Resnik and Kushner for the classification of their examples as indeed (intuitively) explanatory? Besides simply claiming that these proofs "would seem to qualify as explanatory if any do" (Resnik & Kushner 1987, p. 147), it is contended with some – albeit rather vague – reference to mathematical/logical practice that Henkin's proof "is generally regarded as really showing what goes on in the completeness theorem and the proof-idea has been used again and again in obtaining results about other logical systems" (Resnik & Kushner 1987, p. 149). And with respect to the proof of the intermediate value theorem the authors "find it hard to see how someone could understand this proof and yet ask *why* the theorem is true (or what makes it true)" (*Ibid.*) and hence it has to be counted as explanatory. Yet we are not given any hint as to what exactly the explanatory feature(s) of this proof are supposed to consist in.

For counterexamples to Steiner's theory to carry real weight they would have to be much more closely related to mathematical practice. Contrary to what Resnik and Kushner claim (p. 151), mathematicians *often* describe themselves and other mathematicians as explaining. And their judgments concerning explanatory vs. non-explanatory proofs (and other varieties of explanation in mathematics as the case may be) has to figure as the basic evidence, however subjective or context dependent they may be. Claims to the effect that certain proofs are explanatory come from within mathematics not from philosophers of mathematics. Their sources, working mathematicians, are furthermore precisely identifiable, and the case for explanatoriness will be even stronger, if a certain proof is put forward explicitly with the aim to explain a "mathematical phenomenon", which has been acknowledged for a long time to be mysterious and puzzling by (a subgroup of) the mathematical community. A case of mathematical explanation rooted in this way in mathematical practice can justifiably serve as a test case for Steiner's account. It certainly cannot be dismissed easily if it should amount to a refutation of that account. And it is such a test case coming from the work of Alfred Pringsheim in the theory of infinite series which we want to present and discuss in the following.

6. KUMMER'S CONVERGENCE TEST

The following exposition is adapted from Pringsheim (1916). In order to make it more readable and clearly bring out the points which are relevant in our context we have simplified Pringsheim's account by stating some results in a slightly less general form than they could be formulated. But nothing essential is lost because of that (cf. footnote 7).

Let's start with some preliminary observations concerning infinite series. We will confine ourselves to infinite series $\sum_{n=1}^{\infty} a_n$ of positive terms,[6] i.e. $a_n > 0$ for $n = 1, 2, 3, \ldots$ and we will consider different convergence and divergence tests for them. Of fundamental importance are the following comparison tests.

(1) If $\sum c_n$ is a *convergent* series such that the terms of $\sum a_n$ satisfy $a_n \leq c_n$ for all n (or at least for all values of n greater than some fixed value m), then $\sum a_n$ is also convergent.

Similarly we have:

(2) If $\sum d_n$ is a *divergent* series such that the terms of $\sum a_n$ satisfy $d_n \leq a_n$ for all n (or at least for all values of n greater than some fixed value m), then $\sum a_n$ is also divergent.

It turns out that the comparison tests are often easier to work with in practice when they are stated in a slightly different form. In order to simplify the exposition we will for the remainder of this section adopt the convention to denote arbitrary infinite series by '$\sum a_n$', convergent ones by '$\sum c_n$', and divergent ones by '$\sum d_n$'. Let $C_n = \frac{1}{c_n}$, $D_n = \frac{1}{d_n}$ and let g and G be two positive numbers (g can be thought of as arbitrarily small and G as arbitrarily large). Now suppose for all n (or at least for all $n \geq m$, for some m)

$$a_n \leq G \cdot c_n \qquad \text{or} \qquad a_n \geq g \cdot d_n.$$

Then we have

$$\text{(3)} \qquad \frac{1}{c_n} \cdot a_n = C_n \cdot a_n \leq G \implies \sum a_n \text{ converges.}$$

and

$$\text{(4)} \qquad \frac{1}{d_n} \cdot a_n = D_n \cdot a_n \geq g \implies \sum a_n \text{ diverges.}$$

Under the assumption that $\lim_{n\to\infty} C_n \cdot a_n$ and $\lim_{n\to\infty} D_n \cdot a_n$ exist,[7] hence $\lim_{n\to\infty} C_n \cdot a_n \leq G < \infty$ and $\lim_{n\to\infty} D_n \cdot a_n \geq g > 0$, we arrive finally at the following formulations.

$$\text{(5)} \qquad \lim_{n\to\infty} C_n \cdot a_n < \infty \implies \sum a_n \text{ converges.}$$

$$\text{(6)} \qquad \lim_{n\to\infty} D_n \cdot a_n > 0 \implies \sum a_n \text{ diverges.}$$

Tests (1) through (6) commonly also known as *comparison tests of the* 1^{st} *kind* arise from a direct comparison of the terms a_n with c_n or d_n. In contrast *comparison tests of the* 2^{nd} *kind* are based on quotients of two consecutive terms of the series and their comparison. This method is frequently very convenient since for many series of practical importance the quotient $\frac{a_n}{a_{n+1}}$ happens to be simpler than the general term a_n. With our conventions of denoting convergent and divergent series in place we can state these tests concisely as follows.

(7) If for all n (or all $n \geq m$, for some m) $\frac{a_{n+1}}{a_n} \leq \frac{c_{n+1}}{c_n}$, then $\sum a_n$ converges.

And

(8) If for all n (or all $n \geq m$, for some m) $\frac{a_{n+1}}{a_n} \geq \frac{d_{n+1}}{d_n}$, then $\sum a_n$ diverges.

They easily follow from the direct comparison of terms of the series involved[8] and again they can be reformulated in different ways. Simple transformations of the conditions in (7) and (8) yield $\frac{a_{n+1}}{a_n} \leq \frac{C_n}{C_{n+1}}$ and $\frac{a_{n+1}}{a_n} \geq \frac{D_n}{D_{n+1}}$ and then in turn we get

$$\text{(9)} \qquad \text{For all } n \geq m, \ (C_n \cdot \frac{a_n}{a_{n+1}} - C_{n+1}) \geq 0 \implies \sum a_n \text{ converges.}$$

$$(10) \qquad \text{For all } n \geq m,\ (D_n \cdot \frac{a_n}{a_{n+1}} - D_{n+1}) \leq 0 \implies \sum a_n \text{ diverges.}$$

And finally by assuming the existence of the involved limits,

$$(11) \qquad \lim_{n\to\infty}(C_n \cdot \frac{a_n}{a_{n+1}} - C_{n+1}) > 0 \implies \sum a_n \text{ converges.}$$

$$(12) \qquad \lim_{n\to\infty}(D_n \cdot \frac{a_n}{a_{n+1}} - D_{n+1}) < 0 \implies \sum a_n \text{ diverges.}$$

Having established the general form of comparison tests of the 1^{st} and 2^{nd} kind it now remains to determine concrete examples of convergent and divergent series, $\sum c_n$ and $\sum d_n$, which can be substituted in those tests in specific applications. For our purposes we don't need to proceed any further in this direction, instead we focus our attention on another formulation of a comparison test of the 2^{nd} kind due to Ernst Kummer.[9] Letting (B_n) be an *arbitrary* sequence of positive numbers Kummer's test can be stated as follows.

$$(13) \qquad \lim_{n\to\infty}(B_n \cdot \frac{a_n}{a_{n+1}} - B_{n+1}) > 0 \implies \sum a_n \text{ converges.}$$

This test is rather striking because of the extreme generality or arbitrariness of the sequence (B_n) occurring in it. Whereas the tests (11) and (12) above require the use of sequences (C_n) and (D_n) which derive, respectively, from convergent and divergent series, *any old sequence* (B_n) will do in (13). Pringsheim calls it a "most remarkable criterion" (Pringsheim 1916, p. 379) of "indeed surprising generality" (p. VI) that stands in need of explanation or clarification [Aufklärung] (p. 379). Pringsheim's opinion was by no means exceptional, many mathematicians must have been similarly puzzled and left unsatisfied by Kummer's original proof of his criterion in 1835. As Knopp notes it wasn't until 1885 that O. Stolz gave an "extremely simple proof, by means of which the criterion was first rendered fully intelligible" (Knopp 1928, p. 311, fn. 52). Moreover, even after another 30 years had passed this criterion was apparently still viewed as an anomaly of sorts defying smooth integration into the theory of infinite series. Pringsheim notices (in 1916) that Kummer's criterion "appeared as totally erratic in other accounts [of convergence and divergence tests], seemingly lacking any analogue among the convergence criteria of the 1^{st} kind"[10] and he thus aimed at presenting it "freed from this mysterious isolation" (p. VI). Pringsheim gives two different proofs of it, one explanatory and another one which only "proves the correctness of the criterion *a posteriori* in a simpler way" (p. 379). Let's begin with the latter; it is essentially due to Stolz and it's indeed very simple.

If $\lim_{n\to\infty}(B_n \cdot \frac{a_n}{a_{n+1}} - B_{n+1}) > 0$, then there exists a ρ such that from some stage m on, $n \geq m$ implies

$$B_n \cdot \frac{a_n}{a_{n+1}} - B_{n+1} \geq \rho > 0$$

hence

$$B_n \cdot a_n - B_{n+1} \cdot a_{n+1} \geq \rho a_{n+1}. \tag{14}$$

Since the difference on the left hand side is thus positive it follows that the products $B_n \cdot a_n$ form a monotone decreasing sequence (of positive terms). So this sequence has a limit, say

$$\lim_{n\to\infty}(B_n \cdot a_n) = \alpha \geq 0.$$

If we now add, respectively, the left hand side and the right hand side terms in inequality (14) from stage m to k we get

$$(B_m \cdot a_m - B_{m+1} \cdot a_{m+1}) + (B_{m+1} \cdot a_{m+1} - B_{m+2} \cdot a_{m+2}) + \ldots$$
$$+ (B_{k-1} \cdot a_{k-1} - B_k \cdot a_k) \geq \rho a_{m+1} + \ldots + \rho a_k$$

which reduces to

$$(B_m \cdot a_m - B_k \cdot a_k) \geq \rho(a_{m+1} + \ldots + a_k).$$

Consequently, for $k \to \infty$

$$(B_m \cdot a_m - \alpha) \geq \rho \cdot \sum_{j=m+1}^{\infty} a_j$$

Which shows that $\sum a_n$ is indeed convergent. This proof certainly establishes its result, i.e. it shows that Kummer's test works. But it fails to explain or even address the very aspect of this test which makes it so puzzling. – How come that the C_n in (11) can be replaced by terms B_n of a *completely arbitrary* sequence (as long as they are positive) and we still get a convergence test? Here is Pringsheim's explanation.

We first note elementary results concerning the representation of the terms c_n and d_n of convergent resp. divergent series.[11] For any $\sum c_n$ we can pick a strictly increasing sequence (M_n) of positive numbers satisfying $\lim_{n\to\infty} M_n = +\infty$ such that

$$c_n = \frac{M_n - M_{n-1}}{M_n \cdot M_{n-1}}. \tag{15}$$

And conversely, every series whose terms are defined in this way is convergent.

In case of a divergent series $\sum d_n$ we can find a sequence (M_n) as above such that

$$d_n = \frac{M_n - M_{n-1}}{M_{n-1}}. \tag{16}$$

And conversely, every series whose terms are defined in this way is divergent.

Now let's assume that for some sequence (B_n) of positive numbers we have

$$\lim_{n\to\infty}(B_n \cdot \frac{a_n}{a_{n+1}} - B_{n+1}) > 0. \tag{17}$$

We have to show that $\sum a_n$ converges.

Considering $\sum b_n$, where $b_n = \frac{1}{B_n}$, there are only two cases possible. Either $\sum b_n$ converges, i.e. the sequence (B_n) is of type (C_n), then $\sum a_n$ converges because of criterion (11). Or, on the other hand, $\sum b_n$ is divergent, hence (B_n) is of type (D_n) and we can reformulate our assumption (17) thus

$$\lim_{n\to\infty}(D_n \cdot \frac{a_n}{a_{n+1}} - D_{n+1}) > 0.$$

This implies that there is a $\rho > 0$ such that for appropriate $m \geq 1$ we have for all $n \geq m$

$$D_n \cdot \frac{a_n}{a_{n+1}} - D_{n+1} \geq \rho$$

equivalently

$$\frac{1}{\rho} \cdot D_n \cdot \frac{a_n}{a_{n+1}} - \frac{1}{\rho} \cdot D_{n+1} \geq 1. \tag{18}$$

Now, clearly, if $\sum d_n$ is divergent then so is $\sum \rho \cdot d_n$. Hence the terms $\rho \cdot d_n$ can be expressed by means of a sequence (M_n) according to (16) in the following way

$$\rho \cdot d_n = \frac{M_n - M_{n-1}}{M_{n-1}}$$

which yields

$$\frac{1}{\rho} \cdot D_n = \frac{1}{\rho \cdot d_n} = \frac{M_{n-1}}{M_n - M_{n-1}}.$$

By substitution for $\frac{1}{\rho} \cdot D_n$ in (18) we get

$$\frac{M_{n-1}}{M_n - M_{n-1}} \cdot \frac{a_n}{a_{n+1}} - \frac{M_n}{M_{n+1} - M_n} \geq 1.$$

Subtracting 1 and multiplying by M_n gives

$$\frac{M_n \cdot M_{n-1}}{M_n - M_{n-1}} \cdot \frac{a_n}{a_{n+1}} - M_n \cdot (1 + \frac{M_n}{M_{n+1} - M_n}) \geq 0$$

that is

$$\frac{M_n \cdot M_{n-1}}{M_n - M_{n-1}} \cdot \frac{a_n}{a_{n+1}} - \frac{M_{n+1} \cdot M_n}{M_{n+1} - M_n} \geq 0. \tag{19}$$

Yet according to the converse statement following (15) the terms $\frac{M_n \cdot M_{n-1}}{M_n - M_{n-1}}$ and $\frac{M_{n+1} \cdot M_n}{M_{n+1} - M_n}$ define terms $C_n = \frac{1}{c_n}$ and $C_{n+1} = \frac{1}{c_{n+1}}$ such that $\sum c_n$ converges. In other words, (19) can be written in the form

$$C_n \cdot \frac{a_n}{a_{n+1}} - C_{n+1} \geq 0$$

from which the convergence of $\sum a_n$ follows because of (9).
This finishes the proof of Kummer's test:

$$\lim_{n\to\infty} (B_n \cdot \frac{a_n}{a_{n+1}} - B_{n+1}) > 0 \quad \Longrightarrow \quad \sum a_n \ \text{ converges}.$$

According to Pringsheim this proof gives "the true reason why the C_n which naturally occur in (5) can eventually be replaced by *completely arbitrary positive* numbers B_n" (Pringsheim 1916, p. 379).

Although Pringsheim's proof of Kummer's test explains why an arbitrary sequence (B_n) occurs in it, it does not by itself solve a further mystery about Kummer's test, i.e. its apparent isolation within the general theory of convergence tests. According to Pringsheim (as already quoted above) Kummer's test seemed totally erratic because of its surprising generality and because it completely lacks, as a convergence test of the 2^{nd} kind, any analogue among the convergence tests of the 1^{st} kind. Pringsheim wants to free it from this (apparent) isolation and "show how it naturally fits into a systematically developed general theory". (Pringsheim 1916, p. VI)[12] To be sure, Pringsheim's explanatory proof already achieves something towards this goal of integration by making fully explicit how this test is connected with the basic form of comparison tests (9)-(12), but it doesn't relate it in any way to comparison tests of the 1^{st} kind. In order to do that and to remove the structural asymmetry Pringsheim supplies the missing analogue to Kummer's test by constructing a test of the 1^{st} kind exhibiting the same extreme generality. What Pringsheim is engaged in here is yet another explanatory project which goes beyond giving explanatory proofs. Rather, he aims at a "global" explanation of Kummer's test by embedding it in a reorganized theory. This kind of explanatory concern ties in very well with Pringsheim's approach to the foundations of complex analysis (cf. Mancosu 2001), and it also shows, again, that explanations in mathematical practice come in a wide variety. It certainly deserves to be analyzed in more detail and we refer the interested reader to part II of the appendix where we provide a derivation of Pringsheim's analogue to Kummer's test; however, since Steiner addresses almost exclusively *proofs* and their explanatoriness, we will focus in what follows on Pringsheim's proof of Kummer's test.

7. A TEST CASE FOR STEINER'S THEORY

How well can Steiner account for Pringsheim's explanation? An analysis of the explanatory nature of Pringsheim's proof would have to proceed from a characterizing property of some entity or structure in the result to be proved, i.e. in Kummer's convergence test (13). The proof counts as explanatory according to Steiner only if it makes it evident that the conclusion depends on this property. But here we already face a major difficulty. All "entities" in Kummer's test are *generic*, no concrete objects are mentioned in it (apart from the number 0 of course, but the proof is clearly not based on any characterizing property of 0). This generality makes it hard to come up with a property that uniquely determines some entity within a family of them. Indeed, the *complete arbitrariness* of the sequence (B_n) in (13) makes Steiner's account come unstuck. It is obvious that this arbitrary sequence (B_n) is the focus of Pringsheim's proof. After all, it is the very feature of Kummer's test that makes it so puzzling, thus prompting Pringsheim to provide an explanatory proof (different from Stolz's proof which verifies but doesn't explain the result). Yet, (B_n) cannot be "characterized" in any way – the imposition of any constraining property would obviously result in *non*-arbitrariness! An arbitrary sequence simply cannot be distinguished – qua *arbitrary* sequence – within the family of all sequences by any property. That's just what it means to be arbitrary. Hence one couldn't base any proof on a characterizing property of (B_n) (nor of (a_n) for that matter, which are equally arbitrary), and so it's no surprise that no such property appears in Pringsheim's proof. Consequently, Steiner's account renders it non-explanatory because it fails to satisfy a necessary condition for explanatoriness. In other words, with respect to Pringsheim's proof Steiner finds himself plainly at odds with the *practice of explanation* in mathematics.

At this point one might object the following.[13] Although (B_n) stands for an arbitrary sequence of positive terms, any such sequence has the property of giving rise to a series which is either convergent or divergent. And this in turn holds if and only if the terms B_n can be represented according to the formulas (15) or (16) respectively. These representational facts are central to Pringsheim's proof. Exploiting them distinguishes it from Stolz's proof and constitutes a distinctive feature of it as an *explanatory* proof – as Pringsheim would argue. However, Steiner could maintain his account and make it work based on the following disjunctive property $C(x)$ *or* $D(x)$, which also incorporates the representation expressed by (16). Define $C(x)$ to be true of a sequence (B_n) if and only if for all $n \geq 1$, $B_n > 0$ and $\Sigma \frac{1}{B_n}$ converges; and define $D(x)$ to be true of a sequence (B_n) if and only if for any $\rho > 0$ there exists a strictly increasing sequence (M_n), $n = 0, 1, 2, \ldots$, of positive numbers satisfying $\lim_{n\to\infty} M_n = +\infty$ such that for all $n \geq 1$, $\frac{1}{\rho} \cdot B_n = \frac{M_{n-1}}{M_n - M_{n-1}}$.

