New Perspectives on Mathematical Practices
Essays in Philosophy and History of Mathematics
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EDITORIAL PREFACE

The Second Perspectives on Mathematical Practices Conference (PMP2007) was held at the Free University of Brussels (VUB), Belgium, from 26 to 28 March, 2007. This volume contains texts issuing from talks delivered at that occasion which particularly focused on the historical dimension of mathematical practice, the core subject of the conference. All papers gathered here address aspects of the question how the philosophy of mathematics relates to the history of mathematics. Nature and goals of this type of inquiry have been most clearly stated by José Ferreirós and Jeremy Gray in the introduction to their seminal reader The Architecture of Modern Mathematics (Oxford University Press, 2006), opposing with great sensitivity the ahistorical received view in the philosophy of mathematics to a recently emerging trend of studies in contextualized mathematical practices. We subscribe to the programme set out by them, and hope to provide here with a modest contribution to it. Incidentally, both editors have participated to PMP2007, and one may find Gray’s paper included here, while that by Ferreirós is part of a companion volume consisting of rather philosophically laden texts, to appear with College Publications.

Let me give, at the outset of this collection, an overview of the articles included, which have been loosely organized in chronological order of the period covered by them. Algebra is the topic of the first triple of papers, opening with that by Albrecht Heeffer (Ghent), in which it is contended that the traditional three-stage division of the development of algebra, viz. into rhetorical, syncopated and symbolic phases, is not adequate. As is argued, it would better be replaced by an alternative account, in which non-symbolic, proto-symbolic and symbolic phases succeed one another. The first covers the algorithmic type of algebra dealing with numerical values or a non-symbolic model, e.g. Greek geometrical algebra. The second is home to algebras employing words or abbreviations for the unknown but not therefore being symbolic in character, such as Diophantan and early Abbacus algebra. The third and last phase, that of (truly) symbolic algebra, viz. allowing for manipulations on the symbolic level of symbols only, starts around 1560.
Ad Meskens (Antwerp) in his contribution explains that Diophantos had complete mastery over the methods of solution for solving linear equations, for indeterminate quadratic equations and for systems of equations of first and second degree, while for higher degree equations he only sometimes had a solution method. When in 1971 an Arabic version of Diophantos was found, it came as a shock that in this book some previously unknown material was found. In it, Diophantos apparently used the methods for solving higher-degree problems described in other books to their limits. The structure of a Diophantine problem follows a general rule set out by Proklos. Problems are put a general way, using indeterminate numbers. But solutions are provided for specific numbers given at the outset. We can see a partial analogy with geometrical construction: applied to a specific figure, though posed generally. Even if the specific example allows it, Diophantos never gives general methods. During the elaboration of the example he sometimes adds a restriction. In some other cases, where there is a need for restrictions, he does not impose them. It is unclear whether this is due to ignorance about the need of such a restriction or to the impossibility to correctly formulate it.

According to Jens Høyrup (Roskilde), Italian fourteenth- and fifteenth-century abbacus algebra presents us with a number of deviations from what we would consider normal mathematical practice and proper mathematical behaviour: the invention of completely false algebraic rules for the solution of cubic and quartic equations, and of rules that pretend to be generally valid but in fact only hold in very special cases; and (in modern terms) an attempt to expand the multiplicative semi-group of non-negative algebraic powers into a complete group by identifying roots with negative powers. In both false-rule cases, the authors of the fallacies must have known they were cheating. Certain abbacus writers seem to have discovered, however, that something was wrong, and devised alternative approaches to the cubics and quartics; they also developed safeguards against the misconceived extension. In his paper, Høyrup analyses both phenomena, and correlates them with the general practice and norm system of abbacus mathematics as this can be extracted from the more elementary level of the abbacus treatises.

Matthew Parker’s (London) contribution considers Cantor’s extension of the concept of number to the transfinite, and the resolution this supplies for what has been called “Galileo’s Paradox”, namely that the square numbers seem to be at once fewer than and equal to the positive integers. Galileo’s Paradox is held to have been resolved by the articulation of numerosity into distinct concepts, including those of proper inclusion,
Anzahl, and power. Power has become the basis of an elegant and useful theory and has proven especially useful in addressing the motivations common to Galileo, Bolzano and Cantor, namely, to grasp the relations between numerosity and geometric magnitude, to defend the analysis of the continuum into points, and to explain physical phenomena. As Parker explains, it is in virtue of its success in serving such motivations that Cantor’s theory of transfinite numbers constitutes a solution to some of the deeper philosophical problems posed by Galileo’s Paradox. But there are alternatives. For example, Anzahl too can be considered as a notion of numerosity. In order to analyze this matter, Parker proposes a Method of Conceptual Articulation.

During the first part of the nineteenth century, the mathematical disciplines of analysis and algebra developed tremendously, exploring new techniques and questions to arrive at far-reaching and sometimes surprising new results. According to Henrik Kragh Sørensen (Aarhus), a central part of this development involved changes in the role and use of representations. Mathematicians often work with representations in order to access and manipulate mathematical objects such as functions. These uses can satisfy a variety of demands. For instance, representations of implicitly defined functions as infinite series can add to the familiarity of these new functions by anchoring them within existing ways of accessing and manipulating functions. In different contexts, the question of whether a given function can or cannot be represented in a specific form opens the door for new results such as impossibility proofs. In his paper, Kragh Sørensen analyses such multiple roles of representations of functions from a Wittgenstein-inspired perspective as “aspects” of functions. By comparing important results from algebra (algebraic unsolvability of the general quintic equation) and analysis (representations of elliptic functions) in the context of Abel’s mathematics, representations are highlighted both as means and ends in themselves.

Point of departure of the next article, by Jeremy J. Gray (Milton Keynes), is the observation that, on the one hand, historians and philosophers of mathematics share an interest in the nature of mathematics (what it is, what features affect its growth, how it informs other disciplines), but that, on the other hand, much of the work done in history and philosophy of mathematics shows that the two groups largely work in isolation. A reconsideration of the history of mathematical analysis in the nineteenth century, according to Gray, suggests that history and philosophy of mathematics can be done together to the advantage of both, and also how legitimately different enquiries need not drive them apart.
The last few decades have witnessed a broadening of the philosophy of mathematics, beyond narrowly foundational and metaphysical issues, and towards the inclusion of more general questions concerning methodology and practice. Part of this broadening, although a part that remains relatively close to foundational and metaphysical issues, is the turn towards a “new epistemology” for mathematics, including the study of topics such as the role of visualization in mathematics, the use of computers in proving mathematical theorems, and the notion of explanation as applied to mathematics.

Erich H. Reck’s (Riverside, CA) paper is a contribution to such a new epistemology. More particularly, it is an attempt to bring into sharper focus, and to argue for the relevance of, two related themes: “structural reasoning” and “mathematical understanding”. As the notion of understanding is vague and slippery in general, as well as very loaded in philosophical discussions of the sciences, the label is handled with care. Similarly, while talking about “structural” reasoning in mathematics may be suggestive, that term too requires further elaboration. Reck’s clarifications and elaborations are tied to a specific historical figure and period, Richard Dedekind, and his contributions to algebraic number theory in the nineteenth century, which proves to be all but an incidental choice.

Eduard Glas (Delft) undertakes a comparison between mathematician Felix Klein and philosopher Imre Lakatos. Klein, Glas argues, is perhaps the most outstanding example of an eminently fruitful mathematician opposing the one-sided obsession of most mathematicians of his generation with purity and rigor, an obsession through which the discipline increasingly tended to fall apart into disparate, self-contained specialties. In contrast to the adepts of rigor and purity within the leading schools, who eschewed reliance on intuitive or quasi-empirical insights, Klein’s methodology was based on the use of geometric and even physical models and thought experiments, a methodology which certainly qualifies as ‘quasi-empirical’. Klein’s successes depended in large measure on his exceptional versatility in the mental visualization even of the most abstract mathematical objects and relations. Throughout his career, Klein kept insisting that intuition, especially spatial intuition, is indispensable in all mathematical endeavours, which also makes for their rootedness in concrete experience. According to Glas, Klein was as much a maverick in the eyes of ‘pure’ mathematicians as Imre Lakatos would become in the eyes of mainstream philosophers of mathematics. Like Lakatos, Klein insisted that progress in mathematics relies on methods that are very much akin to those of natural science, especially as concerns the use of models and (thought) experiments. He in
fact practised a model-based, quasi-empirical method of investigation that
indeed tallies nicely with Lakatos’ quasi-empiricist methodology.

The idea that formal geometry derives from intuitive notions of space
has appeared in many guises, most notably in Kant’s argument from geo-
metry. Kant claimed that a priori knowledge of spatial relationships both
allows and constrains formal geometry: it serves as the actual source of our
cognition of the principles of geometry and as a basis for its further cultural
development. The development of non-Euclidean geometries, however, un-
dermined the idea that there is some privileged relationship between our
spatial intuitions and mathematical theory. The aim of the paper by Helen De Cruz
(Leuven) is to look at this longstanding philosophical issue
through the lens of cognitive science. Drawing on recent evidence from cog-
nitive ethology, developmental psychology, neuroscience and anthropology,
she argues for an enhanced, more informed version of the argument from
geometry: humans share with other species evolved, innate intuitions of
space which serve as a vital precondition for geometry as a formal science.

In line with the general spirit of the underlying conference, Ronny
Desmet (Brussels) observes that it is part of the growing awareness that
historical, social and psychophysical processes precede the cut and dried
results of mathematics, even those which have been presented as the ob-
vvious starting points of all pure mathematics. And as with all fashionable
currents, he continues, the shift from foundations to practices in the phi-
losophy of mathematics has its heroes. Indeed, Lakatos and Wittgenstein
immediately come to mind in this respect. If an author were to say that,
given their mutual influence, Wittgenstein’s view on mathematics can be
identified with Russell’s, dissent would follow. But if he were to say that,
given their intense collaboration Whitehead’s view on mathematics can
be identified with Russell’s, this claim would normally pass without much
protest. Desmet, however, could not disagree more. In his paper, he argues
that Whitehead, like Wittgenstein, should be differentiated from Russell,
and given his own niche in philosophy of mathematics, and that, further-
more, Whitehead’s writings, which were based on his own mathematical
experience, offer a perspective on mathematical practices equalling, or even
surpassing, that provided by Wittgenstein.

In discussions of mathematical practice, Dirk Schlimm (Montréal)
points out, the role axiomatics has often been confined to providing the
starting points for formal proofs, with little or no effect on the discovery or
creation of new mathematics. Nevertheless, it is undeniable that axiomatic
systems have played an essential role in a number of mathematical innova-
tions. Moreover, it was not only through the investigation and modification of given systems of axioms that new mathematical notions were introduced, but also by using axiomatic characterizations to express analogies and to discover new ones. In his contribution, which closes this volume, Schlimm however draws our attention to a different use of axiomatics in mathematical practice, namely that of being a vehicle for bridging theories belonging to previously unrelated areas. How axioms have been instrumental in linking mathematical theories is illustrated by the investigations of Boole, Stone, and Tarski, all of which revolve around the notion of Boolean algebra.

As already mentioned, a companion volume of (more philosophically oriented) PMP2007 proceedings papers is to be published by College Publications, London. At the outset of the present one, allow me to express my gratitude to an array of people. For naturally, this proceedings volume did not come about because of the efforts of the editor alone. To begin with, I am extremely grateful for all institutional and personal contributions to what in my eyes was a very successful PMP2007 conference. This includes the generous sponsors of the event: Research Foundation — Flanders, Brussels Capital-Region, National Centre for Research in Logic — Belgium, as well as its (co-)organizers Belgian Society for Logic and Philosophy of Science, Wissenschaftliches Netzwerk PhiMSAMP, and Centre for Logic and Philosophy of Science at Free University of Brussels. Further, I was very fortunate to leave local organization largely in the safe hands of my precious colleagues Patrick Allo, Ronny Desmet, and Karen François. And as there is simply no event whatsoever without an interested and interesting audience, let me hereby also thank all participants to PMP2007 (including the authors of this book). I sincerely hope that this conference series, started in 2002 with the initial PMP, may live on, possibly at other locations. On a more personal level, I also feel indebted to the bodies that have funded my academic work during the past years: Research Foundation — Flanders, Free University of Brussels (through BAP and GOA49), and Alexander von Humboldt-Foundation — Germany (with special thanks to my dear colleague and friend Thomas Müller). Finally, I would love to dedicate this volume to Jean Paul Van Bendegem, who has been my faithful and trusting mentor for a decade now, and to whom, philosophically but also beyond, I owe so much.

Bart Van Kerkhove
Brussels, Belgium
(editor)

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Dirk Schlimm
ON THE NATURE AND ORIGIN
OF ALGEBRAIC SYMBOLISM

ALBRECHT HEEFFER
Centre for Logic and Philosophy of Science
Ghent University
∗E-mail: albrecht.heeffer@ugent.be
http://logica.UGent.be/albrecht/

1. The myth of syncopated algebra

Ever since Nesselmann’s study on “Greek algebra” (1842),¹ historical accounts on algebra draw a distinction between rhetorical, syncopated and symbolic algebra. This tripartite distinction has become such a commonplace depiction of the history of algebraic symbolism that modern-day authors even fail to mention their source (e.g., Boyer,² p. 201; Flegg and Hay;³ Struik⁴). The repeated use of Nesselmann’s distinction in three Entwicklungstufen on the stairs to perfection is odd because it should be considered a highly normative view which cannot be sustained within our current assessment of the history of algebra. Its use in present-day text books can only be explained by an embarrassing absence of any alternative models. There are several problems with Nesselmann’s approach.

1.1. A problem of chronology

Firstly, if seen as steps within a historical development, as is most certainly the view shared by many who have used the distinction, it suffers from some serious chronological problems. Nesselmann¹ (p. 302) places Iamblichus, Arabic algebra, Italian abacus algebra and Regiomontanus under rhetorical algebra (“Die erste und niedrigste Stufe”) and thus covers the period from 250 to 1470. A solution to the quadratic problem of al-Kwārizmī is provided as an illustration. The second phase, called syncopated algebra, spans from Diophantus’s Arithmetica to European algebra until the middle of the seventeenth century, and as such includes Viète, Descartes and
Albrecht Heeffer

van Schooten. Nesselmann discusses problem III.7 of the *Arithmetica* as an example of syncopated algebra. The third phase is purely symbolic and constitutes modern algebra with the symbolism we still use today. Nesselmann repeats the example of al-Kwārizmī in modern symbolic notation to illustrate the third phase, thereby making the point that it is not the procedure or contextual elements but the use of symbols that distinguishes the three phases.

Though little is known for certain about Diophantus, most scholars situate the *Arithmetica* in the third century which is about the same period as Iamblichus (c. 245-325). So, syncopated algebra overlaps with rhetorical algebra for most of its history. This raises serious objections and questions such as “Did these two systems influence each other?” Obviously, historians as Tropfke⁵ (II, p. 14) and Gandz⁶ (p. 271) were struck by this chronological anomaly and formulated an explanation. They claim that Arabic algebra does not rely on Diophantus’ syncopated algebra but descends instead from Egyptian and Babylonian problem-solving methods which were purely rhetorical. However, these arguments are now superseded by the discovery of the Arabic translations of the *Arithmetica* (Sesiano⁷ and Rashed⁸).

Diophantus was known and discussed in the Arab world ever since Qustā ibn Lūqa (c. 860). So if the syncopated algebra of Diophantus was known by the Arabs, why did it not affect their rhetorical algebra? If the Greek manuscripts, used for the Arab translation of the *Arithmetica* contained symbols, we would expect to find some traces of it in the Arab version.

### 1.2. The role of scribes

The earliest extant Greek manuscript, once in the hands of Planudes and used by Tannery,⁹ is Codex Matritensis 4678 (ff. 58-135) of the thirteenth century. The extant Arabic translation published independently by Sesiano⁷ and Rashed⁸ was completed in 1198. So no copies of the *Arithmetica* before the twelfth century are extant. Ten centuries separating the original text from the earliest extant Greek copy is a huge distance. Two important revolutionary changes took place around the ninth century: the transition of papyrus to paper and the replacement of the Greek uncial or majuscule script by a new minuscule one. Especially the transition to the new script was a drastic one. From about 850 every scribe copying a manuscript would almost certainly adopt the minuscule script (Reynolds and Wilson,¹⁰ pp. 66–7). Transcribing an old text into the new text was a laborious and difficult task, certainly not an undertaking to be repeated when a copy in the new script was already somewhere available. It is therefore very likely
that all extant manuscript copies are derived from one Byzantine archetype copy in Greek minuscule. Although contractions were also used in uncial texts, the new minuscule much facilitated the use of ligatures. This practice of combining letters, when performed with some consequence, saved considerable time and therefore money. Imagine the time savings by consistently replacing \( \alpha \rho \iota \theta \mu \omicron \varsigma \), which appears many times for every problem, with \( \varsigma \) in the whole of the *Arithmetica*. The role of professional scribes should therefore not be underestimated. Although we find some occurrences of shorthand notations in papyri, the paleographic evidence we now have on a consistent use of ligatures and abbreviations for mathematical words points to a process initiated by mediaeval scribes much more than to an invention by classic Greek authors. Whatever syncopated nature we can attribute to the *Arithmetica*, it is mostly an unintended achievement of the scribes.\(^a\) The complete lack of any syncopation in the Arabic translation further supports this thesis. The name for the unknown and the powers of the unknown and even numbers are written by words in Arabic translation. The lack of, at that time, well-established Hindu-Arabic numerals seems to indicate that the Arabic translation was faithful to a Greek majuscule archetype. Sesiano\(^7\) (p. 75) argues that the Arabic version relies on the commentary by Hypatia while the Greek versions relate to the original text with some early additions and interpolations. Although the thesis of the reliance on Hypatia’s commentaries is strongly opposed by Rashed\(^8\) (III, p. LXII), and while they disagree on many others issues, both interpretations and translations of the Arabic text concur on the lack of symbolism or syncopation. The \( \alpha \lambda \sigma \gamma \omicron \varsigma \alpha \rho \iota \theta \mu \omicron \varsigma \), or ‘untold number’ of the Greek text, is translated as ‘say’ in Arab, and is thus very similar to the *cosa* of abbaco texts or the *coss* of German cossic texts.

In so far the *Arithmetica* deserves the special status of syncopated algebra, it is very likely that the use of ligatures in Greek texts is a practice that developed since the ninth century and not one by Diophantus during the third century. This overthrows much of the chronology as proposed by Nesselmann.

### 1.3. Symbols or ligatures?

A third problem concerns the interpretation of the qualifications ‘rhetorical’ and ‘syncopated’. Many authors of the twentieth century attribute a

\(^{a}\) This view also has recently been put forward in relation to Archimedes’ works (Netz and Noel\(^{11}\)).
highly symbolic nature to the *Arithmetica* (e.g. Kline,\textsuperscript{12} I, pp. 139–40). Let us take Cajori\textsuperscript{13} (I, pp. 71–4) as the most quoted reference on the history of mathematical notations. Typical for Cajori’s approach is the methodological mistake of starting from modern mathematical concepts and operations and looking for corresponding historical ones. He finds in Diophantus no symbol for multiplication, and addition is expressed by juxtaposition. For subtraction the symbol is an inverted $\psi$. As an example he writes the polynomial

$$x^3 + 13x^2 + 5x + 2 \quad \text{as} \quad K^{Y} \alpha \Delta^{Y} \varsigma \chi \varsigma \epsilon \bar{M} \bar{\beta}$$

where $K^{Y}$, $\Delta^{Y}$, $\varsigma$ are the third, second and first power of the unknown and $\bar{M}$ represents the units. Higher order powers of the unknown are used by Diophantus as additive combination of the first to third powers.

Cajori makes no distinction between symbols, notations or abbreviations. In fact, his contribution to the history of mathematics is titled *A History of Mathematical Notations*. In order to investigate the specific nature of mathematical symbolism one has to make the distinction somewhere between symbolic and non-symbolic mathematics. This was, after all, the purpose of Nesselmann’s distinction. We take the position together with Heath,\textsuperscript{14} Ver Eecke\textsuperscript{15} and Jacob Klein, that the letter abbreviations in the *Arithmetica* should be understood purely as ligatures (Klein,\textsuperscript{16} p. 146):

We must not forget that all the signs which Diophantus uses are merely word abbreviations. This is true, in particular for the sign of “lacking”, $\uparrow$, and for the sign of the unknown number, $\varsigma$, which (as Heath has convincingly shown) represents nothing but a ligature for $\alpha \varsigma \rho \iota \tau \mu \sigma$.\textsuperscript{b}

Even Nesselmann\textsuperscript{1} acknowledges that the ‘symbols’ in the *Arithmetica* are just word abbreviations (“sie bedient sich für gewisse oft wiederkehrende Begriffe und Operationen constanter Abbreviaturen statt der vollen Worte”). In his excellent French literal translation of Diophantus, Ver Eecke\textsuperscript{15} consequently omits all abbreviations and provides a fully rhetorical rendering of the text as in “Partager un carré proposé en deux carrés” (II.8), which makes it probably the most faithful interpretation of the original text.\textsuperscript{b}

\textsuperscript{b} This problem led Fermat to add the marginal note in his copy of Bachet’s translation “Cubum autem in duos cubos, aut quadratoquadratum in duos quadratoquadratos, et
This objection marks our most important critique on the threefold distinction: symbols are not just abbreviations or practical short-hand notations. Algebraic symbolism is a sort of representation which allows abstractions and new kinds of operations. This symbolic way of thinking can use words, ligatures or symbols, as we will argue further. The distinction between words, word abbreviations and symbols is in some way irrelevant with regards to the symbolic nature of algebra.

1.4. Counter-examples

A final problem for Nesselmann’s tripartite distinction is that now, almost two centuries later, we have a much better understanding of the history of symbolic algebra. Nesselmann relied mostly on the Jesuit historian Cossali for a historical account of Italian algebra before the sixteenth century. Except for a text by Rafello Canacci, Cossali does not discuss much the algebra as it was practiced within the abbacus tradition of the fourteenth and fifteenth century. Guillaume Libri, who had collected many manuscripts from this tradition, describes and published several transcriptions in his *Histoire des sciences mathématiques en Italie* published in 1838. Oddly, the well-informed Nesselmann does not seem to know Libri’s work and thus remains ignorant of the continuous practice of algebra in Italy since Fibonacci and the first Latin translations of al-Kwārizmī. It is only since the last few decades that we have a more complete picture of abbacus algebra, thanks to the work of Gino Arrighi, Warren van Egmond, the Centro studi della matematica medioevale of Sienna and Høyrup’s recent book (Høyrup). In our understanding, symbolic algebra is an invention of the sixteenth century which was prepared by the algebraic practice of the abbacus tradition. At least abbacus algebra has to be called syncopated in the interpretation of Nesselmann. A lot of abbacus manuscripts use abbreviations and ligatures for *cosa*, the unknown (as *c*, *co.* or *g*), cens**o** or cien**s**o, the second power of the unknown (ce. or *c*) cubo, the third power (cu.) and beyond. Also plus, minus and the square root are often abbreviated, such as in *p*, *m* and *R* (with an upper or lower dash). From the fifteenth century we also find manuscripts that explicitly refer to a method of solving problems that is different from the regular rhetorical method. In an anonymous manuscript of c. 1437, the author solves several standard problems in two ways. One
he calls symbolic (figuratamente) and the other rhetorical (per scrittura). Possibly the practice of solving a problem figuratamente existed before, but in any case it was not found suitable to be written down in a treatise. Here however, the anonymous author believes a symbolic notation contributes to a better understanding of the solution as he writes:

I showed this symbolically as you can understand from the above, not to make things harder but rather for you to understand it better. I intend to give it to you by means of writing as you will see soon.\footnote{f. 59r: “Ora io telo mostrata figuratuiamente come puoi comprendere di sopra bene che e lla ti sia malagievole ma per che tulla intenda meglio. Io intende di dartela a intendere per scrittura come apresso vedrai”}

He then repeats the solution in a rhetorical form as we know it from other abbacus texts. This is the first occasion in the history of algebra where an author makes an explicit reference to two different kinds of problem solving, which we would now call symbolic and non-symbolical.

This manuscript or related copies may have influenced the German cossists. Regiomontanus, who maintained close contacts with practitioners of algebra in Italy, adopts the same symbolic way of solving problems. In his correspondence with Johannes Bianchini of 1463 we find problems very similar to the abbacus text: divide 10 into two parts so that one divided by the other together with the other divided by the first equals 25.\footnote{The correspondence is kept in Nürnberg, City Library, Cent. V. 56c, ff. 11r-83v. The transcription is by Curtze\textsuperscript{21} (pp. 232–4): “Divisi 10 in duos, quorum maiorem per minorem divis, item minorem per maiorem. Numeros quotiens coniunxi, et fuit summa 25: quero, que sint partes”. The corresponding problem in Magl. Cl. XI. 119 is on f. 61v but uses a sum of 50 instead of 25.} In modern symbolic notation the problem can be formulated as in Fig. 1:

Regiomontanus solves the problem in the same manner of abbacus algebra but adopts only the symbolic version. He uses symbols for cosa and censo which we typically find in German cossist algebra from 1460 for a period of about 160 years.

While we see in later abbacus algebra and Regiomontanus the roots of symbolic algebra, Nesselmann places both within the stage of rhetorical algebra. According to Nesselmann’s own definition these two instances of algebraic practice should at least be called syncopated.
Fig. 1. The solution of an Arabic division problem by Regiomontanus (c. 1460, Nürnberg Cent. V 56c, f. 23) with literal transcription in modern symbolism

1.5. Conclusion

We have argued that the interpretation of rhetorical, syncopated and symbolic algebra as three historical phases in the development of algebra cannot be sustained. The special status given to syncopated algebra seems to be an invention to provide the *Arithmetica* of Diophantus with a privileged status. Diophantus has always been difficult to place within the history of algebra. The humanist project of reviving ancient Greek science and mathematics played a crucial role in the creation of an identity for the European intellectual tradition. Beginning with Regiomontanus’ 1464 lecture at Padua, humanist writers distanced themselves from “barbaric” influences and created the myth that all mathematics, including algebra, descended from the ancient Greeks. Later writers such as Ramus, Peletier, Viète and Clavius participated in a systematic program to set up sixteenth-century mathematics on Greek foundations. The late discovered *Arithmetica* of Diophantus was taken as an opportunity by Viète to restore algebra to a fictitious pure form. The special status of syncopated algebra should be understood within this context. A symbolic interpretation of Diophantus’s *Arithmetica* as a work of algebra by Bombelli, Stevin and Viète was made possible only through the developments before its rediscovery. Diophantus became important for algebra because symbolic algebra was already established by 1560.

Ironically, Renaissance humanists may be wrong about the Greek origin

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\[ \text{For a discussion on the creation of this new identity see Hoyrup}^{22} \text{ and Heeffer.}^{23} \]
of the *Arithmetica* after all. Diophantus lived in Alexandria and there is no evidence that he was Greek. Hankel posed the provocative thesis that he was Arab.\(^8\) Had the *Arithmetica* not been written in Greek, no one would have attributed it to the Greek tradition. Others conjectured he was a Hellenized Babylonian (Burton\(^{25}\)). Precisely because the *Arithmetica* does not connect well with the Greek tradition of arithmetic and logistic, it provides impetus to the thesis of a non-Greek origin.

As the tripartite distinction has obscured the true history of the development of symbolic algebra, we propose an alternative one.