Pringsheim's proof clearly invokes and relies on the property $C(x)$ *or* $D(x)$, one can as it were "read it off" the proof structure directly.[14] Moreover this property is both necessary and sufficient for being an (arbitrary) sequence of positive numbers. Thus we have apparently managed to identify a characterizing property of (B_n) after all.

This, however, is not the case. Steiner's account cannot be salvaged in this way. On closer inspection it turns out that $C(x)$ *or* $D(x)$ won't do as a characterizing property. To begin with, we should like to point out how the *problem of characterizing arbitrariness* recurs with respect to $C(x)$ *or* $D(x)$, which can be seen as the dual difficulty of the one mentioned above. Let's recall Steiner's definition of 'characterizing property'. It is defined as "a property unique to a given entity or structure within a *family* or domain of such entities or structures" (Steiner 1978, p. 142), i.e. such a property "picks out one from a *family*" (Steiner 1978, p. 147). One of Steiner's own paradigm examples, as mentioned already earlier, is "having a certain prime power expansion", which uniquely determines a number n within the domain of all natural numbers. Now, it is obvious that the property $C(x)$ *or* $D(x)$ is *not* a characterizing property according to this definition, it *fails* to pick out any *particular* sequence of positive numbers. In this respect it is analogous for instance to the property "n is even or n is odd", which does not single out any particular element from the set of natural numbers. So $C(x)$ *or* $D(x)$ cannot be used by Steiner to account for the explanatoriness of Pringsheim's proof; as a (supposedly) *characterizing* property of sequences, being true of *every* sequence in the domain, it fails as badly as it is possible for a property to fail. We can now sum up Steiner's predicament as follows. No property which is indeed unique to a certain sequence. i.e. which in fact "picks out one from a family", can characterize arbitrary sequences in general. On the other hand, a property like $C(x)$ *or* $D(x)$ which holds true of *all* (and only) sequences of positive numbers fails to be characterizing in Steiner's sense.

This conclusion is based on the most straightforward understanding of the notion *characterizing property* in our context, namely as a property applying to an individual sequence. It might be tempting to think that the above predicament could be avoided by an appropriate reconstrual of that notion. So we have to explore in detail other options of interpreting 'characterizing property' and point out why none of them works. More precisely, we will show that neither construing $C(x)$ *or* $D(x)$ as characterizing a *set* of sequences (as opposed to an individual sequence), nor the weakening of *characterization* to *partial characterization* (of individual sequences) succeeds in the twofold task of (i) rendering $C(x)$ *or* $D(x)$ a characterizing property and, in turn, Pringsheim's proof explanatory; while (ii) remaining consistent with Steiner's theory in other respects especially concerning his own examples of characterizing properties and of explanatory as well as non-explanatory proofs. But before taking this up we need to address an even more basic problem, which is *completely independent* of how we construe the notion of characterizing property, yet whose solution is a *prerequisite* for a precise statement of Steiner's theory in the first place.

However 'characterizing property' might be defined in particular, it has to be first of all a property of "an entity or structure *mentioned in the theorem*" (Steiner 1978, p. 143 our italics, cf. also p. 147). And here we come up against a difficulty in Steiner's theory. Failing to provide any definitions of 'entity','structure', and most important 'mention in a theorem' Steiner left his theory vague or incomplete in crucial respects. In the absence of clear criteria to determine which, if any entities or structures are indeed mentioned in a theorem we may be unable in certain cases to even get started on applying Steiner's theory. What, for instance, is mentioned in Kummer's test? Certainly no object like *the* generic arbitrary sequence (whatever that may be); earlier we were speaking loosely when we said that apart from the number 0 all entities (or rather "entities") mentioned in Kummer's test (13) were generic. There are no singular terms in (13) referring to (particular or generic) sequences. The expression 'B_n' is to be construed as a *variable* in the scope of a universal quantifier (and the same holds for the expression 'a_n'). Hence unless we take (the elements in) the domain of discourse over which the quantifiers range as something which is "mentioned in a theorem" – and prima facie it is by no means clear whether this is the right way to go – there is *no explicit mention of sequences* in Kummer's test. Consequently, if we should have good reasons *not* to count quantifier ranges among what is mentioned in a theorem, then the whole issue as to whether or not $C(x)$ *or* $D(x)$ is a characterizing property would simply be preempted – there being no appropriate, i.e. mentioned, entity in Kummer's test which it could be the property of. In other words this attempt to make Steiner's account work vis à vis Pringsheim's proof would seem wrong-headed from the very start, and the same goes for any other attempt based on a supposedly characterizing property of sequences.

It is important to emphasize that we are dealing here not just with a marginal problem which comes up only with respect to quantifier ranges or in the context of Kummer's test. The problem is much more general. Take for instance a theorem containing the predicate 'x is prime'. Does this theorem mention the *property* (or the *concept*) of being prime, the *set* of all prime numbers, all the *individual prime numbers*, or none of the foregoing? Steiner remains silent on how to answer questions like this one in general; and some of the examples he provides rather than clarifying things add even further to the confusion – witness his remarks concerning explanatory proofs of the summation theorem

$$\text{(20)} \qquad \text{For all } n, \quad 1+2+\cdots+n = \frac{n(n+1)}{2}.$$

Steiner's remarks imply that he apparently takes the symmetry properties as well as the geometrical properties of the sum $1+2+\cdots+n$ as something – entities or structures? – *mentioned* in (20). This is very puzzling indeed and just highlights the need for precise definitions here. In the absence of such definitions, to repeat our point from above, we don't even have a clear enough grasp of Steiner's theory in order to apply and assess it in general.

Luckily, for our purpose of assessing Steiner's theory vis à vis Pringsheim's proof we don't need to solve the general problem. And concerning the question whether or not (the elements in) the range of quantifiers should be taken, on Steiner's view, to be indeed – explicitly or perhaps implicitly – mentioned in Kummer's test we don't have to resort to mere speculation either, since Steiner provides an answer to a question exactly parallel to ours when he discusses the inductive proof of theorem (20) above. Steiner argues that this proof is not explanatory because it lacks a characterizing property.

> "The proof by induction does not characterize anything mentioned in the theorem. Induction, it is true characterizes the *set* of all natural numbers; but this *set* is not mentioned in the theorem" (Steiner 1978, p. 145, emphasis in the original).

The set **N** of natural numbers is the range of the universal quantifier in (20) as the set **B** of sequences of positive numbers is the range of the universal quantifier in (13), Kummer's test, (once its quantificational structure has been made fully explicit). Moreover, although $C(x)$ *or* $D(x)$ clearly fails as a characterizing property of any particular sequence it can be argued, very much in line with one of Steiner's own examples,[15] that it does characterize the set **B** since we have for every sequence s of real numbers

$$C(s) \textit{ or } D(s) \leftrightarrow s \in \mathbf{B}.$$

However, if according to Steiner the principle of induction does not characterize anything mentioned in (20), then by the same token neither $C(x)$ *or* $D(x)$ nor, for that matter, any other property true of all and only sequences of positive numbers characterizes anything mentioned in (13). To paraphrase Steiner: $C(x)$ *or* $D(x)$, it is true, characterizes the *set* **B** (within some family of sets of sequences); but this *set* is not mentioned in Kummer's test.[16]

This presents a real stumbling block for any attempt to account for the explanatoriness of Pringsheim's proof based on a property true of all and only sequences of positive numbers. Insisting that any such property is indeed a (characterizing) property of something mentioned in (13) implies, to repeat our point, by parity of reason, the rejection of Steiner's explicit claim that the principle of induction fails to be a property of anything mentioned in (20). Now, Steiner might be willing to give up his position here in order to account, in turn, for the explanatoriness of Pringsheim's proof (that is, pending its deformability into related proofs), since prima facie this concession might seem a relatively small price to pay.[17]

After all, it is *not* tantamount to pronouncing the inductive proof of (20) explanatory – which would indeed be very counterintuitive! More would be needed for that as Steiner himself emphasizes.

> "[...] a characterizing property is not enough to make an explanatory proof. One must be able to generate new, related proofs by varying the property and reasoning again. Inductive proofs usually do not allow deformation, since before one reasons one must have already conjectured the theorem" (Steiner 1978, p. 151 fn. 11).

Unfortunately for Steiner, though, the inductive proof of (20) *does* allow for deformation. Let us briefly sketch how it works. The property to be varied is the principle of induction which characterizes **N** within the family of, say, sets in the power-set of **N**. As a property of sets it contains a free set variable X.

$$1 \in X \ \&\ \forall x(x \in X \rightarrow (x+1) \in X)\ \&$$
$$\forall P[(P(1)\ \&\ \forall x(P(x) \rightarrow P(x+1))) \rightarrow (\forall x \in X,\ P(x))]$$

We'll use '$IND(1, x+1)$' as a convenient shorthand thus also clearly displaying its parameters. It should be obvious that $IND(1, x+1)$ besides characterizing **N** also passes Steiner's *dependence test* which is necessary to make a proof explanatory. This test requires

> "[...] that from the proof it is evident that the result depends on the [characterizing] property. It must be evident, that is, that if we substitute in the proof a different object of the same domain, the theorem collapses" (Steiner 1978, p. 143).

Trivially, theorem (20) could not be established by an inductive proof without $IND(1, x+1)$. In other words, if we substitute in the proof a different set of our domain, i.e. a proper subset of **N**, and a corresponding, different (restricted) induction principle, then we are clearly blocked from concluding (20).

Let's now turn to "deformations" of the principle of induction. Appropriate variation of $IND(1, x+1)$ yields characterizing properties of different sets in the given family. Below we list in pairs deformed induction principles and the respective sets characterized by them (a and b denote natural numbers).

$IND(2, x+2)$	$\mathbf{E} = \{2, 4, 6, 8, \ldots\}$
$IND(3, x+3)$	$\mathbf{T} = \{3, 6, 9, 12, \ldots\}$
$IND(a, x+a)$	$\mathbf{M}_a = \{a, 2a, 3a, 4a, \ldots\}$
$IND(2a, x+2a)$	$\mathbf{E}_a = \{2a, 4a, 6a, 8a, \ldots\}$
$IND(1, x+2)$	$\mathbf{O} = \{1, 3, 5, 7, \ldots\}$
$IND(a, x+2a)$	$\mathbf{O}_a = \{a, 3a, 5a, 7a, \ldots\}$
$IND(a, x+b)$	$\mathbf{L}_{a,b} = \{a,\ a+b,\ a+2b,\ a+3b, \ldots\}$
$IND(2, x+1+\sqrt{4x+1})$	$\mathbf{Q} = \{1\cdot 2,\ 2\cdot 3,\ 3\cdot 4,\ 4\cdot 5, \ldots\}$

We use lower case letters as variables ranging over the elements in the sets named by the respective upper case letters. For any variable 'v' and its respective range V, we use 'v^+' as notation for the successor of v in V. The following rendering of theorem (20), which incorporates this successor notation, will be the basis for the array of related theorems obtained by a process of deformation.

$$\text{(21)} \qquad \text{For all } n, \quad 1+2+\cdots+n = \frac{n\cdot n^+}{2} = \frac{n\cdot n^+}{2(n^+ - n)}.$$

And here are the related theorems.

$$\text{(22)} \qquad \text{For all } e, \quad 2+4+\cdots+e = \frac{e\cdot e^+}{2(e^+ - e)} = \frac{e\cdot e^+}{4}.$$

$$\text{(23)} \qquad \text{For all } t, \quad 3+6+\cdots+t = \frac{t\cdot t^+}{2(t^+ - t)} = \frac{t\cdot t^+}{6}.$$

$$\text{(24)} \qquad \text{For all } m_a, \quad a+2a+\cdots+m_a = \frac{m_a\cdot m_a^+}{2(m_a^+ - m_a)} = \frac{m_a\cdot m_a^+}{2a}.$$

(25) $$\text{For all } e_a, \quad 2a+4a+\cdots+e_a = \frac{e_a \cdot e_a^+}{2(e_a^+ - e_a)} = \frac{e_a \cdot e_a^+}{4a}.$$

(26) $$\text{For all } o, \quad 1+3+\cdots+o = \frac{o \cdot o^+ + 1}{2(o^+ - o)} = \frac{o \cdot o^+ + 1}{4}.$$

(27) $$\text{For all } o_a, \quad a+3a+\cdots+o_a = \frac{o_a \cdot o_a^+ + a^2}{2(o_a^+ - o_a)} = \frac{o_a \cdot o_a^+ + a^2}{4a}.$$

(28) $$\begin{aligned}\text{For all } l_{a,b}, \quad a+(a+b)+\cdots+l_{a,b} &= \frac{l_{a,b} \cdot l_{a,b}^+ + ab - a^2}{2(l_{a,b}^+ - l_{a,b})} \\ &= \frac{l_{a,b} \cdot l_{a,b}^+ + ab - a^2}{2b}.\end{aligned}$$

(29) $$\text{For all } q, \quad 2+\cdots+q = \frac{q \cdot q^+}{\frac{3}{2}(q^+ - q)}.$$

A few comments are in order. Each of the theorems results from deforming the inductive proof of (20) by substituting a different subset of **N** together with its corresponding induction principle. Throughout the array of these proofs the "proof idea", induction (in various forms), is held constant. Although theorems (22), (23), (24), and (25) show in a straightforward way how theorem (20) changes in response to substituting in place of **N**, respectively, the set of even numbers, the set of multiples of 3, then more generally the set of multiples of a, and the set of even multiples of a; it has to be kept in mind that as a rule the process of "deformation" involves reworking, "not just mechanical substitution" (Steiner 1978, p. 147). In the case of (26) concerning the set **O** of odd numbers we need to observe that the recursive characterization of the members of **O** by $IND(1, x+2)$ yields $o = 1+2k$, $o^+ = (1+2k)+2$, for some $k \geq 0$. Hence $\frac{o \cdot o^+}{2(o^+ - o)} = \frac{4k^2+8k+3}{4}$. Each summand in the numerator, except 3, is divisible by 4, so in order to ensure getting an integer as a result we add 1 to the numerator and thus arrive at formula (26), which is then proved by induction according to $IND(1, x+2)$. (Of course, subtracting 3 may seem, prima facie, an equally plausible alternative here, but adding 1 is favored by staying closer to the original form of the summation theorem, i.e. by keeping the deformation minimal. Also the choice between between "adding 1" and "subtracting 3" can be decided by checking the resulting formulas against the summation of $1+\cdots+o$ letting $o=1$ (and $o^{+}=3$). It has to be stressed, however, that this slight element of trial and error can be completely avoided once the theorems (20) and (22) have been established.[18]) Deformations of a very similar kind[19] lead to further generalizations expressed in (27) and (28). The latter is a general theorem covering the summation of *arbitrary linear progressions* of natural numbers. Moreover, one can generalize even beyond linear progressions, as shown by (29), if one doesn't stick exclusively to deformations by means of additive terms involving only constants (and parameters).[20]

Although we could continue our list of generalizations of theorem (20) we stop here because the point should be clear by now. The inductive proof of theorem (20) meets all of Steiner's requirements to count as explanatory[21] – *provided*, that is, quantifier ranges are indeed taken to be entities which are *mentioned* in theorems. This puts Steiner in a dilemma. If he maintains that in general theorems make *no* mention of quantifier ranges, then $C(x)$ *or* $D(x)$ is ruled out out as a characterizing property. And since this is the most promising, perhaps even the only, candidate for such a property that could render Pringsheim's proof explanatory, Steiner's account seems bound to *undergenerate*, i.e. it seems thus blocked from fully capturing the intuitive notion of explanatory proof operative in mathematical practice. On the other hand, including quantifier ranges among the entities mentioned in theorems results in *overgeneration* by declaring, as we have just seen, the inductive proof of (20) explanatory, which it clearly isn't – neither by Steiner's own lights nor, as a rule, according to the understanding of working mathematicians (some mathematicians even take inductive proofs to be *paradigms of non-explanatory* proofs). So either way Steiner's theory runs counter to mathematical praxis.

Let us note, for the records, that this gives rise to an independent criticism of Steiner's account, since we can easily restate theorem (20), without any changes to its proof, avoiding sorted variables and making sure **N** is explicitly mentioned in it.

$$\text{For all } x \text{ in } \mathbf{N}, \quad 1+2+\cdots+x=\frac{x(x+1)}{2}.$$

Now overgeneration is inevitable. Furthermore, it seems quite odd that Steiner's theory *qua* theory of the explanatoriness of *proofs* should turn out to be so overly sensitive to what appears to be a rather minor detail in the exact wording of a theorem which doesn't affect its proof.

Setting aside now the issue concerning quantifier ranges, let us investigate further how Steiner's account fares in the attempt to render Pringsheim's proof explanatory in terms of $C(x)$ *or* $D(x)$ as a characterizing property of the set **B**. After all, despite the fact that Steiner's account overgenerates there is still a question of independent interest as to whether or not it undergenerates as well. So let us grant that the set **B** of sequences of positive numbers is in some way or other indeed mentioned in Kummer's test. Then $C(x)$ *or* $D(x)$ does characterize **B**, and this property is also clearly exploited in Pringsheim's proof. But Steiner requires more, i.e. $C(x)$ *or* $D(x)$ has to pass Steiner's *dependence test*. In other words, it must be evident "that if we substitute in the proof a different object of the same domain, the theorem collapses" (Steiner 1978, p. 143). This raises the question, first of all, what the domain should be taken to consist of. When Pringsheim gives his proof of Kummer's test he is working exclusively with sequences of positive numbers, hence it appears most natural to take the power-set of **B** as the domain – from which **B** is then singled out by our characterizing property. However, this is already as far as we can get within Steiner's theory, since $C(x)$ *or* $D(x)$ obviously fails the dependence test. Once again it is the extreme generality of Kummer's test which creates a problem here. Since this convergence test works for *arbitrary* sequences (B_n) of positive numbers, it clearly *won't collapse* no matter what subset of **B** gets substituted and its proof won't really be affected by it either! In order to make Kummer's test collapse we have to go outside of **B** and allow sequences to contain arbitrary real numbers $\neq 0$, positive and negative.[22] This constitutes already a deviation from Pringsheim's original setting yet even further adjustments are needed to make Steiner's theory work. Letting **S** be the set of arbitrary sequences of non-zero real numbers and $\mathcal{P}$ the power-set of **S**, we could, as a first try, take our domain $\mathcal{D}$ to contain the elements of $\mathcal{P}$ minus all the proper subsets of **B**. However, a closer look at Kummer's test, which is stated in terms of a *limit*, and at Pringsheim's proof reveals that neither of them demands (B_n) to consist *exclusively* of positive numbers. Kummer's test still holds good and Pringsheim's proof goes through if we only require that *all but finitely many* terms of (B_n) are positive, i.e. that there exists an m such that for all $n \geq m$, $B_n > 0$; finite initial segments of (B_n) don't matter. In other words, substituting for **B** the set $\mathbf{B}^*$, the superset of **B** which comprises all such "eventually positive" sequences, won't make the theorem (nor the proof of it) collapse. Hence $C(x)$ *or* $D(x)$ still fails the dependence test with respect to domain $\mathcal{D}$, that is, it does not fully capture – neither in the technical sense of Steiner's theory nor in the intuitive sense – what property of (B_n) Kummer's test really depends on.

At this point Steiner has two options.[23] He could either further tailor the domain $\mathcal{D}$ to the purpose at hand by simply excluding $\mathbf{B}^*$ (and various other sets) from it, thus ensuring by brute force that $C(x)$ *or* $D(x)$ passes the dependence test. But this is unacceptable not only because it amounts to a completely artificial, *ad hoc* "immunization manoeuvre" to save his theory in the face of recalcitrant data. More importantly, such a move goes against the spirit of Steiner's theory. On his account the explanation provided by a proof consists (besides generalizability) in showing that and how the proved theorem depends on a certain characterizing property. In other words, an explanatory proof makes it evident that the characterizing property in question pinpoints the reason why, "essentially", the theorem is true. As we have seen, restricting quantification to elements of $\mathbf{B}$ is not essential for the truth of Kummer's test, it is a sufficient but not a necessary condition. So, pronouncing Pringsheim's proof explanatory in virtue of a spurious dependence of Kummer's test on the property $C(x)$ *or* $D(x)$ yields a correct result for a wrong reason.

Steiner's other option is to first *generalize* Kummer's test by explicitly turning it into a convergence test quantifying over sequences from $\mathbf{B}^*$, i.e. to get the dependence right, and then account with his theory for the explanatoriness of an – equally generalized – proof of it. This would then have to be done in terms of a correspondingly generalized property $C^*(x)$ *or* $D^*(x)$. But now the property $C(x)$ *or* $D(x)$ as well as Pringsheim's original proof are out of the picture, instead we are dealing with a *different* proof (and a *different* theorem), even though the difference consists merely in a slight generalization. Steiner can't claim that the two proofs are "essentially the same", since one turns out to be explanatory (if everything works out) while the other one doesn't. So we have to take them as in fact two distinct proofs. But in this case rendering one of them explanatory doesn't tell us anything about the explanatoriness of the other. Hence Pringsheim's proof still escapes Steiner's theory.

Let us finally look at the interpretation of $C(x)$ *or* $D(x)$ as a *partially characterizing* property of sequences – if only to point out why it won't help. Steiner concedes that in order to account for the explanatoriness of certain proofs the notion of *characterization* has to be weakened to that of *partial characterization*. It is quite common

> "to study domain X by assigning a counterpart Y to each object in X. The object in Y need not uniquely characterize anything in X; examples are Galois theory and algebraic topology" (Steiner 1978, pp. 149f).

One worry one might have here at the outset concerning the introduction of partially characterizing properties is the danger of inflation. Although Steiner doesn't give us much to go on, presumably *any* property counts as partially characterizing unless, in the extreme cases, a property happens to be either empty or true of everything of the domain (which seem to be the only cases in which we can't plausibly claim that such a property characterizes anything at all even partially). Thus once partially characterizing properties are admitted to account for explanatoriness Steiner may find himself on the slippery slope to a vast overgeneration of his account.

Given the plausible restrictions on the notion of *partial characterization* just mentioned it follows that $C(x)$ *or* $D(x)$, being true of *every* sequence in Pringsheim's domain, won't even pass as a partially characterizing property of sequences. Which shouldn't come as a surprise since the arbitrariness of (B_n) excludes even partial characterization within the domain **B**. Again we have to move beyond **B** and, in turn, the problem of passing the dependence test recurs.

So far all our attempts to get Steiner's theory off the ground vis à vis Pringsheim's proof have failed. Our best candidate for a (partially) characterizing property turned out either not to be (even partially) characterizing at all or still unable to do the job of rendering Pringsheim's proof explanatory. And in the absence of a characterizing property it doesn't even make sense to ask whether or not Steiner's second main criterion for explanatory proofs, generalizability, is satisfied. Because generalizability *presupposes* that there is in fact a characterizing property on which the theorem depends such that if we substitute (the characterizing property of) a different object of the domain we get a related "deformed" theorem. One has to be careful here, however, to distinguish generalizability from mere generality. It is well known that Kummer's test, because of its generality, is the source of many other convergence tests. By substituting specific sequences for (B_n) one can obtain from it (or from Pringsheim's proof) as special cases, for instance, D'Alembert's test, Raabe's test, and Bertrand's test (cf. Tong 1994). But these tests are *special cases* of Kummer's test they are *not* gotten by generalizing it in the relevant sense of Steiner's theory. And Steiner himself is very clear about it. He states with respect to an analogous situation

> "The new result is contained within the old. The point is, however, that generalizability through varying a characterizing property is what makes a proof explanatory, not simple generality" (Steiner 1978, p. 146).

In other words, what has turned out again and again to be a difficult problem for Steiner's account, namely the generality of Kummer's test, cannot simply be declared a virtue which renders, by itself, Pringsheim's proof explanatory. There is no such "shortcut" in Steiner's theory from mere generality to explanatoriness.

Although there are many more features of Steiner's theory that deserve thorough reconstruction and critical assessment they are not of major importance in the given context so we conclude our discussion at this point. Enough has been said to bring out the substantial difficulties Steiner's theory has to account for the explanatoriness of Pringsheim's proof of Kummer's test – besides other problems of a general nature which came to light in the course of our investigations. It is our hope that this kind of testing theories of mathematical explanation against the practice of mathematical explanation will pave the the way to further studies in the same vein. This seems to us the most promising approach for making progress in this treacherous area.

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APPENDIX

Part I.

We show how to arrive at the equations (15) and (16) above, i.e. how to represent the terms c_n and d_n of convergent resp. divergent series by means of the positive terms M_n of strictly increasing divergent sequences (cf. Pringsheim 1916, pp. 326ff and 332).

(A) Let c_n be the terms of a convergent series, i. e. $\sum_{n=1}^{\infty} c_n = s$. We set $s_0 = 0$ and, for $n = 1,2,3,\ldots$, $s_n = c_1 + \ldots + c_n$. Notice that $s - s_n > 0$ for all n, since the c_n are positive so $s > s_n$ for all n. We can therefore define, for $n = 0,1,2,\ldots$

$$M_n = \frac{1}{s - s_n}.$$

Since the sequence $(s - s_n)$ is strictly decreasing and converges to 0 it follows that (M_n) is a strictly increasing sequence such that $\lim_{n\to\infty} M_n = +\infty$. Furthermore, we have

$$s - s_n = \frac{1}{M_n}$$

and, for $n = 1,2,...$

$$s - s_{n-1} = \frac{1}{M_{n-1}}$$

Now, for $n = 1,2,...$ it holds that $s_n = s_{n-1} + c_n$ and we can thus write

$$c_n = s_n - s_{n-1} = (s - s_{n-1}) - (s - s_n) = \frac{1}{M_{n-1}} - \frac{1}{M_n} = \frac{M_n - M_{n-1}}{M_n \cdot M_{n-1}}.$$

To show the converse, assume (M_n) to be a strictly increasing sequence $(n = 0, 1, 2, \ldots)$ of positive numbers such that $\lim_{n\to\infty} M_n = +\infty$. Let $c_n = \frac{M_n - M_{n-1}}{M_n \cdot M_{n-1}} = \frac{1}{M_{n-1}} - \frac{1}{M_n}$. Then

$$\sum_{n=1}^{k} c_n = \sum_{n=1}^{k} \left(\frac{1}{M_{n-1}} - \frac{1}{M_n}\right) = \frac{1}{M_0} - \frac{1}{M_k}.$$

As $\lim_{k\to\infty} \frac{1}{M_k} = 0$,

$$\sum_{n=1}^{\infty} \left(\frac{1}{M_{n-1}} - \frac{1}{M_n}\right) = \frac{1}{M_0}$$

hence $\sum c_n$ is convergent.

(B) Let's now turn to the case of a divergent series $\sum_{n=1}^{\infty} d_n$ (such that $d_n > 0$). We first observe that

$$(1+d_1)(1+d_2)\cdots(1+d_k) \geq 1 + \sum_{n=1}^{k} d_n.$$

Since $\sum d_n$ is divergent, the left hand side also diverges as $k \to +\infty$. Furthermore, every factor $(1+d_i)$ in the product is > 1, hence the sequence (M_n) as defined by

$$M_0 = 1$$
$$M_n = (1+d_1)\cdots(1+d_n)$$

is strictly increasing. For $n > 1$ we have $M_{n-1} = (1+d_1)\cdots(1+d_{n-1})$ and by division we get

$$\frac{M_n}{M_{n-1}} = 1 + d_n$$

hence

$$d_n = \frac{M_n}{M_{n-1}} - 1 = \frac{M_n - M_{n-1}}{M_{n-1}}.$$

This equation also holds for $n = 1$ by definition of M_0.

Conversely, let (M_n) be a strictly increasing sequence $(n = 0, 1, 2, \ldots)$ of positive numbers such that $\lim_{n\to\infty} M_n = +\infty$. We have to show that $\sum d_n$ is divergent, where $d_n = \frac{M_n - M_{n-1}}{M_{n-1}}$. We start by noting that $\sum(M_n - M_{n-1})$ is divergent.[24] Because

$$\sum_{n=1}^{k} (M_n - M_{n-1}) = M_k - M_0$$

hence

$$\sum_{n=1}^{\infty} (M_n - M_{n-1}) = (\lim_{n\to\infty} M_n) - M_0 = +\infty.$$

Applying the logarithm function to the terms M_n yields a divergent sequence $(\log M_n)$. Hence the previous result implies that also $\sum(\log M_n - \log M_{n-1})$ diverges. On the other hand, because of the equation

$$\log x < x - 1 \quad \text{for} \ \ x > 0, \ \ x \neq 1$$

and the fact that, for all n, $\frac{M_n}{M_{n-1}} > 1$ we have

$$\log M_n - \log M_{n-1} = \log \frac{M_n}{M_{n-1}} < \frac{M_n}{M_{n-1}} - 1 = \frac{M_n - M_{n-1}}{M_{n-1}}.$$

So by the comparison test (2) we conclude that a series $\sum d_n$ is indeed divergent if its terms satisfy

$$d_n = \frac{M_n - M_{n-1}}{M_{n-1}}.$$

Part II.

In the construction of a convergence test of the 1^{st} kind that exhibits the same kind of generality as Kummer's test Pringsheim proceeds as follows.[25]
As a special case of comparison test (3) we have

$$\text{If for all} \ \ n \geq m, \ \ \text{for some} \ \ m, \ \ C_n \cdot a_n < 1 \quad \Longrightarrow \quad \sum a_n \ \ \text{converges.}$$

Since all partial sums $s_n = \sum_{k=1}^n c_k$ are positive, this is equivalent to

$$\text{If for all} \ \ n \geq m, \ \ \text{for some} \ \ m, \ \ (C_n \cdot a_n)^{\frac{1}{s_n}} < 1 \quad \Longrightarrow \quad \sum a_n \ \ \text{converges.}$$

And by assuming the existence of the involved limit we get

$$\lim_{n \to \infty} (C_n \cdot a_n)^{\frac{1}{s_n}} < 1 \quad \Longrightarrow \quad \sum a_n \ \ \text{converges.} \tag{30}$$

On the other hand, letting M_n as before denote the positive terms of a strictly increasing divergent sequence, we can show the following. (Its proof, though not difficult, is a bit more involved hence we postpone it for the sake of greater perspicuity of the main argument.)

$$\lim_{n \to \infty} \left(\frac{a_n}{M_n - M_{n-1}}\right)^{\frac{1}{M_n}} < 1 \quad \Longrightarrow \quad \sum a_n \ \ \text{converges.} \tag{31}$$

By setting

$$d_1 = M_1 \quad \text{and} \quad d_n = M_n - M_{n-1} \ \ (n = 2, 3, 4, \ldots) \tag{32}$$

we obtain terms of a divergent series.[26] Furthermore we have

$$M_n = M_1 + \sum_{k=2}^{n} (M_k - M_{k-1}) = \sum_{k=1}^{n} d_k = s_n.$$

Thus by observing that $D_n = \frac{1}{M_n - M_{n-1}}$ convergence test (31) can be stated as follows

$$\lim_{n\to\infty} (D_n \cdot a_n)^{\frac{1}{s_n}} < 1 \quad \Longrightarrow \quad \sum a_n \quad \text{converges.} \tag{33}$$

The construction of the terms d_n (resp. D_n) out of the given sequence (M_n) does not - contrary to how it may appear - impose a constraint on the nature of the divergent sequence that can occur in (33), since the terms of *any* divergent sequence $\sum d_n$ admit of such a representation (32) by simply defining the required sequence (M_n) thus

$$M_n = \sum_{k=1}^{n} d_k.$$

(In effect, what we have obtained here is another, simpler and more straightforward, representation of the terms d_n than the one given by (16) above.) Now we are in a position to state a most general convergence test by combining (30) and (33). We only need to note that, obviously, any *arbitrary* positive sequence (B_n) is either of type (C_n) or type (D_n). So we finally arrive at

$$\lim_{n\to\infty} (B_n \cdot a_n)^{\frac{1}{s_n}} < 1 \quad \Longrightarrow \quad \sum a_n \quad \text{converges}$$

where $s_n = \sum_{k=1}^{n} b_k$.

This is the most general convergence test of the 1^{st} kind and with regard to its surpassing generality it thus represents in Pringsheim's theory "the perfect analogue to Kummer's test" (Pringsheim 1916, p. 344).

To complete the foregoing proof it remains to establish proposition (31). We first show that if $\alpha > 1$, $q > 0$, and (M_n) a strictly increasing divergent sequence of positive terms, then α^{M_n} eventually dominates M_n^q, i.e. there exists an m such that for all $n \geq m$

$$\alpha^{M_n} > M_n^q.$$

To this end we start from the elementary inequality

$$e^x > x \quad (\text{for } \; x > 0).$$

Setting $x = \frac{p}{q+1} \cdot M_n$ for arbitrary but fixed $p > 0$, $q > 0$ yields

$$e^{\frac{p}{q+1} \cdot M_n} > \frac{p}{q+1} \cdot M_n \quad (\text{for each } \; n).$$

By raising both sides to the $(q+1)^{st}$ power we get

$$e^{p \cdot M_n} > \left(\frac{p}{q+1}\right)^{q+1} \cdot M_n^{q+1}$$

hence

$$\frac{e^{p \cdot M_n}}{M_n^q} > (\frac{p}{q+1})^{q+1} \cdot M_n.$$

Since $\lim_{n\to\infty} M_n = +\infty$, there is an m such that for all $n \geq m$ the right hand side is greater than 1, thus

$$e^{p \cdot M_n} > M_n^q \quad (\text{for all} \;\; n \geq m).$$

If $\alpha > 1$, then $\log\alpha > 0$ so we can set $p = \log\alpha$ and conclude

$$\alpha^{M_n} = e^{\log\alpha \cdot M_n} > M_n^q \quad (\text{for all} \;\; n \geq m).$$

By letting now $q = 2$ and using the fact that for all n, $M_{n-1} < M_n$ we infer further

$$\alpha^{M_n} > M_n^2 > M_n \cdot M_{n-1} \quad (\text{for all} \;\; n \geq m)$$

hence

$$\frac{1}{\alpha^{M_n}} < \frac{1}{M_n \cdot M_{n-1}} \quad (\text{for all} \;\; n \geq m)$$

and by multiplying by the (positive) factor $M_n - M_{n-1}$

$$\frac{M_n - M_{n-1}}{\alpha^{M_n}} < \frac{M_n - M_{n-1}}{M_n \cdot M_{n-1}} \quad (\text{for all} \;\; n \geq m).$$

We know already (cf. *Part I* above) that the terms on the right hand side are terms c_n of a convergent series, so comparison test (1) implies that also the terms on the left hand side are of type c_n. Substituting them in test (1) yields the following (for $\alpha > 1$)

$$\text{If for all} \;\; n \geq m, \;\; \text{for some} \;\; m, \;\; a_n \leq \frac{M_n - M_{n-1}}{\alpha^{M_n}} \quad \Longrightarrow \quad \sum a_n \quad \text{converges.}$$

Equivalently

$$\text{If for all} \;\; n \geq m, \;\; \text{for some} \;\; m, \;\; (\frac{a_n}{M_n - M_{n-1}})^{\frac{1}{M_n}} \leq \frac{1}{\alpha} \quad \Longrightarrow \quad \sum a_n \quad \text{converges.}$$

Under the assumption that the limit below exists and observing that $\frac{1}{\alpha} < 1$ we eventually obtain proposition (31)

$$\lim_{n\to\infty} (\frac{a_n}{M_n - M_{n-1}})^{\frac{1}{M_n}} < 1 \quad \Longrightarrow \quad \sum a_n \quad \text{converges.}$$

NOTES

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[1]For a discussion of unificationist theories of explanation, such as Kitcher's, see Tappenden's contribution in this volume.

[2]This is far from obvious, see Sandborg 1998.

[3]Kitcher 1984 seems to accept the heterogeneity of mathematical explanations. In his book "The Nature of Mathematical Knowledge" (1984) he recognizes that mathematical explanations "appear heterogeneous": "Thus, at first sight, mathematical explanations, like scientific explanations, appear heterogeneous. Whether we shall some day achieve a single model which covers all cases of scientific explanation - or even of mathematical explanation - I do not know. However, we suggest that any adequate account of explanation in general should apply to the mathematical cases ("data") presented here." (p. 227) However, his later work seems to go against the grain of the previous approach and to imply that a unification account of scientific explanation will be able to account for mathematical explanation in general – "the fact that the unification approach provides an account of explanation, and explanatory asymmetries, in mathematics stands to its credit" (p. 437 of 1989).

[4]Sandborg 1997, chapter 3, developed a similar project but we envisage a different taxonomy.

[5]The proof is given by Steiner, the proof idea being due to G. Kreisel.

[6]Since in what follows we are dealing exclusively with series and sequences of positive real numbers, the qualification "of positive terms" will be omitted throughout.

[7]As a matter of fact this existence assumption is not really needed. The criteria in question can be stated, more generally, in terms of upper limit, in (5), and lower limit, in (6), in place of limits (cf. Pringsheim 1916, p. 318; Bromwich 1942, p. 30). However, the use of the weaker formulations allows us to simplify the exposition without losing anything important in our context. The same goes for (11), (12), and (13) below.

[8]In the case of the convergence test the condition $\frac{a_{n+1}}{a_n} \leq \frac{c_{n+1}}{c_n}$ implies $\frac{a_{n+1}}{c_{n+1}} \leq \frac{a_n}{c_n}$, hence the sequence $\frac{a_n}{c_n}$ decreases monotonically and there is some number γ such that $\frac{a_n}{c_n} \leq \gamma$. Consequently $a_n \leq \gamma \cdot c_n$ for $n \geq m$, for some m, which implies the convergence of $\sum a_n$. The argument in case of the divergence test is analogous.

[9]Its history is somewhat entangled. When Kummer published it in 1835 he imposed the condition that $\lim(b_n a_n) = 0$, which was shown to be superfluous by U. Dini. According to Knopp this test was later "rediscovered several times and gave rise, as late as 1888, to violent contentions on questions of priority" (Knopp 1928, p. 311 fn. 52). Dini as well as Pringsheim improved it (cf. Bromwich 1942, p.37/38).

[10][... daß das Kummersche Konvergenzkriterium] bei der sonstigen Darstellungsweise völlig abseits stand und keinerlei Analogon unter den Kriterien *erster* Art zu besitzen schien (Pringsheim 1916, p. VI).