### 2. An alternative distinction

The term ‘symbolic algebra’ was introduced by the Cambridge wrangler George Peacock in *A treatise on algebra* (1830).\(^{26}\) Peacock makes the distinction between arithmetical and symbolical algebra, devoting a volume to each. Both kinds of algebra use symbols, but in arithmetical algebra “we consider symbols as representing numbers, and the operations to which they are submitted as included in the same definitions” (p. iv). In arithmetical algebra he allows only operations that are closed within this algebra, thus avoiding negative and imaginary numbers. A quadratic equation is therefore no part of arithmetical algebra. Symbolical algebra is then considered to be a generalization of arithmetical algebra lifting the restrictions posed on operators. Though his book initiated work on the logical foundations of algebra, the restrictions set on arithmetical algebra are completely arbitrary and do not contribute to a historical assessment of symbolic algebra.

In 1881 Léon Rodet questioned the threefold distinction by Nesselsmann and proposed instead to draw the line between symbolic algebra and one dealing with abbreviations and numerical data (Rodet,\(^{27}\) pp. 69–70). Klein\(^{16}\) (p. 146) took this as a point of departure for his seminal work on the emergence of symbolic algebra. Still, the focus on the use of symbols as a prerequisite leads to a limited view of symbolic algebra. As we will argue, a symbolic approach to algebra is perfectly possible without symbols. Moreover, symbols are usually introduced in a later stage towards symbolic algebra. Essentially, there is only a distinction between symbolic and non-symbolic algebra, but to account for historical periods with symbolic

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\(^{8}\) “Wäre eine Conjectur erlaubt, ich würde sagen, er war kein Grieche; vielleicht stammte er von den Barbaren, welche später Europa bevölkerten; wären seine Schriften nicht in griechischer Sprache geschrieben, Niemand würde auf den Gedanken kommen, dass sie aus griechischer Cultur entsprossen wären” (Hankel,\(^{24}\) p. 157).
practice without the use of symbols we propose the following threefold distinc-
tion:

1. Non-symbolic algebra: this is an algorithmic type of algebra dealing
with numerical values only, or with a non-symbolic model. Typical exam-
pies are Greek geometrical algebra or the Chinese method for solving linear
problems with multiple unknowns (Fāng chéng).

2. Proto-symbolic algebra: algebra which uses words or abbreviations
for the unknown but is not symbolic in character. This would include Dio-
phantus, Arabic algebra, early Abbacus algebra and early German cossic
algebra.

3. Symbolic algebra: algebra using a symbolic model, which allows for
manipulations on the level of symbols only. Established around 1560 and
prepared by later abbacus and cossic algebra, Michael Stifel, Girolamo Car-
dano and the French algebraic tradition.

We now proceed to clarify the specificity of the symbolic mode of algebraic
practice.

2.1. The constructive function of symbolism

What is so specific about symbolic reasoning? What makes symbolism so
powerful that it has completely conquered mathematical and scientific dis-
course since the seventeenth century? Many philosophers, from Descartes
and Leibniz to Charles Sanders Peirce and Ernst Cassirer, have written ex-
tensively about the role of symbolism in mathematical problem solving. We
will only touch upon some points of this long tradition. Our focus will be
on the role of symbolism in the formation of new concepts in mathematics.

2.2. Symbols vs. notations

Part of the explanation for the emergence of symbolic algebra lies in the
differentiation of the functions of symbols and notations. Both have a rep-
resentative function but the role attributed to symbols surpasses its direct
representational function. Notations have grown out of shorthand writing
or abbreviations of words. As such, they directly represent the operations
and concepts behind the abbreviation. Symbols add an extra to the function
of notations, a distinction which has mostly been neglected in the history
Let us look at the function of some very essential symbols when they were first introduced.

2.3. **The first occurrence of the plus sign**

Troplke\(^5\) (II, pp. 14–8) describes how the addition operation was introduced. The + sign was first used in a printed arithmetic book by Johannes Widmann\(^29\) (f. I vi'; see Fig. 2).\(^b\)

![Fig. 2. The first use of the + and − sign in print from Widmann\(^29\)](image)

One could ask why the notation first appeared in Germany and not within the Italian abacus tradition. We believe there is a valid explanation

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\(^a\) Montucla, in his *Histoire des mathématiques* (Montucla,\(^28\) I, p. 587), sees no difference at all between words used for operations and symbols: “Notre algèbre ne diffère en aucune manière de ce qu’on vient de voir. Il y a seulement ceci de plus, que les modernes affectant de mettre tout en signes, en ont imaginé pour désigner l’addition, la soustraction des grandeurs et leurs égalité. Les premiers algébristes du siezème siècle les indiquèrent par les lettres initiales de plus, moins, égal. Nous le faisons aujourd’hui par les signes +, −, =”.

\(^b\) This page of Widmann’s book has lead to wild speculations about the origin of the signs (see Cajori,\(^13\) I, 232–4). The meaning here is only one of aggregation but not relevant for our further discussion.
for this. The early abacus tradition was fully vernacular. Later abacus treatises use

\[ \bar{p} \text{ and } \bar{m} \]

for the Italian words *plus* and *meno*. The German cossic tradition was originally Latin-based. Widmann found the sign in the manuscript collections he consulted for his arithmetic, particularly in the Dresden C80 (Wappler,\textsuperscript{30} p. 13).

This manuscript uses the + notation as a ligature in a section written c. 1486. The plus sign is shorthand for the Latin word *et*. The form of the crossed lines is evidently derived from the letter *t* in *et*. The use of + as ligature was not only as a mathematical operation but also in the meaning of the word ‘and’ (e.g. Wappler,\textsuperscript{30} p. 15). It first occurs within an algebraic context, as shown in Fig. 3.

\[ x^3 + 2x^2 \]

Fig. 3. The first appearance of the + sign in the Dresden codex C80, f. 350\textsuperscript{vv}, written around 1486 (from Tropfke,\textsuperscript{5} II, p. 17)

Because a ligature is essentially a shorthand notation, we would expect signs, which are based on ligatures, to be mathematical notations rather than symbols. This leads us to a key question: is the plus sign a notation or a symbol here? The answer will depend on the context. When printed in an early fifteenth century arithmetic book, the + sign in ‘3 + 5 makes 8’ would be interpreted as a shorthand for ‘and’, meaning the addition of five to three. Here, ‘plus’ describes an operation, a mental or even physical action. There is some temporal element present in the description ‘3 + 5 makes 8’. First you have three; after adding five, you find out that you have eight. The + sign in this context is thus a direct representation of the action of adding things together. Therefore, we would not consider it as a symbol in the case of an arithmetic book. Interestingly, Widmann,\textsuperscript{29} who uses the signs in an arithmetic book, does not so in the introductory chapters on the basic operations of addition and subtraction. Instead, he first employs the signs after 166 pages on mercantile problems. Which brings us to the context in which signs and symbols occur.
2.4. Contextual elements in the use of signs

The context in which the + sign first appeared, illustrated in Fig. 3, is very different from that of Widmann’s arithmetic. Two important elements of this first occurrence are also present in the first use of operations on equations by Cardano.\textsuperscript{31,c}

Firstly, the + sign is introduced within the context of algebra, and not in arithmetic. It is part of a binomial expression with two cubic terms, $x^3$ and $2x^2$. One interpretation is to just see it as the addition of these two terms, similar to the arithmetical example above. However, the context of this important manuscript, where operations on polynomials are introduced, asks for a more adequate interpretation.\textsuperscript{d} The plus sign here has the additional function of creating a structure on which can be operated. For the abacus masters and the early cossists binomials are more like primitive structures. In the late fourteenth century algebra treatises typically have a section dealing with the addition, subtraction and multiplication of binomials. These binomials can be algebraic, as in the example from the illustration, but often also irrational. For example, Maestro Gilio, in the Siena L.IX.28 (Franci,\textsuperscript{35} p. 7) has his algebra preceded by a “trattato delle radice” demonstrating how to add, subtract and multiply irrational binomials:

Se avessi a multipricare 7 e R di 8 per 7 meno R di 8 fa così: multiprica 7 via 7 fa 49, et della multipricagione della R di 8 in meno R di 8 si ne perviene meno 8, tralo di 49 resta 41, e della multipricagione di 7 in più R di 8 e di 7 meno R di 8 si nne perviene ragiunti insieme nulla, adomque multipricando 7 per R di 8 in 7 meno R di 8 si nne viene 41.

The rule corresponds with the general formula:

$$(a + \sqrt{b})(a - \sqrt{b}) = a^2 - b$$

In abacus treatises a binomial is considered a primitive element, a mathematical entity such as a number or a proportion. The plus sign is a constructive operator in the formation of an algebraic binomial. While

\textsuperscript{c} As argued in Heeffer,\textsuperscript{32} Cardano\textsuperscript{31} was the first to show an explicit operation on an equation and the first to subtract two equations (Cardano\textsuperscript{33}). Both instances are shown as marginal notations in the first printed editions.

\textsuperscript{d} Grammateus\textsuperscript{34} also first uses the signs in his chapter on algebra, see Fig. 5 and the discussion below.
the plus sign is derived from a ligature, its use for the representation of a binomial within an algebraic context points at a symbolic function.

A second contextual element in the introduction of algebraic symbolism is the function of clarification and illustration. Elsewhere we have shown how the first addition of two equations by Cardano appeared as an illustration in the *Ars Magna*. Also in the Dresden C80, the expression $x^3 + 2x^2$ functions as an illustration between the text lines. The use of + in this illustration emphasizes the aggregate function of the sign. The binomial is ‘constructed’ by placing the sign in between the two terms. The temporal aspect of adding numbers together is absent here. The structure of the binomial is depending on the + as a connector. These contextual elements bring us to an interpretation of the + sign as a symbol rather than as a shorthand notation.

2.5. *Paradoxes of symbolism?*

During the seventeenth century we find discussions concerning conceptual difficulties with some basic features of symbolic reasoning we now take for granted. Mersenne\(^37\) (p. 522) talks about “a strange paradox” when discussing the rules of signs:

> Or plusiers s’étonnent comment il est possible que – multiplié par –, c’est-à-dire moins par moins fasse +, et que P multiplié par M, ou M par P fasse M, ce qui semble estre contre toute sorte de raison. Sur quoi vous pouvez voir Clavius au 6 chap. de son Algebre; neantmoins i’en ay veu qui nient cette proposition, sur laquelle ie ne m’arresterai pas davantage.

While the discussion of such “paradoxes” may seem idle to Mersenne, they increasingly appear during the seventeenth century. Antoine Arnauld, who wrote an important philosophical work known as *The logic of Port-Royal* (Arnauld\(^38\)), also published a *Geometry* (Arnauld\(^39\)). In that book he includes an example of symbolic rules that he considers to be against our basic intuitions on magnitudes and proportions. His reasoning goes as follows. Suppose we have two numbers, a larger and a smaller one. The proportion of the larger to the smaller one should evidently be larger than the proportion of the smaller to the larger one. But if we use 1 as the larger number and $-1$ as the smaller one this would lead to

\(^3\) See Heeffer.\(^32\) The illustration, an essential contribution to symbolic algebra, is omitted in the English edition by Witmer.\(^36\)
which is against the rules of algebra. Witnessing the multiple instances in which this discussion turns up during the seventeenth century, the clash between symbolic reasoning and classic proportion theory, taught within the quadrivium, was experienced as problematic. Leibniz found it important enough to write an article about (Leibniz, 40 p. 167). He acknowledges the problem as a genuine one, but states that the division should be performed as a symbolic calculation, the same way as we do with imaginary numbers. Indeed, when blindly applying the rules of signs there is no problem at all. When dividing a positive number by a negative one, the result is negative, and dividing a negative number by a positive one, the result is also negative. Therefore

\[
\frac{-1}{-1} > \frac{1}{1}
\]

The discussion was not closed by Leibniz. Several eighteenth-century authors return to the question. E.g. Rolle\(^{41}\) (pp. 14–22), Newton\(^{42}\) (p. 3), Maclaurin\(^{43}\) (pp. 6–7) and d’Alembert.\(^{44}\)

### 2.6. Symbolic reasoning without symbols

Interestingly, the application of these rules posed no problems in the abacus tradition before 1500. In the Summa, Pacioli lists the rules of signs for the arithmetical operations of addition, subtraction, multiplication and division. Dividing a positive by a negative produces a negative. Dividing a negative by a positive leads to a negative (“A partire piu per meno neven meno. A partire meno per piu neven meno”) (Pacioli,\(^{45}\) f. 113r); see Fig. 4.

![Pacioli's rules of signs for division](image-url)

These rules were known implicitly and have been applied within the
abbacus tradition, for example in the multiplication of irrational binomials by Fibonacci (1202; see Boncompagni,\textsuperscript{46} p. 370; Sigler,\textsuperscript{47} p. 510):

\[(4 - \sqrt{2})(5 - \sqrt{8}) = 24 - 4\sqrt{8} - 5\sqrt{2}\]

However, an explicit treatment was impeded by the immature status of negative quantities. As far as we know, Pacioli was the first to list these formal rules for the basic operations of arithmetic.\textsuperscript{5} Importantly, Pacioli introduced these rules in distinction 8, as a preparation to his treatment of algebra. In contrast with the discussion of the basic operations of arithmetic, the rules of signs have a more formal and general character. Except for an illustrating example with numbers, the formulation of the rules does not refer to any sort of quantities, integers, irrational binomials or cossic numbers. The rules only refer to ‘the negative’ and ‘the positive’. Despite the absence of any symbolism, we consider this an early instance of symbolic reasoning. Except for the ligatures $\bar{p}$ and $\bar{m}$, no symbols are used for plus and minus. Still, its use in the “formalism” of these rules makes $\text{piu}$ and $\text{meno}$ qualify as symbols.

2.7. 	extit{Towards operational symbolism}

After Pacioli, the rules of signs appear more frequently in algebra textbooks. Exemplary is the anonymous Vienna codex 5277, written between 1500 and 1518 (Kaunzner\textsuperscript{48}). Here the rules of signs are introduced in relation to operations on polynomials and use the + and $\text{minus}$ sign introduced some decades before in the Dresden C80. For multiplication we find (f. 6r; Kaunzner,\textsuperscript{48} p. 132):

\[
\text{Si multiplicis} \begin{cases} 
+ \text{ per } + & \text{fit } + \\
- \text{ per } - & \\
+ \text{ per } - \text{ et contra } & -
\end{cases}
\]

Here, the rule appears to be symbolical, because we recognize our current symbols, but it is conceptually identical with that of Pacioli. Where we have previously denoted a constructive function to the plus sign in the Dresden C80, we can here discern an additional operative function. Not only can + and $\text{minus}$ be used to construct binomials, the signs now come

\textsuperscript{5}We checked about thirty transcriptions of abbacus manuscripts published by Gino Arrighi and the Center for the Study of Medieval Mathematics of Siena. Also Tropfke\textsuperscript{5} (II, pp. 124–8) lists no sources prior to Pacioli.\textsuperscript{45}
into relation with the terms of the polynomials in which they appear. The example added in the Vienna 5277 show how to multiply two binomials:

\[(6x + 8\phi)(5x - 7\phi)\]

The rule describes the following:

Cumque in unitate + \(\phi\) repetitur et in altera − \(\phi\), ducta \(x\) per + \(\phi\),
excitur + \(x\). Si augetur \(x\) per − \(\phi\) (ut praecedens edocuit regula),
edocitur − \(x\). Sed ex − per + vel + per − semper − perficiecitur,
sicut sequens docebit exemplum.

This is the first documented instance in which the minus sign, which was originally introduced for the construction of binomials, is used to denote a negative algebraic term! The text describes the multiplication of the positive term \(x\) with the negative − \(x\). Where previous uses of negative values were highly problematic, we now witness how the use of symbolism facilitates the acceptance of negative terms. The modern interpretation of subtraction as the addition of a negative term now becomes realized. This is exemplified where the author introduces the first rule of algebra (on linear equations) with the following cautela (f. 13v; Kaunzner, p. 139):

Si radix in latera continet + \(\phi\), tunc is numerus. Quo radix sub-
abundat, ex numero, cui radix aequatur, subtrahur. Si vero − \(\phi\) \(x\) continuaret, tunc addatur.

Where the original al-jabr operation from early Arabic algebra cannot be interpreted as the addition of a term to both sides of an equation to eliminate a negative term, such interpretation now becomes justified. This rule describes that to solve the linear equation, for example,

\[ax + b = c + dx\]

you proceed by adding \(cx\) to both parts and subtracting \(b\) from both parts. By means of a symbolism for representing negative terms, a basic operation on equations now becomes commonplace.

The story does not end here. The Vienna codex is innovating in yet another respect. The problem of a man making three business trips is one

---

8 Codex Vindobonensis 5277, f. 6v; Kaunzner, p. 132. We replaced the cossic sign of the unknown by \(x\). The sign \(\phi\) is used for units and has to be interpreted as \(x^0\). The habit of using \(\phi\) or \(\Phi\) in German algebra textbooks is abandoned by the end of the sixteenth century.

h See Oaks and Alkhateeb for such an interpretation of al-jabr.
of the examples illustrating the first rule. At each trip he doubles his income but spends 4 additional florenos. He ends up with nothing. The problem asks for the capital he started with. The author solves the problem by constructing an equation as follows. Take \( x \) for the original capital. After the first trip he has \( 2x - 4 \). After the second he arrives at \( 4x - 8 - 4 \), and after the third he end up with \( 8x - 16 - 8 - 4 \). Using the new symbol for negative terms, the manuscript reads:

\[
8 \ x - 16 \phi - 8 \phi - 4 \phi \ hoc \ totum \ est \ aequale \ 0 \ x^0. 
\]

Though lacking a symbolic expression for the equation, the author puts the constructed polynomial equal to zero. This is highly uncommon for the beginning of the sixteenth century. This Vienna codex is an example of a sudden leap in the evolution towards algebraic symbolism. While it only adopts the + and − signs from a previous manuscript, the new symbols advance several conceptual steps: a symbolic expression for the rules of signs, the elimination of negative terms in an equation and the equation of a polynomial to zero.

2.8. The spread of operative symbolism

We have evidence that the Vienna codex was consulted by both Heinrich Schreyber (Grammateus) and Christoff Rudolf. Grammateus\(^{34}\) (f. Gvi\(^r\)) uses the + and − signs and mentions the rules of signs while introducing operations on polynomials as shown in Fig. 5.

The \textit{cautela} for the first rule from the Vienna codex is reproduced literally in a German translation by Grammateus.\(^{k}\) That Rudolf in his \textit{Coss}

---

\(^{1}\) Better known as the monkey and coconut problem; see Heeffer.\(^{49}\)

\(^{j}\) Vienna 5277, f. 14\(^r\); Kaunzner,\(^{48}\) p. 139: “Est quidam mercator, qui emit aliquot talenta piperis, et iterum vendit, et lucratur tantum, quantum summa valebat capitalis, et exponit 4 florenos. Cum residuo consimiliter tantum consequitur lucri, quantum restabat, et 4fl expendit. Itidem tertio modo facit, et 4fl exponit, et demum nihil, vel lucri, vel summae capitalis, remanit. Quaeritur iam de summa pecunia originali. Sit 1 \( x \), et lucratur 1 \( x \), ergo erunt 2 \( x \). Ex his aufer 4 \( \phi \) vel fl, restat 2 \( x - 4 \phi \). Deinceps, cum eo lucratur totidem, quantum restabat, fiunt per consequens 4 \( \phi - 8 \phi \). Ex quibus demendi sunt 4 fl, Stabit residuum 4 \( x - 8 \phi - 4 \phi \). Tertio iterum tantum hicratur, quantum restabat, fiunt itaque 4 \( x - 16 \phi - 8 \phi \). Ex his ultimo auferantur 4fl, et relinquentur 8 \( x - 16 \phi - 8 \phi - 4 \phi \) hoc totum est aequale 0 \( \phi \). Secundum cautelam addendi sunt 16 \( \phi \) 8 \( \phi \) 4 \( \phi \) ad 0 \( \phi \). Summa, scilicet 28 \( \phi \), dividatur per 8 \( x \), Quia aequivalent. Et quotiens, scilicet \( 31/2 \), dicit florenorum in primo habitorum numerum”.

\(^{k}\) Grammateus\(^{34}\) (f. Jiiii\(^r\)): “Wann do stet in ainer position der zwayer die sich mit ainander vorgleichen das zaich + so subtrahir sein zal von sienem gleichen in der andern position wird aber funden – so addire die selbig zal zu der in der andern position”.
collected most of his material from Vienna manuscripts was known and discussed already in the sixteenth century. With Pacioli, Grammateus and Rudolf, the symbolic approach to operate on positive and negative terms was spread all over Europe. This heralded the use of operative symbolism in algebra.

2.9. **How new concepts are created by operative symbolism**

Let us return to the apparent paradox of Arnauld. Leibniz argued that symbolic reasoning resolves the paradox. We have shown that such kind of reasoning was common practice in the abacus tradition of the late fifteenth century. Pacioli would respond to the discussion that

\[
\frac{1}{-1} \text{ equals } -\frac{1}{1} \text{ and } \frac{-1}{-1} \text{ also equals } -\frac{1}{1}, \text{ therefore } -\frac{1}{1} = \frac{-1}{1}.
\]

Although we do not find the symbols for division, negative numbers and equations in Pacioli or his predecessors, the common application of these operations provides evidence for a symbolic mode of reasoning. We

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\(^1\) See the introduction by Stifel in the 1553 edition.
will now discuss how symbolism has had a decisive role in the formation of three important new mathematical concepts of the sixteenth century. The first one relates to the discussion on negative quantities.

2.10. Negative numbers

The Vienna 5277 manuscript was the first to apply the $-$ sign to an algebraic term. We have pointed out the symbolic function of the sign. While the minus sign has been abstracted from the subtraction operation, it now incorporates the extra function of negation. By placing a $-$ sign before an algebraic term, the term becomes negated. Two centuries later, d’Alembert will define ‘negative’ in the Encyclopédie as “the affection of term by the sign $-$” (Diderot and d’Alembert,51 XXII, p. 289). d’Alembert rebukes “those who pretend that the ratio between 1 and $-1$ is different from the ratio $-1$ and 1”. They are wrong in two respects, he claims. Firstly, because in algebra the division by negative numbers is common practice, and secondly because the value of the product of 1 and 1 is the same as the value of the product of $-1$ and $-1$.

His characterization ‘affecting’ is interesting. He makes the distinction between negation used for an isolated quantity (or term) and its use in the sense of $a - b$. A negative value must be understood in the first meaning, not in the second. He opposes the view of a negative quantity as a quantity less than zero, “as most mathematicians do”.

We believe this is indeed the meaning attributed to a negative quantity by the early cossists. The first appearance of the negative symbol has the intention of affecting an algebraic term. The Vienna 5277 manuscript uses the minus sign to create negative quantities. Studies as by Sesiano52 and Gericke,53 discussing several instances of so-called negative values from Fibonacci to the sixteenth century, can be criticized for their all too casual interpretation of negatives. The concept of a negative quantity as a value smaller than zero was an unacceptable and even ridiculous idea before the seventeenth century. However, the symbolic affection of an algebraic term did lead to the concept of a negative number. Negative numbers have become possible with the introduction of the minus sign. Where the $-$ sign originally had the function of a constructive operator for binomials, in the early sixteenth century it became an operative symbol for the negation of algebraic terms. Once negative quantities had become established by this symbolic construction, the elimination of negative terms from an equation by addition became a common operation.
2.11. Defining imaginary numbers by operational symbolism

Bombelli was the first to define imaginary numbers by the eight combinatorial operations that are possible with the products of the negative and positive roots of plus and minus one. Note the correspondence with Pacioli’s rules of sign when Bombelli lists the following operations in Fig. 6 (Bombelli,\textsuperscript{54} f. 169\textsuperscript{r}). These operations defined imaginary numbers within the symbolic model. The interpretation of their arithmetical equivalence still remained a mystery. It took two more centuries to arrive at a geometrical interpretation of complex numbers. This story is well covered by Barry Mazur,\textsuperscript{55} paying surprisingly much attention to the conceptual evolutions which have lead to imaginary numbers. Mazur makes an interesting observation with regard to possible forms of notation. In discussing Dal Ferro’s formula for one case of the cubic equation he remarks (Mazur,\textsuperscript{55} pp. 124–5):

In discussing the “easy” case in which the indicator $d = c^2 - \frac{b^3}{27}$ is positive, I said that the manner in which Dal Ferro’s expression is written tells us how to compute it (extract, as indicated, the roots and make the arithmetic operations requested by the formula). The expression doesn’t provide a specific method for the extraction of those roots, but once we have such a method, the expression is itself interpretable as a possible algorithm for the production of a real number. It is often the case that our expressions for specific numbers suggest algorithms, or partial algorithms, for their computation. To take a random example, the number $2^{21} - 1$ happens to equal $7 \times (300,000 - 407)$, and this number written in decimal notation is 2097151. Each way of writing this number hints at a specific strategy for its calculation (e.g., if you express the number as $2^{21} - 1$, the form of this expression bids you do what it tells you to do to calculate the number: raise 2 to the twenty-first power and then subtract 1 from the result).

In different terms, Mazur here describes the same mechanism we have proposed to explain the function of symbolism. The “symbolic expression is itself interpretable as a possible algorithm for the production” of instances of the concept it represents. By using different symbolic expressions for a same number, we represent different algorithms or strategies for its computation. In other words, the possible combinatory operations on the object become embedded with the representation. The symbolism performs the task of representing these possible operations. We will now look at what
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2.12. The equality symbol as the crown jewel of symbolic algebra

The equality sign evidently refers to the arithmetical equivalence of two expressions left and right from the sign. For example, the expression $3 + 5 = 8$ denotes the arithmetical equivalence of the sum of three and five with eight, as well as of eight with its partitioning into the numbers three and five. However, if we look at the historical moment at which the equality sign was introduced, we arrive at a very different picture. The equality sign as we now use it, was introduced in a book on algebra by Robert Recorde (see Fig. 7). He chose the sign of two parallel lines ‘because no two things can be more equal’. This often quoted citation ignores the more important motivation for introducing the sign. Firstly, the equation sign was not introduced, either in his lengthy introduction, discussing the basic operations of arithmetic and extraction of roots, or in the dialogue on operations on polynomials or the rule of proportion. Instead, he introduced the sign in the chapter on the resolution of algebraic equations “For easie alteration of equations . . . And to avoid the tediouse repetition of these woordes: is equalle to: I will sette as I doe often in woorke use, a paire of parralle . . . lines of one lengthe, thus : ==, bicause noe 2, thynges, can be moare equalle” (Recorde, 56 fol. FFiv).

The use of the sign is thus specifically motivated by the alteration, or manipulation of equations. From this quote we can read the specific representational function that makes the equality sign the prime symbol of
the concept of an equation. In addition to its direct reference to arithmetical equivalence, the equality symbol represents the combinatorial operations which are possible on an equation. These operations include adding or subtracting homogeneous terms to both sides of the equation, dividing or multiplying an equation by a constant or unknown (introduced by Cardano) and adding or subtracting two equations (introduced by Cardano and perfected by Peletier and Buteo). The equality sign symbolizes the algebraic equation. We have argued elsewhere that the concept of an equation fully emerged around 1560 (Heeffer). We also stated that symbols are introduced as a result of symbolic thinking. The introduction of the equality symbol provides historical evidence for the introduction of a symbol representing a newly emerged mathematical concept.

The introduction of the equation symbol completes the basic stage of development towards symbolic algebra, as initiated in Germany by the end of the fifteenth century. The time of the introduction, 1557, coincides perfectly with our conceptual analyses of algebra textbooks of the sixteenth century.