[11]Proofs of these results are presented in the appendix, part I.

[12][... das Kummersche Konvergenzkriterium] aus dieser rätselhaften Isolierung befreit als natürliches Glied einer folgerichtig aufgebauten allgemeinen Theorie erscheinen zu lassen. (Pringsheim 1916, p. VI)

[13]This possible objection was in fact suggested to us by Klaus Jørgensen.

[14]Another property, $L(x)$, which can be drawn from Kummer's test itself – as well as from Pringsheim's proof – and which appears to characterize perhaps even more precisely than $C(x)$ *or* $D(x)$ those sequences which Kummer's test and its proof are really about, is defined as follows. Let $L(x)$ hold of a sequence (B_n) if and only if for all $n \geq 1$, $B_n > 0$ and there exists a sequence (a_n) of positive terms such that $\lim_n(B_n \cdot \frac{a_n}{a_{n+1}} - B_{n+1}) > 0$. However, we can find for *any* (B_n) a sequence (a_n) such that $\lim_n(B_n \cdot \frac{a_n}{a_{n+1}} - B_{n+1}) = 1 > 0$ by setting $a_1 = 1$ and $a_{n+1} = a_n \cdot (\frac{B_n}{B_{n+1}+1})$. Hence $L(x)$ and $C(x)$ *or* $D(x)$ turn out to be co-extensional after all and any of our arguments below concerning the latter property equally applies to the former. So we will just focus on $C(x)$ *or* $D(x)$.

[15]With respect to an explanatory proof of the Pythagorean Theorem Steiner points out that a right-angled triangle is characterized by the property of being decomposable into two triangles similar to each other and to the whole (Steiner 1978, p. 144). Evidently, this property does not pick out any individual right-angled triangle, the only way to render it in fact *characterizing* seems by taking it as defining the *set* of right-angled triangles.

[16]Nothing hinges on the fact that the principle of induction as a property of sets picks out **N** "directly", i.e. characterizes it in a top-down way whereas $C(x)$ *or* $D(x)$ characterizes S in a bottom-up way via its members. It is clear that $C(x)$ *or* $D(x)$ characterizes, if anything, the *set* of sequences of positive numbers but certainly not any particular such sequence.

[17]It should indeed be a rather small concession on Steiner's part, given that he has to make an analogous move in the context of the Pythagorean Theorem anyway. As pointed out in footnote 15 Steiner declares a property characterizing which picks out the set of right-angled triangles, and that set is no more explicitly mentioned in the Pythagorean Theorem than **N** is mentioned in (20).

[18]The deformation at the level of characterizing properties, or sets, which occurs in the move from $IND(1, x+1)$ to $IND(1, x+2)$, or from **N** to **O**, which is thus evidently effected by *skipping the even numbers* (i.e. the members of **E**) translates directly into the following *subtraction* at the level of summation formulas. $1 + \cdots + o = \frac{n \cdot n^+}{2} - \frac{e \cdot e^+}{4}$, where $n = o$, $e = o - 1$, $n^+ = e^+ = o + 1$. So we have

$$1 + \cdots + o = \frac{2o(o+1) - (o-1)(o+1)}{4} = \frac{o^2 + 2o + 1}{4} = \frac{o(o+2) + 1}{4} = \frac{o \cdot o^+ + 1}{4}.$$

In an analogous way we obtain formula (27) directly from (24) and (25).

[19]In case of (28) we can prove, on the one hand, that adding $(ab-a^2)$ effects a deformation which is *minimal* relative to a family of prima facie equally plausible alternatives. On the other hand one may proceed, again, at the level of sets from the deformation of **N** into $\mathbf{L}_{a,b}$, keeping track of which elements of **N** get skipped. This is then paralleled by a corresponding deformation of the summation formula in the following way. We start by applying formula (20) to $1+\cdots+l_{a,b}$ taking into account the necessary subtractions which correspond to the skipping of numbers in $\mathbf{L}_{a,b}$ (with respect to **N**). To increase perspicuity we abbreviate '$l_{a,b}$' by 'l', denote $1+\cdots+l$ by 'Σ' and also make further use of (20). Thus we readily arrive at the equation

$$\Sigma = \frac{l(l+1)}{2} - [\frac{(a-1)a}{2} + (b-1)(\Sigma - l) + k \cdot \frac{(b-1)b}{2}].$$

The number k appearing here is some natural number ≥ 0 such that $l = a+kb$. From this equation we now work out the resulting deformation of the formula $\frac{l(l+1)}{2}$ step by step.

$$\Sigma + (b-1)\Sigma = \frac{l(l+1)-(a-1)a+2(b-1)l-k(b-1)b}{2}$$

$$\Sigma = \frac{l(l+1)+(b-1)l-a^2+a+(b-1)(l-kb)}{2b}$$

$$\Sigma = \frac{l(l+b)-a^2+a+(b-1)a}{2b} = \frac{l \cdot l^+ + ab - a^2}{2b}.$$

[20]Here is how we arrive at formula (29). $\frac{q \cdot q^+}{2(q^+-q)} = \frac{q(q+1+\sqrt{4q+1})}{2(\sqrt{4q+1}+1)} = \frac{q(\sqrt{4q+1}+3)}{8}$. Checking against one or two concrete summations of elements of **Q** indicates the change to $\frac{q(\sqrt{4q+1}+3)}{6}$. We try here, as before, to make do with just additive constants in order to keep the deformation minimal. Expressing this change in terms of q and q^+ yields $\frac{q \cdot q^+}{\frac{3}{2}(q^+-q)}$, which is then proved by induction according to $IND(2, x+1+\sqrt{4x+1})$.

[21]It is important to note that nothing more is demanded of deformation (or generalizability) in Steiner's account than that it should lead to related theorems; other than that it is explicitly left undefined (cf. Steiner 1978, p. 147). In particular questions as to e.g. the efficiency (or "naturalness", whatever that may mean) either of the process of deformation or of the resulting proofs compared to other methods don't enter at all into Steiner's criteria for explanatoriness. Hence they need not concern us here.

[22]To construct a counterexample let e.g. $a_n = 1$ for all n, and $B_n = -n$.

[23]It should be clear that simply dropping the dependence requirement from the account is *not* an option for Steiner. Dispensing with it leads immediately to overgeneration, i.e. it allows the construction of easy recipes for churning out "explanatory" proofs. For instance, take any proof of a theorem of the form 'For all x, $\varphi(x)$', specialize to some element a in the domain and add on as an idle element in the proof some characterizing property $\psi_a(x)$ of a. Thus we obtain an "explanatory"

proof of '$\varphi(a)$' (no matter how non-explanatory the proof may actually appear to us intuitively): it makes reference to a characterizing property and can also be deformed to yield related results '$\varphi(b)$', '$\varphi(c)$', ... by specializing to other elements b, c, ... in the domain, substituting for $\psi_a(x)$ equally idle characteristic properties $\psi_b(x)$, $\psi_c(x)$, ...

[24]The divergence of (M_n) is the only property that is needed here, neither monotonicity nor the positivity of its terms come in.

[25]We'll focus only on the main steps in the derivation and, as before, simplify matters slightly. For the strongest formulation of the results the reader is referred to Pringsheim 1916, pp. 337-334.

[26]For a proof of the divergence of $\sum d_n$ see part (B) of *Part I* above.

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REVIEL NETZ

THE AESTHETICS OF MATHEMATICS: A STUDY

To the memory of Heda Segvic

1. THE PROBLEM MOTIVATED

Let us start with a trivial example, which however already suggests the outlines of the problem at hand. Imagine I have collected my lunch at a self-service cafeteria so that now my tray holds, say, a paper plate with a sandwich on it, another one with fruit, and finally, a soda in a large cup (the kind known as "small"). Now, as I prepare to detach myself from the counter, I arrange the three objects on the tray. This can be approached through several theoretical perspectives.

First, there is the mathematical-physical perspective, employing the specific field of *statics* (pioneered, as we shall note again below, by Archimedes). The task is to arrange three objects on a plane, so that their individual centres of gravity, and the centre of gravity of the system as a whole, will ensure maximum stability. One should in particular consider the problem of the system's robustness, i.e. how it may react with the disturbances it is likely to undergo as I move towards a table. This is a very complex problem, and the fact that we very often (not always) solve it in effective ways, may indicate our powers of unconscious computation.

The mention of the "unconscious" immediately brings to mind a further relevant theoretical perspective. It may be suggested that the desire to arrange objects in neat, ordered ways could reflect either an obsessive-compulsive disorder, or its more or less universal incipient form. Whatever one thinks of any particular form of *psychoanalysis*, it is clearly a possible way of explaining my acts as I rearrange the objects on the tray. While the mathematical perspective provides a possible functional role for the *arrangement obtained*, the psychoanalytic perspective provides a possible functional role for the *act of arrangement* itself – by uncovering the desires and needs which find their outlet in that act.

Finally, regardless of the desires that motivate the act of arrangement, and regardless of the physical function of that act, one can study the formal properties of the arrangement obtained, this time adopting the perspective of *aesthetics*. Merely as a visual pattern on the tray, the objects possess properties such as symmetry and composition. The driving force that makes *me* align my plates along a precise geometrical configuration is perhaps best understood by the psychoanalyst. However, some of the properties of the alignment I achieve are not psychological, but aesthetic: they belong, so to speak,

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not to the psychopathology of everyday life, but to the *aesthetics* of everyday life. Anxiety was momentarily warded off; but on the tray itself, we observe not the absence of anxiety, but the presence of, say, the golden section. In general, then, *statics* studies the function of the arrangement obtained; *psychoanalysis* studies the function of the act of arrangement; and *aesthetics* studies certain objective features of the arrangement obtained (which are of course difficult to characterize precisely). We may perhaps say (if only so as to have the word "function" available for our use on all occasions) that aesthetics studies the *aesthetic* function of an object. This is useful because now we can note that the various functions differ as to their dominance in different contexts. We may observe, for instance, how people rearrange their trays as they sit down on the table: the tray safely in place, the physical function lost its dominance and the aesthetic function is dominant instead.

I have introduced two theses. One is that the aesthetic function is ubiquitous; the second is that, in different contexts, it may be more or less dominant. Such theses have long been current (their best statement probably remains Mukarovsky (1970), translation of a work written in 1936), though perhaps interest in the aesthetic function of literary works has more recently waned in the English-speaking world. At any rate, the trivial example I have delineated is meant to introduce the idea of the aesthetic function of *mathematical texts*.

Thus, we should not be concerned about the fact that mathematical texts have obvious, overt functions (akin to the static features of the tray in my example), e.g. to obtain the truth of mathematical results for some possible mathematical or physical applications. This overt function can be separated, analytically, from the aesthetic features of a mathematical text. (Of course, there might be interesting interactions between such overt functions and the aesthetic function.) Further, we need not be concerned about another fact, that mathematical texts – like all texts – are motivated by all sorts of external forces, such as the sociological realities of publication and tenure, comparable to the psychological processes suggested to underlie my ordering of plates on the tray. (Once again, though, sociological factors may interestingly interact with the aesthetic factors.) Finally, I wish to stress – this is the main point of my trivial example – the mundane nature of the aesthetic. At least to begin with, in this article I do not intend to wax lyrical about the beauty of mathematics. Mathematical works are sometimes great works of art, sometimes (even when they are of considerable mathematical value) their presentation is boring and pedestrian. It is not my contention that mathematical texts are particularly beautiful, more so than other types of human expression. Rather, like all types of human expression, they possess, among other things, an aesthetic dimension.

Yet the question of mathematical beauty is of special urgency. Mathematicians – it seems, more than most other scientists – often claim to be motivated by the aesthetic dimension. To take the most famous example, Hardy insisted that "The mathematician's patterns, like the painter's or the poet's, must be *beautiful*... Beauty is the first test: there is no permanent place in the world for ugly mathematics" (Hardy, 1967, 85).[1] Hardy then went on to contrast the beauty of mathematics with its – as he claimed – inutility. To be precise, Hardy argued that the utility of a given piece of mathematics is inversely related to its beauty (so that, say, the multiplication table - perhaps the most 'useful' part of 'mathematics' – is so devoid of beauty as hardly to deserve the name 'mathematics'). In other words, Hardy considered the aesthetic dimension as dominant in mathematics. We should not follow judgements such as Hardy's blindly; what we need to do is to have some way to position them in the reality of mathematical experience. What is the objective feature that authors such as Hardy identify in mathematics, when they identify a 'beauty' within it? Only after we have answered such questions, we can return to answer usefully the question, why Hardy chose to value this feature above others. One purpose of this article is as a prolegomenon for such questions.

Another purpose lies within the history and philosophy of mathematics themselves. It appears that there is a certain difference between arguing that a certain piece of mathematics was created in order to get tenure, and arguing that it was created in order to produce beauty. My intuition is that, in the first case, we learn something about tenure, while in the second case we learn something about mathematics. A mathematician, QUA mathematician, may aim at truth, necessity, generality, and many other epistemic values – and at the same time, and still QUA mathematician, he or she may also aim at non-epistemic values such as beauty. Then again, he or she may aim at tenure – but there we may be inclined to drop the 'QUA mathematician' clause.

This then may be the contribution of the following discussion to the history and philosophy of mathematics. Working in this discipline, we naturally tend to concentrate on the *epistemic* values of mathematical activity – which were of course at the heart of the philosophy of mathematics from its inception with Plato onwards. If any non-epistemic values may be recognized, their role might be acknowledged, but then they are considered as extrinsic to mathematics itself. I would suggest that aesthetic value is a key example of a non-epistemic value that, however, is *intrinsic* to mathematics. The thrust of the articles collected at this volume is, I believe, to widen our picture of the field of mathematical practice as a rational activity: one that appeals to the visual and not merely to the symbolic, that aims at explanation and not merely at proof. It also appeals, I suggest, to the aesthetic. Among other things – and still as rational practitioners – mathematicians aim at beauty.

2. SOURCES OF BEAUTY IN MATHEMATICS

2.1. An Outline

I propose in this section a typology of sources of mathematical beauty. However, I warn immediately of the simplifications I adopt, selecting from the complexity of the problem to focus on what is, I hope, a tractable and still significant domain.

To start with, the problem of mathematical beauty might be addressed at several levels, as beauty is encountered throughout the mathematical life. First, most mathematicians feel that there are aesthetic qualities to the mathematical pursuit itself. The states of mind accompanying the search for mathematical results are often felt as sublime; an aesthetic study seems warranted. This then is mathematical beauty as a property of states of mind. Second, beauty resides in the products of this pursuit – in mathematical theorems and treatises. This then is mathematical beauty as a property of texts. Finally, beauty resides in the entities studied by those theorems and treatises – in the many mathematical worlds – groups, spaces, numbers and sets... This then is mathematical beauty as a property of the ontological realm of mathematics. This ontological interpretation is perhaps the main context in which we think of "mathematical beauty".

In this article I focus on beauty as a property of mathematical texts. I do this for two extrinsic reasons and for one intrinsic reason. The first extrinsic reason is that texts are most readily available for our study: we have a clear and well-defined corpus for investigation. The second extrinsic reason – closely related to the first one – is that there is already a body of theory, in poetics, which I can take as suggestive for the study of beauty in mathematical texts.[2] Finally, and more intrinsically, I suggest that the study of beauty in mathematical texts may shed some light on the question of beauty in the mathematical pursuit and in the mathematical world. I shall try to argue for this in the conclusion, when we have completed the typology.

One simplification, then, is to focus on a single *layer* of mathematical beauty. Simultaneously, I limit myself to a single *field*. In recent years, it has been a tacit assumption of much of the work in the philosophy of mathematics that mathematical practice is heterogeneous. The nature of mathematics changes, depending on the discipline, the time and the place. In this investigation, we concentrate on properties of mathematical *texts*, which are even more obviously dependent on culturally specific settings than mathematical "ideas", say, are. Thus it seems prudent to start not from some global overview of the beauty in mathematical texts as such, but instead from a single genre of texts. In this article, I concentrate on ancient Greek mathematics, in particular geometry. This, once again, has extrinsic and intrinsic reasons. The extrinsic reason is that I am most familiar with this genre; the intrinsic reasons are that this genre is the foundation of western mathematics – and is often invoked as a model for the role of beauty in mathematics[3].

Briefly, then, this article offers a typology of the aesthetic issues in Greek mathematical texts. We now finally come to the subject matter itself, and let me explain how I intend to carve up this large field into a typology.

A very obvious initial distinction to be made is between the large scale and the small scale. On the one hand, beauty is felt at the level of whole treatises (or at the level of a proposition, taken as a whole). On the other hand, beauty is felt at the level of the mathematical text as it unfolds – in the immediacy of the texture of read words. The main difference suggested by this comparison, it seems to me, has to do not with scale itself as with the different kinds of experience it implies. At the large scale, beauty has to do more with the ways in which mathematical *contents* are arranged; at the small scale, the contents are less important, and the *form* of the arrangement becomes more important. To offer a rough analogue, one can liken the large-scale structure of a treatise to the *narrative* structure of prose works, e.g. novels – which of course is to a large extent an arrangement of the *contents* signified by the novels. On the other hand, one can liken the small-scale structure of a single mathematical statement to the *prosodic* structure of poems – which of course to a large extent has to do with phonological *form* independent of content. In the next two subsections, I shall discuss first the "narrative" properties of mathematical works (subsection 2.2) and then their "prosodic" properties (subsection 2.3). That this crude analogue is of service is part of what I need to show.

Narrative has to do with content; prosody has to do with form. Those are the two essential layers of any discourse, and it is often suggested that aesthetics has to do precisely with the *clashes* between layers of discourse.[4] If so, we should expect the relationship between form and content, itself, to be a source of beauty in mathematical texts. I try to show that this is the case in subsection 2.4 below. This area, of the relationship between form and content – signifier and signified – does not lend itself to an easy label but, for reasons which will be made clearer in subsection 2.4 itself, I title it "correspondence".

2.2. *"Narrative"*

The question of narrative often enters contemporary poetics in the form of narrative as a *process*: what may be called *narration*. Thus for instance one may note the distances between writers, authors and narrators, so as to follow the aesthetics of ironies and gaps (Booth, 1961). A defining feature of Greek mathematics is its implicit claim to transcend subjective perspectives: this approach is thus largely irrelevant for mathematics.[5] Narrative enters mathematics not as process, but as *structure*: ignoring the question of the identity of the narrator, something is being narrated, and we may note how elements are selected and combined along this narrative.[6]

Take for example Archimedes' first book on *Sphere and Cylinder*. This has for starting point a discursive introduction (addressed to Dositheus, a colleague), where the goal of the treatise is set out explicitly. Archimedes proudly says he had discovered fundamental results about the sphere, in particular that its surface is four times its great circle, and that its volume is two-thirds the cylinder enclosing it. Having said that, he moves on to offer a set of axioms or postulates (none of which is very closely related to the sphere), and plunges into the mathematical detail.

There is nothing about spheres or cylinders, their volumes or their surfaces. The main substantial sequence of results (propositions 2-6) deals with polygons and circles in proportion. Next, propositions 7-12 deal with surfaces of *pyramids*; propositions 13-20 – the surfaces of *cones* (and of various figures composed of segments of cones). Still no word of the sphere (though, with cones, we at least move into something resembling the *cylinder*). Then the following two propositions 21-22 move out to a totally new territory. Instead of having anything to do with three-dimensional figures, they return to the polygons of propositions 2-6 and state for them very complex and special results, having to do with proportions of lines drawn through the polygons. Those lines do not seem to have any relevance to anything – certainly not to spheres. (fig. 1).