As the minus sign facilitated the acceptance of negative numbers, so did the equation sign contribute to the further development of algebra towards the study of the structure of equations. That the equation sign, as introduced by Recorde, was not universally accepted for another century, is irrelevant for our argumentation. Other signs or even words functioned as the equation symbol in the same way as the two parallel lines had done.
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Thomas Harriot, in his manuscripts, placed two short strokes between the parallel lines resembling ‘II’ and introduced the < and > signs as they are used today (Stedall, 59 p. 8). This was later abandoned in the printed edition, and through its further use by Oughtred’s Clavis mathematicae, the equation sign became generally accepted in England.

3. Conclusion

The history of the emergence of symbolism in mathematics, and particularly in algebra, has been obscured by serious methodological mistakes. Historians of mathematics, such as Cajori, start from modern mathematical concepts and operations and look for corresponding ones in historical sources. With respect to algebraic symbolism, Cajori only had eye for the first appearance of modern symbols. In this paper we argued that the appearance of symbols in the history of algebra is the result of a process of mathematical practice in which symbolic reasoning matured and developed. The use of symbols is a result of the development of new concepts and methods of symbolic reasoning, not the start of it. We have illustrated this with the equality symbol, which was introduced at a time when the concept of a symbolic equation was already established. The algebraic practice of abacus masters before the sixteenth century has been crucial in paving the way for the emergence of symbolic algebra. All sixteenth-century symbols, even the arithmetical ones for addition and subtraction, have been introduced within an algebraic context. Symbols are more than handy shorthand notations. We have shown that operative symbolism leads to new methods and concepts. The first acceptance of isolated negative numbers should be understood within the context of symbolic reasoning.

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41. M. Rolle, Traité d’algebre; ou, Principes generaux pour resoudre les questions de mathematique (Chez Etienne Michallet, Paris, 1690).

43. C. MacLaurin, *A treatise of algebra in three parts: containing, I. The fundamental rules and operations, II. The composition and resolution of equations of all degrees, and the different affections of their roots, III. The application of algebra and geometry to each other: to which is added an appendix concerning the general properties of geometrical lines* (A. Millar and J. Nourse, London, 1748).


56. R. Recorde, *The whetstone of witte, whiche is the seconde parte of Arithmetike: containing the extraction of roots: the cossike practise, with the rule of equation: and the woorkes of surde numbers. Though many stones doe beare greate price, the whetstone is for exersice ... and to your self be not unkinde,* (By Ihon Kyngston, London, 1557).

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1. Diophantos

The only thing we can be reasonably sure of in the case of Diophantos (Διόφαντος Ἀλεξανδρεύς) is that Alexandria was his place of origin or his permanent domicile. Only two books are ascribed to Diophantos: the Arithmetika, or Arithmetic and a treatise on polygonal numbers, while three others are conjectured to have existed, Porismata, Moriastika, and Arithmetika Stoicheisis. Contrary to most ancient texts no reference to other mathematicians is made in the Arithmetika. This immediately rules out an a posteriori dating of the treatise. An a priori dating is also very difficult, since Diophantos is only referred to by Theon of Alexandria (Heath, p. 2; Tannery, II, p. 35–6). Theon lived in the fourth century, so we can safely put 400 AD as an upper limit in dating the Arithmetika (Tannery, II, p. 35, I.8). Where it is very obvious that the Arithmetic can be ascribed to Diophantos, this is not the case for the book on polygonal numbers. If we accept this ascription, then we can use the reference to Hypsikles, who lived around 150 BC.

In any case this means that we cannot date the life of Diophantos but

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a On Diophantos see Heath, Ver Eecke, Swift and Schappacher.
b Mentioned in problems V.3, V.5, V.16. In this and the following we refer to problems in Diophantos’ Arithmetika with a Roman numeral, followed by a figure. The Roman numeral refers to the number of the book in the series of Greek books, while the figure is refers to the number of the problem within the book.
c Moriastika is mentioned in a scholium to Nicomachus’ Arithmetica.
within an interval of well over five hundred years, which nearly coincides with the Roman rule over Egypt. It has already been noticed by Netz\(^7\) (pp. 215–6) that in Classical Greece mathematicians who are cited by other mathematicians often differ in age no more than one generation. Knowing this we could, on the basis of Theon’s reference, tentatively put Diophantos’ life about 300 AD.

One of the earliest references to Diophantos, albeit without real biographical importance, is in the *Suidas Lexicon*. The *Suida* is a tenth-century alphabetically organized encyclopedia, with about 30000 articles. The articles are based on earlier sources, but they are not always very trustworthy. Somehow however Diophantos’ reputation must have lived on for hundreds of years, or it became a byword for a logistics teacher. Under the lemma *A drachma raining hail* (δ 1491), we find

> In the case of Diophantos a drachma (dragme) became the subject of speculation. When the hail stopped at that minute (falling) from the upper air, they joked it was a handful (dragme) of hailstones.\(^3\)

The word drachma has three meanings: a handful, an old Athenian coin or a weight (Heron, *Geometrika*, p. 411). Referring to the sometimes unrealistic nature of problems of logistic, it seems to be a play on the meaning of the words. “How much hail has fallen?”, “Why, a drachma, a handful, which weighs a drachma and is worth a drachma…”

One of the oldest references to Diophantos as a person is by the Byzantine intellectual Michael Psellos (1018–1081?). Psellos wrote a large number of treatises on very diverse subjects such as philosophy, theology and the sciences. In a letter Psellos refers to the treatise *The Egyptian method for numbers*, written by Anatolios, and dedicated to Diophantos (Tannery,\(^6\) II, pp. 37–42). Tannery identifies this Anatolios with Anatolius of Alexandria, who was bishop of Laodicea (on the Syrian coast) about 270-280. This bishop has indeed written mathematical treatises of which fragments have been preserved. He was a student of Dyonisius of Alexandria, the later Saint Dyonisius.\(^9\) If one concludes, like Tannery, that a treatise can only be ded-
icated to a person if this person is still alive, then Diophantos would have lived in the third century. Which fits in with the above remark on references to him. Moreover this Dionysius may well be Diophantos’ dedicatee.

Klein and Knorr propose another interpretation of the Psellosfragment. Klein argues that Psellos is referring to the differences in symbolism between Anatolios and Diophantos. In this way Anatolios’ reference becomes an a posteriori dating: Diophantos lived before him. He also argues that Anatolios’ treatise is dedicated to another Diophantos. If Dionysius can be identified with Dionysius of Alexandria then, Knorr concludes, Diophantos should be dated a generation earlier, i.e. around 240 AD. He even finds it possible that Diophantos lived in the first century AD (Knorr, p. 156–7).

An argument to contradict these propositions is that Diophantos is cited neither by Nicomachus (ca. 100 AD), Theon of Smyrna (ca. 130 AD) or Iamblichus (end third century AD). The use of the word leipsis also indicates a later dating of Diophantos. This word was used quite late in classical Greek and its first known use is in the second century AD. The word hyparkxis, which is used by Diophantos in a mathematical context, is a term which was often used by the neoplatonic school of Alexandria (ca. 200 AD) in philosophical treatises.

Since 1500, more than a thousand years after his death, different authors have speculated on the life of Diophantos, naming him as an Arab, jew, converted Greek of hellenized Babylonian. None of these characterizations withstands criticism. What we know of Diophantos is very little and we will have to live with it that we in fact know practically nothing of one of Easter date and the Institutes of Arithmetic, in ten books. Part of this book has been inserted into Heron’s Definitions (138), presumably by a Byzantine scribe.

1 The keyword in the Psellosfragment is heterós, which according to Knorr (p. 184) should be read as heteroi. Klein translates this sentence as: Diophantos treated this very precisely, but the very learned Anatolios collected the most essential parts of this theory, as described by Diophantos, in another way. Kleinh (pp. 244–6) translates: . . . but the very learned Anatolios collected the most essential parts of this theory, in another way than Diophantos. He devotes a whole paragraph to the possible translations. Knorr on the other hand thinks the fragment should be read as: but the very learned Anatolios, who had collected the most essential parts of this theory, dedicated it to that other Diophantos.

2 There may be some confusion here with Diophantus the Arab, Libanius’ teacher, who lived during the reign of Julian the Apostate. See also Suda λ486 and Lieu in MacLeod, pp. 129–30.

3 E.g. by Spengler, Tannery and Burton. See Schappacher for a discussion of the claims by these authors.
the most original mathematicians of Antiquity.

It has long been recognized that the Greek manuscripts of the Arithmetic, which have come down to us, belong to two distinct classes: the Planudean one and the non-Planudean. The Planudean class of manuscripts contains scholia which were inserted by the thirteenth century monk Maximos Planudes. Twenty-seven manuscripts and four important excerpts have been studied. None of these manuscripts withstand the test of mathematical rigour, a fact already noticed by Planudes. The manuscripts are corrupted throughout, which is remarkable and makes this text arguably one of the most corrupted of known Greek mathematical texts.

Much of the Greek (and other) mathematical corpus has been lost and is only known through references in texts that withstood the tooth of time. A number of smaller works, such as Aristarchos’ About the size and the distance of the Moon and the Sun were preserved because they were part of courses of a some late-Roman or Byzantine curriculum. In the intellectual centres at the borders of the Byzantine Empire, such as Edessa, Harran and Ras el-Ain, all close to the Persian Empire, translators had been active from the fifth century onwards. Many of the Greek texts were translated into Syriac. The influence of these centres on later Arabo-Persian intellectual life, especially in Baghdad, has hardly been studied. Yet these Syriac translations are a crucial link in the transmission of Greek knowledge to the Arabic civilization. In fact most of the translators from Greek or Syriac into Arab were christians (Benoit and Micheau, pp. 192–202).

In view of the enormous amount of non-described and non-inventoriated Arabic manuscript it cannot come as a surprise that every once and a while an hitherto unknown manuscript is identified. That was the case in 1971 when an Arabic version of Diophantos was found. It came as a shock when it was realized that in this book some of the unknown of the thirteen books could be found. In these books it becomes clear that Diophantos uses the methods which he has described in the other books to their limits in solving higher-degree problems. The study of these Arabic books and their analysis is the work of two Arabists: Roshdi Rashed and Jacques Sesiano. The

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1 The last notable study was that by Allard in the 1970s. Unfortunately, this study has never been published, apart from a very limited edition which were photocopies of his doctoral thesis. Its publication was announced by Les Belles Lettres but it never materialized. Some of the manuscripts have been described in scattered articles and monographs (Allard).

2 See Meskiens, Van der Auwera and Tytgat. According to Pappos this treatise was part of the introduction to Ptolomy’s Syntaxis.
manuscript has 80 folios measuring 168 x 129 mm. This Arab translation consists of books IV to VII, but the Arab books with numbers IV, V, VI are not the same as the Byzantine books with the same number. It appears that the order of the books of the *Arithmetika* is: first the Greek books I, II, III, then the Arab books IV, V, VI, VII followed by the Greek books IV, V, VI. What the exact order of the last three Greek books is among the books VIII-XIII is not certain.

2. The structure of a Diophantine problem

Just as Euclidean theorems have a specific structure, so do the Diophantine problems. In fact in many places both are quite comparable. Euclidean problems (especially those dealing with quadratic equations), even if they are interpreted as the geometrical translation of an essentially algebraic problem are always put in terms of lines, squares and rectangles (Fowler\(^\text{18}\)).

Generalisations usually deal with parallelograms. The theorems are put in a general fashion and are, on the basis of postulates and preceding theorems, proved rigorously. The result, a line, can be incommensurable with the original lines, in other words irrational solutions are permitted.

The structure of a mathematical problem is described by Proklos.\(^a\) A problem begins with *protasis*, which in the ideal case states a condition and a result which follows from the condition. The *protasis* is always general, which distinguishes it from the other parts.\(^b\) In the *ekthesis* a particular condition is set, the *diorismos* states a (geometrical) relation, which is the desired result. What is still lacking is a proof, but to make this possible the objects of the proof must be constructed, which is done in the *kataskeue*. With these objects one can then prove the *protasis*, this is the *apodeixis*, which ends when the result stated in the *diorismos* is obtained. Now the conclusion, *sumperasma*, is drawn, mostly this is a repetition of the *protasis* with the word ‘therefore’ added.

The structure of a Diophantine problem follows this general rule set out by Proklos. Diophantos puts his problems in a general way and indeterminate numbers (*protase*). Solutions are given for specific numbers (*ekthese*), which are given at the outset of the solution. We can see a partial analogy with geometrical construction, in this way that they will be applied to a

\(^{a}\) See Netz\(^{19}\) referring to Proklos’ *Primum Euclidis Elementorum Librum Commentaria* Prologus 203.1–207.25. He also gives etymological explanations for the Greek words.

\(^{b}\) And this is why some constructions are put generally, but are not necessarily proven generally. On Greek methods of proof see Hintikka and Remes.\(^{20}\)
specific figure, even though they are posed generally.

Diophantos never gives, even if the specific example allows it, a general method. During the elaboration of the example he sometimes adds a restriction. In some other cases, where there is a need for a restriction, he does not impose them. Whether this is ignorance on the need of such a restriction or the impossibility to correctly formulate the restriction, e.g. in terms of characterization of the numbers, is not clear. On the other hand Diophantos has no problems of adding numbers which are different in kind, e.g. “Find a perpendicular triangle of which the area, added to one of its perpendiculars, is a square and the perimeter is a cube” (VI.19). Unlike Euclid however, Diophantos does not allow irrational numbers as solutions.

The solution of a problem begins with putting the unknown equal to certain expressions, which guarantee rational solutions (construction) after which the problem is solved (demonstration). The choice of the unknown is deliberate, he chooses it in such a way that the expressions can be expressed in one unknown. With this unknown and the specified numbers, expressions satisfying some of the conditions of the problem statement are formed (hypostataseis). The conditions of the problem are compared to the expressions and these result in an equation. The expression has to be equalled to a square or a cube, and a “suitable” expression for this square or cube is chosen. When this new expression is inserted into the equation a more or less “easy” equation is found. This equation can be solved using Diophantos’ general rules, which he proposed in the introduction. This leads to a particular solution for the problem at hand. Usually there is not a formal conclusion, but just a simple remark that the problem is solved.

The structure, as it is described above, is nearly classical for many Greek mathematical treatises. The Arithmetika is therefore a book which fits the Greek tradition seamlessly, although the contents is remarkable, original and isolated within that tradition. We can take the famous problem II.8 as an example.
II.8 To divide a square into two squares.

I propose to divide 16 into two squares.

I put it that the first number is $\delta^\upsilon$, then the other is $16\mu - \delta^\upsilon$.

So it is necessary that $16\mu - \delta^\upsilon$ is a square.

Take the square of a random multiple of $\varsigma$ of which the square root of 16 is subtracted.

Take for instance $2\varsigma - 4\mu$, the square of which equals $4\delta^\upsilon + 16\mu - 16\varsigma$.

We put this equal to $16\mu - \delta^\upsilon$.

If we add the lacking numbers on both sides and if we subtract equals from equals, we find that $5\delta^\upsilon$ equals $16\varsigma$ and $\varsigma = \frac{16}{5}$.

From which it follows that one of the numbers is equal to $\frac{256}{25}$ and the other to $\frac{144}{25}$.

So the sum of the numbers is $\frac{400}{25}$.

3. Symbolism in the Arithmetic

As is the case with most Greek treatises we only know the Arithmetic through Byzantine and mediaeval copies. This means that the text has gone through a process of minisculization. We can therefore only guess if and which Diophantine symbols were capital letters in the original documents. What we do know is that Diophantos’ sign for minus, more or less resembling ⌢, was also used by Heron, which indicates a wider use (Ca-
All comments on style and notation therefore are in first instance comments on the Byzantine versions. This filtering process, put together with the uniqueness of the *Arithmetic* makes it virtually impossible to look further back in time than the Byzantine era. Only when we can compare Diophantos with original Graeco-Roman papyri is it possible to say something more definite. The most important characteristic of the *Arithmetic* is the system which is being used to write what we know as polynomials, in which abbreviations of the unknown are used.\(^b\)

<table>
<thead>
<tr>
<th>Unity</th>
<th>Number</th>
<th>Square</th>
<th>Cube</th>
<th>Square</th>
<th>Cube</th>
<th>Cube</th>
</tr>
</thead>
<tbody>
<tr>
<td>(x^0)</td>
<td>(x^1)</td>
<td>(x^2)</td>
<td>(x^3)</td>
<td>(x^4)</td>
<td>(x^5)</td>
<td>(x^6)</td>
</tr>
</tbody>
</table>

The following convention is used to write a polynomial:

1. The coefficients are written in Ionian fashion after the unknown or its power.
2. All terms which are to be subtracted are written after \(\ddagger\).
3. The terms which have to be added are written, without summation sign, before the \(\ddagger\).

E.g.,

\[
\begin{align*}
\delta^v \delta \mu \pi \pi \pi &= 4x^2 + 25 \\
\delta^v \delta \pi \mu \pi \nu \ddagger \delta^v \pi &= x^4 - 50x + 800 \\
\kappa^v \beta \pi \nu \nu \nu \ddagger \beta \mu \pi &= 2x^3 - 5x^2 + 8x - 1
\end{align*}
\]

Signs similar to \(\ddagger\) seem to have been in general use for a subtraction. In *Papyrus graecus Vindobonensis 19996* the sigel \(\odot\) appears (Gerstinger and Vogel,\(^{32}\) p. 14, p. 22).

Diophantos used \(\mu\) to refer to numbers. It is regarded as nothing more than a symbol. The symbol is necessary to indicate that one is dealing with the independent term (Tannery,\(^{33}\) III, p. 160; Heath,\(^1\) p.39). If \(\mu\) were omitted confusion would be possible, especially with sloppy handwriting. Is \(\xi \nu \gamma\) (underlined omitted) equal to 23\(x\) or 20\(x\) + 3? Writing \(\xi \mu \nu \gamma\) omits

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\(^b\) In the Tannery transcription capital letters are used. Here we follow Allard’s transcription,\(^{24}\) which uses small letters. Different translators always have had the difficulty of ‘translating’ this notation. Heath\(^1\) and Tannery\(^6\) do not translate the text, but rather paraphrase it in modern mathematical terms including modern mathematical notation. Ver Eecke\(^3\) resolves the abbreviations and writes *l’arithme* for \(\xi\), Meskens and Van der Auwera\(^31\) translate the text, but use an \(x\) for the arithmos and modern exponentiation in their translation. Allard,\(^{24}\) in his hard to find translation, is perhaps most faithful to the text using renaissance-like abbreviations such as Q (quadrat) and C (cube).
Ad Meskens

this problem. This interpretation is not followed by Klein\textsuperscript{10} (p. 131), who sees \(\overline{\mu}\) as an abbreviation of \textit{monas} (unity), necessary in the notation as a consequence of the meaning of the word \textit{arithmos}, this is a certain number of something. Klein refers to a similar, non-abbreviated word use by Heron. The \textit{arithmoi} calculated by Diophantos are according to Klein numbers of pure units or “all numbers are composed of a certain number of units” (Diophantos I, introduction). In this interpretation, according to Klein, Diophantos can view a monas as divisible in parts.

Diophantos’ \textit{arithmos} can therefore be regarded as a positive rational number. Irrational numbers — although they are present — and negative numbers are not considered to be \textit{arithmos}. On the basis of symbolism in the Renaissance, we are inclined to follow Klein’s interpretation. In Renaissance texts, which do not have the supposed problem of sloppy notation, a letter is added to the independent term, although this seems completely useless. \(\dot{\alpha}\varphi\theta\mu\omega\zeta\), the number, is used by Diophantos to indicate the unknown. It is usually written as an abbreviation, \(\varsigma\), or a letter sign closely resembling it. “But the number that has none of these characteristics, but consists of an undetermined number of units, we call the \textit{arithmos} and her sign is \(\varsigma^n\)” (Diophantos I, introduction). The earliest known use of the symbol \(\varsigma\) is in papyrus Michigan 620, dating back to the first century or the beginning of the second (Robbins and Robbins,\textsuperscript{34} Vogel\textsuperscript{35}). If there has to be a declension of the word \(\dot{\alpha}\varphi\theta\mu\omega\zeta\) this is indicated in an exponent. For instance \(\varsigma^n\) means \(\dot{\alpha}\varphi\theta\mu\omega\zeta\). The symbol is duplicated when there is a plural \(\varsigma\varsigma\), \(\varsigma\sigma\varsigma\), \(\varsigma\varsigma\), \(\varsigma\varsigma\varsigma\). The symbol is immediately followed by the number, thus \(\varsigma\varsigma\lambda\beta\) means \(32\times\). On the basis of all these considerations Heath\textsuperscript{1} (pp. 32–7) concludes that the sign cannot be interpreted as an algebraic symbol.

To denote powers of the unknowns two symbols are used by Diophantos: \(\delta^v\) and \(\kappa^v\), resp. the \textit{dynamis} and the cube of the unknown. These symbols are not equivalent to the exponent, but to the power of the exponent itself. Thus \(\delta^v\) does not stand for the \(2\) in \(x^2\), but for the expression \(x^2\). The word \textit{dynamis}, \(\delta\sigma\mu\alpha\zeta\), has been interpreted in different ways by translators. In colloquial Greek it has the meaning \textit{power}. When translating a mathematical text this word is frequently used in the translation. For instance in Bailly’s dictionary we find: \textit{t. d’arithm puissance d’un nombre, particul. le carré} (Séchan and Chantraine,\textsuperscript{36} p. 542). However in mathematical usage it is always used as \(a\ \text{square}\). The verb \(\delta\sigma\alpha\sigma\tau\dot{\alpha}\alpha\), \textit{dynastai}, was originally used for transformations of surfaces in the plane. Thus a rectangle was trans
formed into a square of the same area (tetragonismos). The dynamis is thus obtained by finding or constructing the mean proportional of length and width of the rectangle. Because the transformation of a rectangle may result in a side of a square which is not measurable in length it seems to have been desirable to measure these sides by their squares instead of their lengths (Szabo, p. 103). The $\delta^v, \delta^v\delta^v$, has a special place in Diophantine word use, in the sense that, contrary to the other powers, it always refers to the square of the unknown. The square of a number is usually referred to as tetragon, τετράγωνον.

Higher powers can be referred to by juxtaposing these symbols. The fourth power is $\delta^v\delta, \delta^v\delta^v\delta^v$. It was already used in this sense by Heron (Sakalis, p. 43) and Hippolytus. Although in the Greek manuscript no powers higher than six are present, it becomes clear from the Arabic books that the symbolism is retained. In the Arab version expressions like ‘square square square square’, representing $x^8$ can be found, which possibly was written in Greek as $\delta^v\delta^v\delta^v\delta^v$. We notice that this type of notation requires two symbols $\delta^v$ and $\kappa^v$ to denote the powers. It is obvious that any number $n$ can be written as $2x + 3y$. Thus $\delta n^v$ represents $x^5$ and $\kappa^v\kappa$ represents $x^6$. Notice the fact that the notation is additional, like the exponents in our notation.

Although the origin of the terminology is not clear, it seems to have been widely used. The same terminology can for instance be found in a surveyor’s text attributed to Varro (Bubnov, pp. 495–503). Marcus Terentio Varro (116-27 B.C.) was the editor of an encyclopedia De Disciplinis, in 9 books, of which book 4 treated geometry. Although the text is written in Latin, the powers are referred to in transliterated Greek: dynamus (problem 19), kybus (problem 14, 22), dynamodynamus (problem 20), dynamokybus (problem 20) and kybokybus (problem 22). We may reasonably assume then that these terms were in widespread mathematical use by the third century.

More often than not in Greek mathematics fractions are written as a sum of unit fractions. The use of unit fractions in this way is not present with Diophantos (unless it was omitted by copiists). Nevertheless he sometimes uses unit fractions, including those with an unknown in the denominator,

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See Szabo (pp. 36–55). For an opposing view see Caveing (pp. 1342–62). The discussion has not stopped, other authors, David Fowler and Jens Hoyrup to name but a few, have since contributed their views.

d See Hippolytus (1.2.10). Hippolytus lived around 200 AD. He is regarded as the first antipope. He died in 235, reconciled with the Church.

e If $n = 2m$ then $x = m, y = 0$, if $n = 2m + 1 = 2(m - 1) + 3$, then $m = m - 1, y = 1$. 
for which he says: “and each of these fractions has the same symbol with
above it the sign $\chi$ to clarify the meaning” (Diophantos, introduction).\textsuperscript{f}

The symbol $\gamma \chi$ or $\gamma'$ represents $\frac{1}{3}$.\textsuperscript{g}

The use of unit fractions is not general, sometimes they are written in
words, such as $\pi\epsilon\nu\pi\chi\alpha$, one fourth (e.g. in I.22)\textsuperscript{h} or as a fraction written as
a number and a unit fraction, e.g. $\gamma\gamma' \iota \gamma'$ or $3 \frac{1}{2} x + 3 \frac{1}{2}$. For $\frac{1}{4}$ Diophantos
has, according to Greek tradition, a separate symbol $\angle'$. For the inverses
of the unknown and its square we find $\varsigma' \gamma$ (e.g. in III.10 and III.11) and $\delta \upsilon'$
(e.g. in VI.3).

We find the notation with the denominator above the nominator, like $\frac{\gamma}{\epsilon}$
or $\frac{3}{5}$, $\mu \iota \gamma \tau \varsigma \alpha$ or $\frac{13}{3} - x$ (IV.32). The horizontal bar is not to be regarded
as a vinculum, but as a line above the letters to indicate that the expression
is a number. A few times one finds a unit fraction in this denominator
above nominator notation, e.g. $\frac{1}{512}$ for $\frac{1}{\delta \upsilon \alpha}$ (IV.28).

In this semi-semantic notation Diophantos also manipulates what we
would call polynomial fractions: $\mu \gamma \theta \mu \eta \theta \nu \sigma \zeta \alpha \omega \upsilon \alpha \sigma \delta \upsilon \alpha \theta \gamma \nu \chi \alpha$
or $\frac{8}{x^2 + x}$ and $\delta \upsilon \alpha \zeta \alpha \omega \upsilon \alpha \eta \theta \nu \sigma \zeta \alpha \omega \upsilon \alpha \sigma \delta \upsilon \alpha \theta \gamma \nu \chi \alpha$
or $\frac{x^2 + x + 8}{x^2 + 3x + 4}$ (IV.25). Interestingly he considers
polynomial fractions as fractions, for which he has general rules for adding
and multiplying. In IV.36 he finds the fractions equivalent to $\frac{3x}{x - 3}$ and
$\frac{4x}{x - 4}$.

4. The algebra of the *Arithmetika*

We also face the problem as to what Diophantos’ algebraic knowledge was.
Like Euclid’s *Elements* the *Arithmetika* is a tightly organized book that
deals with problems of linear and quadratic equations and problems concern-
ing rational perpendicular triangles. The problem is always posed in a
very general way, but the solution is always given in a numerical example.

\textsuperscript{f} Allard’s transcription\textsuperscript{24} makes use of an accent $'$.\textsuperscript{g}

\textsuperscript{h} Tannery\textsuperscript{6} sometimes transcribes this as $\delta \upsilon \upsilon$.\textsuperscript{h}
The values which are chosen by Diophantos to solve the problem are almost perfectly chosen, which gives rise to the question whether he had an idea of a general solution method. The answer is most probably ‘yes, he did’ and he also realized that the solution he gave in most cases was not the only one possible (when dealing with indeterminate equations). In III.19 he writes “Now we have learned to divide a square into two squares, in an infinite number of ways”. The way Diophantos deals with linear and quadratic equations suggests that the special cases he uses are nothing more than archetypical examples of the solution method. Because of his style, with examples, it is however very difficult to get an idea of Diophantos’ mathematical reasoning.