Then, in proposition 23, we are asked to make a thought experiment. We rotate the circle, polygon and lines from fig. 1, and obtain in this way a sphere in which is enclosed a figure composed of segments of cones. It now becomes obvious that the results concerning polygons, and the results concerning cones, can be put together and (with the aid of the specific claims made about proportion, as well as about pyramids), can immediately give rise to the proportions determining the surface and volume of a sphere. The seemingly irrelevant and long preparation – just about half the book – is suddenly found to be directly relevant so that, indeed, the main line of reasoning

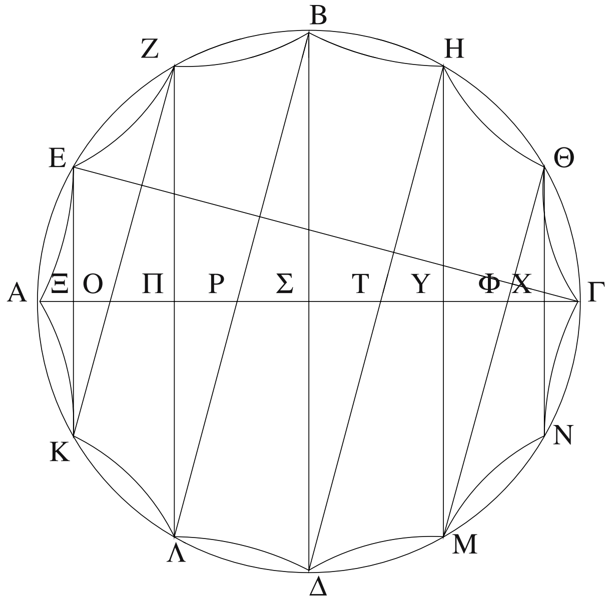

FIGURE 1.

can now proceed quickly to obtain Archimedes' main results in propositions 33-34.

As I promised already, it is not my intention to wax lyrical about mathematics. That Archimedes was a genius of narrative is what I subjectively feel. In more objective terms, however, all I am concerned with is that narrative structure is indeed a proper perspective through which to analyze Archimedes' performance. The simplest way to show this is by noting that he had *alternative ways of presenting his argument*. The most obvious one – and the one with potentially the greatest harm for his narrative achievement – would have been to *start* with the thought-experiment of proposition 23. Clearly then the sense of a brilliant master-stroke would have been completely eroded. Thus we notice a fundamental fact: the mathematical kernel of an argument – whatever we take *this* to be – only very weakly underdetermines the form it may take. The mathematician makes decisions for the form, decisions that are mathematically undetermined (in a traditional, narrow sense of mathematics) and therefore may well be dominated by the aesthetic function.

We should not think of Archimedes' *Sphere and Cylinder* as representing the only type of mathematical narrative structure. As one further example, let us now take Euclid's *Elements* book I. We may first note that Euclid does not use the device of setting out his goal explicitly. Instead, his work starts truly in medias res, with some definitions, postulates and common notions. The text has to start from nowhere, but it is quickly infused with momentum. A subject matter is implicitly defined – the triangle – and a string of ever-stronger results follows, very often in a neat sequence where one result leads on to the next. Thus we move almost imperceptibly from the state of nil knowledge, at which we start, to relatively remarkable results such as the congruities of triangles (propositions 4 and 8, to begin with). We also see the relationships between angles and sides (the famous proposition 5 – base angles in isosceles triangles are equal – and its converse 6). Problems, showing how to obtain a task, and theorems, showing the truth of a result, are neatly intertwined: problems 1-3, then theorems 4-8, then again problems 9-12. We thus establish a pattern: problems lead on to theorems, which in turn lead on to problems, and then again to theorems: theorems 13-21, and then again problems 22-23. At this stage we get to some very strong and general constructions: a triangle from any given appropriate three sides, an angle equal to any given angle. Now we press on again and, during the next phase of theorems, a variation on the theme of triangles-and-their-angles is offered, with the notion of parallel lines, introduced from proposition 27 onwards. This quickly leads to a problem, in 31, and then an application for the theme of triangles-and-angles (the famous "sum of angles equal to two right angles", in proposition 32), as we move on to widen our field to quadrilateral figures, from proposition 33 onwards. Parallelograms are studied, mainly through the perspective of triangles-and-parallels, until we reach again a sequence of problems on this set of issues, propositions 42-46. (45 is especially strong and general – to construct, in a given angle, a parallelogram equal to a given rectilineal figure – and Mueller (1981, 16) claims this is in some sense the goal of book I.) Finally the book ends with the coda of Pythagoras' theorem (understood through triangles and parallelograms) in proposition 47, with a quick converse, 48.

The movements of the text are all handled implicitly: the author never interferes, never speaks on behalf of the propositions. They do the narrative work on their own: pressing ahead with an even pace, moving from the absolute nothingness of the foundations of geometry and obtaining a full structure, reaching the capstone theorem of I.47[7]. (The architectonic metaphor is hard to avoid.) A few figures are elaborated throughout, gradually evolving. The evolution has a cyclical pattern – from problems to theorems and vice versa – and a linear pattern – from more elementary results, to stronger results based on them. Thus there is an overall structure of a widening spiral where every cycle of theorems and problems is capable of developing further the main themes. Finally we get to Pythagoras' theorem, obviously a most interesting result about triangles: triangles, the main character of the book, make the most remarkable journey, from nothingness to Pythagoras.

This does not work at the level of surprise and irony of Archimedes' *Sphere and Cylinder*, say, and the aesthetic principles are clearly different. Euclid does not aim to startle, in a quick stroke, but to impress, in a stately progression. Once again, Euclid could have made other choices, which would have given the work as a whole a different aspect. He could have introduced the circle at this stage, and so develop simultaneously the elementary results for all main plane figures (instead, he postponed the circle to book III). This would have made this book more comprehensive in scope, but lacking in narrative coherence. Or he could have ended this particular book with the results on parallelograms, for instance, leaving Pythagoras' theorem to another book. This, however, would be to miss on the sense of closure which this theorem provides in its great inherent interest, and in its reverting to the main character, triangles. In such ways, we can begin to substantiate one's immediate impression, that in Euclid's *Elements* I, narrative structure is a dominant organizing principle.

We have thus seen two special examples of narrative structure in Greek mathematical treatises. One can compare them, perhaps, to narrative structures in verbal art in general, for instance in the novel. Some novels are organized in complex structures of suspense and irony, which work by evoking expectations and then playfully subverting them; others are much more directly progressive, and create their sense of structure from a certain balance and directionality about the work as a whole. Archimedes' sudden revelation, (polygon)=(figure composed of conic segments), is perhaps comparable to, say, Charlotte Bronte's sudden revelation in *Jane Eyre*, (cries at night)=(mad wife). An earlier stage of the narrative is suddenly found to have a new, unanticipated meaning, by being retrospectively reinterpreted through a piece of information provided at a later stage of the narrative. Take, on the other hand, the stately progression of the triangle in *Elements* I, going through cycles of theorems and problems, in the process constructing a thick world. This is perhaps comparable to, say, the stately progression of the lives of Russian aristocrats in *Anna Karenin*, moving cyclically from the Anna plot to the Lyovin plot and finally leading to the fulfilment of the strands of narrative with Anna's suicide and Lyovin's family life. The examples are not meant with any great seriousness, and I certainly wouldn't like to suggest that, say, Euclid was a "realist" whereas Archimedes was a "romantic" etc.. I merely wish to point out that one can plausibly point to a variety of types of narrative structures which may be implemented to various aesthetic effects, and which can be found in mathematics just as they can be found in literature. In the above, I have suggested two possible types, and doubtless others can be observed as well.

It is immediately clear that narrative structures can be found in scales smaller than the treatise taken as a whole. In the genre of Greek mathematics, works are composed of a sequence of a few dozen smaller textual segments, today referred to as "propositions". Each of those units has internal structure, and some aspects of it closely mirror the narrative structures suggested already. A proposition is a sequence of statements about objects. The pace in which objects are introduced, and the ways in which statements create expectations, fulfil or subvert them, may all be used for an overall aesthetic function. I have touched upon this topic, from a separate angle, in (Netz, 1999, 198-216), noting the Greek tendency to have smooth, linear progressions in their proofs. I have identified there what I still consider to be the dominant function: the desire to have the proof fit a certain model of persuasion. This may serve as an example of a more basic point. Persuasion, as such, is not an aesthetic function: but the practices of persuasion and of narrative are in fact closely implicated in each other. To persuade, the text must be perceived to have a certain unifying structure - and a structure that may be endowed with aesthetic properties. Furthermore, the very act of persuasion is about the structure of introducing objects, raising expectations about them, and fulfilling those expectations (or perhaps subverting them, e.g. in refutations). The structures that give rise to persuasion are precisely the structures that give rise to narrative structure. Thus, while the aesthetic may not be the dominant function of persuasive texts, it is an *inevitably* relevant function.[8]

I take a quick example. In (Netz, 1999, 213), I have suggested that Archimedes' *Method* 1 is different from most other, "smoother" propositions. Instead of a clear linear structure, it has several hiatuses in the argument and, in particular, it has a very complex, quirky structure near its middle (I numbered the statements in sequence, so that the proof had 34 statements, and the complex passage is statements 13-18). I have suggested that there might have been a particular motive involved: Archimedes introduces here his surprising suggestion (to identify an area with a sum of lines). The structure is all designed to delay this suggestion, and then to bring it out in a startling way: exactly the same structure as we saw in larger scale in *Sphere and Cylinder I* as a whole. We may indeed have identified a feature of Archimedes' *style*.

At any rate, it now seems plausible that narrative may sometimes serve in mathematics as a source of beauty. I shall now briefly suggest, in more metaphysical terms, why this, I think, may be the case.

As noted above, *narration* – the use of narrators' perspectives – does not play a role in Greek mathematics. The medium of truth, par excellence, is ordinary language. It is thus natural that verbal art – the art whose vehicle is language – should so often dramatize the issue of truth and belief, of objectivity and subjectivity. This however does not get dramatized inside *individual* Greek mathematical texts, precisely because Greek mathematical texts markedly dramatize this issue, *when they are taken as a genre*. In quite simple terms, I argue that Greek mathematics was read, partly, against the background of other forms of persuasion. Its claim to possess absolute objectivity and truth is reflected by a rigid form from which perspective-hood, so to speak, has been eliminated. Perhaps this basic decision may be read in aesthetic terms, so that the genre, as a whole, possesses beauty in its sublime impersonality.

However, another kind of narrative structure is allowed: the author may chose to reveal as much or as little of the plot as he or she pleases; he or she may structure this information in many possible sequences. Such choices may possess aesthetic value, and in this way mathematical texts may possess an aesthetic dimension. I now suggest that this aesthetic dimension reveals something fundamental about the relation between mathematics and beauty.

It might perhaps be considered strange that the author has so much choice in mathematics. After all, is not mathematics governed by necessity, so that mathematical truth simply unfolds as a matter of logic? In fact this image is deceptive. It is true that, in a valid argument, the conclusion does follow from the premises. If C follows from the combination of A and B, it is possible to argue "A, and B, therefore C", and C does not only appear to be inevitable: it is inevitable, in the sense that it cannot fail to be *true*. But it can easily fail to be *made*. In general, each mathematical text makes a double set of choices: which premises to assert, and which conclusions to draw *explicitly* from the premises. The fact that a premise is true, just as the fact that a conclusion follows from asserted premises, both do not constrain the mathematician, do not force the mathematician to make them. The mathematician works in absolute freedom – creating a fabric of text that is woven together by the ties of logical necessity.

This dialectic of freedom and necessity is, I suggest, often at the root of the beauty of mathematical narratives. What is, after all, a surprising result in mathematics? It is a result whose perception of inevitability is not determined by the text preceding it, so that it is perceived twice: once for the *freedom* of the author who uncovered it, and then for the *necessity* of logic the author has uncovered. Similarly, "smooth" structures work through the perception of effortless inevitability, which is striking both persuasively and aesthetically. Now, it has been frequently suggested that the dialectic of freedom and necessity is essential to art as such.[9] Narrative art, certainly, has for its protagonists individual persons, and for its form, structured plots. It thus cannot fail to dramatize the theme of freedom versus determinism. Among the many options open to persons, the author selects a single plot; and similarly, among the many options open to mathematical objects, the mathematician selects a single logical thread. Thus, mathematics cannot fail to dramatize the theme of freedom versus necessity. This is one way in which we see not only that mathematics possesses an aesthetic dimension, but also that this dimension is essential to it, and closely implicates it with other verbal forms that are more obviously "artistic".

2.3. "Prosody"

The concept of "narrative" applies almost directly to mathematics, in that mathematical works – just like many other works of verbal art – tell a story: they have characters, and our information about the characters gradually evolves. The same, of course, cannot be said about "prosody". Literally speaking, the prosodic dimension is completely suppressed in Greek mathematics. The sequence of long and short syllables – the foundation of Greek poetic prosody – never seems to be an issue at all.[10] Here however I take the notion of "prosody" in a very metaphorical sense, referring to any compositional device that may be analyzed apart from the meaning of the text, referring purely to its form.

There are many compositional devices we can point out, some of them familiar, indeed, from literature. To begin with, let us return once more to the role of narrative. In literary theory, narrative has not only the global sense of "plot" and "subject" in the work as a whole, but also the local sense of a *narrative textual segment*, as opposed to other types of textual segments, most importantly *description*. One of the main literary compositional devices is this alternation of narrative and description. Some passages – descriptive – add detail to the fictional world, constructing its underpinning of reality; other passages – narrative – unfold the plot that takes place in that fictional world.[11] Description brings up things, narrative brings up the events which happen and which are true of those things. Things and events cross-determine each other, and in general narrative and descriptive passages may work together in interesting ways. The same is true of mathematics. In Greek mathematics, the two types of passages are technically known as *kataskeue*, "construction" and *apodeixis*, "proof". Construction is a descriptive passage where things are brought into existence, proof is a narrative passage where we are told what follows to those things. One should also

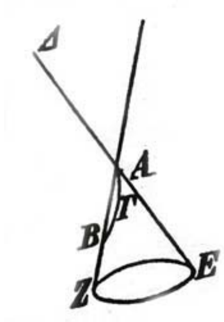

FIGURE 2.

add the (shorter) *ekthesis*, "setting out", which is a descriptive passage, and the (shorter) *diorismos*, "definition of goal", which is narrative.[12] The main difference between the "constructive" and the "argumentative" modes is that the constructive mode is hardly structured in the syntagmatic dimension[13]. The order of constructions is not set out as meaningful: they are merely a sequence of one observation after another, "and let", "and let". The syntagmatic dimension, however, is all-powerful in the "argumentative" mode, strongly structured by the sequence of "since – therefore". (It is perhaps for this reason that the binary structure constructive/argumentative closely resembles the binary structure descriptive/narrative: literary narrative, too, is characterized by strong syntagmatic structure, absent from literary description).

Take for instance the first proposition in Apollonius' *Conics* (I skip the *protasis* or enunciation whose function is separate): "Let there be a conic surface, whose vertex is the point A, and let some point – B – be taken on the conic surface, and let some line – AΓB – be joined". So far we have the "setting out", in mathematical terms a construction and in literary terms a description (or, more precisely, an ekphrasis of the accompanying diagram, fig. 2). "I say that the line AΓB is in the surface". This is the "definition of goal", formulaically employing the first person to introduce the narrative sequence. "For if possible, let it not be" (a meta-narrative statement, hard to classify in terms of "construction or "proof"), "and let the line drawing the surface be ΔE, and the circle, on which EΔ is carried – EZ". (The "construction" proper: the ekphrasis of the diagram is now complete). "So if, the point A remaining in its place, the line DE is carried along the circumference of the circle EZ, it shall also pass through the point B" (the "proof": we were now told a *story*, and here comes its *point*:) "and there will be the same limits

<shared> by two lines" (the end of the story proper) "which is impossible" (a final meta-narrative statement).

David Fowler uses to say that Greek mathematics is about "drawing a figure and telling a story" and Greek mathematical *texts* – to be more precise – are about "describing a figure and telling a story". This is their basic texture. In a miniature such as *Conics* I.1, the aesthetic effect derives from the modal variety itself – the very fact that there are both descriptive and narrative passages. In longer propositions, the alternation of description and narrative can be used for more precise stylistic effects. This is because there is a degree of freedom: the precise sequence of description and narrative is far from rigid. One can chose to have a complete ekphrasis of the diagram first, presented in great detail, then to move on to the proof (where no further constructions are being made). This is often the path taken by Apollonius: in *Conics* I.13, for instance, the "setting out" and "construction" take up (with the brief intervention of the "definition of goal" between them) 20 lines, followed by 30 lines of "proof". The very long and very static stage of the "setting out", in particular, has a certain ponderosity that is very characteristic of the style of Apollonius, and was clearly intentional. Compare this, say, to the third proposition of Aristarchus' *On the Sizes and Distances of the Sun and the Moon*. The text starts with a brief "setting out", immediately moving on to draw a conclusion ("proof" mode) from it, and then back to construction, and so on. With D for description, N for narrative, and the number of lines for each in brackets, the structure is

D (4) – N (1) – D (4) – N (1) – D (5) – N (15) – D (8)[14] – N (4)

Aristarchus, correspondingly, has a much more "lively", discursive style.

The binary structure of description and narrative is thus the chief compositional device of Greek mathematics. There are many other, more local compositional devices, all due to the fact that the mathematician actively selects from a variety of available modes. To begin with, mathematical arguments are characterized by their sources of validity. Some claims are based on visual considerations unpacking the diagram. Others are based on more formal, linguistic manipulations (e.g., that if A is to B as C is to D, then as A is to C as B is to D: proved in *Elements* V.16 and frequently used in Greek mathematics). In more general, there is a tool box of results the Greek mathematician knew well, and this tool box clearly has an internal structure: some results fall together to form clusters; (Netz, 1999, 216-235) (e.g., as we have seen for Book I of the *Elements*, elementary results about the triangle are more closely related to elementary results about parallels, than to elementary results about circles.) Thus the mathematical argument works by using sources of necessity of different *kinds*: a palette, from which the mathematician chooses and combines. This introduces the aesthetic dimension of *variety*. Consider for instance the capstone theorem to the last book of Euclid's *Elements* (which is an appendix to XIII.18), proving that there are only five regular solids. I quote a passage:

"For a solid angle cannot be constructed with two triangles, or indeed planes" (a direct visual intuition) "With three triangles the angle of the pyramid is constructed, with four the angle of the octahedron, and with five the angle of the icosahedron"; (we enumerate numerically, going through the ordinal sequence, certainty secured by the finite, inspectable nature of that sequence) "but a solid angle cannot be formed by six equilateral and equiangular triangles placed together at one point, for, the angle of the equilateral triangle being two thirds of a right angle, the six will be equal to four right angles": (this uses the properties of the triangle of Book I – together with a quick calculation, which is yet another source of necessity) "which is impossible, for any solid angle is contained by angles less than four right angles" (this is proved in book XI, and is thus a very distinct part of the tool box). This brief passage works then through visual intuition; through numbers perceived as ordinals and as an object of calculation; through results from book I and from book XI; all coming to function together organically. An obvious contrastive comparison would be propositions such as Euclid's *Elements* I.5, which work through an iterative application of a single source of necessity. *Elements* I.5 is often felt to be dull (it is the famous *pons asinoroum*), whereas the appendix to book XIII is obviously delightful. The difference is essentially that, by the time he has reached book XIII, Euclid has enormously widened his palette – he now has thirteen books to draw on whereas, in I.5, he had only a handful of basic presuppositions.

"Variety" has to do with *texture*, but it is "prosodic" only in a very metaphorical sense. My next example is nearly literally prosodic. For while the rhythm of long and short syllables is not itself a marked feature of Greek mathematical texts, the texts are marked by other rhythmic patterns, which are of clear aesthetic significance. The rhythmic pattern of verse represents the fact that verse is built from clearly defined units – lines – that participate in larger-scale structures – stanzas – and possess an internal structure – feet. Greek mathematical texts – perhaps more than any other prose style – are similarly built from clearly defined units, which allow a similar structural analysis. This is especially true of proofs which (as mentioned above) possess the strong syntagmatic structure of the "since – therefore" sequence. This sequence works on *assertions*, and combines them into *arguments*.

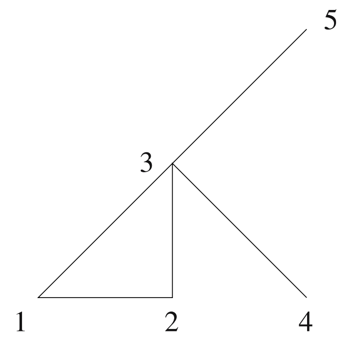

FIGURE 3.

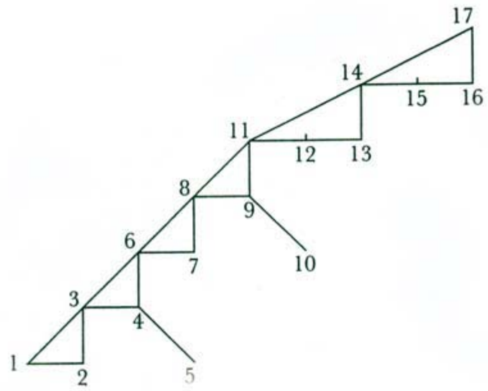

FIGURE 4.

When analyzing mathematical proofs, I number the assertions and draw the "trees" of the structure of the proof, e.g. representing an argument such as "(1) and (2), therefore (3) (for (4), too), hence (5), as well" by fig. 3

Thus we can compare the logical structure of *Elements* II.5 (fig. 4) – a prototypically "smooth" Euclidean proof – with that of *Method* 1 (fig. 5) – a complex proof I have mentioned above. To begin with, we can see how the notion of a "smooth" proof can be given concrete form. We may also begin to note further features of this, quasi-prosodic structure. First of all, the proof alternates between starting-points (assertions which are unargued for inside the proof itself and appear in the tree "on top of nothing" – in Euclid's *Elements* II.5 these are 1, 2, 5, 7, 10, 12, 13, 15, 16) and conclusions (assertions which follow from other assertions and which thus appear in the tree "on

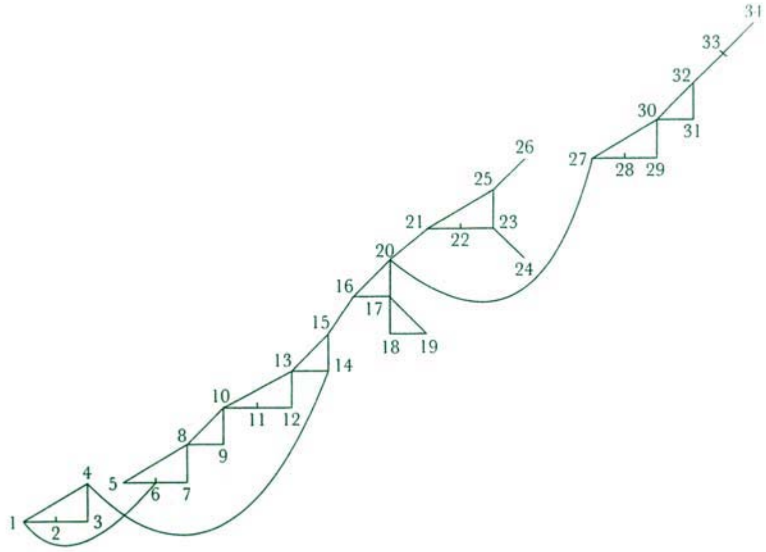

FIGURE 5.

top of" other assertions). Although one could, as a matter of logic, structure proofs so that all the starting-points are asserted first, followed by all the conclusions, this is in fact awkward both for the cognitive computation of the validity and for the aesthetic appreciation. The structure adopted instead is a constant interplay between starting-points and conclusions, which form together minimal units: *arguments*.[15] (Thus, in a tree, every triangle, or independent line, stands for an *argument*). Starting-points are the moments in which the proof is recharged, building its energy for the charge of the conclusion. I have offered a comparison of the mathematical assertion to a line of verse; I now suggest a further analogue – which I consider to be relevant almost in a literal way – comparing the structure of starting-points, conclusions and arguments, to the structure of unstressed syllables, stressed syllables and feet. It is indeed difficult not to think of structures of proof in terms of their "flow" or lack thereof (the presence or absence of regular meter) of becoming "quicker" or "slower" (shorter or longer feet). Consider figures 4 and 5: the precise pattern of figure 4 (iamb-troche-iamb, iamb-troche-iamb, anapest-anapest – if you see what I mean), the very complex structure of figure 5 (a radical free verse, where feet are consistently changed, though notice a gradual transition, from a very rough meter to start with, to a sequence without hiatuses in assertions 14-34, and in particular a sequence of three "iambs" right towards the end[16]). It is clear that some sort of rhythmic patterning is going on, and my intuition as a reader, at least, is that this pattern contributes to my appreciation of the text. Following a proof as it unfolds, you are *carried along* it; the structure of this intellectual motion has significance in both cognitive and aesthetic terms.

Moving back from rhythmic patterns as such to more general relations between signs, one finally notes the following. In Greek mathematical texts, signs are constantly being reformulated and co-related with each other, thus creating a rich texture. Consider for instance a passage from the construction in Euclid's *Elements* II.5:

"... [A]nd, through the <point> Δ, let a line, ΔH, be drawn parallel to either of the <lines> ΓE, BZ, and, through the <point> Θ, let again a line, <namely the line> KM, be drawn parallel to either of the <lines> AB, EZ, and again, through the <point> A, let a line, <namely the line> AK, be drawn parallel to either of the <lines> ΓΛ, BM".

It will be seen that we have here the same formula (for drawing a parallel) repeated three times, with the substitution of different letters and the addition of connecting particles. The effect in this case is probably one of sheer monotony, but the formulaic nature of the text is aesthetically significant. The Greek mathematical text is composed of nearly fixed expressions, such as the formula for drawing a parallel above. These stand to each other in several relations. First, they allow some freedom (for instance, the words "to either of", in the example above, are a local variation on the more standard formula which makes lines parallel only to a *single* other line). Second, formulae are subject to substitutions (as, in this example, that of letters). Because of these two factors, different tokens of the same formula type tend to be different (indeed, otherwise there would hardly be a mathematical point in repeating them). Third, such tokens are often related in meaningful way. This does not happen in the example above, as it is from the construction, where the syntagmatic arrangement is weak. A typical structure in which formulae figure inside proofs is, for instance, the result of *Elements* V.16, mentioned already:

"As A is to B so is C to D. Therefore, as A is to C so is B to D".

(In actual appearance, A, B, C and D would be spelled out as some geometrical object, providing the text with a richer pattern). We can now see that this is a sequence of two tokens of the same formula (that of proportion), arranged in the relation "since – therefore". This is a textbook case of a patterning of the syntagmatic and the paradeigmatic dimensions. This patterning is essential to Greek mathematics:[17] it is also "prosodic" in a rather direct sense, in that it is (at the abstract level suggested here) a form of *alliteration* ("cat, therefore mat"). Why is that *beautiful*? Partly, the answer has to do with the sheer presence of structure, but partly it has to do with the basic relation between sign and signified, and in this respect we shall return to discuss alliteration in the next subsection.

Before moving on, we need to point out the general moral of this subsection. The phenomena described are in a way heterogeneous. I have discussed several levels of formal structure – of selection and arrangement – the alternations of construction and proof, of various sources of necessity, of starting-point and conclusion, of different tokens of the same formula-type. They are all similar only in that they are all alternations; no deeper unity combines them all, and no generalization is possible at the level of their contents. But the very fact that mathematical texts support many layers of structure indicates an *essential* reason for the presence of an aesthetic dimension. Aesthetic appreciation is often based on the perception of structures inside the artistic object. A mere concatenation of objects, devoid of any structure, cannot function as a vehicle of communication, let alone a vehicle of beauty. Any perception is structural; by imposing structures on the sequence of objects, perception makes them meaningful – and opens up the possibility of aesthetic value.

Now mathematical perception, particularly in its Greek form, imposes not merely structure, but some very definite structure. Bringing in logical categories, its boundaries and markings are extraordinarily sharp. The assertion begins *here* and ends *there*; it is *definitely* a proof and not a construction; it is a conclusion from precisely *those* premises; it works through precisely *this* type of reasoning; it is made precisely of *this* sequence of formula-tokens that belong to precisely *the same* type as that used above.[18] Note further that it is in the nature of mathematics to make the signs themselves, taken formally, contribute to the logical sequence (A:B::C:D therefore A:C::B:D! - More on this below). Thus, the kinds of structural relations picked up by mathematical perception are directly relations of signs, and thus are directly of potential aesthetic significance. Nothing surprising, then, in that mathematical texts may display aesthetically pleasing forms: the more structured a text is, the more it is implicated in the pattern of objects and structures – that is, in an aesthetic pattern.

We saw above that mathematical texts are essentially implicated in the dialectic of freedom and necessity; now we see that they are essentially implicated in the dialectic of object and structure. I move on to discuss what I call here "correspondence", the locus of yet another dialectic.

2.4. "Correspondence"

"Correspondence" is a thick jungle, and, to make some progress, I start with Jakobson's helpful terminology (which I have already used above without explanation).

Jakobson stressed the bipolar structure of language: selection and combination, similarity and contiguity, the paradeigmatic and the syntagmatic. For the notions of "selection" and "combination" note that, in a text, the speaker (a) *selects*, for each slot in the text, a unit of speech out of a large pool of available candidates, and also (b) *combines* the selected units in a certain order. Thus two kinds of structure are at work: *similarity* (of the various candidates for a single slot) and *contiguity* (of units which happen to lie next to each other). The "similarity" kind of structure is known as "*paradeigmatic*", the "contiguity" as "*syntagmatic*". Now, in even more general terms, we may say this. One possible textual device is to represent an object through its possible equivalents or near-equivalents, in other words through that to which it stands in the relation of similarity – and this is what Jakobson calls "*metaphor*". Another device would be to represent one object through that to which it stands in the relation of contiguity; this naturally would be Jakobsonian *metonym*.[19] While very typical of a certain theoretical approach, this set of notions is at bottom an analytical, indeed terminological exercise, in itself theory-free. Stripped of all jargon (and of the cognitive and linguistic assumptions which do make it stronger and more interesting) Jakobson's theory of metaphor and metonym is very simple indeed. Some signs are similar to each other, some are contiguous to each other; one of the devices of art is to put such similarities and contiguities on display. It is especially on the paradigmatic kind of correspondence that I concentrate here.[20]

Many mathematical signs stand to each other in close paradeigmatic relations. We already began to see this in the preceding subsection: several tokens of the same formulaic expression, say

A is to B as C is to D, C is to D as E is to F, A is to B as E is to F

are like several inflections of the same verbal root. In full form (where the A, B etc. are spelled out as full geometrical objects) this may be obscured at the level of performed text – just as, in ordinary language, the words sharing the same root may appear, in phonological form, rather distinct. Here, for instance, is a passage from Apollonius' *Conics* IV.46:

"Since it is: as the <square> on MΨ to the <square> on ΨI, so the <rectangle contained> by AΠB to the <rectangle contained> by ΔΠE, but as the <rectangle contained> by AΠB to the <rectangle contained> by ΔΠE, so the <square> on ΛT to the <square> on TI, therefore also: as the <square> on MΨ to the <square> on ΨI, so is the <square> on ΛT to the <square> on TI".

This is nothing more than a series of paradigmatically related signs, identical in some important ways. Now, the perception of hidden paradigmatic identity is a tool often used in poetic alliteration – say, the "hundred visions and revisions" of T.S. Eliot's *Prufrock* – and now we see that it is an essential feature of mathematical perception, as well. Quite simply, mathematics cannot work as a deductive exercise without the constant re-identification of signs. It would be difficult to show that this has a specifically aesthetic effect in mathematics, just because the deductive function is so central. As one reads, one constantly notes with satisfaction, "yes, it is indeed the same". This satisfaction of sameness recognized is sung most loudly by the bass section, exulting over the validity of the derivation; my own intuition is that, listening carefully, one can also discern, in the chorus of one's recognition, an alt voice rejoicing over the finding of sameness in difference.

This, at any rate, is an example where paradigmatically related signs are placed in syntagmatic order. We move a bit closer to "metaphor" when considering the structure induced on the text by the presence of paradigmatic relations that do *not* have syntagmatic meaning. To put this in simple terms, mathematical texts often return to speak about the very same topic, and thus they contain an element of repetition. Such repetitions may be handled in various ways, and thus we have an aesthetically significant choice. Parellalism is an extreme and therefore an illuminating case. As noted by Jakobson (in the same fundamental study mentioned above): "Rich material for the study of [metaphor and metonym] is to be found in verse patterns which require a compulsory parallelism between adjacent lines, for example in biblical poetry... This provides an objective criterion of what in a given community acts as a correspondence" (Jakobson, 1987, 110-111) . Greek mathematics possessed one such pattern of compulsory parallelism, namely the relation between general "enunciation" and particular "setting-out" and "definition of goal". So for instance the first proposition in Euclid's *Elements*:

[Enunciation] "On a given finite straight line to construct an equilateral triangle".

[Setting-out] "Let there be the finite straight line, AB".

[Definition of goal] "Thus it is required to construct an equilateral triangle on the straight line AB".

The two parts – enunciation on the one hand, setting-out and definition of goal on the other hand – are related paradigmatically. They are two inflections, general and particular, of the same root meaning; two signs differing in their relation to a single signified. They stand to each other, in this case, in very close explicit correspondence (although, of course, the "inflection" demands considerable rearrangement). This is the simplest, least ambitious type of metaphorical relationship, namely near-*synonymy* (compare, e.g. Psalms 2.1: "Why do *the nations* conspire / and *the peoples* plot in vain?"). Close parallelisms are a feature of Euclid's style, and while they seem to have a didactic motivation, they are also important for the fabric of the Euclidean text, contributing to its serenity and gravity.

In other authors, metaphor is often much more ambitious. I now quote from Archimedes' *Balancing Planes*, proposition 6 (this, incidentally, is the theorem we use when balancing plates on a tray):

[Enunciation] "Commensurable magnitudes balance at reciprocal distances having the same ratio as the weights".

[Setting-out] "Let there be commensurable magnitudes A, B whose centres are A, B, and let there be a certain distance, E Δ, and let it be: as A to B, so the distance Δ Γ to the distance Γ E";

[Definition of Goal] "it is to be proved that Γ is centre of the weight of the magnitude composed of both A, B."

Now the transformation requires an unpacking of the mathematical *meaning* of the enunciation. Instead of mere synonymy, we have two separate signs, whose only connection is their shared signified. Thus the text displays the paradigmatic structure of signs.

This relationship, between enunciation and the sequence of setting-out and definition of goal, is a special case of a very general feature of mathematical texts. They repeatedly need to speak about roughly the same objects, saying roughly the same things. Several propositions all use the same piece of construction; or they all rely on the same local argument (which did not get enshrined as a separate lemma, and therefore gets repeated from one proposition to another). Or, in the analysis-and-synthesis mode (used a few dozen times in extant Hellenistic mathematics), the proof goes through two parallel stages, inversely related, the analysis and the synthesis. In all such cases, the mathematician has several options. They range between two extreme positions that are, in themselves, aesthetically empty: exact repetition (where no paradigmatic distance opens up at all), and the explicit deletion of a passage by "as has been said in the previous proposition" (where paradigmatic distance becomes infinite, between expression and non-expression). In between lie various forms of variation that may or may not be intended to be perceived as such: in short, another avenue for aesthetic effect. (In more general, an aesthetic effect may be obtained by the overall pattern of decisions about which kind of repetition to employ – full, zero, or some metaphorical repetition).

We saw several cases where the problem of repetition arises from some mathematical functional constraint; the solution to this problem may then involve an aesthetic function. In other cases, the mathematical function of the repetition is much less obvious, and the aesthetic function may therefore be dominant. The clearest example is the phenomenon of alternative proofs. Inside a single book, say Archimedes' second book on *Sphere and Cylinder*, one may find the same theorem proved twice. Proposition 8 shows that, given two unequal segments of the sphere, the ratio of the greater volume to the smaller is smaller than the (what we would call) the square of the ratio of the surfaces, but greater than (what we would call) the 1.5 power of the same. Having proved this remarkable result, the text goes on to prove *the same result, once again*. Heiberg, the great editor of Greek mathematics, considered this alternative proof to be spurious, and of course he may have been right. (Heiberg, 1913, 217 n.1) It is always possible to argue that an alternative proof resulted not from the decision of a single author to produce more than a single proof, but from the decision of some later mathematician to try his or her hand at finding an alternative proof, and then from the decision of yet another later scribe, to put the two proofs together. Whether this is the case or not is significant in historical terms (and has some bearing on our aesthetic judgement), but it does not touch upon the basic aesthetic interpretation of alternative proofs. We merely need to transpose the locus of aesthetic judgement, from the original author to the later mathematicians (who went on to offer what is, in mathematically functional terms, a "redundant" proof), and the later scribes (who considered a juxtaposition of several proofs, whose result is identical, to be of interest). Now here we touch on a major theme of the history of mathematics. The development of mathematics is frequently motivated not by the desire to solve open problems, but by the desire to solve problems *that are solved already* - the most famous case in Greek mathematics is the duplication of the cube, for which see Eutocius' catalogue of solutions (Heiberg, 1915, 54–106). (In general, for the Greek accumulation of problems, see the fundamental study Knorr (1986).) Mathematicians went on proving the same enunciation, just as painters went on painting the same annunciation; in the first case as in the second, the desire to replicate sustained a perfection of styles and techniques.

Needless to say, the desire is not to replicate in a strict sense. Once again, we see that exact replication is aesthetically empty. The desire, instead, is to replicate-with-a-difference – to achieve the same result (or paint the same scene) through a different line of reasoning (or through a different mode of painting). I shall now try to explain why this may indeed be of such aesthetic significance. Before that, however, we need to widen even further our field, to further possible relations between signs and signifieds.