Let us have a look at some of the general methods of Diophantos. First degree equations are solved by Diophantos without any problems, but the numbers are always chosen such that the solutions are positive. He also has a remarkable sense of reducing systems of equations to one equation. Indeterminate equations of the first degree are not treated by Diophantos. For quadratic equations, he has a standard technique, indicating he knew the general algorithm. In the Arithmetika he never considers more than one root, even if there are two positive solutions, which he seems to know. This may be because he is only interested in one solution, not all solutions. Of the indeterminate equations only those of the type \( ax^2 + bx + c = 0 \) in which — when applying the algorithm — \( a \) or \( c \) disappear occur. The method Diophantos uses only allows to solve the general equation when \( a \) or \( c \) or \( b^2 - 4ac \) are a positive square, or if one solution is already known.

Consider the equation \( ax^2 + bx + c = \alpha^2 \) in which either \( a = a'^2 \) or \( c = c'^2 \), then the solution is easily found. Suppose \( a = a'^2 \) the equation becomes

\[
a'^2x^2 + bx + c = \alpha^2
\]

Put \( \alpha = a'x + k \) and the equation becomes

\[
a'^2x^2 + bx + c = a'^2x^2 + 2a'kx + k^2
\]

resulting in

\[
bx + c = 2a'kx + k^2
\]

\[a\] On indeterminate equations of the first degree in Greek mathematics, see Christianides.42
with solution

\[ x = \frac{k^2 - c}{b - 2a'k} \]

for any value of \( k \).

Diophantos never uses a parameter \( k \), but always a specific chosen number, for which the solution always turns out to be positive.

Suppose \( c = c'^2 \)

the equation becomes

\[ ax^2 + bx + c'^2 = \alpha^2 \]

Put \( \alpha = kx + c' \)

resulting in

\[ ax^2 + bx = k^2x^2 + 2c'kx \]

which is, after factoring, equivalent with the linear equations

\[ x = 0 \text{ or } ax + b = k^2x + 2c'k \]

Diophantos never considers \( x = 0 \), but immediately arrives at the second equation. The solution to this equation is

\[ x = \frac{2kc' - b}{a - k^2} \]

Again Diophantos uses a specific chosen number for \( k \).

If neither \( a = a'^2 \) or \( c = c'^2 \) Diophantos draws attention to the fact that the first choice of parametrization of \( \alpha^2 \) may not lead to a rational solution. For example in IV.31 he finds the expression \( 3x + 18 - x^2 \) which has to equal a square. At first he equals the square to \( 4x^2 \), leading to the equation \( 3x + 18 = 5x^2 \). He remarks that the solution is not rational in this case.\(^b\) He then puts that we have to find a square which, if added to 1, multiplied by 18 and then added to \( 2\frac{1}{4} \), is again a square. Diophantos’ expression seems enigmatic. Let us therefore transcribe it into modern notation.

\(^b\) The solution is \( x_{1,2} = \frac{3 \pm \sqrt{369}}{10} \).
We have the equation $3x + 18 - x^2 = a$ square $= \alpha^2$.
Put $\alpha = dx$.
Then
\[
3x + 18 - x^2 = d^2 x^2 \\
\iff -(1 + d^2)x^2 + 3x + 18 = 0
\]
with $D = 9 + 4.18(1 + d^2)$. The quadratic equation will have a rational solution if $D$ is a perfect square.
Thus $9 + 4.18(1 + d^2) = f^2$ or $\frac{9}{4} + 18(1 + d^2) = \left(\frac{f}{2}\right)^2$, which is nothing else than the condition expressed by Diophantos.

More generally expressed this becomes:
\[
ax^2 + bx + c = \alpha^2
\]
with
\[
y^2 = d^2 x^2
\]
from which
\[
(a - d^2)x^2 + bx + c = 0
\]
with discriminant
\[
D = b^2 - 4(a - d^2)c
\]
Diophantos wants this last expression to be a perfect square: $D = f^2$.
Putting
\[
D' = b^2 - 4ac
\]
it follows that
\[
D' + 4d^2 c = f^2
\]
If $D'$ is a perfect square, the previous method, immediately imposes a substitution for $f$, with which the equation can be solved.
If this is not the case, then one of the solutions has to be known, to construct a pencil of solutions.

\[\text{c In the above case: } D = 9 - 4(-1 - d^2)18 = 9 + 4.18(1 + d^2) = f^2 \Rightarrow \frac{9}{4} + 18(1 + d^2) = \left(\frac{f}{2}\right)^2\]
A very specific method, which Diophantos uses is the so-called double equation. This is a system of two equations in one unknown, which both have to equal a square. The solution is based on factoring the polynomials. For instance for the system of linear equations:

\[
\begin{align*}
ax + b &= \alpha^2 \\
Cx + d &= \beta^2
\end{align*}
\]

by subtracting both equations, we find

\[(a - c)x + (b - d) = \alpha^2 - \beta^2\]

Now suppose that

\[(a - c)x + (b - d) = p.q\]

(for instance by considering \((a - c)\) as a common factor) we can write

\[p.q = \left(\frac{p + q}{2}\right)^2 - \left(\frac{p - q}{2}\right)^2 = \alpha^2 - \beta^2 = (\alpha - \beta)(\alpha + \beta)\]

Diophantos immediately puts\(^d\)

\[
\alpha = \frac{p + q}{2} \quad \text{and} \quad \beta = \frac{p - q}{2}
\]

Diophantos uses the method for quadratic equations as well. Whereas for linear equations a solution in the rationals is always possible, this is not the case for quadratic equations. Diophantos treats justs a few examples, which always give rise to a difference which can be factored.

For example equations of the type

\[
\begin{align*}
a^2x^2 + bx + c &= \alpha^2 \\
a^2x^2 + b'x + c' &= \beta^2
\end{align*}
\]

leading to

\[(b - b')x + (c - c') = \alpha^2 - \beta^2\]

which has already been solved.

\(^d\) Omitting, or not recognizing that one solution is given by

\[
\begin{align*}
p &= \alpha - \beta \\
q &= \alpha + \beta
\end{align*}
\]
An example is G.IV.23 in which

\[
\begin{align*}
  x^2 + x - 1 &= \alpha^2 \\
  x^2 &- 1 = \beta^2
\end{align*}
\]

leading to \( x = \alpha^2 - \beta^2 \).

A second form is

\[
\begin{align*}
  x^2 + bx + c &= \alpha^2 \\
  b'x + c &= \beta^2
\end{align*}
\]

from which

\[
x^2 + (b - b')x = \alpha^2 - \beta^2
\]

with the evident resolution into factors:

\[
x(x + (b - b')) = \alpha^2 - \beta^2
\]

The space this article allows is too small to deal with all of Diophantos’ methods. However we can state that Diophantos has complete mastery over the methods of solution for solving linear equations, for indeterminate quadratic equations and for systems of equations of first and second degree. For higher degree equations he sometimes knows a solution method. In some cases however one can think of a fortuitous choice of parametrization. For instance problem A.VI.17 in the Arab version is: “Find three squares of which the sum is a square, in such a way that the first is the square root of the second and the second the square root of the third” (Rashed, IV, p. 65; Sesiano, p. 149). Diophantos’ solution to this problem is \((\frac{1}{4}, \frac{1}{16}, \frac{1}{256})\). Excepting trivial solutions and variants with minus signs, this is the only solution. We may wonder whether Diophantos was aware of this fact. Clearly Diophantos’ method leads to a solution, which was enough for him.

\[
\begin{align*}
  x^2 + y^2 + z^2 &= \alpha^2 \\
  x^2 &= y \\
  y^2 &= z
\end{align*}
\]

from which \( x^8 + x^4 + x^2 = \alpha^2 \).

Put \( \alpha = x^4 + \frac{1}{2} \)

from which \( \alpha^2 = x^8 + x^4 + \frac{1}{4} \)

and \( x^2 = \frac{1}{4} \) and \( \alpha = \frac{9}{16} \).

(See Sesiano, p. 149).
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46  Ad Meskens

WHAT DID THE ABBACUS TEACHERS AIM AT WHEN THEY (SOMETIMES) ENDED UP DOING MATHEMATICS?
AN INVESTIGATION OF THE INCENTIVES AND NORMS OF A DISTINCT MATHEMATICAL PRACTICE

JENS HØYRUP

Section for Philosophy and Science Studies
Roskilde University
*E-mail: jensh@ruc.dk
http://www.akira.ruc.dk/~jensh

In memoriam Hubert L. L. Busard

1. Kreuger, Enron and scandalous abbacus algebra: three instances of fraud

There may still be Swedes who consider Ivar Kreuger a businessman of genius (at least when I was young there were). After his suicide in 1932 and the opening of his books the rest of the world, in so far as it remembers him and his attempt to create a world monopoly of matches, tends to agree that he was a crook blown up into heroic wide-screen format. That he succeeded as a star for so long — and that the Enron directors did so seven decades later* — depended on the construction of a scheme so complex that nobody was able to look through it.

The history of abbacus mathematics presents us with a similar episode, and some members of the tribe of historians of mathematics wave a patriotism that recalls that of certain Swedes — a phenomenon which illuminates particular features of the mathematical endeavour, just as Kreuger and Enron illuminate particular aspects of the market economy. But before that

* I abstain from referring to corresponding Danish affairs, not because I do not recognize that they exist (they do, and mostly have as protagonists leading members of our major, “liberal” government party, reduced to ex-members only after they have been discovered or convicted) but because non-Danes know so little about them that they are allowed to continue their good deeds abroad after having been convicted at home.
story can be told, the notion of “abbacus mathematics” should itself be explained.

Abbacus mathematics (Italian _abbaco_) is a distinct mathematical practice, known from Italy (primarily from the region between the Genova-Milan-Venice arc to the north and Umbria to the south) from the late thirteenth to the mid-sixteenth century (but with an aftermath which makes much of its contents familiar to anybody who learned arithmetic in junior secondary school in the 1950s, as I did). Its social base was the “abbacus school”, a school frequented by merchant and artisan youth (but also sons of the aristocracy) for two years around age 11–13. In smaller cities, the abbacus masters were often employees of the city; in large cities like Florence and Venice schools were run on a completely private basis.

It has been commonly assumed that the abbacus school and its mathematics descended, at most with minor secondary contributions, from Leonardo Fibonacci, his _Liber abbaci_ and his _Pratica de geometria_. Thus, according to Elisabetta Ulivi (p. 10), the _libri d’abbaco_ “were written in the vernaculars of the various regions, often in Tuscan vernacular, taking as their models the two important works of Leonardo Pisano, the _Liber abaci_ and the _Practica geometriae_” — while, as Warren Van Egmond sees it, all abacus writings “can be regarded as [. . .] direct descendants of Leonardo’s book” (Van Egmond, p. 7).

On close analysis of the texts involved — early Italian abacus books, texts of a similar kind from the Ibero-Provençal area, and the _Liber abbaci_ — this turns out to be a mistake, due to what at another occasion I called “the syndrome of the Great Book”: the “conviction that every intellectual current has to descend from a Great Book that is known to us” (Høyrup, p. 11). Instead, as argued in Høyrup, the beginning of abacus mathematics must be traced to an environment which precedes the _Liber abaci_; which was known to Fibonacci; which (if it had not fully reached Italy in his days) he may have encountered in Provençal area; but which is likely to have spanned both sides of the maritime and the religious divide of the Mediterranean world. The beginning of abacus _algebra_, taking place in the early fourteenth century, seems to be inspired by borrowings from an environment located in the Provençal-Catalan area, with a Catalan rather than a Provençal barycentre. This is argued in Høyrup, on which I draw for the following outline of the events. The precise location of the area is unimportant for what follows; it is more important that the inspiration

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b A convenient survey of the topic is Ulivi.
did not come directly from Arabic “scientific” algebra as represented for instance by the treatises of al-Khwārizmī, Abū Kāmil and al-Karajī.

The “scandal” belongs precisely within the field of algebra. The earliest extant treatment of the subject (and plausibly the earliest treatment at all in an Italian vernacular) is found in Jacopo da Firenze’s *Tractatus algorismi*, written in Montpellier in 1307. In what in my transcription of the manuscript is labelled chapters 16–17 — the algebra section proper — rules are given for the following cases

\[
\begin{align*}
(1) \quad & \alpha t = n \\
(2) \quad & \alpha C = n \\
(3) \quad & \alpha C = \beta t \\
(4) \quad & \alpha C + \beta t = n \\
(5) \quad & \beta t = \alpha C + n \\
(6) \quad & \alpha C = \beta t + n \\
(7) \quad & \alpha K = n \\
(8) \quad & \alpha K = \beta t \\
(9) \quad & \alpha K = \beta C \\
(10) \quad & \alpha K + \beta C = \gamma t \\
(11) \quad & \beta C = \alpha K + \gamma t \\
(12) \quad & \alpha K = \beta C + \gamma t \\
(13) \quad & \alpha CC = n \\
(14) \quad & \alpha CC = \beta t \\
(15) \quad & \alpha CC = \beta C \\
(16) \quad & \alpha CC = \beta K \\
(17) \quad & \alpha CC + \beta K = \gamma C \\
(18) \quad & \beta K = \alpha CC + \gamma C \\
(19) \quad & \alpha CC = \beta K + \gamma C \\
(20) \quad & \alpha CC + \beta C = n
\end{align*}
\]

Here, \( t \) stands for *thing* (cosa), \( C \) for *censo*, \( n \) for *number* (numero), \( K \) for *cube* (cubo), \( CC \) for *censo di censo*. *Censo* is the product of *thing* with *thing*, *cubo* the product of *censo* with *thing*, and *censo di censo* the product of *censo* with *censo*. This shorthand has the advantage over \( x, c, \ldots \)

---

\(^{c}\) Heyrup\(^{9}\) is an edition of the algebraic chapter with mathematical commentary. Heyrup\(^{9}\) is a preliminary transcription of the complete Vatican manuscript (Vat. lat. 4826); the other two extant manuscripts of the treatise (Milan, Trivulziana MS 90; Florence, Riccardiana MS 2236), of which Heyrup\(^{9}\) is a semi-critical edition, represent a redaction from which the algebra chapter is eliminated. Both are also included in Heyrup.\(^{7}\)

\(^{d}\) These terms come from Arabic algebra, which is the evident ultimate root of all abbacus algebra. *Cosa* translates *šay‘*, *censo* comes from Latin *census*, a translation of *māl*, “possession” or “amount of money”. Originally, Arabic *al-jabr* was centred around riddles dealing with a possession and its (square) root, for instance “possession and ten of its roots equal 39 dinars”. Al-Khwārizmī, in his presentation of the topic (which may be the earliest presentation at all in a systematic written treatise) still remembers this: when he has found the root, he multiplies it by itself in order to find also the possession. But already in his treatise these riddles with their solutions serve as representation of second-degree problems, in which the fundamental unknown is a *šay‘*, whose second power is identified with a *māl* (whence the *šay‘* becomes its root). Almost all abbacus algorithms do as Jacopo: the “roots” are replaced by “things” in the formulation of the rules, and the number is a number, not (as in the Latin translations of al-Khwārizmī) a quantity of dragmas. This is one of several reasons that abbacus algebra (in particular Jacopo’s algebra) can be seen not to descend from the “learned” level of Arabic algebra but from...
that it leaves it as non-obvious for us as it was for the medieval reckoner that the cases (8–12), (14–20) are reducible.

For the first six cases, one or more illustrating examples are given, for the rest only rules. All twenty rules are valid, since all the cubic and quartic cases (7)–(20) are either homogeneous, biquadratic or reducible to one of the cases (1)–(6) through division. No mathematical scandal so far.

Yet scandal was not far away, in neither time nor space. In 1328, and still in Montpellier, a certain Paolo Gherardi wrote a *Libro di ragioni*, another abbacus book containing an algebra section. Gherardi repeats most of Jacopo’s rules and examples — dropping however those of the fourth degree, offering only one example for each case, changing the numerical parameters in some cases, and replacing two of the examples by entirely different ones.

The important innovations are two. Firstly, Gherardi introduces four new cases, one of which (G1) is rather trivial and the other three (G2–G4) not resolvable by means of techniques known at the time:

\[
\begin{align*}
G1: \quad & \alpha K = \sqrt{n} \\
G2: \quad & \alpha K = \beta t + n \\
G3: \quad & \alpha K = \beta C + n \\
G4: \quad & \alpha K = \gamma t + \beta C + n
\end{align*}
\]

For the latter three, Gherardi gives rules modelled after those for the second degree — for (G2) and (G3) those which hold if $K$ is replaced by $C$, for (G4) the rule for the equation

\[aC = \beta t + (\gamma + n)\]

Finally, Gherardi offers illustrative examples for those higher-degree cases where Jacopo had given none — all of a kind that is easily constructed (see imminently), whereas some of those proposed by Jacopo are so intricate that a modern reader does not immediately see that they lead to second-degree equations. For instance, Jacopo’s illustration of case (6) runs as follows (ed. and transl. Høyrup, p. 318f):

Somebody has 40 *fiorini* of gold and changed them to *venetiani*. And then from those *venetiani* he grasped 60 and changed them back into *fiorini* at one *venetiano* more per *fiorino* than he changed them at first for me. And when he has changed thus, he found that the *venetiani*
which remained with him when he detracted 60, and the fiorini he got for the 60 veneziani, joined together made 100. I want to know how much was worth the fiorino in veneziani.

Gherardi’s examples for the third-degree cases all follow the model used in Jacopo’s illustration for case (3) (ed. and transl. Hoyrup,\textsuperscript{7} p. 307):

Find me 2 numbers that are in the same proportion as is 4 of 9. And when one is multiplied against the other, it makes as much as when they are joined together. I want to know which are these numbers.

Such illustrations are of course easily constructed for any given polynomial equation and look more complex than the equation itself without really being so — for instance Gherardi’s illustration (ed. Arrighi,\textsuperscript{11} p. 107) of the case (G4) “cubes are equal to things and censi and number” (the translation is mine, as all translations in the following where no translator is identified):

Find me three numbers which are in proportion as 2 to 3 and as 3 to 4, and that the first multiplied by itself and then by the [same] number makes as much as when the second is multiplied by itself and the third number is added above, and then 12 are added above.

As we shall see, Gherardi cannot have invented all of this, he must have copied it from an earlier source. We may ask why he did not discover that he was filling his treatise with nonsense. The answer is that all the wrong solutions contain irreducible radicals, and that Gherardi made no attempt to find the approximate value of the solutions. This was no idiosyncrasy. Even Jacopo, when finding a correct irrational solution to one of his examples, leaves things there. Being satisfied with exactly expressed but irrational solutions remained the habit of abbacus algebra. In contrast, abbacus geometry always approximated the square roots that turned up in applications of the “Pythagorean rule” (as it must be called in a context where it was always presented as a rule without proof).\textsuperscript{4} This difference already tells us that algebra and geometry served different purposes: geometry (as a whole, not necessarily each single problem) had to lead to results that were ap-

\textsuperscript{4} This role of square roots and their approximation was so important for geometry that the topic was mostly taught in the geometry chapter of abbacus treatises (when these were ordered in separate chapters and geometry was actually covered). In the Latin algorithms, in contrast, root extraction (not approximation) was one of the arithmetical “species”; they contain no geometry.
plicable in practice, and which could thus be compared to the reading of a yard stick. Abacus algebra, at least beyond the first degree, must in some sense (which we shall get closer at below) have been a purely theoretical discipline without intended practical application.

2. Almost honest business

If Gherardi does not represent the beginning of false solutions, nor is he their end. In 1344, Master Dardi da Pisa (as unknown as Jacopo and Gherardi) wrote a treatise Alibraa argibra, the earliest extant European-vernacular treatise dedicated to algebra alone.\footnote{The extant complete manuscripts are younger. One is from c. 1395 (Vatican, Chigi M.VIII 170), one from 1429 (Arizona State University Library, Tempe), and one from c. 1470 (Siena, Biblioteca Comunale, I.VII.17). Apart from lost sheets and some reordering of the material in the last manuscript, there are no major differences between the three. Of a fourth manuscript from c. 1495 (Florence, Biblioteca Mediceo-Laurenziana, Ash. 1199) I have only seen the extract in Libri\textsuperscript{17} (III, pp. 349–56), but to judge from this it appears to be very close to the Siena manuscript.} After presenting the arithmetic of roots and binomials and giving geometric demonstrations for the correctness of the rules (the latter are very rare in abacus algebra, and in particular not present in any of the earlier treatises) Dardi deals with 194 “regular” cases and 4 whose rules are told only to hold under special conditions (conditions which are not analyzed).\footnote{Van Egmond\textsuperscript{18} lists all the cases in symbolic transcription.} The huge number of regular cases (all with the exception of two lapses solved correctly) is reached because of ample use of radicals — for instance in these ways:

\[
\begin{align*}
\alpha t + \beta \sqrt{K} &= \gamma C \\
\alpha CC &= n + \sqrt[3]{m}
\end{align*}
\]

\[
\begin{align*}
\alpha t + \beta \sqrt{C} &= \gamma C \\
\alpha CC + n + \sqrt[3]{m} &= \beta C
\end{align*}
\]

For a generation which has come to see no difference between rational and irrational numbers, to see all the “cossic numbers” (as thing, censo etc. were to be called when abacus algebra reached Germany under the name of Coss) as powers of the same unknown and to express everything in symbols and not in words, these are trivial extensions — and by reducing many of the cases to other cases that are dealt with previously, Dardi shows that he understood things in the same way without having access to our tools (tools without which the extensions are often not trivial).

All of these cases are illustrated by one or more examples. All are pure-number problems, with a few exceptions either about a single number,
about two numbers with sum 10, or about numbers in given proportion.

Then there are the four “irregular” cases, cases governed by non-general rules. It is clear from Dardi’s words that he knows these rules to be valid only when the equations to which they correspond have particular properties — but he states that “by some accident the said rules may appear in some computation”. The cases in question are these:

\[
\begin{align*}
(I1) \ & \ \gamma t + \beta C + \alpha K = n \\
(I2) \ & \ \delta t + \gamma C + \beta K + \alpha CC = n \\
(I3) \ & \ \alpha t + \gamma C + \alpha CC = n + \beta K \\
(I4) \ & \ \delta t + \alpha CC = n + \gamma C + \beta K
\end{align*}
\]

All four are provided with examples, the former two of which reveal how the rules have been found. We may look at the first example — a capital grows in three years with composite interest from 100 £ to 150 £ (Jacopo has the same problem, only with two years; it illustrates his case (4)). If the value of the capital after 1 year — or, even simpler, the value of 1 £ after one year — had been taken as the thing, we would have been led to a homogeneous equation,

\[
t^3 = 1500000 \text{ respectively } \ t^3 = 1^{1/2}.
\]

Instead, Dardi takes the monthly interest of 1 £ expressed in δ as his thing. The yearly interest of 1 £ is therefore \(1/20\) thing £. The same choice had been made by Jacopo, and in Dardi’s present case it leads to the equation

\[
100 + 15t + \frac{3}{4} C + \frac{3}{80} K = 150.
\]

The rule used to solve it is

\[
t = \sqrt[3]{\left(\frac{\gamma}{\alpha}\right)^3 + \frac{n}{\alpha} - \frac{\beta}{\alpha}},
\]

— or rather, since the rule first tells to divide by (the coefficient of) the cubes and afterwards speaks only of the resulting new coefficients,

\[
t = \sqrt[3]{\left(\frac{\gamma'}{\alpha'}\right)^3 + n' - \frac{\beta'}{\alpha'}},
\]

where \(\beta' = \beta/\alpha\), etc. At first view, this may seem an astonishingly good guess (since it works), but it requires nothing beyond some training in the arithmetic of polynomials and awareness that a different position for the

\footnote{£ stands for lira/lire. 1 £ = 20 β (soldi), 1 β = 12 δ (denari). Whoever is familiar with the traditional British pound-shilling-penny system will recognize it.}
thing leads to a homogeneous equation. For simplicity, let us consider the homogeneous equation

\[ \sqrt[3]{(t + \phi)} = \mu, \]

(in the actual problem, \( \phi = 20, \mu = 12000 \)). Performing the multiplication we get

\[ \phi^3 + 3\phi^2 t + 3\phi C + K = \mu, \text{ or} \]

\[ 3\phi^2 t + 3\phi C + K = \mu - \phi^3, \]

which should correspond to

\[ \gamma' t + \beta' C + K = n'. \]

Therefore,

\[ \phi = \gamma' / \beta', \quad n' = \mu - \phi^3, \quad \text{whence} \]

\[ \mu = \phi^3 + n' = (\gamma' / \beta')^3 + n'. \]

Now, the solution obtained from the homogeneous equation is

\[ t = \sqrt[3]{\mu - \phi}, \text{ that is} \]

\[ t = \sqrt[3]{(\frac{\gamma'}{\beta'})^3 + n' - \frac{\gamma'}{\beta'}}, \]

exactly Dardi’s rule. Whoever invented the rule must have done so from a numerical example, but following the numerical steps precisely and seeing from which operations the coefficients arise it would not be too difficult to see that the 20 of our example results, in the words of the rule, “when [the coefficient of] the things [is/are] divided by [the coefficient of] the censi”; similarly for the rest of the rule — and similarly for the remaining three irregular rules.

The inventor of Gherardi’s rules may have been a pure bluffer — for imitating the rules for the second degree it was not even necessary to know how these were derived, all that was needed was to know the rules themselves. In contrast, the rules for Dardi’s irregular cases, guesses though they are in a certain sense, can only have been arrived at by someone who understood polynomial operations quite well.

The irregular rules turn up in many later manuscripts, mostly without the warning about their restricted validity. One of these, an anonymous *Libro di conti e mercatanzie* from c. 1395 (ed. Gregori and Grugnetti\textsuperscript{13}),
is related to Gherardi’s *Libro di ragioni* in a way which shows them to build on common sources (also shared with an equally anonymous *Trattato dell’alcibra amuchabile* from c. 1365 (ed. Simi\textsuperscript{14}).\textsuperscript{d} Quite apart from the internal evidence (the use of a business dress when all other examples are in pure numbers, and the reservations expressed by Dardi himself), this is strong evidence that these rules were borrowed by Dardi and thus that they antedate 1344, just as the false rules in Gherardi’s *Libro di ragioni* must have been borrowed by Gherardi from a source shared with the *Trattato dell’alcibra amuchabile*. We may conclude that the presence of regular higher-degree cases in Jacopo’s algebra created a fashion or a need to do even better — a need which was then fulfilled, first by the invention of false rules that could not be checked,\textsuperscript{e} and then by the construction of irregular rules that worked if tested on the proposed example.\textsuperscript{f} We shall discuss this process below, but for the moment only observe that false solutions survived for long. Luca Pacioli, after having made the check proposed in note e (this section), pointed out in his *Summa de arithmetica* (Pacioli,\textsuperscript{15} p. 150\textsuperscript{r}) that so far no rule had been found for the solution of cases where, as he says, the three algebraic powers that are present are not “equidistant”. On that background, del Ferro’s genuine solution of the cubic equation and Cardano’s publication of a corresponding proof can be seen not only to be mathematically impressing but to deliver what others were known by then to have promised in vain for two centuries. But Pacioli’s book did not kill off the fraud completely — in 1555, the Portuguese Bento Fernandes still included them in his *Tratado da arte de arismetica* (Silva,\textsuperscript{16} p. 16, pp. 30–3).

3. Aiming high — and failing honestly

If we are to learn from the abbacus masters about what mathematics is or may be as a practice it does not serve to consider solely such aspects of their activity as correspond to what we routinely expect from a mathematician. The false solutions constitute one aspect of abbacus mathematics which is anomalous with respect to our routine expectations. We may go on with

\textsuperscript{d} For this, see Høyrup\textsuperscript{6} (pp. 18–25).
\textsuperscript{e} That is, unless one constructed alternative examples with (most conveniently from) a known integer solution; and that seems not to have been a widespread idea.
\textsuperscript{f} This test was easy: In the example that was analyzed above, \( t = \sqrt[3]{12000} - 20 \). Since this is the yearly interest, the yearly growth factor of the capital is \( 1 + \frac{1}{20} = 1 + 0.05 \). After three years the capital is thus multiplied by \( \frac{3}{2} \), just as required. An abbacus master would have had to perform the calculation stepwise, but the principle is the same.
another anomaly. It is found in yet another Vatican manuscript, Vat. Lat. 10488 (fol. 29\textsuperscript{v}–30\textsuperscript{v}).\textsuperscript{a}

\textit{Algebra}

1. These are some computations collected from a book made by the hand of Giovanni di Davizzo dell’abbaco from Florence written the 15th of September 1339, and this is 1424.

2. Know that to multiply number by cube makes cube and number by censo makes censo and number by thing makes thing.

3. And plus times plus makes plus and less times less makes plus and plus times less makes less and less times plus makes less.

4. And know that a thing times a thing makes 1 censo and censo times censo makes censo of censo and thing times censo makes cube and cube times cube makes cube of cube and censo times cube makes censo of cube.

5. And know that dividing number by thing gives number and dividing number by censo gives root and dividing thing by censo gives number and dividing number by cube gives cube root and dividing thing by cube gives root and dividing censo by cube gives number and dividing number by censo of censo gives root of root and dividing thing by censo of censo gives cube root and dividing censo by censo of censo gives root of root and dividing number by censo of censo of censo of censo gives root of root of root of root and dividing number by cube of cube of cube of cube gives cube root of cube root of cube root.

\textsuperscript{a} I use the most recent of the two discordant foliations.
What Did the Abbacus Teachers Aim at?

¶[5]
If you want to multiply root by root, multiply root of 9 times root of 9, say, 9 times 9 makes 81, and it will make the root of 81, and it is done.
To divide root of 40 by root of 8, divide 40 by 8, it gives 5, and root of 5 let it be.
To divide root of 25 by root of 9, divide 25 by 9, it gives root of $2^2/9$, done.
If you want to multiply 7 less root of 6 by itself, do 7 times 7, it makes 49, join 6 with 49, it makes 55, and 7 times 6 makes 42, then multiply 7 times 42, it makes 294, and multiply then 4 times 294, it makes 1176, I say that 55 less root of 1176 will it make when 7 less root of 6 is multiplied by itself.

¶[6]
If you want to detract root of 8 from root of 18, do 8 times 18, it makes 144, its root is 12, and say, 8 and 18 makes 26, detract 24 from 26, and root of 2 will remain, done.
If you want to join root of 8 with root of 18, do 8 times 18, it makes 144, its root is 12, and say, 12 and 12 makes 24, and say, 8 and 18 makes 26, and join 24 and 26, it makes 50, and root of 50 will the number be.
If you want to multiply 5 and root of 4 times 5 less root of 4, do thus and say, 5 times 5 makes 25, and say, 5 times root of 4, do thus, bring 5 to root, it makes 25, and do root of 25 times root of 4, it makes root of 100, and make 5 times less root of 4, it makes less root of 100, 25 still remains, now detract 4 from 25, 21 remains, and 21 they make.
If you want to multiply 7 and root of 9 times 7 and root of 9, do 7 times 7, it makes 49, put (above) this 9, you have 58, and 9 times 49 makes 441, multiply by 4, it makes 1764, you have that it will make 58 and root of 1764, which is 42, done.
If you want to divide 35 by root of 4 and by root of 9, do thus, from 4 to 9 there is 5, multiply 5 times 5, it makes 25, and say, bring 35 to root, it makes 1225, now say, 4 times 1225 makes 4900, divide by 25, it makes 196, and do 9 times 1225, it makes 11025, divide by 25, it gives 441. We have that dividing 35 by root of 4 and by root of 9 gives root of 441 less root of 196, and it is done.

This is followed by 19 rules for solving reduced equations of the first, second, third and fourth degree: Jacopo’s 20 cases, with two omissions, and a new false case which cannot be read because somebody discovered that it did not work and glued a paper slip over it; this slip has been removed or fallen off, but the glue has made the paper as dark as the ink.

First of all we should know that Giovanni’s composition of the “cossic numbers” is multiplicative and not made by nesting; cube of cube stands for $t^3 \cdot t^3$, not for $(t^3)^3$. This corresponds to what we find with Diophantos.
and in Arabic algebra. Once we know this we see that §1 and §3 present what we might call the multiplicative semi-group of non-negative algebraic powers through examples; the interrupting §2 gives the “sign rules”. So far, everything goes well; from the correction made in the Vatican manuscript at a later point (§4) it is also clear that the author of this manuscript understood it well, and was able to perform divisions within the semi-group to the extent they can be performed.

But Giovanni does not stop here. Skipping the divisions that correspond to multiplications within the semi-group (which he may have considered unproblematic) he jumps to those that have no such solution. Obviously what he does is wrong, and he should have discovered that if he had been a bit careful. Indeed, if “dividing number by thing gives number”, then, since the quotient multiplied by the divisor gives the dividend (any abacus algebraist would know that, it is often told explicitly in the texts), number multiplied by thing should give number. But “number by thing makes thing”, as Giovanni knows well and states in §1.

However, the nonsense conceals a system. If in this paragraph and nowhere else we read “root” as $t^{-2}$, “cube root” as $t^{-3}$, if we compose these “roots” multiplicatively, and if we finally interpret “number” when occurring as a result as $t^{-1}$ — then everything is perfect, and the semi-group is extended into a group.

We shall return to the implications of Giovanni’s undertaking as a whole. At this point we may try to trace how he thought. The background appears

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\[b\] In the present case κυβ ´o κυβος respectively ka’b ka’b, None of these have the semantic implications of the Latin and Italian genitive — the Greek term because it is no more a genitive than “Anglo-American”, ka’b ka’b because the Arabic “genitive” is of much wider use than the IndoEuropean homonymous case, being employed for any sequence of two consecutive nouns, including those which in Greek (and English) are composed with o. None of them therefore suggests nesting, as does the genitive used in the Italian and Latin translations. In the short run this caused a problem to the abacus writers; for instance, it probably lays behind Dardi’s two lapses (cf. Van Egmond,\(^\text{18}\) p. 417). In the longer run, however, the linguistic trouble was probably what drove the trend toward an interpretation through nesting (common in the later fifteenth century, and practised for instance by Pacioli). Since the creation of new names for the fifth and seventh power (etc.) then caused new confusion, this may have been one of the driving forces behind the eventual introduction of numerical exponents (first in Chuquet and Bombelli).

\[c\] From later versions it can be seen that this line was originally “and dividing censo of cube by cube of cube gives number”. Somewhere in the process, this had become “and dividing censo of cube by cube gives number”. Noticing the error, somebody — almost certainly the writer of the manuscript, since the correction is made there — discovered that this was wrong, and stated a correct result (but of a division Giovanni had not intended).
to be an intuitive and only implicit arithmetization of the series of algebraic powers. Multiplying by censo, so more or less he may have reasoned, we take two steps “upwards”; multiplying by cube we take three steps. Multiplying cube by censo we get censo of cube (this is stated). Dividing censo of cube by censo we therefore get cube, two steps “downwards”. Dividing instead by cube we have to take three step downwards. Now multiplying the thing by itself we get a censo, and taking the root of the censo we return to the thing; similarly, the cube root of a cube is a thing. Therefore “root” must be some kind of opposite of the censo, and cube root some kind of opposite of cube. Taking two steps upwards from number (number by censo) gives us censo, taking two steps downwards (number divided by censo) therefore its opposite, root; taking three steps downward must give us cube root.

This explains everything except those rules where the result is “number” — for instance “dividing censo by cube”. Here the idea must more or less have been that “root” is a “second root”, just as censo is a second (being thing times thing, and corresponding to two steps in multiplication and division); correspondingly, the cube root is a “third root”. Therefore, the result of censo divided by cube must be a “first root”, which Giovanni then identifies with number (perhaps because it seemed to him that “thing” was an impossible choice, being the result of the division of cube by censo). If we take care that this is only the meaning of “number” when it results from a division, everything becomes correct — but like Hogarth’s famous false-perspective engraving only locally correct, and absurd as soon as one tries to move back and forth through the whole network of possible operations.

Even Giovanni’s fallacies were borrowed faithfully. As we have seen, his text was copied in 1424 by somebody who understood it well enough to repair a copying error correctly. Later (earlier than c. 1480) Giovanni’s system turns up in Piero della Francesca’s Trattato d’abaco (ed. Arrighi,19 p. 84f) with some change of the order and without the mistake discussed in note c (this section), and almost identically in Giovanni Guiducci’s Libro d’arismetricha from c. 1465 — see Giusti20 (p. 205). Finally, Giovanni’s first 15 rules turn up in exactly the same order in Bento Fernandes’ Tratado da arte de arismetica from 1555 (Silva,16 p. 14); Fernandes thus stops just before the corrupted line, which may be no accident. Piero, like Fernandes, also repeats the false algebraic rules, apparently without suspecting that something is rotten. Evidently Piero, claiming to write about “certain abacus things that are necessary for merchants” (ed. Arrighi,19 p. 39), could do so because neither he nor any merchant had the least operatory need for it.
4. A better intuition

Intuitions like those which can be read from Giovanni’s system can be found in other abacus writings, and they often work better. One which is also made much more explicit gives a proof for the sign rule “less times less makes plus”. The earliest known occurrence is in Dardi’s *Aliaabra argibra*:

Now I want to demonstrate by number how less times less makes plus, so that every time you have in a construction to multiply less times less you see with certainty that it makes plus, of which I shall give you an obvious example. 8 times 8 makes 64, and this 8 is 2 less than 10, and to multiply by the other 8, which is still 2 less than 10, it should similarly make 64. This is the proof. Multiply 10 by 10, it makes 100, and 10 times 2 less makes 20 less, and the other 10 times 2 less makes 40 less, which 40 less detract from 100, and there remains 60. Now it is left for the completion of the multiplication to multiply 2 less times 2 less, it amounts to 4 plus, which 4 plus join above 60, it amounts to 64. And if 2 less times two less had been 4 less, this 4 less should have been detracted from 60, and 56 would remain, and thus it would appear that 10 less 2 times 10 less 2 had been 56, which is not true. And so also if 2 less times 2 less had been nothing, then the multiplication of 10 less 2 times 10 less 2 would come to be 60, which is still false. Hence less times less by necessity comes to be plus.

The passage is followed by a diagram:

![Diagram](image)

The reason this must be characterized at least to some extent as an intuition and not as a genuine piece of analysis is the final part: instead of finding that the contribution of less 2 by less 2 must be the lacking $64 - 60 = \ldots$
4. Dardi expects (from similarity) that it must be either an additive or a subtractive contribution of 4, or possibly nothing at all, and then eliminates the second and the last possibility, leaving only the first one.

Luca Pacioli repeats the argument in his *Summa*, now with the diagram in the margin, and with an explicit reference to the cross-multiplication (Pacioli,15 p. 113'). He finds the very concept to be *absurd* and an abuse but none the less necessary — Pacioli, indeed, thinks in terms of *negative numbers*, not merely subtractive contributions to an equation as does Dardi (he explains that a number of this kind is “less than zero and in consequence a debt”). Apart from that the only innovation is that the alternative to the alternative is now \((-2)\cdot(-2) = -2\), not \((-2)\cdot(-2) = 0\).

5. **An alternative to the false solutions**

Some abbacus authors thus had better intuitions than others. Similarly, some of them understood better than others that the false solutions to the higher-degree equations *were* false and even devised alternatives.

One such alternative is described in yet another anonymous manuscript, this one from the fourteenth century (Florence, Biblioteca Nazionale, Fond. princ. II.V.152). After the presentation of the “22 rules of algebra” (Jacopo’s 20 rules, and the two biquadratics that are absent from his list), the author goes on to explain that other rules can be made for certain other cases (ed. Franci and Pancanti,21 p. 98). He continues:

Wanting to treat of this it is needed first to show how there are other roots than those one normally speaks about, that is, there are other roots than square roots and cube roots, and among these there is one which is called cube root with addition of some number, and about this I intend to show something.

The concept is then explained through several examples, starting with “the cube root of 44 with addition of 5”. This root is 4, because \(4^3 = 44+5\cdot4\); in general, expressed in our shorthand, the cube root of \(n\) with addition \(\alpha\) — say, \(\sqrt[3]{(\alpha,n)}\) — is \(t\) if

\[K = n+at.\]

(We recognize the normalized version of equation (G2)). Evidently, this allows us to give a name to the solution of the above equation; but if we

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\(^a\) See also Franci.23
follow Pascal’s advice about how one should understand definitions, this name is just an abbreviation of “the solution to the equation \( K = n + at \), which makes the whole thing rather circular.

However, several further observations must be added to this. Firstly, as long as irrational square and cube roots were not approximated in abbacus algebra, expressing the solution to the equation \( C^3 = 3 \) as “root of 3" was just as circular. Secondly, the trick is also used in much more recent mathematics — elliptic functions could be said to suffer from the same defect. What makes square roots and elliptic functions mathematically interesting (beyond the possibility of numerical approximation) is the network of relations they allow us to establish.

What can we say about our author and his “cube root with addition” in this respect? Firstly, that he\(^b\) must have been aware of the objection just discussed. He does not find it worthwhile to discuss a single problem of the type which is immediately solved by his particular root; instead he explains that it is of limited use, since for many numbers this root cannot be expressed. What he does beyond that is to establish a (limited) network of relations: he gives (correct) rules for reducing equations of the types \( K + \beta C = m \), \( K = \beta C + m \) and \( \beta C = K + m \) to the form \( K = n + at \), and in the ensuing example he then makes use of the cube root with addition.\(^c\) He also shows in the examples that solutions may exist even if the number term turns out to be “a debt”, that is, negative. In order to find this reduction rule, the author must have performed manipulations similar to those behind Dardi’s first irregular rule.\(^d\) The author must have been an adroit

\(^b\) Or the one from whom he borrows — a reservation which must always be made for the abbacus authors when they seem to be original; I shall not repeat it but ask the reader to keep it in mind.  
\(^c\) He does not show that \( \alpha \) can be eliminated and thus that a single table of \( \sqrt[\alpha]{(a, n)} \) is all that is needed. The reason could be that tables did not enter his mind, but it could also be that the transformation was too difficult. It asks indeed for a substitution \( z = t/\sqrt[\alpha]{a} \), which gives the equation
\[
z^3 = \frac{n}{a\sqrt[\alpha]{a}} + z.\]

This is more difficult to find and explain without symbolic algebra than the additive substitutions needed for the transformations which are explained: finding the transformation factor to be \( \sqrt[\alpha]{a} \) asks for manipulation of several powers of two variables at a time, something which was so far beyond the horizon of abbacus algebra that even Bombelli when creating his new formalism happened to exclude it (cf. Section 9). Vive Descartes!  
\(^d\) This is not fully explicit, but obvious from the detailed appearance of the rule. If, for convenience, we reformulate the first equation as \( t^3 + 3at^2 = n \), completion gives \((t+a)^3 = n+a^3+3at^2 = n+a^3+3a^2(t+a)-3a^2\cdot a\), which is exactly what the rule tells, in this order and without contraction of any kind of the expression \( n+a^3+3a^2(t+a)-3a^2\cdot a \).
mathematician.\footnote{He was also more honest than many colleagues. He not only avoids the false rules, when dealing with the problem type to which Dardi applies his second irregular rule, the present author (ed. Franci and Pancanti,\textsuperscript{21} p. 76) takes the \textit{thing} to be the value of the capital after one year, thus showing that the problem is fundamentally homogeneous. Further, when presenting (ed. Franci and Pancanti,\textsuperscript{21} p. 3–6) the arithmetic of the algebraic powers he accompanies the rules by numerical examples that show how things really work. If Giovanni di Davizzo had done that, his marvellous construction would have collapsed immediately.}

Luca Pacioli may have heard about the solution of particular higher cases by means of these specious roots, but in that case he does not seem to have appreciated them. In any case he goes on, after the statement that cases where the three algebraic powers that are present are not “equidistant” had not been solved so far, to admit that certain particular cases can be solved \textit{a tastoni}, “feeling one’s way”. There is another trace in Pacioli’s text of these solutions by special roots, which however he may not have recognized as such. Our anonymous author, as we remember, refers to “other roots than those one normally speaks about” in the plural, but only mentions one. In particular he does not speak about the \textit{radice pronica} which is referred to by several authors. Pacioli\textsuperscript{15} (p. 155\textsuperscript{v}) explains that by “pronic root”

one normally understands a number multiplied by itself and above its square add the root of the said number; of this sum that number is called the pronic root. As 9 multiplied by itself makes 81, and above 81 add the root of 9, which is 3, makes 84, the pronic root is said by practitioners to be 9.

This does not seem very useful, and does not seem even loosely related to the notion of “pronic numbers”, numbers of the type $n \cdot (n+1)$. However, in Pierpaolo Muscharello’s \textit{Algorismus} from 1478 [ed. Chiarini et al.,\textsuperscript{22} p. 163] we read that

Pronic root is as if you say, 9 times 9 makes 81. And now take the root of 9, which is 3, and this 3 is added above 81: it makes 84, so that the pronic root of 84 is said to be 3.

This makes better sense — according to Muscharello, $n$ is the pronic root of $n^4+n=n^3\cdot (n+1)$. Moreover, as we see, this pronic root can be used to “solve” equations of the type $CC+\alpha t = n$. It therefore seems plausible

Similarly for the other two cases.
that the cube root with addition was not the only non-fraudulent attack on higher-degree equations made by abacus authors before Pacioli's time.\footnote{Benedetto da Firenze [ed. Pieraccini,\textsuperscript{24} p. 26] also mentions the pronic root in his discussion of Biagio il Vecchio’s solution of the problem $CC + t = 18$, which he points out to be valid only for this particular parameter. It is not clear, however, whether the pronic root to which he refers is 4 (as Pacioli would have it), 2 (in agreement with Muscharello), or perhaps Biagio’s solution $\sqrt{18 + \left(\frac{1}{2}\right)^2} - \left(\frac{1}{2}\right)^2$, which is 4 (not 2, as claimed by Pieraccini in her preface (p. vi)); she overlooks that what Biagio asks for and expresses in that solution is a number which is posited as C). The coincidence of Biagio’s formula with Pacioli’s interpretation depends on the specific parameter 18, it should be noted.}

6. Some general characteristics of abacus mathematics

Before we discuss the implications of the material presented so far — which after all represents only a small although prestigious corner of abacus mathematics,\footnote{An expression of the prestige of algebra is found in Pacioli’s words when he comes to the presentation of the rules for the algebraic cases (Pacioli,\textsuperscript{15} p. 144r), “having come with the help of God to the much desired place, that is, to the mother of all the cases called popularly “the rule of the thing”, or the Great art, that is, a theoretical practice also called Algebra and almucabala in the Arabic tongue.”. The words “theoretical practice” (\textit{practica speculativa}) confirm what was derived from internal evidence on p. 4, viz. that algebra was “a purely theoretical discipline without intended practical application”. We shall still need to ascribe a more precise meaning to this.} far too difficult to be taught to the young students of the standard two-year course — we need to discuss some general characteristics of abacus mathematics.

Some of the orderly abacus treatises start by presenting the Hindu-Arabic numerals and their use (in multiplication tables, in the algorithms for numerical computation, and/or in particular divisions); others start directly by the rule of three.\footnote{Still other treatises are less ordered problem collections. Finally, some of the “abacus books” are not treatises at all in even the vaguest sense but private notebooks.} In both cases they show how things are or are to be done, without giving arguments for this. In particular in the case of the rule of three, this is noteworthy. We may look at the way the presentation is done in Jacopo’s \textit{Tractatus algorismi} (ed. Høyrup,\textsuperscript{7} p. 236f):

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.

Then follows the first example (\textit{tornesi} and \textit{parigini} are coins minted in Tours and Paris, respectively):

\begin{quote}
Benedetto da Firenze [ed. Pieraccini,\textsuperscript{24} p. 26] also mentions the pronic root in his discussion of Biagio il Vecchio’s solution of the problem $CC + t = 18$, which he points out to be valid only for this particular parameter. It is not clear, however, whether the pronic root to which he refers is 4 (as Pacioli would have it), 2 (in agreement with Muscharello), or perhaps Biagio’s solution $\sqrt{18 + \left(\frac{1}{2}\right)^2} - \left(\frac{1}{2}\right)^2$, which is 4 (not 2, as claimed by Pieraccini in her preface (p. vi)); she overlooks that what Biagio asks for and expresses in that solution is a number which is posited as C). The coincidence of Biagio’s formula with Pacioli’s interpretation depends on the specific parameter 18, it should be noted.
\end{quote}
vii tornesi are worth viii parigini. 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 $5/7$. And 25 parigini and $5/7$ will 20 tornesi be worth.

We notice that the intermediate product has no concrete interpretation (apart from the awkward and intuitively unattractive “as many times 9 parigini as there are tornesi in 20 tornesi”). If instead the division had been performed first, it would have been easy to explain $9/7$ to be the value of 1 tornese in parigini, for which reason 20 tornesi must be worth 20-(9/7) parigini. Alternatively, one might have explained that 20 tornesi must be worth 20/7 times as much as 7 tornesi, and hence (20/7)-9. These methods are not totally absent from the abbacus record, but they are uncommon.\(^c\)

In a somewhat similar vein, the Pythagorean rule is always presented as a naked rule, and the perimeter of the circle is simply stated to be $3^{1/7}$ times the diameter.

This does not mean that the abbacus treatises contain nothing but isolated rules. Firstly, dressed problems have to be analyzed in such a way that they can be reduced to the application of a standard rule, which means that abbacus mathematics is argued though not as thoroughly so as philosophers or modern mathematicians might prefer; secondly, the rules may also serve in more theoretical contexts. For instance, when Dardi wants to show how to divide 8 by $3+\sqrt{4}$,\(^d\) he first makes the calculation $(3+\sqrt{4})(3-\sqrt{4}) = 5$ and concludes that 5 divided by $3+\sqrt{4}$ gives $3-\sqrt{4}$. What, he next asks, will result if 8 is divided similarly, finding the answer by means of the rule of three ($5, 3-\sqrt{4}$ and 8 being the three numbers involved).\(^e\)

Even though abbacus mathematics does not in any way attempt to construct an axiomatic structure, these rules offered without proof but serving to justify other procedures hence function as axioms or postulates.\(^f\)

\(^c\) In contrast, the latter method is so common in Arabic treatises belonging to the same genre that it has a specific name, namely “by *nisba*” (“relation”, specifically “ratio”); thus, for instance, al-Karaji in the *Käft* (ed. and transl. Hochheim,\(^23\) II, p. 17), who states that he prefers it over the (rule-of-three) solution by “multiplication and division”.

\(^d\) Vatican manuscript, Chigi M.VIII.170, fol. 12v.

\(^e\) Similarly, the *Istratti di ragioni* from c. 1440 (ed. Arrighi,\(^26\) p. 26), purportedly consisting of extracts from Paolo dell’Abbaco who wrote a century before, teaches how to divide $4/5$ by $1/4$ by means of the rule of three.

\(^f\) A rather explicit and very simple instance of this function is found in Jacopo’s *Tractatus*
Jens Høyrup

But this is not the sole expression of the norm that mathematics should be not only argued but also consistent. Firstly, when different ways to solve a problem are presented, the identity of the outcomes may be followed by an explanation like the one Jacopo gives after having found the circular area first according to the normal “Arabic” formula \((1 - 1/2 \cdot 1/7) \text{diameter}^2\) and next as \((\text{diameter} \times \text{perimeter})/4\) (ed. Høyrup, p. 352):

And you see that it becomes as the one above, which we make without knowing the circulation around, which is also \textit{braccia} 44, and they become the same. And therefore I have made this beside that, so that you understand well one as well as the other, and that one as well as the other is a valid rule. And they go well.

Secondly, solutions to problems are regularly followed by numerical proofs (in the sense of “verifications”). At times these check directly that the application of the rule actually gives what is asked for; at times, however, only a more indirect check is possible, which means that the proof shows the compatibility of two approaches. In one such case, the Milan-Florence redaction of Jacopo’s \textit{Tractatus} observes (ed. Høyrup, p. 454) that “Thus we have made the alloy well, since we have found again the said 700 \(\delta\). It would have been a pity if we had found more or less”.

At times, the computations lead to approximate results — not only when the roots of non-square numbers are found but also, for instance,

\textit{algorithm} 15.2 (ed. Høyrup, p. 285) when the circle is treated:

\begin{align*}
\text{Always do, that when you know its circumference around, that is, its measure, and you want to know how much is its straight in middle, then divide its circumference by 3 and } & 1/7. \text{ And that which results from it, so much will its diameter be, that is, the straight in middle. And similarly when you know the straight in middle of a circumference and you want to know in how much it goes around, then multiply the straight in middle by 3 and } & 1/7, \text{ and as much as it makes, in so much does the said round go around. And if you should want to know for which cause you divide and multiply by 3 and } & 1/7, \text{ then I say to you that the reason is that every round of whatever measure it might be is around } & 3 \text{ times and } 1/7 \text{ as much as is its diameter, that is, the straight in middle. And for this cause you have to multiply and divide as I have said to you above.}
\end{align*}

There are indeed good reasons to maintain that being reasoned is an “institutional imperative” (Merton) for any institutionalized and cognitively autonomous teaching of mathematics (see Høyrup, pp. 109–12).

In modern elementary arithmetic we are accustomed to the need for rounding called forth by the use of decimal fractions, for which reason checks of many practical calculations will not be exact however correct the calculations. Since abacus mathematics operated with genuine fractions it did not encounter that problem, and exactness was therefore possible.
when discounting of a debt is computed by means of an iterative procedure or in application of *welsche Praktik* (a kind of combined division-cum-multiplication by stepwise emptying used in practical trade). In such cases I do not remember ever to have seen a proof. Consistency was apparently meant to be exact, and once approximations were made exactness could no longer be expected — approximation, so to speak, was a one-way street leading away from the world of consistency toward that of measurement and business.

All in all (and many more arguments could be given from the texts), the following norms or expectations\(^1\) can be seen to have regulated abbacus mathematics:\(^2\)

- it should, *in so far as authors and users could it respectively follow it*, be argued;
- it should be consistent;
- and it should be exact, unless some real-world application asked for approximation.

### 7. False rules revisited

How do these norms agree with the invention of the false algebraic rules? At the surface of things, not at all. Those of Gherardi can never have been argued in a pertinent way, and their inventor should have known so. Dardi’s irregular rules were certainly derived from arguments, but arguments which could never be told publicly because they would show *how* restricted their validity was, while only Dardi and those who copied them from him reveal at all that their validity was restricted. They were never tested by the inventors (or if they were the inventors did not betray themselves by telling the negative outcome), so the consistency they fulfil is merely that of superficial similarity.

\(^1\) In particular, I have never seen an analogue of the reversal of the approximate determination of a diagonal in the Old Babylonian text BM 96957+VAT 6598 x xxv (ed. Robson,\(^2\) p. 259), made by reversal of the approximation formula.

\(^2\) “Norms or expectations”: indeed, expectations concerning the object of the activity of abbacus masters (“mathematics”) are involved along with norms for the way these masters should act. It might be better to speak of an “ideology” belonging with abbaco mathematical practice, since an ideology is exactly to be characterized as an inextricable fusion of descriptive and prescriptive (supposed) knowledge (see Hayrup,\(^3\) p. 342).