So far, we saw several ways in which the mathematical text contains a multitude of signs, all referring to the same signified. This was true at the level of the *text*, in the strict sense: we have dealt purely with the modality of written language. This is in some sense perhaps the main modality of the signs of Greek mathematics (it is at this level that Greek mathmatical texts are either true or false). But this is not the only modality of the signs of Greek mathematics, and an appreciation of its aesthetics must refer to this presence of many modalities. Greek mathematics relies essentially on at least two modalities, language and diagram. It thus involves simultaneously verbal and visual perception. Not only is it possible to have two written signs refer to the same object, then: we also have the possibility of having an object referred to simultaneously by both verbal and visual signs.

Greek mathematical texts, apart from their general enunciations, refer throughout to a diagram labeled by letters. The A, B, Γ of Greek mathematical proofs participate simultaneously in two semiotic systems, the text (where they are manipulated inside expressions), and the diagram (where they are spatially configured). I have argued in (Netz, 1999, Ch. 1), that, in logical terms, the two can not be understood separately: the text does not function unless we read it in the light of the diagram, the diagram is incomplete unless we interpret it through the text. We may now notice the aesthetic significance of this situation. Put simply: the diagram is *read*; the text is *visualized*. Greek mathematics relies, therefore, on a kind of synesthesia. In fact, the synesthetic structure is probably more complicated than that. The diagram is, in reality, statically present to the eye, but it is also discussed as if it were dynamically manipulated and constructed, in a language suggestive of motion in and through it. In other words, the verbal and the visual are also accompanied by the *kinesthetic*. An obvious case is the way in which parallel lines are mentioned so that they appear to "flow in the same direction" ("AB is parallel to CD", in fig. 6). Consider an even more beautiful example – an expression of a very common type. I quote from Apollonius' *Conics* I.41:

FIGURE 6.

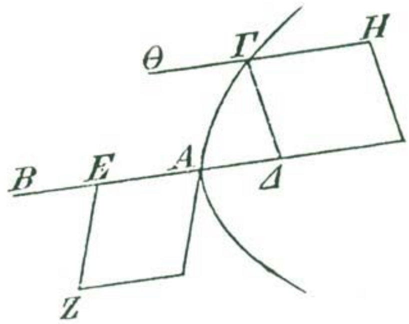

FIGURE 7.

"... ΔΓ has to ΓH the ratio composed of the <ratio> that ΔΓ has to ΓΘ, and the <ratio> that ΘΓ has to ΓH". (Fig. 7).

Note how the three modalities interact: the statement works as a structural manipulation of verbal signs (it is a complex structured formulaic expression). It is also premised on the visual expression of the same signs – without which, indeed, the statement is hardly interpretable. But finally, note the very typical switch in *directionality*. ΓΘ is transformed into ΘΓ, in a kinesthetic metaphor (one can hardly avoid the term) for the canceling-out involved in the operation. The motion "in and out", across the single line ΓΘ, introduces it and then cancels it.

Synesthesia is a key concept in romantic aesthetics. In its apparent irrationality, untrammeled sensuality, it answers to a certain kind of aesthetic temperament.[21] Parenthetically, I note that many modern mathematicians seem to have an enormous interest in synesthesia: in fact, I suspect it is fundamental to their reports of the mathematical process as beautiful. Friedberg (1968), for instance, in a very personal introduction to number theory, starts with a long and fascinating excursus about the author's polymodal synesthetic perception of numbers – colored, tactile, acoustic, what have you.[22] Now, it is of course more difficult to show that such aesthetic concerns motivated the Greeks. They definitely did not seek, Baudelaire-like, the expansion of perceptual experience for its own sake. A more modest claim, however, would be that we have here another kind of *variety*. The richness of modalities – that are *felt to be organically related* – is very different from the intended, jarring effect of the romantic juxtaposition of incommensurables. Greek mathematical synesthesia thus comes down to yet another form of the multiplicity of signs for a single signified, with the added complication that the different signs cone from separate modalities, and can only function as a whole: none stands on its own.

This mode of operation – simultaneously perceiving an object through several systems – occurs in mathematics in other, more technical ways. Even inside a given modality, two domains often interact. Most significantly, Greek mathematics translates the synesthesia of the linguistic and the visual into a dual set of *mathematical* domains: proportion theory, and geometry. Very often, a Greek mathematical proof operates by thinking of an object, simultaneously, through the more abstract properties revealed through proportion theory, and through the more concrete properties revealed through geometry: it is simultaneously a *line in space*, and a *magnitude in proportion*. Further global metaphors were offered in Greek mathematics, especially in its interface with physics: for instance, rays of vision are lines (optics), musical harmonies are numerical ratios (music), the motion of stars is a configuration of circles (astronomy). Archimedes, to mention one example, pioneered the science of statics with the global metaphor of balance as the (composite domain of) geometrical proportions. He first used this metaphor in *On Balancing Planes*, to obtain results on the physical balance. Then, he went on, in the *Method,* to use this metaphor in the reverse direction, now applying the results of *On Balancing Planes* to obtain new results about pure geometrical objects (seen now through the metaphor of the balance). It seems to me that this double metaphor is often considered Archimedes' most beautiful achievement.

The alignment of separate domains is mathematically functional: it allows different kinds of understanding to operate simultaneously and thus to generate results which would have been impossible with only one of the kinds. The whole theory of conic sections, for instance, would be impossible to develop on the basis of either geometry or proportion theory alone. At the same time, this global metaphorical structure is clearly perceived in aesthetic terms: the duality of the concrete and the abstract, in particular, seems to lie at the core of the Platonic fascination with mathematics.

In Greek mathematics, those bimodalities can be definitely mapped: between proportion theory and geometry, inside mathematics; between mathematics and physics, outside it. The same cannot be said for modern mathematics: just as modern mathematicians are fascinated by the specific notion of synesthesia, they also set out systematically, in the 19^{th} century and even more in the 20^{th} century, to seek out the global equivalences between domains. This quest shaped the content of modern mathematics, which is a tight network of poly-isomorphic disciplines.[23] It is also, I would suggest, at the core of the modern mathematical sense of beauty. This is very different from the much more static structure of Greek mathematics, where isomorphism is much less actively sought after: in this respect, it appears that different aesthetic temperaments are visible in ancient and modern mathematics.

We have made a long detour through synesthesia and global equivalences, but ultimately we return to the very basic phenomenon of the multiplicity of signs for a single signified: the paradigmatic dimension. The detour, however, may help us to perceive the special role this dimension has in mathematics. Why is it that it so obvious to us that several mathematical discourses are about 'the same thing'?

In fact, with mathematics we seem to stand in an easier position than with other verbal forms. The paradigmatic is especially easy to identify in mathematics. Why?

In literary texts we always face a central question about the paradigmatic: how is it to be defined? It would be natural to think of it in terms of a shared "core" between the two verbal segments standing in the paradigmatic relation – "cat" and "feline", or "cat" and "dog", or perhaps even "cat" and "mat" – but, as the examples show, "core", generally speaking, is a slippery concept. Equivalence is perhaps indefinable in the natural lexicon. But now it becomes clear that, in the mathematical context, the concept of "core" has a special relevance. Consider "alternative proofs": two proofs share a core meaning, if they prove the same result, i.e. if they are *mathematically equivalent*. Here is the concept organizing the paradigmatic dimension in mathematics. As we have mentioned already in the preceding subsection, the logical categories employed by mathematics make its structures much sharper than those of ordinary discourse, and the same goes for paradigmatic structure. "Equivalence" is a very clear term in mathematics, as perhaps nowhere else.

However, even mathematical equivalence is a complex object. Hence its aesthetic significance.

Let us try to approach in aesthetic terms the most basic phenomenon of mathematical equivalence, *derivation*. This is the major arranging principle of a mathematical text: P→Q.

We have in fact mentioned derivations already, in the context of "rhythm", where I have suggested that the metrical pattern of a mathematical proposition is marked by the sequence of "unstressed", argued assertions, alternating with "stressed", argued assertions. Thus, I suggested that the rhythmic pattern of the proposition is given by its pattern of derivations. I now suggest that this is also its main bind of correspondence. We thus see a certain dual level for derivations: at the large-scale level, they create the pattern governing the proposition; at the immediate, small scale, they are a strongly marked correspondence. This duality is very intriguing, since exactly the same holds with *rhyme*. Derivation, one may say, is the rhyme of mathematics. For rhyme, too, creates the strophic pattern of a (rhymed) poem - while being its most strongly felt correspondence.

This analogy has an even more direct application: derivations, like rhymes, are aesthetically effective where there is sufficient distance between the syllables/assertions – no "knight"/"night", P→P.[24] This is the principle that tautologies must be avoided. Nor of course should distance be too large: the rhyme must be heard, the derivation must be seen to be valid. Notice that the constraints on derivation are aesthetic rather than logical. A "trivial" derivation (P→P, or nearly so) is logically valid; while a logically valid derivation from P to Q, whose validity is, as stated, impossible to perceive, fails not as a matter of logic but as a matter of persuasion and pleasure. Briefly then, when following a derivation, we must see both that P and Q are distinct, and that they are identical. The two signs must both refer to the same signified, *without* being identical.

Consider e.g. the following type of derivation:[25]

"Triangle ABC is similar to triangle DEF. Therefore as AB to DE, so BC to EF".[26]

There is a beauty here, I feel: that two statements – one on a geometrical shape, another on a much more abstract proportion – are found to be closely related, indeed nearly "identical". (The first implies the second; the second does not fall much short from implying the first). Of course they are not identical: they say different things, in very different ways; they belong to different modalities; yet they are also nearly the same. Compare Larkin:

"Man hands on misery to man:
It deepens like a coastal shelf.
Get out as early as you can,
And don't have any kids yourself".

Once again: there is beauty in the startling, irrational apposition of "shelf" (metaphorical to begin with) and "self": the two, suddenly, the concrete and the abstract, are found to be somehow "nearly the same".

The relation between rhyme and derivation merits a closer look for it may, I think, offer a key to the more general relation between poetry and mathematics. Both rhyme and derivation share the same combination of difference and identity: they reveal that two entities, seemingly different, are at some level identical. In both cases, this can be done because the entities, to begin with, subsist at two separate levels – the sign and the signified. Thus we are shown that two sign/signified combinations are identical in one respect, different in another.

Rhyme and derivation are thus similar; but they are also different or, more precisely, *complementary*. Rhyme works by having two sign/signified combinations that are similar as signs and dissimilar as signifieds; derivation works by having two sign/signified combinations that are similar as signifieds and dissimilar as signs.

Rhyme:	sign $\rightarrow$ signified$_1$, signified$_2$	("shelf" / "self")
Derivation:	sign$_1$, sign$_2$ $\rightarrow$ signified	(similarity / proportion)

The two sides of a mathematical derivation are very dissimilar in form; they approach each other at the level of content. In a complementary fashion, the two rhyming words are very dissimilar in meaning; they approach each other at the level of form. The mathematical relationship is anchored in meaning, marks the meaning; the poetic relationship is anchored in form, marks the form. In sum, then, mathematics and poetry both utilize the binary nature of the sign/signified relationship, to combine identity and difference; in mathematics, the identity is at the "signified" level, in poetry, it is at the "sign" level. The patterns of identity and difference are similar but complementary. Inasmuch as they are similar – merely as patterns of identity and difference – they both yield a pleasing aesthetic relation. But inasmuch as they are complementary – in the different levels they mark – they tend to have very different effects.

Let us see what – in a similar metaphysical level of abstraction – Jakobson had to say on the nature of poetry. I quote the conclusion of his article "What is Poetry?"[27]:

"Why is [poetry] necessary? Why is it necessary to make a special point of the fact that sign does not fall together with object? Because, besides the direct awareness of the identity between sign and object (A is A_1), there is a necessity for the direct awareness of the inadequacy of that identity (A is not A_1). The reason this antinomy is essential is that without contradiction there is no mobility of concepts, no mobility of signs, and the relationship between concept and sign becomes automatized. Activity comes to a halt, and the awareness of reality dies out".

Art, according to Jakobson, is about subverting our ordinary, automatic acceptance of reality – in this case our ordinary, automatic acceptance of the sign/signified relationship. Because poetry creates a web of relations that mark the *sign* aspect of the sign/signified combination, it subverts this very relationship. Why is that? Because in the ordinary, automatic acceptance of speech, we take it for granted that the relation is

$$\text{sign} \rightarrow \text{signified}$$

That is, the sign is there merely to mark the signified, and does not have a significance of its own. The sign is supposed to do no more than invoke the signified – that is, determine the signified.

But in poetry, this determination of signified by sign is subverted: it has the structure

$$\text{sign} \rightarrow \text{signified}_1, \text{signified}_2$$

I.e. similar signs yield very different signifieds and the determination fails. The very function

$$\text{sign} \rightarrow \text{signified}$$

Is thus being questioned: poetry, in this way, is a critique of language.

Mathematics, on the other hand, does nothing of the kind. It is fully anchored on the signified, and its structure

$$\text{sign}_1, \text{sign}_2 \rightarrow \text{signified}$$

Supports the intuition that signs are no more than entries into signifieds. The combination of sign/signified is not subverted, but supported. Mathematics is not a critique of language, but its affirmation.

Such considerations may seem perhaps rather removed from actual experience; perhaps they are. Yet this kind of metaphysical politics – the politics of abstract subversion, as it were – is central to contemporary literary theory. And certainly the sheer surprise of irrationality, of the breaking down of the relation between form and meaning, is part of aesthetic experience. This is especially true for a certain kind of romantic (or modernist) aesthetic temperament. Perhaps one might even suggest the following. If poetic correspondences *undermine* the notion of rational correspondence, while mathematical correspondences *affirm* the notion of rational correspondence, we should predict that, to some temperaments, poetry would seem suspect while mathematics would seem praiseworthy, indeed a model. Such may have been Plato's temperament.

With all such differences, however, the main result is this: that mathematics is shot through with the notion of correspondence. It fully partakes in the dialectic of identity and difference. Thus it creates a pattern, of potential aesthetic significance. Arguably, nowhere else is the dialectic of identity and difference so rich and visible as in mathematics. Perhaps the best evidence for this is, once again, the recent quotation from Jakobson:

"... Because, besides the direct awareness of the identity between sign and object (A is A_1), there is a necessity for the direct awareness of the inadequacy of that identity (A is not A_1)".

Jakobson, of course, was not above using quasi-mathematical notation to enhance the scientific credibility of his methodology. But could he really have chosen a better way to express the notions of identity and its absence? Nowhere else are those notions so central, so clear. Mathematicians keep affirming just that: that A equals B. No one else – not even poets – affirms such claims as often. The presence of the dialectic of identity and difference in mathematics is far from accidental: it is, quite simply, what mathematics is about.

3. CONCLUSION

I have offered a typology of possible sources of beauty in Greek mathematical texts. They fell into three main categories. The first, "narrative", is a consequence of the fact that mathematical texts are freely written, and yet display necessary connections. This allows mathematical texts to display all kinds of combinations of surprise, invention and retrospective inevitability. That is what I call the dialectic of freedom and necessity, a dialectic that often seems to speak to our sense of beauty.

The second, "prosody", is a consequence of the fact that mathematical perception organizes its reality in well-defined units that are strongly structured by a web of relations. This allows mathematical texts to display rich structures, in many interacting layers. That is what I call the dialectic of object and structure, which is at the heart of art and indeed communication in general.

Finally, "correspondence" is a consequence of the fact that mathematical texts constantly restate their contents in equivalent ways. Statements are subtly transformed and restated in derivations, and objects are perceived in sequence through several separate perspectives. This may be at the heart of mathematical beauty since this constant re-shuffling of equivalent statements is what allows mathematical texts to display, finally, both the combinations of surprise and necessity mentioned in the context of "narrative", and the rich structures mentioned in the context of "prosody". In a more narrow sense, the relations displayed in mathematical texts – true identities that bridge truly different objects – somehow pick up a kind of surprise and structure that is of special value. That is the dialectic of identity and difference, which is perhaps one of the major themes of the aesthetic experience. At any rate, in a sense, this dialectic is most perfectly instantiated in mathematics.

In this article, I narrowed the questions of mathematical beauty to the question of beauty in mathematical texts (concentrating on Greek mathematical texts). I have largely ignored the question of beauty as a property of mathematical states of mind, and of beauty as a property of mathematical objects.

I shall not try to offer here any generalizations across historical periods. I did make a few suggestions for possible historical *dis*continuities: the appearance of a personal voice in some early modern genres of mathematics; the valuation of synesthesia and metaphor as such in some fields of modern mathematics. I suspect the typology offered here has considerable continuity with many other genres inside the western tradition, if only because of their genetic dependence upon Greek mathematics. But it will be necessary to study each genre separately, uncovering its own internal aesthetic principles. In the study of experience there are no shortcuts.

Further, I have little to say on beauty as a feature of states of mind. Seen in an abstract light, such states of mind are "text", as well, but texts to which our only access is the mathematician's introspection. This I do not possess, and I can only salute Polya or Poincare, Hardy or Hadamard. The study of such mathematician's reports is important, and may, with caution, be used in a historical study (more on this below). I shall not try to pursue this here except noting that, once again, I suspect there are continuities between the texts of mathematics and "texts" of mathematical intuition. Perhaps surprise and inevitability, the concrete and the abstract, conspire first in the mathematician's imagination, bringing forth in his or her mind what will later on be enacted in writing.

Something similar may be said with greater confidence on the question of the beauty of mathematical objects. Here, I would suggest, we have a special or limiting case of the forces shaping the beauty of mathematical texts. This is most obvious with one major type of mathematical objects, namely mathematical *facts*. Mathematical facts (or results), such as Pythagoras' theorem, or that a sphere's surface is four times its greatest circle, are all clearly beautiful. They are also, simply, a limiting case of a narrative. A result is a narrative, stripped to its bare structure: instead of telling the elaborate story of the first book of Euclid's *Elements*, or of Archimedes' first book on *Sphere and Cylinder*, you reach directly for the punch line. But what makes this beautiful is the promise of a narrative. Any fool can tell you that a sphere's surface is four times its great circle – or indeed that it is three times its great circle (which sounds even nicer). The beauty resides in the statement's being demonstrably true. It is surprise and inevitability combined that make a mathematical statement beautiful: exactly the "narrative" mechanism we saw operating in the beauty of mathematical texts.