\(^3\) It may seem somewhat circular to read norms from a text corpus and then (as we shall do) apply them to understand the mathematical practice on which the same the same corpus is based. However, the norms are read out of one part of the corpus (the scattered casual remarks, the basic level), and we shall discuss their impact on other parts of the corpus, in particular the algebra.
The display of wrong results is thus not to be understood (as is the sweeping unacknowledged copying from the writings of predecessors) as “what was generally done and accepted at the time/within the environment”. Instead it should be understood as a parallel of scientific fraud nowadays, which also exists, in spite of its conflict with what is expected from its perpetrators, and in the likeness of the economic fraud of Kreuger and the managers of Enron and Parmalat. The background is also the same. Abacus masters were in liberal profession, and had to impress municipal authorities or the fathers of prospective students if they wanted to earn their living. That could at best be done by solving problems that were too difficult for competitors; the prestige of algebraic problem solving (see note a, Section 6) made it an adequate instrument in that fight for distinction, and the inability of the judges to distinguish gilt lead from gold made it profitable to choose the easy way of fraud.

However, the fraud could only succeed because of the existence of those very norms which it violated. The general predilection for exactness barred checks of the approximate validity of the false solutions, and faith was instilled by the expectation that abacus authors had arguments for their mathematical claims even if their public — whether municipal councillors or fathers, perhaps even less brash competitors — felt themselves to be unable to follow these.\footnote{More or less in the same vein, readers of the present pages probably suppose that I have really consulted the unpublished manuscripts I quote and to which they have no access (I promise I have!).} In the same way, Enron could generate faith by being the client of prestigious accountant firms like Arthur Andersen and PriceWaterhouseCooper,\footnote{See, for instance, McNamee.} and by being apparently successful operators on a market supposed to be transparent by nature even though common citizens cannot look through it.\footnote{This is one aspect of what Robert Merton\textsuperscript{32} (pp. 439–59) baptized the “Matthew effect”. Similarly, because Cyril Burt was already famous when he started making his glaring statistical fabrications, for decades nobody noticed their character (admittedly, it also played a role that his “conclusions” — the intellectual superiority of the better classes — were politically convenient). Cf. Kamin\textsuperscript{33} (passim).}

Norm systems, indeed, are double-edged. They keep together a social body and regulate the behaviour of most members of the body; but they also allow those who hide behind them without complying with them to be far more successful than they could have been without the trust of others in the norms and their effectiveness — beyond regulation, norm systems
provide expectations, namely regarding the behaviour of others. No Tartuffe without religion and reverence for it!

8. Understanding Giovanni, and understanding more through Giovanni

Giovanni di Davizzo apparently did not know that his marvellous complete group had “no existence, if not that on the paper”, in Georg Cantor’s vicious words (Cantor, p. 501) about Veronese’s transfinite numbers. In so far he may have profited from the cover of the norm system without actually knowing that he disobeyed it. This is not very illuminating, scientific mistakes still are made today, and if nobody discovers them to be mistakes their authors may earn degrees, positions and prestige from them in good faith.

But there is something more to say about what Giovanni did. His expansion of the semi-group may be seen as a search for consistency — but then not only for consistency as a condition that had to be obeyed but as something which should be actively created. Since his scheme was taken over by others, a fair number of abbacus writers seem to have shared the norm that mathematical knowledge ought to expand — since the scheme was completely useless for practical as well as mathematically-theoretical purposes, they can have adopted it for no other reason. This norm agrees well with a passage in the introduction to Jacopo’s Tractatus (ed. Hoyrup, p. 195), copied more often than any other introduction by other abbacus authors and thus likely to correspond to prevailing moods:

...by mind and good and subtle intelligence men make many investigations and compose many treatises which were not made by other people, and know to make many artifices and written arguments which for us bring to greater perfection things that were made by the first men.

This wish for expansion of the art throws further light on the creation of the false solutions: whereas being able to solve (or give pretended solutions) to complicated algebraic problems gave prestige, prestige (probably more prestige) was specifically conferred to those who expanded the reach of existing algebraic knowledge. This is also the reason that many historians of mathematics tend spontaneously to see the fraud as praiseworthy because of the cognitive ambition it reveals, as pointing toward the breakthroughs of del Ferro, Tartaglia and Cardano. However, we should rather reverse this verdict. Those who committed the fraud consciously had no ambition to
Jens Høyrup

expand knowledge, just as modern scientific swindlers they were parasites on the cognitive ambition of others. They gained their prestige because of an existing norm system — but in fact, in so far as they succeeded in having their fraud accepted as good knowledge (and the abacus frauds went undetected much longer than the Piltdown fabrication), they undermined the creation of genuine new knowledge.

9. The power of the norm system

Some palaeontologists doubted the Piltdown man from the very beginning, and in the end this notorious potpourri of man and ape was exposed. Similarly, the invention of the “cube root with addition” shows us that not all abacus authors believed in the Gherardi solution to equation (G2). Further, the reductions of other equation types in the anonymous treatise in which we find this peculiar “root” explained shows that the norm for expanding the art consistently could lead to genuinely extensions of mathematical insight — extensions which, when combined with the breakthrough of del Ferro etc., led to the solution of all cubics and quartics in the sixteenth century.

A similar argument could be made (now in contrast to Giovanni’s “group”) around the way the same text (as well as Pacioli in his Summa) correlates the algebraic powers with powers of a number (see note f, Section 5). This led directly toward the arithmetization of the sequence of such powers — for instance, Bombelli’s arithmetical notation for powers, in which

\[ x^n \]

corresponds to our \( x^n \).

Not to be contrasted with any fraud or fallacy is the use of purely formal algebraic operations — another consequence of the faith in the consistency and expandability of mathematics.

In the above-mentioned Trattato dell’alcibra amuchabile from c. 1365 it is stated in direct words (ed. Simi, p. 41f) that the addition \( \frac{100}{a^2} \times 5 + \frac{100}{a^2} \times \frac{6}{4} \) is to be performed “in the mode of a fraction”, explained with the parallel \( \frac{24}{4} + \frac{24}{6} \). It is thus taken for granted that operations with

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\( ^a \) J. V. Field (private communication) suggests as a possible reference J. S. Weiner, The Piltdown Forgery (Oxford University Press, 1955), read with thrill in young age. My own original familiarity with the affair comes from Danish popular-science articles from the same epoch. A recent very full bibliography is Turrittin. \(^{35} \)
algebraic expressions could be handled exactly as numbers, and thus that for instance the notation for fractions was a mere form that could be filled out by any contents, numerical as well as algebraic. This formal use of the fraction notation could not be used by Dardi, since he had already chosen to use the same notation for multiples of \( \frac{\text{censo}}{\text{cosa}} \) (or \( \frac{\text{thing}}{\text{thing}} \)), writing the "denominator" below the "numerator" with a stroke in between — for instance, \( \frac{10}{3} \) for "10 things". Nor was the usage broadly accepted at first (nor understood by all those who copied material where it was used). In the longer run, however, mathematical writers got accustomed to it, and when Viète makes use of it in his In artem analyticen isagoge (ed. van Schooten, p. 7f), all he feels the need to explain is his geometrical interpretation — for instance, that \( \frac{\text{Bcubus}}{\text{Apiano}} \) is "the latitude which B cube makes when applied to A plane". When coming to the arithmetic of such fractions he just prescribes the customary operations for numerical fractions without mentioning this parallel as an argument — he appears simply to discover no difference.

The norm system which governed the practice of abacus mathematics was not identical with that of Greek-inspired Humanist and university mathematics, and it could not be already because the practices they governed were different in spite of similarities. For instance, a request for exactness could not mean the same in numerical computation and in geometry made exclusively by ruler and compass. But the two systems were suffi-
ciently similar to one another to allow a merger, not only of the two types of mathematical knowledge but also of the two norm sets. We may remember that both Maurolico and Clavius in their voluminous production also wrote on abacus matters although from the Humanist perspective, and that Clavius’s stance on the matter of exactness was more tolerant than that of, for example, Viète and the classicist Kepler, at least for a while. Without the partial merger of norms for what constituted legitimate practice, seventeenth-century scientific mathematics would hardly have been able to integrate the tools created by abacus algebra — and without the heritage from abacus algebra, it would have remained restricted to the possibility of finding something more of the same kind (perhaps brilliant, but not very much more) with respect to the Greek heritage, just as had been the case for medieval Islamic theoretical geometry. The total transformation of the mathematical enterprise taking place from Descartes to (say) Bernoulli would not have been possible.

Bibliography


† See Bos (pp. 33–5) for a convenient confrontation, and Bos (pp. 159–66) for the details of Clavius’s “idealizations of practical methods".
15. L. Pacioli, Summa de arithmetica geometrica proportioni et proportionalita (Paganino de Paganini, Venezia, 1494).


PHILOSOPHICAL METHOD AND
GALILEO’S PARADOX OF INFINITY

MATTHEW PARKER
Department of Philosophy, Logic and Scientific Method
London School of Economics
* E-mail: M.Parker@lse.ac.uk
http://lse.ac.uk/collections/philosophyLogicAndScientificMethod/

You are free, therefore choose — that is to say, invent.
Sartre, L’existentialisme est un humanisme

1. Introduction

Philosophy, and especially metaphysics, has often been attacked on either epistemic or semantic grounds. Anything outside of experience and the laws of logic is said to be unknowable, and according to Wittgenstein and the logical positivists, there are no such things to know; metaphysical disputes are either meaningless or merely verbal. This was thought to explain philosophy’s supposed lack of progress: philosophers argue endlessly and fruitlessly precisely because they are not really saying anything about matters of fact (Wittgenstein,1 Remark 402; Carnap2).

Since the mid-twentieth century, the tide has been against such views, and metaphysics has re-established itself within the analytic tradition. Ontology, essentialism, and de re necessity have regained credibility in many eyes and are often investigated by excavating intuitions of obscure origin. Relatedly, externalist semantic theories have claimed that meaning or reference has a secret life of its own, largely unfettered by our understanding and intentions (Kripke3,4 Putnam5,6). ‘Water’, it is claimed, would denote H₂O even if we had never discovered that particular molecular structure, and this is allied with the view that such structure is metaphysically essential to water — that water could not have been otherwise (Kripke3,4).

I wish to explore a third way, an approach to philosophical problems that is sympathetic to Wittgenstein and the positivists’ diagnosis of philosophy.
(“[P]hilosophical problems arise when language goes on holiday”; Wittgenstein, Remark 38), while rejecting their gloomy prognosis and Wittgenstein’s anti-interventionist prescription (“Philosophy leaves everything as it is”; Wittgenstein, Remark 124). I will call this third way the Method of Conceptual Articulation (MCA). In general, it consists in refining or modifying concepts, or engineering altogether new ones, so that an apparently “empty” question acquires a satisfying answer — or if you prefer, so that some related, more specific question emerges that has a definite answer and is relevant to some motivation. When we find that we have posed a question that we ourselves do not entirely understand, we should not demand, ‘Still, what is the true answer?’ but step back and ask, ‘What more precisely would we really like to know?’ In this way, even questions that are metaphysical in the pejorative Viennese sense, questions with no factual answers (if such there be), can nonetheless be answered, for they can be given cognitive content, and perhaps in a well motivated way. By refining or modifying our concepts and questions, I think we can “fill” some initially empty questions, and even solve philosophical problems, which I define here as finding definite answers that are relevant to our motivations.

This approach is partly inspired by, and endorses, a certain libertarianism that one finds in the views and practices of at least some modern mathematicians, namely the view that we are free to develop concepts and introduce objects as we wish, provided they do not spawn inconsistency. As Cantor put it, “Mathematics is entirely free in its development, bound only by the self-evident concern that its concepts be both internally without contradic-tion and stand in definite relations, organized by means of definitions, to previously formed, already existing and proven concepts” (Cantor, p. 79). On that view, whatever we can consistently define is a legitimate object of study. More recently, Wilder wrote of modern abstract algebra,

From this it is evident that the modern mathematician has lost the qualms of his forebears regarding the ‘reality’ of a ‘number’

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a The MCA has many precedents, perhaps most clearly in Carnap. But there the connection between ontology and language choice was treated as another way to dismiss metaphysics rather than rehabilitate it.

b Of course, this depends on how we individuate questions. If we suppose that any change in cognitive content (induced by a change in language or theory) implies that we are dealing with a different question, then trivially the content of a question can never change. But this seems to be a purely verbal issue; I do not think anything here rides on it.
(or other mathematical entity). His criteria of acceptance are of a completely different sort, involving such matters as consistency, utility of the concept, and the like. (Wilder, p. 148)

On that view, the reputed uncritical realism of working mathematicians does not in general limit their freedom, for it is no longer reserved for intuitively appealing structures like the natural numbers or Euclidean space. One internally consistent concept or mathematical theory is no more true or real than another. Hence Cantor wrote that if a proposed object satisfies his above conditions, “mathematics can and must regard it as existent and real” (op. cit.). To be sure, many mathematicians are concerned not only with consistency but with the legitimacy of the objects they introduce, in some broader sense that may depend on utility, intuition, elegance, and so on. Such practical and aesthetic concerns may even provide some evidence of consistency, which itself is usually quite difficult to prove, but they are not the same thing as consistency, much less reality or truth. For the libertarian, there is no question of the truth for a definition or axiom, for such things make no claim of fact. They only stipulate linguistic conventions and determine a domain of discourse.

As evidence that this has become a popular view among mathematicians, I would cite the emergence of non-Euclidean geometries, the trend in function theory from more to less restricted concepts of function (Jourdain; Maddy10,11) and the ascendance of the big-tent notion of set that Maddy calls Combinatorialism (ibid.), the latter two of which Cantor himself played important parts in. But if the reader is unconvinced, no matter; nothing I have to say here depends on it. I mention this libertarianism only to illustrate the kind of approach I have in mind, and as a significant element in Cantor’s views, which we will discuss at length. If libertarianism and the related MCA do not reflect the views and practices of most mathematicians, I would urge mathematicians to reconsider. We should study mathematical practice to determine what works and improve our understanding of mathematics. We should not regard the prevailing practice as sacred.

I believe that several philosophical problems have already been solved by means approximating the MCA, but rarely deliberately. Those who have solved philosophical problems by articulating new concepts have typically thought that they were discovering deep facts, not stipulating definitions. Still, in several cases, a problem was in fact solved by articulating concepts that addressed concerns more specific than the initial question. One example was the problem of the world systems, ultimately put to bed by
Newton’s refinement of the concept of motion and his successful theory of gravitation. Another lies in recent extensions of decidability to the continuous context (Myrvold; Parker). A more overtly metaphysical example that quite clearly employs the MCA is Parfit’s work on personal identity (Parfit). I hope to discuss these and other examples elsewhere. The one I will consider here is Cantor’s extension of the concept of number to the transfinite, and the resolution this supplies for “Galileo’s Paradox” (Galileo), namely that the square numbers seem to be at once fewer than and equal to the positive integers.

There, too, the MCA was not applied deliberately. The historical figures discussed below — Galileo, Bolzano, and Cantor — did not see themselves as altogether freely stipulating useful new conventions, but either as drawing conclusions about the relative size of infinite collections, or, in Cantor’s case, as extending the concept of numerosity in a constrained way. Nonetheless, key elements of the MCA are reflected in some of their remarks and arguments. I will argue in light of their writings that, whatever those authors may have thought, questions of transfinite numerosity were in certain senses indeterminate, and Cantor’s extensions of numerosity were stipulated more freely than some of his remarks would suggest. His stipulations — in particular the notion of power — not only served to resolve Galileo’s Paradox (which Galileo and Bolzano had also done, in different ways), but at least partially solved the broader philosophical problem of transfinite numerosity insofar as it helped to address major background concerns. The main evidence that the MCA can work, then, is that it has worked. (Note that I do not claim that the MCA is part of standard math-

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\(^c\) DiSalle reads Newton’s Scholium to the Definitions in the *Principia* as giving a *definition* (presumably stipulative) of absolute space and absolute motion. This would fit wonderfully with my methodology, but it does not seem to fit Newton’s text. Newton seems rather to have made metaphysical *claims about* absolute space and motion. Still, such claims performed the function of giving those notions empirical content and rendering the Copernican question determinate.

\(^d\) The paradox far pre-dates Galileo; see footnote a, Section 3. I use the word ‘paradox’ throughout in the sense of a contradiction engendered by otherwise plausible suppositions. (Assuming the law of non-contradiction, I do not see what else a paradox could be.)

\(^e\) Cantor himself paid little attention to that paradox, but he did present a version of it (with the square numbers replaced by the even numbers), not as a paradox but merely an illustration of a property of powers (Cantor, pp. 242–3). He also alluded to the same phenomenon in a couple of other places, as we will see.

\(^f\) Throughout this essay I use ‘numerosity’ to denote the general notion of cardinal number, i.e., *number-of-elements*, without presupposing Cantor’s analysis of that concept. Cantor’s “cardinal number” will be called power, as he initially called it.
If indeed the MCA has sometimes been successful in the realm of mathematics, there is a further question as to whether it can be (or as I suggest, already has been) useful in more general metaphysics. Of course, the problem of the infinite is traditional metaphysics par excellence. Nonetheless, I will not argue here for the broader applicability of the MCA. I have mentioned some applications that I wish to discuss elsewhere, and I hope to apply it to others as well. The broad success of the method can then be evaluated in terms of those applications. Here let us bracket that question and focus on mathematics.

In Section 2, I further articulate and contextualize the method. I describe a roughly Wittgenstinian picture of concepts and Cantor’s related notion of concept splitting. I then state a naïve version of the method, raise some possible objections, and finally mention a modified method that avoids most of the difficulties. Section 3 reviews Galileo’s Paradox and his motivations for presenting it. There I argue that, in concluding that the concept of relative size cannot be applied to the infinite, he was in a certain sense right, and Cantor, in claiming that there is no contradiction in cases like Galileo’s Paradox, was wrong. In Section 4, I review Bolzano’s position, that proper subsets are always smaller and bijection is not sufficient for equinumerosity. I argue from Duggan’s order extension theorem that Bolzano was not simply mistaken; the relation of proper subset can be extended to a general concept of number quite different from Cantor’s. Section 5 evaluates Cantor’s methods, his own perspective on his work, and the success of his theory in addressing some major concerns common to all three of our historical figures. Section 6 briefly criticizes Gödel’s arguments for absolutism about the concept of cardinal number and touches on some more recent debates about realism in mathematics. Section 5 concludes with brief summary remarks.

2. The method

The approach to philosophical problems considered here emerges from a certain picture of concepts and of how philosophical problems arise, a picture

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8 This is not an attack on Cantor’s theory. The point is just that his theory required a conceptual innovation in order to escape Galileo’s Paradox (and it was not the only such innovation possible).
derived from Wittgenstein, Waismann, and to some extent Kant. (Kant suggested that the antinomies arise from stretching our concepts beyond their proper domains ([Critique of Pure Reason] Aii ff.), an idea anticipated by Galileo\(^\text{20}\) (p. 31, quoted below).) Elements of this picture can even be found in Cantor’s remarks, as we will see. Wittgenstein pointed out in the early sections of the *Investigations*\(^1\) that many of our concepts do not have any tidy set of necessary and sufficient conditions. He used the metaphor of a rope, which derives its unity from its many overlapping fibers rather than a single pervasive thread. Another apt metaphor would be that of a well worn rag. Typically, our informal concepts are woven of many strands: various conditions or properties, similarities between instances, and different approximately equivalent characterizations. They fray at the edges, where borderline cases arise (“degree vagueness”, as Alston\(^\text{23}\) called it in 1964). They also have holes: cases that do not lie on a fuzzy boundary but rather are omitted from classification altogether (“combination-of-conditions vagueness”, ibid.).\(^a\) They can be stretched to cover new cases, but, as Cantor noticed, *stretched too far they will tear* (Fig. 1). When we extend a concept beyond its usual domain, we may find that it comes apart, so that some characteristic conditions are no longer mutually consistent, or various formulations are no longer equivalent.

Cantor pointed out just such a case. He of course extended the concept of number in two directions, that of *Anzahl*\(^b\) (later called ordinal number)

\(^a\) Alston’s “degree vagueness” is the sorites type, consisting in “the lack of a precise cut-off point along some dimension” (Alston,\(^\text{23}\) p. 87), while “combination-of-conditions vagueness” consists simply in the indeterminacy of the truth conditions for a term (pp. 87–88). Waismann’s\(^\text{24}\) famous notion of open texture is related. Originally called *Porosität* — literally, ‘porosity’ — it is, on one reading, just what I mean by holes, i.e., combination-of-conditions vagueness. But as Ackerman\(^\text{25}\) points out, Waismann further distinguishes open texture as *ineliminable*; no definition can completely remove it. Our rag picture is partly motivated by a suspicion that such *ineliminable* vagueness exists and is even the rule, but that is inessential to the present considerations. Such vagueness should also be distinguished from outright category errors. The emptiness of ‘What time is it on the sun?’, for example, is not due to vagueness but to a *define* inapplicability. Nonetheless, it seems we could modify the concept of time-of-day to cover that case.

\(^b\) Cantor also described *order type* (a generalization of *Anzahl*) as the natural extension of the concept of number (Cantor,\(^\text{26}\) p. 117). There is some disagreement about the sense of ‘*Anzahl*’ in Cantor’s hand. Ordinarily this word is translated as ‘number’, ‘cardinal number’, or ‘number of elements’, contradicting Cantor’s later designation of his *Anzahlen* as the *ordinal* numbers. Tait on the other hand, reads *Anzahl* as ‘counting number’ (Tait\(^\text{27}\)\), and elsewhere (Tait\(^\text{28}\)) treats this as a synonym for ‘ordinal’. Cantor did refer to an *Anzahl* as the result of counting (Cantor,\(^\text{7}\) p. 75), but note that this is consistent with regarding it as a measure of numerosity (relative to an ordering). To avoid any anachronism or prejudice, I will simply use ‘*Anzahl*’.

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Matthew Parker

Figure 1. A concept that has been stretched too far

and that of power (Mächtigkeit, later called cardinal number). For any finite set, Cantor observed, Anzahl and power coincide and determine each other, but not so for the infinite; two infinite sets can have the same power but different Anzahlen. Hence, as Cantor put it, “the whole concept of number . . . in a certain sense splits up into two concepts when we ascend to the infinite” (Cantor,\textsuperscript{7} p. 78, Cantor’s emphasis). In fact, it splits into more concepts than that, for other notions of transfinite number are possible, as I will explain in Section 4. (Besides those discussed here, another alternative notion is given in Buzaglo.\textsuperscript{29})

When concepts split, we may be puzzled as to what we really had in mind in the first place. Which criteria truly characterize the original notion? But this is often a misguided question. The original appeal of the concept, in its established domain of application, may have been due to the concurrence of several conditions or characterizations. For example, part of the value of ordinal number, in the sense of ‘position in a sequence’,\textsuperscript{6} is that for finite sets it coincides with cardinal number or numerosity. This after all is what makes it possible to count; we correlate finite numbers with the elements of the set being counted, and the last position we reach in the sequence of numbers

\textsuperscript{6} Cantor’s “ordinal numbers” are not merely positions in a sequence. They are ordered sets of “units” that represent the “order type”, in effect the structure, of a well-ordered set (Cantor\textsuperscript{26}). They are not the grammarian’s ordinals.
indicates the numerosity of the set. Thus, both cardinal number and ordinal, and the fact that they concur, and as well the condition that proper subsets are always smaller — all of these and more are what truly characterize the original concept of number. When such concurrent conditions diverge, there may be no uniquely right way to extend the concept, and hence a question that stretches a concept beyond its usual domain may have no uniquely correct answer. To obtain answers, we have to refine or modify the concepts involved.

So far we have made free use of the notion of “the concept of X”, but this requires clarification on a number of fronts. In general we will use ‘concept’ to denote some disjunction of conditions. Like Frege, we have in mind a logical object, not a psychological one. But to speak of the concept of number, for example, leaves open the question of which conditions count. They might be those associated with the word ‘number’ by everyone in some community, or by the “competent” speakers of a language (which requires further clarification), or by a particular individual. They may be the conditions regarded as constituting the meaning of ‘number’, or they may include all commonly held beliefs about number. Or, the concept of number might be something more objective, a somehow distinguished set of conditions that we may not even be aware of. These distinctions will be helpful in understanding the views of our historical figures. Like typical concepts, the MCA can be refined in various ways. One naïve version is as follows:

**The Naïve Method of Motivational Analysis (NMMA)**

1. Establish that the question at hand, as stated, has no uniquely determined answer.
2. Identify background motivations for the question, either practical or theoretical.
3. Refine or extend the concepts involved in the question so that under the amended concepts, the question does have a determinate answer and is relevant to the background motivations.

Some clarifications are in order.

There are different senses in which a question can have no unique answer. Olson suggests that even vague questions have definite answers, for if a case is vague, then the assertion of vagueness is itself the uniquely correct answer. But if we ask, as in our example below, ‘Is A greater than B’, then ‘The question is vague’ is not in the normal sense an answer to the question, for the question presupposes an answer of ‘yes’ or ‘no’. The
assertion of vagueness is just a way of denying that there is a uniquely correct answer. Other responses, such as ‘Those terms do not apply’, and ‘Only to degree x’, are quite different from the former. They do not assert semantic indeterminacy, but a category error; they imply an established usage that definitely rules out a simple yes-or-no answer. Such differences impinge on the nature of a conceptual amendment, for if the question is genuinely indeterminate, there is room to refine or extend concepts without transgressing established limits on usage, but not if the question commits a definite category error. Still, even if a particular application of a concept or word is definitively ruled out, we may be able to extend its domain nonetheless, changing its content. (I will explain shortly why this is not obviously true.) The main purpose of step (1), besides removing the temptation to keep looking for straightforward answers (e.g., yes or no), is to prevent us revising concepts that are already doing good work (though sometimes non-cumulative revisions are necessary). But there does not appear to be much danger in extending a concept beyond previously imposed limitations of scope, however definite, so long as logical consistency is maintained.

A question that arises about step (2) is, whose motivations should we consider? The method is intended to serve those who apply it, so in using it one should consider one’s own motives. But we can choose our motives, and one might choose to address someone else’s concerns. So in general, the goals considered might be anyone’s or even no one’s, but the success of a conceptual innovation or refinement — the question of whether it constitutes a solution in the sense I have given — is then relativized to those goals. To address the historical question of whether the method was successfully applied to a particular problem, we merely ask whether anyone’s motives were considered, and whether the sharpened question addressed those motives. It matters little, for that purpose, whose motives they were, but then the resulting answer is only a solution relative to those motives. In the present case, we will see that all three of our historical figures had related motives that were indeed addressed by Cantor’s solution.

When we can apply the NMMA, we really ought to. The steps themselves imply that the method will succeed in a way that addresses what is important to us, as long as the steps can be carried out, and step (1) protects us from doing damage to other useful concepts and theories. But there are several reasons to worry whether the method can be executed. The presupposition that we can determine whether a given question has a

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\^ Thanks to an anonymous referee for raising this issue.
unique answer is challenged by Quine’s\textsuperscript{31} attack on the analytic/synthetic distinction (1951), externalist semantics (Kripke;\textsuperscript{3,4} Putnam\textsuperscript{5,6}), and the mere fact that analytic philosophers have struggled a hundred years or more to discern meanings and meaningfulness, with limited success. The threat from externalism is that we might not have epistemic access to the meaning or reference of our own expressions. If water denoted $\text{H}_2\text{O}$ long before hydrogen and oxygen were even discovered, as Kripke and Putnam claim, then how can we be sure what our own words denote, and hence whether a given question is really empty or not? The NMMA also presupposes the apparent truism that we are masters of our own language — that we can revise our concepts to make our expressions mean whatever we want. This too is challenged by a form of externalism, namely Lewis’s notion of a reference magnet, something that draws reference to it in virtue of its natural “eligibility” (Lewis;\textsuperscript{32,33} Hodes;\textsuperscript{34} Sider\textsuperscript{35,36}). How strong after all are these magnets, and can we override them?

A further worry is that we might not be able to perform step (2), to identify motivations more specific than the initial question. In fact, it is characteristic of philosophy and pure science that the main goal is extremely general: to understand. In the spirit of pure investigation we often pose puzzles without knowing exactly what we are looking for or having any specific purpose in mind. Wittgenstein provides an apt (and peculiarly bawdy) illustration: many problems are “like the problem set by the king in the fairy tale who told the princess to come neither naked nor dressed, and she came wearing fishnet . . . He didn’t really know what he wanted her to do, but when she came thus he was forced to accept it” (from a lecture quoted in Ambrose\textsuperscript{37}).\textsuperscript{e} Moreover, even if there are distinct background motivations, it may be very difficult to discern them until after a solution is given. Otherwise there would not be much of a philosophical problem.

Supposing we can identify motivations, the final step is to construct concepts that will make our question determinate and relevant. In some cases this may be easy, but in others, it may require superhuman foresight, and this likely describes Cantor’s case. Among the background motivations for his theory were desires to understand the structure of continuous spaces and other infinite point sets, the representability and integrability of functions, and the relations between numerosity and geometric magnitude (e.g., length or volume).\textsuperscript{f} To foresee that the concept of power would be so useful

\textsuperscript{e} I do not, like Wittgenstein, think this characterizes all mathematical problems, but many philosophical ones.

\textsuperscript{f} As we will discuss, Ferreirós\textsuperscript{38} argues that Cantor’s dominant motivations lay not in
to those ends would have required genius beyond even Cantor’s, and as we will see, Cantor only recognized the great importance of the concept, and adopted it as a notion of number, gradually, as applications occurred and a rich theory developed.

A final grave worry is that the method does not accurately describe the history of the example under consideration: Galileo’s Paradox and its resolution. Indeed, for the most part it does not fit the views of the participants. But I will argue that in fact, extensions of the concept of numerosity were freely stipulated; that Cantor developed his concepts of numerosity gradually, in light of motivations, applications, and results; that he had certain motivations in common with Galileo and Bolzano; and that his concepts of numerosity proved particularly pertinent to those motivations.

Some of the above difficulties can be avoided by restricting our attention to matters of logical consequence rather than truth. Given a particular set of concepts, expressed as a set of propositions, i.e., a theory, we may well be able to determine whether or not an answer to a given question follows from that theory, along with other, uncontroversially determinate propositions and the standard laws of logic (or some other set of laws if you like). We need not distinguish between definitions in the theory and factual hypotheses; just throw them all in. Whether or not there is a genuine analytic/synthetic distinction, the determinacy of the answer to a question relative to a given theory does not depend on it. We need only remember that such determinacy or indeterminacy is then relative to a theory (and the laws of logic employed, if those are not in fact immutable). Furthermore, reference no longer enters into the matter. Even if we cannot tell whether reference determines an answer to our question, we may still be able to establish logical independence. We need only find two models of the theory which give different answers to the question.

We could generalize our method further and avoid even more of the difficulties. Given a seemingly unanswerable question, we might proceed roughly as follows:

mainstream mathematical concerns like function theory but in metaphysics and natural philosophy. However, he certainly had these more specific mathematical goals as well, and some were stimulated by his broader motivations.

We may simply stipulate a set of propositions assumed to have determinate truth values. For the logical positivists, these were observations or sense data; for Newton’s problem of giving empirical meaning to absolute motion, they would have been the propositions of relative position and motion; here they include the relations of relative size among finite sets, and the relations of proper subset-hood and 1-1 correspondence among infinite sets.
The Generalized Method of Theory Revison (GMTR)\textsuperscript{b}

(1) Propose a theory.
(2) Attempt to deduce an answer to the question.
(3) Evaluate the fruits of the theory (especially the motivations that it serves).
(4) As with shampoo, repeat as necessary.

Here we omit the step of showing that the question has no determinate answer, for even if it does, we can, if we wish, just propose a new theory that better serves our motivations. We also avoid the problem of identifying our background motivations and engineering appropriate concepts in advance. We can just as well propose a theory first and then examine the interests that it serves. Note that the purpose of step (3), ‘Evaluate the fruits’, is not to judge the legitimacy of a theory or conceptual innovation, much less its truth or reality. In the context of mathematics, that would contradict the libertarianism I have advocated above: the claim that one logically consistent mathematical concept is no more real or true than another. But we are concerned here with developing concepts or theories that serve our motives. The point of step (3) is to determine whether the problem has been solved in that sense.

The question of whether we can override reference magnets is still troubling. We may propose a theory that answers our initially mysterious question, but reference magnetism might imply that the resulting theory is not in fact about its intended subject, and it may consequently be false even if it is true of its intended subject. However, it is hard to see how such considerations would bear on our understanding. Say for example we want to have a theory about a clear liquid with molecular structure XYZ. We construct a theory of XYZ and deduce lots of enlightening consequences. But suppose that, despite our intentions, the theory is really about H\textsubscript{2}O, because H\textsubscript{2}O is a very strong reference magnet, and suppose our theory is false with regard to H\textsubscript{2}O. What does it matter? The consequences we have deduced are still true of their intended referent, as desired. If somehow they are not true simpliciter, that would seem, in this case, to be an irrelevant technicality. How can reference matter here if it plays no role in

\textsuperscript{b} Of course, this is just the standard hypothetico-deductive method of empirical science, with the usual step of testing predictions radically generalized to “Evaluate the fruits”, but it is meant to apply as well to mathematical and philosophical theories, in order to evaluate, not their truth, but their interest and usefulness.
our understanding or our use of language? In any case, the GMTR fits more easily than the NMMA with historical examples, including the one discussed below. Cantor did not argue that the question posed by Galileo’s Paradox was initially empty, nor regard himself as freely stipulating the nature and existence of transfinite numbers, but he certainly did propose a new theory that provided a solution to the paradox, derive consequences, and evaluate the fruits. Of course, this is not saying much. The GMTR is so loose that nearly any theoretical development will instantiate it. The important point is that mysterious philosophical questions can thus be made determinate and relevant to our concerns. By augmenting or revising our language or theory, we can obtain answers that bear on our broader purposes.

However, the GMTR does not so much resolve our difficulties as dodge them, and by abandoning step (1) of the NMMA, it threatens to undo with willy-nilly revisions as much progress as it achieves. In what follows I will leave the GMTR aside and try to exhibit the extent to which the resolution of Galileo’s Paradox can be assimilated to the NMMA or something close to it. But I will focus on logical implication, ignoring worries about externalism and reference magnets until we reach the discussion of Gödel’s realism. After all, if, as I argue, something close to the NMMA has in fact worked, there is little to fear from the objections I have mentioned.

3. Galileo

Galileo points out in his last dialogue (Galileo, p. 32) that the square numbers (1, 4, 9, 16, . . . ) are clearly fewer than the “numbers” (the positive integers 1, 2, 3, 4, . . . ), for the latter include the squares as well as many

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1 I have ignored here serious questions about just what a theory of reference is supposed to assert — whether it makes genuine claims of fact or rather proposes a convention of interpretation, whether it is supposed to be completely adequate or a limited toy model, etc. (One might argue that we can diagonalize our way out of any given theory of reference just by stipulating that in certain cases reference will work differently.)

3 There are further questions as to which propositions are essential to a theory, and which ones are not only determined by the theory but meaningful in some further sense. We might, for example, add to Newton’s theory of gravitation the statement ‘Discontent is orange’, making that sentence part of a useful theory, but in a clearly ad hoc and unhelpful way. We would like to have some way of distinguishing such inessential appendages to a theory from its integral, functional elements, but that is essentially the problem that hobbled logical positivism (Hempel39) even before Quine’s “Two Dogmas” 31. Perhaps the “evaluate” and “repeat” steps of the GMTR can help, but I make no attempt to resolve the problem here. I only make the modest claim that a statement can sometimes gain determinacy and relevance in virtue of a new concept or theory.
more. Yet, he goes on to show, these two collections are equal, since they
 can be placed in a one-to-one correspondence; just match each square with
 its root. So the two collections are at once equal and unequal.

Galileo’s protagonist Salviati concludes that infinities “transcend our finite un-
derstanding” (Galileo, p. 26), and “the attributes ‘equal’, ‘greater’, and ‘less’,
are not applicable to infinite, but only to finite, quantities” (p. 32).

Some may regard this as simply a naïve mistake, though excusably so,
given its date. Not all readers will agree, but often it is taken for granted
that Cantorian set theory resolves the paradox in the only way possible.

As we now know, some will say, two sets are equal in numerosity if and
only if they can be placed in one-to-one correspondence. The fact that
the set of integers is thus equal to a proper subset of itself is just an odd
phenomenon characteristic of infinite sets, which any suitably modern and
open-minded individual will accept once accustomed to it. Gödel held
this view, and Cantor himself said, “There is no contradiction when, as
often happens with infinite aggregates, two aggregates of which one is a
part of the other have the same cardinal number”, and further, “I regard
the non-recognition of this fact as the principal obstacle to the introdution
of transfinite numbers” (quoted in Jourdain, p. 75; my emphasis).

But in a clear sense, Salviati was right and Cantor was wrong.

The concept of relative size with which Salviati and his author were
equipped, taken in whole, cannot be applied consistently to infinite sets.
For Galileo, this concept involved at least two principles:

**Euclid’s Principle** (Common Notion 5): The whole is greater than the
part (i.e., strictly greater than any proper part).

**Hume’s Principle**: Two collections are equal in numerosity if and only

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a A nearly continuous family of similar paradoxes going back to the distantly
related Wheel Paradox from around the 4th century BCE (Sambursky; Murdoch;
Gardies; Duhem; Thomas; Rabinovitch). Notably, Duns Scotus, around 1302,
compared the odd and the even numbers to the whole numbers and even anticipated
Cantor in rejecting what we will call Euclid’s Principle (Gardies, pp. 45–6). Gregory
of Rimini, around 1346, adopted an approach surprisingly close to the MCA: he dis-
tinguished two senses of ‘larger’, the “improper” one corresponding to Euclid’s Prin-
ciple, and the “proper” corresponding to what we will call Hume’s Principle (Duhem,
pp. 111–2). However, he offered this more as a conceptual analysis than an innovation,
and still suggested by his ‘proper/improper’ terminology that the Humean notion of
larger was the uniquely correct one.

b According to Duhem (p. 89 ff.), this was also held by Duns Scotus and several
subsequent medievals.
if their members can be put in one-to-one correspondence.

In daily life, the collections we reckon about are mostly finite (if conceived as collections — excluding regions of space and the like), and like Anzahl and power, the above principles always agree in such cases. For those not studied in these matters, the two principles are not distinguished at all; they are integral parts of a single concept, which we divide only in post hoc “analysis”. Galileo’s own inability to separate the two principles is evidence for the unity of this pre-theoretic concept. Though he does implicitly suggest the conflict between them, and thus their distinctness, he never considers the possibility of a notion of size under which one of the principles fails. Both principles appear to have been firmly entrenched in his conception of numerosity, and experience with students shows that the same is true for many today. Such a concept of numerosity, as Galileo showed, cannot be applied to the infinite.

Cantor was wrong in that there is a contradiction when an aggregate and a proper part of it have the same cardinal number, namely the contradiction between the above two principles. Cantor only avoided this by abandoning the first. Of course, he might have meant that there is no contradiction under his technical concept of cardinality, but then to call this cardinal number, in the general sense of numerosity, just ignores the fact that Euclid’s Principle was so deeply ingrained in our intuitive notion of (or entrenched beliefs about) numerosity. Furthermore, such a reading would make his next remark about “the non-recognition of this fact” very strange. How could anyone have recognized a fact about his technical concept before he introduced it?

The dogmatic view that Cantor’s analysis was right and those of Galileo and Bolzano (the latter discussed below) mistaken is fairly common today,

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\(^c\) Galileo did not spell out these principles, let alone call them by these names, but they are clearly implicit in his statement of the paradox. In recent debates on neo-logicism (e.g., Hale and Wright;\(^{48}\) Demopoulos\(^{49}\)), ‘Hume’s Principle’ usually refers to Frege’s implicit definition of the numbers, stating that two classes have the same number if and only if they can be put in 1-1 correspondence (Frege,\(^{50}\) p. 73). What Hume actually wrote is, “When two numbers are so combin’d as that one has always an unite answering to every unite of the other, we pronounce them equal” (A Treatise of Human Nature I, iii, I). ‘Number’ here is taken to mean ‘set’ or something like it (Tait,\(^{28}\) p. 241; Demopoulos,\(^{59}\) p. 109), so Hume is merely defining equality, not introducing numbers as objects. Tait\(^{28}\) objects to the phrase ‘Hume’s Principle’ in application to infinite sets, since Hume himself disavowed the infinite, but for us, the question of whether, and how univocally, to extend Hume’s finitary principle to the infinite is at issue.
perhaps due in part to Gödel’s arguments (also discussed below).\footnote{Frege and Russell were also committed to power as the essential concept of numerosity (Frege,\textsuperscript{50} p. 73, p. 98; Russell,\textsuperscript{51} chapter IX).} The MCA suggests a more pluralistic resolution of the paradox (one that some readers may regard as obvious and standard, but others will disagree). There are at least two ways to characterize the relative size of sets, namely Euclid’s Principle and Hume’s. Euclid’s defines what we might call the ‘greater’ of inclusion: one set is $\text{greater}^{\text{inc}}$ than another if it properly includes the other. (This obviously has very limited application, but as we will see, it can be extended.) Hume’s suggests the ‘greater’ of power: one set is $\text{greater}^{\text{pow}}$ than another if there is a bijection between the latter and a subset of the former, but not between the former and a subset of the latter. Only $\text{greater}^{\text{pow}}$ has generated a rich theory of relative size for arbitrary sets, and only $\text{greater}^{\text{pow}}$ concerns the intrinsic size of sets as sets, independent of ordering or any other property imposed on a set or derived from the nature of its elements. Nonetheless, both notions are in some degree legitimate heirs to the pre-theoretic notion of numerosity, in virtue of the entrenchment and seemingly analytic status of our two principles. Cantor’s notion of cardinality is not the uniquely right concept, but a particularly elegant and useful one. This pluralistic resolution harmonizes with Cantor’s own notion of concept splitting and his professed conceptual libertarianism, yet it is clear from the above quotes, and other considerations below, that this is not how Cantor himself always saw the matter.

I have said Galileo was right that his concept of number did not apply to the infinite, but this does not contradict the conceptual pluralism I am endorsing. Galileo was right about his concept, understood in terms of his tacit commitment to both principles, i.e., his disinclination to consider rejecting one. Further, this was apparently the conception of many, for several before him drew the same conclusion, that ‘more’ and so on do not apply to the infinite, and few suggested the possibility of a ‘more’ that violates one of the principles (Duhem;\footnote{cf. notes a, b, Section 3}) Yet, understood in terms of common, explicit conventions, “the concept” of numerosity was unsettled on the question of whether both principles must be upheld simultaneously in the infinite case. The matter was disputed. (See note a, Section 3.)

Galileo came rather close to step (1) of the NMMA. He concluded from his paradox that the notion of numerosity simply does not apply to the infinite. It is not that all infinite sets are equal, as he makes clear in the
above quote and elsewhere (Galileo, p. 33). The concept of relative size does not apply at all. One might take this to mean that the question of whether the squares are fewer than the positive integers or not has no determinate answer, but this is not quite right. Galileo gave an answer: 

“[N]either is the number of squares less than the totality of numbers, nor the latter greater than the former” (p. 32). So for him, the answer to all such questions is ‘No’ — or perhaps, if we do not read the last quote too closely, ‘We cannot speak of such relations among infinities’ (p. 31). In any case, a ‘yes’ is strictly ruled out. If we ask what was the answer according to widely acknowledged, explicit conventions, then there is room for a ‘yes’ or a ‘no’, but Galileo takes the paradox (derived from his tacit principles) to establish a more definite answer.

We can reasonably say that step (2) is present in Galileo’s discussion, for he makes the concerns behind his paradox quite clear. The section of the Dialogues in which it appears proposes a notoriously speculative and unsuccessful explanation for the cohesion of bodies: that a solid contains infinitely many miniscule vacua, and it is nature’s resistance to these vacua that somehow accounts for the rather strong cohesion of solids. (He describes an ingenious experiment showing that the force engendered by a single macroscopic vacuum is not enough.) This account presupposes that a solid is composed of infinitely many indivisible parts. But to that there is an old objection (Murdoch), which Simplicio, the Aristotelian antagonist in the dialogue, puts in terms of lines rather than solids. If a line is composed of infinitely many parts, then a longer line has an even greater infinity of parts, which seems absurd to the pre-Cantorian. “This assigning to an infinite quantity a value greater than infinity”, Simplicio says, “is quite beyond my comprehension” (Galileo, p. 31). Galileo has Salviati reply as follows (anticipating Kant’s idea that antinomies arise from stretching our concepts beyond their proper domains):

This is one of the difficulties which arise when we attempt, with our finite minds, to discuss the infinite, assigning to it those properties which we give to the finite and limited; but this I think is wrong, for we cannot speak of infinite quantities as being the one greater or less than or equal to another. To prove this I have in mind an argument... (ibid.)

The existence of the infinitely many vacua is argued from an ancient relative of the paradox itself, namely the Wheel Paradox of the pseudo-Aristotelian Mechanica.
And here the paradox appears. Hence there is little doubt about its purpose: to show that relative size does not apply to infinite collections, and thus to defeat Simplicio’s objection to the particulate analysis of continuous bodies. More generally, in seeking to escape the line paradox, Galileo was concerned with the puzzling relations between numerosity and geometric magnitude. Thus, what he required from a notion of transfinite numerosity was to illuminate those relations, to determine whether a continuum can coherently be decomposed into infinitely many indivisible parts, and ultimately, to determine whether an infinity of point-vacua can somehow account for the strong cohesion of bodies. As we will see, Bolzano and Cantor too were much interested in the relations between numerosity and magnitude, the nature and structure of continua, and even physical applications.

In effect, Galileo also executed step (3), but with limited success. By declaring that the language of relative size does not apply to the infinite, he was supporting one proposed refinement of the public concept of numerosity. He does not appear to have regarded this as a stipulation; he instead took it to be proved by the paradox, tacitly taking both Euclid’s and Hume’s Principle for granted. Nonetheless, he asserted a clear boundary where none was publicly established, and which he was therefore free to impose without contradicting any established rules or theory. This did at least partly determine an appropriate response to his implicit question whether the squares are fewer than the wholes: either ‘No’ (for no such relations hold among infinite sets) or, ‘That concept does not apply’. However, the bearing of this result on his background motivations was limited. It did imply a kind of degenerate analysis of the relations between magnitude and numerosity, namely that infinite sets have no relations of greater and lesser numerosity, regardless of the magnitudes of the wholes they compose. But

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\[1\] One may wonder how one paradox can refute another very similar one, but Galileo’s Paradox accomplished this in two ways: First, it showed that the line paradox does not arise from the continuous nature of lines, since Salviati’s version concerns the discrete set of whole numbers. Denying the particulate analysis of continua does not resolve the square number paradox, so a more general solution is needed, and Galileo offers one. Secondly, the number paradox provided reason to think that Simplicio’s argument was invalid. Salviati’s paradox involved two collections, the positive integers and the squares, which very plausibly do exist and which do consist of infinitely many parts, with much the same puzzling results as the line paradox. If we accept this much (though one might not, instead denying any actual infinity), then an example of the same form, such as the line paradox, cannot show that composition from infinitely many parts is impossible. Neither of these points depends on Galileo’s particular way of resolving the square number paradox.
this is quite a crude and unenlightening analysis compared to Cantor’s, and besides defusing Simplicio’s objection, Galileo’s solution of the paradox did nothing to clarify the tenability of the analysis of continua into indivisibles, nor the feasibility of Galileo’s hypothesis about vacua and cohesion. It was at best a weak partial solution of the larger philosophical problem.

4. Bolzano

Bolzano\textsuperscript{52} boldly claimed that infinite sets differed in numerosity, and that transfinite numerosity did not satisfy both Euclid’s Principle and Hume’s (though again, see note a, Section 3). He even recognized the divergence of those principles as a necessary and sufficient condition for infinity, though he did not, like Dedekind,\textsuperscript{53} adopt it as a definition. But unlike Cantor, Bolzano saw Euclid’s Principle, not Hume’s, as indispensable to the notion of quantity. Despite the existence of a bijection between two sets, he claimed, they “can still stand in a relation of inequality in the sense that the one is found to be a whole, and the other a part of that whole” (Bolzano,\textsuperscript{52} p. 98).\textsuperscript{a}

\textsuperscript{a} Berg\textsuperscript{54,55} seems to grant Bolzano deathbed absolution by claiming that Bolzano had renounced his allegiance to Euclid’s Principle in his last days. He points out that, in a letter to a pupil dated March 9, 1848, Bolzano retracts the conclusion that \( S_m \) infinitely exceeds \( S_{m+1} \) (see my next paragraph). “Hence”, writes Berg, “it seems that at the last Bolzano confined the doctrine that the whole is greater than its parts to the finite case and accepted \( \text{bijection} \) as a sufficient condition for the identity of powers of infinite sets” (Berg,\textsuperscript{34} p. 177; repeated almost verbatim in Berg\textsuperscript{55}). But Bolzano’s renunciation (published in Bolzano\textsuperscript{56}) is too obscure to establish that he accepted Hume’s Principle. In fact, Bolzano continued to deny that principle in the \textit{Paradoxes}, which he worked on at least until September 30, 1848 (Steele\textsuperscript{57}), more than six months after the cited letter. (On the other hand, the quality of the posthumous editing of the \textit{Paradoxes} has been criticized; see Steele,\textsuperscript{57} pp. 54–5.) Furthermore, Bolzano’s \textit{Paradoxes} does treat a variation on Galileo’s Paradox in a manner that is apparently consistent with the remarks of his letter (and using similar \( S_m \) notation), and yet connects it with lessons learned from the \textit{failure} of Hume’s Principle (Bolzano,\textsuperscript{52} p. 115; cf. pp. 100, 110, 114). There Bolzano does not accept the mere existence of a bijection as sufficient for the equinumerosity of infinite sets; only some connection in the “mode of specification or of generation” is sufficient (p. 98). If one sequence is \textit{produced} from another by squaring each term, for example, then the two sequences have the same number of elements (partly anticipating Gödel’s argument discussed below). For Bolzano, this does not contradict Euclid’s Principle because he distinguishes between terms that have the same value, so that the result of squaring the terms in the sequence 1, 2, 3, … is not a proper subsequence of that sequence. Strange, vague, and problematic as these views are, we have no proof that they cannot be developed into a consistent and interesting theory. Berg’s attribution of a Cantorian view to Bolzano looks suspiciously like a symptom of the Cantorian hegemony.
This he argued from two paradoxes similar to Galileo’s, but involving continuous sets. He showed first that the real numbers in the interval \([0, 5]\) can be matched with those in \([0, 12]\) by means of the equation \(5y = 12x\). Analogously, he exhibited a bijection between the points in a line segment \(ac\) and an arbitrary proper segment \(ab\), a version of Simplicio’s line paradox. In an earlier work (Bolzano\(^5\)), Bolzano considered a generalization of Galileo’s square number paradox (without citing Galileo). To put it in modern terms, he defined the sequences \(S_m = \{n^{2m}\}_{n \in \mathbb{Z}^+}\) for each \(m \in \mathbb{N}\) and argued that each \(S_m\) contains infinitely many more terms than \(S_{m+1}\).

Much like Galileo, Bolzano attributed the “air of paradox” (in the continuous examples at least) to the over-extension of notions from the finite case. When a bijection between finite sets is possible, “then indeed are the two finite sets always equal in respect of multiplicity. The illusion is therefore created that this ought to hold when the sets are no longer finite . . . ” (ibid., p. 98). However, far from concluding that questions of size are vague or indeterminate in the infinite case, or like Galileo, that the notion of size does not apply — far then from initiating our naïve method\(^b\) — Bolzano regarded it as proved by the so-called paradoxes that bijection does not entail equinumerosity. For Bolzano, being a proper part constituted a notion of ‘smaller’. At least twice (p. 95, p. 98) he remarked that an infinite set can be greater than another “in the sense that” the two are related as whole to part.\(^c\)

Despite his own view of the matter, Bolzano was in fact free to choose among Euclid’s principles. The very fact that he took the paradoxes to refute Hume’s Principle in the infinite case is further evidence that before Cantor, Euclid’s Principle was integral to tacit conceptions of number, and Hume’s was not the uniquely essential principle of numerosity. Indeed, there is a clear sense in which there are more whole numbers than perfect squares, for ‘more’ often means ‘additional’. The whole numbers include the squares and more, i.e., others. A notion of numerosity that does not reflect this would seem to be missing something basic. Even in current mathematics, one sometimes uses “small” in a Bolzanian sense. For example, a \(\sigma\)-algebra is often defined as “the smallest” set with certain properties. What is meant in that case is not the set of smallest power (for that does not even pick out a unique set), but rather the unique set, with the specified properties, such

\(^b\) Bolzano did, however, attribute certain mistakes in calculating infinite sums to expressions being “devoid of objective reference” (Bolzano\(^5\), pp. 112–4).

\(^c\) But notice also a hint of pluralism: “in the sense that” suggests the possibility of a different sense.
that no proper subset has those properties. And even Gődel,\(^4\) in the midst of arguing that Cantor’s concepts are forced on us, said that new axioms can “increase the number of decidable propositions” (p. 520). But the set of such propositions always has the power of the integers! What Gődel meant by “increase the number” was just to expand the set of decidable propositions to a proper superset of the former — a Bolzanian use of ‘number’.

The notion of proper inclusion on its own is not a very satisfying notion of ‘greater’, for it leaves vastly many\(^d\) sets incomparable to each other. However, it is not unreasonable to extend the finitary notion of ‘greater’ to a merely partial ordering on the infinite sets. This would at least seem to be an improvement on Galileo’s strict confinement of relative size to the finite. Furthermore, it is possible to extend any partial ordering to a strict weak ordering ‘<’ on the subsets of any well-ordered set, and hence, given the Axiom of Choice, on the subsets of any set (Duggan\(^22\)). In a strict weak ordering, the incomparable sets form equivalence classes, so we can regard any two incomparable sets as equal in “size”. We thus obtain a total preorder ‘≤’ that extends both the relation of ‘no greater than’ on finite sets and the subset relation ‘⊆’. Hence we can define notions of smaller, greater, and equal, as broadly as we like, while respecting Euclid’s Principle (but abandoning Hume’s).

There are two worries about this argument: First, the relations defined might not respect other intuitive principles of size, and may thus seem undeserving of that name. For example, let us say a relation ‘<’ on sets is monotonic if \(A < B\) if and only if \(A \setminus B < B \setminus A\) (a generalization of Euclid’s Principle). Duggan’s powerful extension theorem (Duggan\(^22\)) shows that for a very broad class of properties, binary relations that have those properties can be extended to totality while preserving the properties. As it happens, monotonicity is not one of the properties covered by Duggan’s theorem (since it is not “arc-receptive”), but this in itself does not rule out the possibility that there are total monotonic extensions of the ‘less than’ and proper subset relations, perhaps in virtue of some other provable extension theorem.

If not, so be it. We already know that no extension of the notion of size preserves every property of size that holds for finite sets. An extension

\(^d\) Vastly many, that is, under Cantor’s notion of cardinality, and in any case, infinitely many.