Moving on to "objects" in a more narrow sense – to the worlds of parabolas and perfect numbers – our observations on the mathematical perception in texts become relevant to the mathematical objects themselves. Mathematical perception is structural; aided by logic, it brings sharp contrasts, contours and connections. These may then be beautiful. This principle is true of derivations in the mathematical proof – and of objects in the mathematical world. We are able to perceive, through mathematics, that a parabola has infinitely many diameters, around which it is, in a clearly defined sense, symmetrical; we are able to perceive that a perfect number is precisely equal to the sum of its parts. I have said almost nothing so far about "symmetry", "harmony", "equality", "proportion", perhaps the notions that spring to mind most naturally when considering the beauty of mathematics. These are structural notions; "harmony" is perhaps nothing more than a structure we are able to perceive. At any rate, the continuity between the beauty of texts and the beauty of objects, based in both cases on structural perception, seems plausible. Finally, I would suggest the same for the final source of beauty in mathematical texts – the dialectic of identity and difference. This after all is a way in which texts refer to *objects*. It is the same parabola which is seen both as a geometrical cut in a cone, and as the site for abstract proportions; the same perfect number which is perceived both as a sum of numbers and their multiple. Mathematical perception is not only structural, but multi-layered. In particular, we repeatedly see things – when we see them mathematically – as both concrete and abstract. This is the Greek, and then western, bifocal vision of the mathematical object, simultaneously particular and general. And this is further repeated throughout the mathematical disciplines, whether "pure" (where a visual diagram is simultaneously an abstract, language-defined object), or "applied" (where the same object is simultaneously "mathematical" and "physical" – magnitudes in proportion that are plates on a tray). From Plato onwards, this coincidence of the concrete and the abstract seems to have informed the aesthetic appreciation of mathematical objects.

I now move on to a number of objections to the approach delineated in this article.

I can imagine a perplexed response, wondering how valid my account can be given its novelty. The very fact that mathematics is so little explicitly analyzed in aesthetic terms, while art is, seems to indicate a real distinction between the two. This response has to be qualified – in some ways, an aesthetic appreciation of mathematics is not novel, but commonplace – but it is essentially valid. A theoretical approach to the aesthetics of mathematics should also offer an account of its own absence.

Something has already been said in this respect at the end of the preceding section, where I pointed out the complementary nature of mathematics and poetry. In fact, mathematical texts do differ fundamentally from other forms of verbal art. Because their essential organizing principle is that of logical equivalence, they foreground a set of relations which is in principle independent of specific linguistic form. When the speaker and the audience share a large body of linguistic tools (as was true inside Greek mathematical communication), specific forms such as Greek mathematical formulaic expressions may be used to signal and support the logical relation of equivalence. But when the linguistic tools are no longer shared (as happens, for instance, when a Greek mathematical work gets translated by modern mathematicians), the specific linguistic tools are no longer of help at all. To perceive the logical equivalence, then, the modern mathematician must *substitute new forms for the old ones*. Typically, a modern rendering of an ancient mathematical text would transform it into modern algebraic notation. This would be done – here is a crucial realization – not to *suppress* its relevant aesthetic properties, but to *enhance* them. The modern algebraic notation is the restorer's paint, retouching a surface that became worn with time. There can be no aesthetic object where there is no perception, and the perception of mathematical relations is dependent upon using the tools of mathematical perception available in your own culture. Briefly, then, most aesthetic properties of mathematical texts become visible *only under translation*. This is true for most properties of "narrative" and "correspondence", and even to many properties of "prosody" (the rhythm of a mathematical proof, for instance, can only be perceived when the proof is perceived as a flowing sequence, i.e. translated to your own mathematical language). This is directly the opposite of verbal art par excellence – lyric poetry – where the dominant aesthetic properties reside in the specific linguistic form. It is for this reason that mathematics appears not at all to be a form of verbal art. Indeed it isn't. It is enacted in words, but its dominant aesthetics are located not in the verbal, but in the logical domain.

This, however, does not make it any less of an art. Logical relations are not any less interesting than verbal relations. In fact, they allow rich and yet precise structures, much more than verbal relations do. There should therefore be no surprise that poetics is applicable to science or to mathematics. In particular – just because of its great elegance – the glass slipper of structuralist poetics may, indeed, fit the foot of science even better than it fits that of art. Here I approach another possible reaction against my study. Perhaps one reason why many literary scholars are dissatisfied with the old structuralist model is precisely because of its precision, of its search for structural harmonies. In art, the ambiguous is sometimes as valuable as the precise, the jarring as much as the harmonious.[28] A theoretical model, where works of art are analyzed as elegant solutions to problems, cannot apply directly to works whose theme is failure and inresolution.[29] But – with the interesting exception of *paradoxes* - inresolution is not a theme in mathematics. Solutions are; as are structures and harmonies; hence there is also a poetics – and a rather straightforward one - of mathematics.

I am trying to reassure contemporary literary critics. No, I do not demand of them to find in literature quite the same elegance we find in mathematics. But then I know they can hardly feel reassured. In this article, I have argued for the presence of the aesthetic in mathematics, by arguing that mathematics has a quasi-literary structure. I am not so naïve as to fail to realize that, in contemporary literary theory, the notion of the aesthetic in literature has nearly become a taboo.[30] Nor am I so hypocritical as to deny that, in this article, part of my motivation was to challenge this taboo: I use the metaphor of mathematics as literature as beautiful so as, implicitly, to make more plausible the metaphor of literature as mathematics as beautiful. Nor, finally, am I so naïve as to believe it's as easy as that.

The issue goes to the heart of my suggestion at the introduction to this article, that while non-epistemic, beauty may be also a rational value: to put roughly, that there is some objective reality corresponding to the experience of mathematical beauty, which cannot be reduced to its historical construction. This is forcefully denied by contemporary literary theory, where the historicity of value is often seen as a proved fact.

I believe that, in this respect, contemporary literary scholars have an important and valid perception. Value should be historicized. But this, I believe, is not contradicted by the kind of methodology advocated in this article. To the contrary, I will argue that the approach of this article is a necessary – and currently absent – component of historicism. In conclusion, I shall now try to sketch this argument.

Let us start with the following methodological observation. The typology offered in this article was intended, in the first instance, not as a contribution to the pure metaphysics of beauty (an interesting field in its own right). My purpose was, indeed, historical: to find a way to describe a historical reality so that, among other things – armed with such typologies concerning the various possible perceptions of the aesthetic – we may be able to ask more specific historical questions. So, for instance, we could now finally turn to study, *as historians*, Hardy's aesthetic statements. This must be stressed: to be interested in the aesthetics of mathematics, does not at all entail a blind, uncritical trust of, say, Hardy. Exactly the opposite: it is only after we have built our own analytical tools for dealing with the mathematical aesthetic experience, that we are in a position to approach Hardy in a critical way. We may approach Hardy's mathematics, and the mathematics of his time, *independently* of Hardy: poetics, as it were, gives us a privileged access to texts and to the reality of mathematical experience, an access Hardy never had. We can now study the actual forms of aesthetic experience implicit in this particular genre of 20^{th} century mathematics, comparing it to other genres, from different times, places, and fields. And we may of course compare them to Hardy's words *about* the genre, in this way uncovering the ideological valuation of certain kinds of experience at the expense of others. All of this becomes possible *only after* we have made the aesthetic study.

Even without an aesthetic study, we could study, of course, the (rather obvious) ideological positions adopted by Hardy. But we wouldn't know how to situate those positions: as if we had a schematic map of a terrain we did not know. Hardy had a position where "aesthetic" is opposed to "utilitarian", and this is easy to find and to trace as schematic map. But what is the terrain to which this "aesthetic" refers? This terrain is at the level of experience. We just cannot read our map, then, without reference to an understanding of this level of experience. What was the thing Hardy was referring to when he was speaking about 'beauty'? An answer to this question does not require of us to share Hardy's valuation of the thing; but it does require us to try to analyze the reality of experienced texts underlying Hardy's statements. And therefore – as historians – we need to understand the whole range of phenomena described in this article – narrative and its flow, perception and its structure, the way reality is taken in. It is all very easy, to say that Hardy made an ideological valuation of *something*; the real challenge is to say what this something was.

Of course cultures create their patterns of value. How else could value become part of social existence? But they cannot make such patterns out of nothing, into nothing. They make such patterns out of something (out of the sheer facts of experience), into something (into another objectively felt experience, that of value). The historicism of value is an empty claim as long as it does not confront those – objective – realities, out of which and into which the subjective is made.

Having replied to my colleagues in literary theory, it is finally time for me to turn to my colleagues in the philosophy of mathematics. Indeed I need only glance behind my shoulder, to the preceding articles in this collection. In a sense they do not question my very enterprise in quite the same way the literary critics did. I imagined the literary critics questioning the very notion of 'mathematical beauty'. Not so the philosophers of mathematics. Their worry, it appears to me, is different and – I concede – justified. Having agreed on the existence of the category of 'the beautiful' in mathematics, how does it speak to the concerns of the philosophy of mathematics?

It is of course a contribution to the *aesthetics* of mathematics, and the main claim I now need to make is for the need for such a philosophical investigation, independent of the epistemic concerns that drive the other articles in the collection.

One could address the question, why a mathematician may wish, for instance, to have the narrative effect of surprise – in terms of the epistemic significance such an effect might possess. One may argue perhaps that a surprising result challenges us to uncover the links leading to its conclusion, in this way possessing a specific epistemic value. I do not believe Archimedes, for example, chose to have a surprising narrative in his *Sphere and Cylinder I* for this reason (a plausible historical argument can be made that Archimedes' aim was purely aesthetic), but in principle surprising narratives can definitely be epistemically motivated. Or one may argue the converse (which, in this case is, in my view, historically valid): one may argue that a stately, orderly progression of narrative such as that of Euclid's *Elements I* has a precise pedagogic effect, that helps the reader parse the text as it unfolds, in this way making it easier to follow not the results alone but also their pattern of logical interrelation, so that the text as a whole becomes richer, to the reader, in its explanatory meaning. I think it is likely that Euclid's choice to prefer this model of narrative, then, was pedagogically – that is epistemically – rather than aesthetically motivated.

In other words, one strategy I could have taken while presenting this paper to my colleagues in the philosophy of mathematics was to associate the aesthetic effects discussed to their epistemic correlates, so that my aesthetic styles would translate into epistemic styles in line with those discussed in the preceding articles. This would have been possible and, for certain purposes, valuable. Yet I have avoided, on purpose, doing so. And this was precisely because my philosophical point was to make the claim for the theoretical independence of an aesthetics of mathematics, as a philosophical domain existing apart from the epistemological one.

The issue is partly historiographical, partly philosophical. The historiographical point was already hinted at above: I suggested briefly that I believe Euclid's goal, in his choice of narrative form for *Elements I*, was primarily pedagogic, that is epistemic, while Archimedes' goal, in his choice of narrative form for *Sphere and Cylinder I*, was primarily aesthetic. Euclid wanted his readers to learn something; Archimedes wanted to elicit a gasp of pleasured surprise. I will not try and advance the historical argument here (admittedly certainty is hardly possible with such questions having to do with authorial intention). But the historiographical point is conceptually clear. The position I describe concerning Euclid and Archimedes is obviously a possibility. And, unless we allow the existence of a separate aesthetic realm independent from the epistemic, then we cannot even formulate such a position. For our writing of the history of mathematics, then, the aesthetic is a category we need in order to be able to state the full range of possible motivations of authors in their writings: as simple as that.

The philosophical point – related to the historiographical one – is more subtle. It has to do with the nature of mathematical experience itself.

It might appear strange for me to invoke, at this stage of the argument, mathematical experience. After all I have eschewed the difficult question of the mathematical states of mind, concentrating instead on the objective features of texts. My approach throughout was structuralist, looking at the semiotic properties of texts as vehicles for positively defined aesthetic effects. And yet my goal of course was to begin to stake a ground in this difficult terrain of experience. My approach throughout – here as in my other studies in the cognitive history of mathematics – is to concentrate on the surface details of texts, as offering us the best objective evidence to mathematics as it is actually experienced (as opposed to some logical analysis of its abstract content).

My philosopher colleagues will recognize my intention, if I stress my interest in the *phenomenology* of mathematics. I ask the question: How is mathematics present to the mind? And I concentrate on a more modest question (where there is some useful evidence to work with): how are mathematical texts present to the mind? My claim is that categories such as 'truth' or 'validity', even categories such as 'visual' and 'symbolic', cannot fully provide an answer to this question. Part of the answer has to bring in other categories of experience, such as 'delight', 'pleasure', 'surprise', or, indeed, 'beauty'. All those terms of value are important not so that we may judge mathematics, but so that we can describe it—in its phenomenal reality. A phenomenology that disavows the experiences of value offers an abstracted, synthetic vision of mathematics: as it were, a *disembeautied* vision, which therefore has to be also a *disembodied* vision. And so, to begin to outline the actual phenomenology of mathematics, we must start from its real phenomenal reality—the full range of its experience of value and cognition, perception and appetite. Which indeed reminds me that I should get back to my interrupted lunch.

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NOTES

[1]Contemporary mathematicians often refer to Erdös' famous dictum on 'the book of beautiful proofs' (out of whose enumerable infinity mathematicians seek to find their proofs!) – see Aigner and Ziegler (1998).

[2]As is obvious by now, I am mainly influenced in my thinking by structuralist poetics, broadly construed - mostly because of its tangible, immediate applicability. I do not at all dismiss other possible approaches, and I would be happy to see my typology of sources of beauty enriched by further methodologies. Nor do I think "texts" exhaust the problem. The beauty inherent in states of mind seems to be a central theme in mathematicians' own reports, and thus deserves close study. The notion of the intrinsic beauty of the mathematical realm of being is one of the key issues of western philosophy from Plato onwards; see Burnyeat (1998). I shall briefly return to those general issues in the conclusion.

[3]See e.g. (Lang, 1985, 3): "The Greeks did mathematics for the beauty of it", or the whole of Artmann (1999) – a passionate reading of Euclid by a contemporary mathematician, arguing for the sources of many mathematical values, in particular beauty, in Greek mathematics.

[4]This is the main theme of Lotman (1976).

[5]One should note however the interesting complications of orders of reality, for instance in proof by contradiction, where an alternative reality is entertained "for the sake of the argument" – the perspective adopted and then finally discarded. I also ignore the role of the first person singular in some formulaic expressions such as "I say that", which, because formulaic, perhaps do not have much real force. I give an example of both phenomena at the beginning of the next subsection. Finally, note that in early modern mathematics, the authorial voice frequently interferes in the text, suggesting the line of discovery and playfully interweaving the subjective narrative of the implied author with the objective narrative of the proof. (For a celebrated example, see Descartes' *Geometry*; I thank Heda Segvic for suggesting this comparison.) The implicit claim of the absent perspective characterizes not the aesthetics of mathematics as such, but rather the aesthetics of Greek mathematics.

[6]My choice of the term 'narrative' for this process of selection and combination of information is not obvious. I could equally have called this 'rhetoric', which however I have refrained from doing, wishing to avoid the pointless debate of 'logic vs.

rhetoric'. Obviously the structures represented in mathematical texts are unlike the plots involving human agents which the term 'narrative' brings to mind: I hope to show that, even so, the term 'narrative' remains useful in mathematics as well. (For an interesting discussion of the applicability of the term 'narrative' to mathematics see Thomas (2002).

[7]The term 'capstone theorem' was suggested to me by Henry Mendell. Mendell has also suggested to me the further observation, that most books in Euclid's *Elements* seem to end with such a capstone theorem.

[8]As it were, the considerations of statics and of aesthetics, governing the arrangement of the plates on the tray, are both organized by the same principles (e.g. of simplicity of form and of proportion).

[9]The Kantian aesthetic program is to understand art through the dialectic of the subject's freedom and the world's lawlikeness: see e.g. Krukowski (1992) chapter 1.

[10]That prosody was suppressed in Greek mathematics can be seen from the fate of the Archimedean corpus: originally written in Archimedes' Doric dialect, it was at some point mostly transferred into the *Koine* dialect. Such dialect transformation impacts almost exclusively on prosody: but clearly readers did not consider that the text has lost any meaningful dimension.

[11]Sometimes "narrative" is given a wider sense, so that the contrast is *within* narrative structure, between description and *fabula*: see e.g. (Bal, 1997, 36-43) .

[12]The terms derive from Proclus' *In Eucl. I* 203.

[13]"Syntagmatic" is a technical term of structuralist poetics: I briefly explain a few of those terms at the start of subsection 2.4 below.

[14]The D (8) passage – Heath 362.12-364.4 – is an interesting complication: a mere unpacking of what the diagram stands for (description, then, in literary terms) is of immediate argumentative context (and therefore functions here as part of the proof, in the mathematical sense).

[15]On starting-points and arguments in general see (Netz, 1999, 169–198).

[16]I have noted the tendency to have a "smoother" movement towards the end of a proof, in (Netz, 1999, 206-207) , calling it "the cadenza effect". I have there stressed the possible rhetorical function of this effect; once again, the rhetorical and the aesthetic coincide.

[17]See (Netz, 1999, Ch. 4) for the role of formulaic expressions. I return to discuss these in greater detail below, when introducing the general notion of "correspondence".

[18]Note however that there might, in principle, be aesthetic value not in clarity, but in *ambiguity*. This is in fact exactly parallel to the case of *irony*, mentioned in the preceding subsection. The mathematical text largely forgoes the aesthetic possibilities of ambiguity, just as it largely forgoes the aesthetic possibilities of irony. This is a limitation on the aesthetic range of mathematics – though once again, a certain beauty resides in the limitation itself, providing mathematics with its sharp luminosity.

[19]For all this see especially Jakobson (1987) chapter 8, originally published in 1956.

[20]As noted by Jakobson himself (Jakobson, 1987, 113-114), metaphor is in general easier to understand than metonym. It is in fact difficult to think of examples of metonym in Greek mathematics. I note below the use of particular cases for general statements, which is perhaps metonym-like; probably the clearest example of metonym in mathematics in general is mathematical induction, where the argument relies on the ineritence of properties by objects contiguous to each other (this is a modern method: see Unguru (1991)). Perhaps even: when mathematicians say that proofs by mathematical induction are "strange" and do not reveal the "real reasons", they, among other things, display the typical human preference for metaphor over metonym?

[21]For a historical survey and interesting philosophical observations, see Dann (1998).

[22]On this very widespread experience see Seron et al. (1992).

[23]For the quest and its results, see, e.g., Corry (1996). Note further a special twist on this quest for global metaphor: the brilliant insight of some 20^{th} century mathematical works, that the signified can metaphorically act as sign – so that, for instance, *numbers* are equated with *statements about numbers*.

[24]Indeed the effectiveness of rhyme often has to do with a distance which is not only phonetic, but also semantic: compare e.g. (Wimsatt, 1954, 153–168).

[25]I concentrate on the simple form P→Q, ignoring the complication of the more common structures, P&Q→R, etc. The presence of several premises for a single conclusion is, among other things, a further device for creating a pleasing distance between set of premises and conclusion.

[26]Note that this is not the definition of similarity of triangles – defined by equal angles – but a result (Euclid's *Elements* VI.4). The derivation is thus a substantive equivalence between two different statements (rather than a disguised tautology).

[27](Jakobson, 1987, Ch. 19), translation of an article from 1933-4. Jakobson expresses here standard formalist positions, perhaps first articulated in Shklovsky (1919).

[28]The preference for the disharmonious may be related to the widespread contemporary interest in literature as "subversion" (it seems at any rate that such an interest in "subversion" was at the root of Bakhtin's own distancing from Jakobson's type of structuralist poetics which of course, through Kristeva (1980) and other routes, came to dominate contemporary literary theory).

[29]Of course, we will need the theoretical model to analyze how the effect of inresolution is obtained; but it is true that the basic tenor of structuralist poetics is alien to such an analysis.

[30]See Smith (1988) for a fascinating statement of the doubt in the aesthetic in contemporary literary theory.

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