\(^e\) We can also choose the extension to be compatible, meaning that if two sets in the old domain (the finite sets) were not of equal size (i.e., if not both \(A \leq B\) and \(B \leq A\)), they do not have equal size in the extended relation either (Duggan\(^22\)).
cannot preserve both Hume’s Principle and Euclid’s. As Cantor wrote, some authors “begin by attributing to the numbers in question all the properties of finite numbers, whereas the infinite numbers, if they are to be thinkable in any form, must constitute quite a new kind of number” (Cantor, p. 74). If we wish to speak at all about different sizes of infinity, we must choose the properties of size to preserve.

Secondly, we might worry, especially given Duggan’s appeal to the Axiom of Choice, that total extensions of the subset relation would be quite arbitrary and uninteresting. But given the power of Duggan’s theorem, there may be many possible extensions, and perhaps some among them are especially interesting. In any case, being interesting is a separate concern from being logically possible. I concede that Cantor’s notion of cardinal number is probably the most interesting, elegant, intuitively appealing, and useful extension of numerosity to the infinite. I mainly want to insist that such virtues do not make it the uniquely correct notion of numerosity in the sense of verisimilitude. Unless we presuppose a semantics in which ‘size’ automatically designates some particularly eligible property, of which we might have true or false conceptions, the notion of verisimilitude does not apply. So ignoring such semantic considerations, Bolzano was free to choose without risk of being mistaken.

Bolzano did not make the motivations for his Paradoxes (Bolzano) explicit, but it is clear that some of them were shared with Galileo. As Cantor noted (p. 78), the main purpose of the book was to defend the actual infinite, including the constitution of continuous bodies out of point-like atoms, against many apparent contradictions. Bolzano criticized various leaps of logic that others had made, and he took particular interest in divergent infinite sums as well as time. But like Galileo, he also grappled with the curious relations between numerosity and geometric magnitude, defended the analysis of space and matter into a continuum of points, and even attempted to use this analysis to explain physical phenomena.

Bolzano distinguished the magnitude of a spatial extension from the numerosity of the set of points of which it consists (Bolzano, pp. 134–5), and then asserted various propositions about magnitude and numerosity, such as that if two figures are perfectly similar, the numbers of their points stand in the same ratio as their geometric magnitudes (p. 136). (He defended this from an objection similar to Simplicio’s line paradox, and closer still to the

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7 Though Cantor continues anti-pluralistically, “the nature of this new kind of number is dependent on the nature of things and is an object of investigation, but not of our arbitrariness or our prejudice” (ibid.).
Wheel Paradox, by repeating his rejection of Hume’s Principle (p. 137).

Later in the book, he hypothesized that the whole of infinite space was completely filled with substances, and yet various parts were filled with different degrees of density (p. 161). To defend this, Bolzano urged that “there is no sort of impossibility in one and the same (infinite) set of atoms being distributed, now in a larger region without a single point standing solitary there, now in a second and contracted region without a single point requiring to absorb two atoms” (p. 162), and referred the reader to his versions of Galileo’s Paradox.\(^{6}\) Thus, Bolzano was concerned with relations between magnitude and numerosity, the particulate analysis of continua, and physical applications.

Like Galileo, Bolzano took the paradoxes to prove something, but by taking them to prove one thing rather than another, he imposed a conceptual refinement. No doubt he did so with an eye to some of the motivations noted above. But like Galileo’s, his success was quite limited. He found many applications for his rather vague conception of infinite numerosity in the \textit{Paradoxes}, but most seem to have been incoherent and fruitless.

5. Cantor

Cantor’s approach to the infinite seems to have been closer to the NMMA than those of Galileo or Bolzano. Though Cantor did not explicitly claim that questions of relative size for infinite sets lacked uniquely right answers, he was somewhat pluralistic about concepts of transfinite number, and as I will explain, this suggests that even for him, some questions about relative numerosity were indeterminate until refined. Furthermore, he did, unlike Galileo and Bolzano, regard himself as extending the concept of number, and he did so under the influence of certain specific motives.

By 1887, Cantor clearly endorsed multiple notions of number, including power and \textit{Anzahl} (by then taking the additional names of cardinal and ordinal number), as well as the more general notion of \textit{order type} (Cantor\(^{58}\)). It is often thought that power was always the primary notion of number for Cantor (e.g., Ferreirós\(^{59}\) p. 265, p. 270), who did make several remarks about the basic, general, and intrinsic character of power (Cantor\(^{60}\) p. 150; Grattan-Guinness\(^{61}\) p. 86). Ferreirós points to those remarks

\(^{6}\) The point of referring to the paradoxes was apparently to show that a continuum of atoms, with no gaps, could nonetheless be compressed into a smaller region, increasing its density. But this seems incompatible with his assertion that the ratio between numbers of elements equals the ratio between magnitudes.
as well as the structure of Cantor’s “most mature work”, the *Beiträge* (Cantor\textsuperscript{26}), in which the cardinals are introduced before the ordinals. But the remarks in question leave some room for interpretation and are counteracted by others. In fact, it was *Anzahl* that Cantor first called the “number of elements” of an infinite set,\textsuperscript{a} in the *Grundlagen*, while power retained its less suggestive moniker for some time.\textsuperscript{b} Even in the *Beiträge*, to which Ferreirós appeals, Cantor exalted the notion of order type. That concept, he wrote, “…embraces, in conjunction with the concept of ‘cardinal number’ or ‘power’ introduced in Section 2, everything capable of being numbered that is thinkable, and in this sense cannot be further generalized. It contains nothing arbitrary, but is the natural extension of the concept of number” (p. 117). Power, then, was not unequivocally privileged, for Cantor. The best we could say for it in light of this quote is that it was somehow “conjoined”, to or within, the natural extension of number. The pluralism here, encompassing at least power and order type, is explicit.

Still, one might infer from Cantor’s “ordinal/cardinal” terminology that power was his only concept of numerosity. He even wrote, “The ‘powers’ represent the unique and necessary generalization of the finite ‘cardinal numbers’” (Cantor,\textsuperscript{65} p. 922). But as we have just seen, it was the *Anzahlen* that first took that position. In the *Grundlagen*, Cantor made it clear that he regarded *Anzahl* as a notion of numerosity, relativized to an ordering:

\textsuperscript{a} “In the case of infinite aggregates, on the other hand, absolutely nothing has so far been said, either in my own papers or elsewhere, concerning a precisely defined number of their elements [Anzahl der Elemente]” (Cantor,\textsuperscript{7} p. 71; Cantor,\textsuperscript{62} p. 167). But soon, “Another great gain … is a new concept not previously in existence, the concept of the number of elements [Anzahl der Elemente] of a well-ordered infinite manifold” (Cantor,\textsuperscript{7} p. 71; Cantor,\textsuperscript{62} p. 168). However one translates ‘*Anzahl*’ here, ‘*Anzahl der Elemente*’ strongly suggests a notion of numerosity rather than position in a series.

\textsuperscript{b} His first use of ‘cardinal number’ in print appeared in 1887 (Cantor\textsuperscript{58}). Jourdain\textsuperscript{47} quotes Cantor using it in a lecture of 1883, and a footnote from Cantor attributes the relevant part of Cantor\textsuperscript{58} to a lecture of that year and a letter of 1884 (p. 387). Strangely, though, Cantor did not to my knowledge use ‘cardinal’ in any publications or other letters before 1887. Even in his review of Frege’s *Grundlagen* (Cantor\textsuperscript{63}) and Cantor,\textsuperscript{64} he used ‘ordinal number’ but not ‘cardinal’, and kept power distinctly separate from number; even though Frege had argued (using different words) that Cantor’s *Anzahlen* were ordinal numbers and his powers were cardinals (Frege,\textsuperscript{50} p. 98). It seems plausible, then, that Cantor only inserted the phrase ‘cardinal number’ into the later published form of his lecture (and perhaps likewise for the 1885 letter published as part VIII of Cantor\textsuperscript{26}). Jourdain may have used ‘cardinal number’ anachronistically, as many authors do, believing that Cantor always thought of power as the fundamental notion of numerosity. If so, the cardinal/ordinal terminology was probably spurred by Frege’s remarks and further justified by Cantor’s development of cardinal arithmetic. (See note g, Section 5.)
[A] finite aggregate exhibits the same number of elements [Anzahl von Elementen] for every order of succession that can be given to its elements; on the other hand, different numbers [Anzahlen] will in general have to be attributed to aggregates consisting of infinitely many elements, depending upon the order of succession given to the elements. (Cantor, 7 p. 72; Cantor, 62 p. 168)

This was natural, given the way in which the Anzahlen emerged from Cantor’s theory of point sets. As is well documented (Jourdain 47), they sprang from the indexes on his “derived sets” $P^{(ν)}$, where $P^{(1)}$ is the set of limit points of a set $P$, and $P^{(ν+1)} = P^{(ν)}$ (Cantor 66). Cantor later defined $P^{(∞)}$ as the intersection of all derived sets $P^{(ν)}$ for $ν$ a positive integer (Cantor 67). Intuitively, $P^{(∞)}$ was the result of taking the derivative infinitely many times (once for every positive integer) and $P^{(∞+ν)}$ the result of taking it $ν$ more times. This helps explain how the Anzahlen represented a kind of numerosity; they answered the question, “How many times?”.

Hence there was some pluralism in Cantor’s conception of numerosity. Already in the Grundlagen, the claim that the original concept of number splits in two implied that both power and Anzahl were in some degree legitimate heirs to the title of ‘number’. Such pluralism suggests that even for Cantor, certain questions about the relative size of infinite sets were indeterminate prior to his refinements. Consider the ordered set $(1, 4, 9, 16, \ldots; 2, 3, 5, 6 \ldots)$, i.e., the positive integers arranged so that the squares come first. Is this ordered set bigger than its infinite initial segment $(1, 4, 9, \ldots)$? With respect to Anzahl, yes: The former sequence has the ordinal $2ω$, while the latter has ordinal $ω$. But with respect to power, no. Hence, the question requires refinement. In fact, even the question of the Anzahl alone may be indeterminate, for it depends on the ordering that we apply to a set. In particular, Galileo’s question whether the squares are fewer than the positive integers may be seen as indeterminate, even rejecting Euclid’s Principle, provided that ‘fewer’ can be understood in terms of Anzahl and one does not take the natural orderings for granted. Thus, Cantor’s own ideas implied that some questions of relative size, and by a stretch even Galileo’s, were insufficiently precise to determine an answer.

However, Cantor’s pluralism and libertarianism did not extend to Euclid’s Principle. We have already noted his claim that there is simply no contradiction when a set has the same cardinality as a proper subset. As well, the Beiträge contained a venomous assault on Veronese’s definition of equal-

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\(^c\) Again, this expression strongly suggests numerosity.
ity, which attempted a compromise between Euclid’s Principle and Cantor’s *Anzahlen*: “Numbers whose units correspond to one another uniquely and in the same order and of which the one is neither a part of the other nor equal to a part of the other are equal” (Cantor,\textsuperscript{26} p. 117–8).\textsuperscript{d} Cantor criticized the circularity of this definition (ibid.), but according to Dauben\textsuperscript{69} (p. 234) he also objected to its arbitrariness. “He complained to Peano that Veronese believed the definition of equality, both for numbers and for order types, was entirely at the mercy of one’s choice, which was a heretical suggestion from Cantor’s point of view . . . ” (ibid.). On Dauben’s reading, Cantor was much more the dogmatic essentialist than the *Grundlagen*’s libertarian declarations would suggest.

Hence, Cantor might appear to have been inconsistent on the subject of mathematical freedom. In the *Grundlagen* he defended the actual infinite both by waxing grandiloquent on the freedom of mathematics and by claiming that his theory was forced on him (Cantor,\textsuperscript{7} p. 75). But perhaps he can be seen as occupying a coherent middle ground: He was forced to recognize certain extensions of the notion of number, he might have said, but not to forsake all others. The forcing he refers to is best understood in terms of his derived point sets. Taking repeated derivatives and infinitary unions, Cantor obtained sets with larger and larger transfinite indices, a process he called “necessary” and “free from any arbitrariness” (Cantor,\textsuperscript{67} p. 148). Without these indices, many of his future results on point sets would have been unattainable, including the Cantor-Bendixson Theorem (Cantor\textsuperscript{70}) and the important theorem that any set with a countable $\alpha^{th}$ derivative, for any *Anzahl* $\alpha$, has zero outer content (the lower limit of the total length of any set of covering intervals), and hence, in modern terms, zero measure (Cantor\textsuperscript{71}). Considered independently of point sets, the *Anzahlen* enabled Cantor to establish an infinite hierarchy of infinite powers, having only established two infinite powers before, and to show that there is a unique second infinite power, seemingly a step toward Cantor’s goal of proving the Continuum Hypothesis. Thus Cantor was forced to recognize the *Anzahlen* by their apparent naturalness and his need to employ them, but none of this required him to dismiss other notions of transfinite number. After all, he later incorporated power and order type as additional concepts of number (Cantor\textsuperscript{26,58}).\textsuperscript{e}

\textsuperscript{d} Veronese’s work is now regarded as an important forerunner to that of Robinson, Conway, and Ehrlich, the latter two of which generalize Cantor’s transfinite numbers (Ehrlich\textsuperscript{68}).

\textsuperscript{e} It is not clear, then, why Cantor categorically rejected Euclid’s Principle. Surely it
Moreover, Cantor called \textit{Anzahl} and order type \textit{extensions} of the concept of number (Cantor\textsuperscript{7,26}), not mere descriptions of its transfinite implications. He even called \textit{Anzahl} “a new concept not previously in existence” (Cantor,\textsuperscript{7} p. 71, Cantor’s emphasis) and pointed out that any transfinite number must be “quite a new kind of number” (Jourdain,\textsuperscript{47} p. 74). Hence he did not see himself, like Galileo and Bolzano, as merely deducing facts \textit{about} the sizes of transfinite sets from a well established notion of number, but as defining \textit{new} concepts. If he did not regard this as mere stipulation, it can only be because he thought that some concepts were coherent and counted as concepts of number, while others, not. But that much is perfectly consistent with the NMMA.

If we take Cantor’s words seriously, then, we must conclude that he saw himself as extending the concept of number, and with some degree of freedom. Further, he did so in light of some explicitly acknowledged motivations. He cited a need to employ the \textit{Anzahlen} in the theory of point sets as well as some applications to function theory (Cantor\textsuperscript{7}). But he also stated goals in \textit{philosophy and natural science} that help to explain his interest in both the \textit{Anzahlen} and the powers, namely the resolution of certain difficulties in the philosophical systems of Leibniz and Spinoza, which he thought would help us to develop a rigorous and “organic” account of nature. The standard account of Cantor’s motives largely ignores these remarks and places his main motives within mainstream function theory (e.g., Jourdain;\textsuperscript{9,47} Dauben\textsuperscript{69}). However, Ferreirós\textsuperscript{38} makes a strong case that Cantor was not chiefly concerned with mainstream mathematics but with broad biological, physical, and even spiritual matters. Cantor is quite explicit about this in an 1884 letter:

\begin{quote}
I have been working on this project of a precise deepening into the essence of everything organic for 14 years already. It constitutes the
\end{quote}

was in part because the Euclidean notions of number on offer, such as Bolzano’s and Veronese’s, seemed incoherent. But one is also led to speculate that Cantor’s bouts of dogmatism were partly \textit{because} of his vitriolic debate with Veronese over the coherence of infinitesimals. (The opposite has been suggested, i.e., that Cantor opposed Veronese because he was dogmatic about his concept of number, but Cantor vehemently denied infinitesimals as early as 1878 (Ewald,\textsuperscript{72} p. 867), well before introducing his transfinite numbers.) It is worth noting too that Cantor’s \textit{Anzahlen} made some concessions to Euclid’s Principle. Not every proper part of a well ordered set has smaller \textit{Anzahl} than the whole, but every proper \textit{initial segment} does. The \textit{Anzahlen} thus partly reflect the idea, encoded in Euclid’s Principle, that if we add more elements to an infinite set, we have a larger set, though for \textit{Anzahlen} this only holds if the new elements are added at (or sufficiently near) the end of a sequence.
real motivation why I have confronted, and during this time have never lost sight of, the fatiguing enterprise of investigating point-sets, which promises little recognition. (Ferreirós, 38 pp. 49–50)

Through the background of German Romantic **Naturphilosophie** and Cantor’s own testimony, Ferreirós shows that by “organic” Cantor did not simply mean biological; rather he sought a natural philosophy unifying mechanistic and spiritual elements.

Cantor’s work on this program raised key mathematical goals for his theory of the transfinite, namely to illuminate what we now call the topological and measure-theoretic features of physical space and objects. For example, he proposed an analysis of matter into a countable infinity of point-like corporeal “monads” and a continuum of point-like ethereal monads, and used his theorems on point sets, power, continuity, and outer content to offer explanations of various physical phenomena (Ferreirós 38).

Thus Cantor’s investigation of the transfinite was in considerable part motivated by an interest in the qualitative structural features of space and matter. In particular, he shared with Galileo and Bolzano an interest in the relations between numerosity and geometric magnitude (addressed by Cantor in terms of dimension and outer content), the analysis of matter into a continuum of points, and the application of this analysis to explain physical phenomena.

Cantor did not deliberately design his concept of cardinal number to serve those goals, but that concept developed very gradually under their influence, each conceptual development being spurred by new applications and results. Cantor took up questions about bijection even before 1869 (Ferreirós, 38 p. 52), long before arriving at the notions of power, countability, and last of all cardinal **number**. Soon after he raised the question of a bijection between the whole numbers and the reals, in an 1873 letter to Dedekind, he wrote, “[T] has no special practical interest for me. And I entirely agree with you when you say that for this reason it does not deserve much effort” (Ewald, 72 p. 844). Yet an answer, he pointed out, would yield a new proof of the existence and density of the transcendental numbers, and once Cantor accomplished that, Dedekind remarked that this proved the problem was interesting and worthy of effort (ibid., p. 848). In 1877 Cantor developed the proof that there is a bijection between a line segment and any n-dimensional continuum, and simultaneously began using the word ‘power’ (Ewald, 72 p. 853ff.). In the publication of that result (Cantor 21), he even defined the notions of **smaller and larger** powers, and pointed out the fact that the even numbers have the same power as the positive in-
The term ‘countable’ or ‘denumerable’ did not appear until 1882, when Cantor proved that, in modern terms, an $n$-dimensional space can include at most countably many disjoint open sets (Cantor\(^6\)). At that time, only sets of the first two infinite powers were known. Only after using the ordinals to generate an infinite hierarchy of powers and producing several more theorems on the powers of point sets (including that countable sets have zero content; Cantor\(^7\)) did Cantor adopt the term ‘cardinal number’ (Cantor,\(^8\) from a lecture of late 1883 — but see note d, Section 5). Thus his concept of cardinal number evolved, in a dialectical milieu of applications and innovations, most of which concerned the structure of continuous spaces and point sets within them. If Cantor did not deliberately design the concept to shed light on such matters, those interests nonetheless seem to have exerted a selection pressure on the concept’s evolution. In this respect the concept was tooled to fit its motivations.

The result was tremendously successful. Cantor’s physical hypotheses were as speculative and mistaken as Galileo’s infinity of point-vacua and Bolzano’s smooth plenum of variable density, but still, his theory of transfinite numerosity was much more fruitful than those of Galileo and Bolzano concerning all three of the shared motivations we have identified. Not only did Cantor offer a way out of objections to the particulate analysis of the continuum, such as the supposedly absurd consequence that one obtains infinities of different sizes — all three thinkers managed that — but he also initiated a rich and ultimately coherent theory of such sizes which shed further light on the structure of continua. He was able to show that while there are different sizes of infinity (powers and order types), line segments of different lengths have the same power\(^6\), and if both segments are closed, the same order type (a consequence of Cantor,\(^9\) p. 134), contrary to Simplicio’s argument (Galileo,\(^10\) p. 31). Thus he was able to distinguish clearly between geometric magnitude and “number of elements”. He further showed

\(^1\) Apparently, Cantor had also worked out cardinal arithmetic in a manuscript of 1885. Jourdain\(^11\) (p. 79) reports this, but perhaps based on Cantor’s footnote in Cantor\(^12\) (p. 411), which is in fact a bit vague (“[E]r ist der Hauptsache nach vor bald drei Jahren verfaßt . . . ”). If Cantor did develop cardinal arithmetic before or simultaneously with adopting the term ‘cardinal number’ (also contrary to Jourdain; see note d, Section 5), then this would further explain Cantor’s construal of power as cardinal number, for, one justification he gave for calling his ordinals by the name ‘number’ was their possession of a systematic arithmetic (in an 1882 letter; Ewald,\(^13\) p. 876).

\(^8\) In effect, Bolzano\(^14\) showed this too, but he did not have the notion of power, i.e., of an equivalence class induced by bijection, and he did not take a bijection between sets to show anything significant.
that continua of different dimensions have the same power (Cantor\textsuperscript{21}), and that finite and countable point sets have zero magnitude (Cantor\textsuperscript{60}). And though his own speculations on the structure of matter failed, his theory of the transfinite has in fact afforded some insight into physical phenomena.

The fact that countable sets have measure zero has been especially useful in this regard. Given a measure-zero set of possible states for some physical system, it is generally plausible to assume that the probability of the system taking on one of those states at a given time is zero. (Poincaré\textsuperscript{73} made much use of this assumption in his celestial mechanics, and it plays a key role in statistical mechanics (Sklar\textsuperscript{74}).) The same, then, goes for countable sets of states. In one interesting application, Hadamard\textsuperscript{75} showed that, of the continuum-many bounded geodesics through a given point on a surface of negative curvature, only countably many are asymptotic to closed curves. Hence, “almost all” bounded curves on such a surface — with regard to power, measure, and frequency — are thoroughly non-periodic, and this is reflected in the typically chaotic behavior of many dynamical systems. Thus power sheds light on physical phenomena.

I say that such results — those concerning the structure of continua, the relations between numerosity and magnitude, and physical phenomena — are good reasons to regard Cantor’s theory of the transfinite as successful relative to the goals that he shared with Galileo and Bolzano. Whether this accounts for the popular success of his theory is another question. No doubt the fact that countable sets have zero measure largely explains the introduction of Cantor’s cardinals in many elementary analysis texts, and this in turn must have contributed to their widespread acceptance. Other contributing factors likely include applications of both power and Anzahl in function theory and analysis (Cantor;\textsuperscript{76} p. 260; Mittag-Leffler;\textsuperscript{77} Borel;\textsuperscript{78} cf. Hallett\textsuperscript{79}), the support of Weierstrass, Mittag-Leffler and Hilbert (Dauben;\textsuperscript{80} Hilbert\textsuperscript{81,82}), Frege and Russell’s use of power in their definitions of number (Frege;\textsuperscript{50,83} Russell\textsuperscript{51}), Frege’s proof of the axioms of arithmetic from Hume’s Principle (Frege\textsuperscript{83} cf. Hale and Wright\textsuperscript{48}), and perhaps most of all, the intrinsic nature of power, the fact that it is independent of the nature and ordering of elements (a point emphasized by both Frege\textsuperscript{50} and Russell\textsuperscript{51}). No doubt the last two points are strong reasons to count Cantor’s theory a genuine success, popularity aside, but this is true relative to certain goals, and not the ones we have identified as common to our three historical figures.

To summarize the essential points we have noted about Cantor’s theory, (1) Cantor was pluralistic about concepts of numerosity, within limits; (2)
such pluralism implied indeterminacy in some questions about numerosity;
(3) Cantor explicitly stated certain philosophical and scientific motivations;
(4) each of the three common goals we have attributed to Galileo and
Bolzano was either included in or raised by Cantor’s explicit motivations;
(5) Cantor did devise new concepts of numerosity, though he did not regard
them as entirely free inventions; (6) he did so gradually, while discovering
applications and results that apparently influenced the theory; and (7) the
resulting concepts proved to be very useful for the common goals we have
identified.

6. Gödel

The NMMA (Section 2) takes it for granted that we can identify questions
that lack a determinate answer, and that we are free to extend and refine
concepts and meanings as we wish. Yet some metaphysicians hold that cer-
tain questions, whose answers surely seem indeterminate, nonetheless have
unique factual answers (Lewis; Sider; Olson). One might think
that the reference of numerosity expressions was likewise predetermined,
before Cantor’s work, in such a way that any extension of the number
concept that produced different results would be not only inapt but false.
Gödel’s view was close to this (Gödel). He claimed that Cantor’s concept
of cardinality as well as the axioms of set theory had a self-evident truth
that we cannot help but recognize, if we carefully examine the concepts that
we already loosely grasp. But in arguing that such ideas are forced upon
us, Gödel neglects the fact that they are overdetermined. With the concept
of numerosity as with our other intuitions about sets, the very principles
that once seemed undeniable have led to paradoxes, including Galileo’s,
and cannot be maintained. Gödel (ibid.) had an excuse for the other para-
doxes of set theory: they result from misapplying the intuitive principles to
exotic, impossibly comprehensive collections, such as the set of all sets. If
we admit only sets of pre-established objects (the iterative conception of
set), the paradoxes do not arise. But this is not so for Galileo’s Paradox;
that problem lives right in our back yard, among the whole numbers. If we
want to have any notion of transfinite numerosity we must face up to it
and adopt a notion (or several) that violates either Euclid’s Principle or
Hume’s (or both).

Admittedly, Gödel gives a very compelling argument for Hume’s Prin-
ciple: If two sets can be put in one-to-one correspondence, then we could
conceivably alter the individual elements of one set until they were indistin-
guishable from their counterparts in the other, and then surely the two sets
must have the same numerosity. I say this is very compelling, but nonetheless it is only an intuition pump. Gödel disregards the fact that Euclid’s Principle is also intuitively compelling! If set \( A \) contains everything that is in set \( B \) and also some further things, then it contains more. Both Euclid’s and Hume’s Principles seem forced on us. To have a consistent theory of transfinite numerosity, we must break free of these forces, much as Gauss and Lobachevsky broke free of the parallel postulate. We have learned from them that intuitions do not limit our freedom to form counterintuitive conceptions. Even if Hume’s Principle seems stronger than Euclid’s, no adequate reason has been given to believe that it is unrevisable or a brute fact. It is up to us to choose our preferred principles, or to articulate an arsenal of different concepts incorporating different principles.

On the other hand, Gödel’s realism might supply a reason to regard a conception as false. If one concept satisfying some of our intuitions has an ontological status privileged over others, we might therefore regard that concept as the one that we had loosely grasped all along. In other words, some special “existence” (or some other property) beyond mere consistency might make one concept more eligible than others. If such eligibility helps to determine reference, then there might be a fact about which transfinite relation is really the referent of ‘more’ and other such terms. Again, it seems wisest to ignore such considerations and focus on the motivations for a concept; never mind what the referent of the pre-theoretic term is, let us establish new concepts that bake some bread. But now we may worry that we cannot even do that; the “real” objects may be so much more eligible in virtue of their “reality” that we cannot force our words to mean anything else.

The usual objection to Gödel’s realism is that it makes our possession of mathematical knowledge inexplicable (Benacerraf, cf. Maddy). Gödel proposes a special faculty of mathematical perception, but since abstract objects lack causal efficacy, such a faculty seems impossible. The objection I have raised above gives us further reason for doubt: The fact that our intuitions sometimes lead us into paradox suggests that we have no trustworthy mathematical perception. Why then should we suppose that such “perception” is anything but prejudice?

A standard alternative to Gödel’s form of realism is the indispensability argument. In Quinian terms, it says that if we accept a theory, we are committed to the objects over which its quantifiers range. Insofar as our best empirical theories involve quantification over mathematical objects, we should accept that those objects exist (Quine; Putnam). However,
this faces charges of contradicting mathematical practice, for mathematicians believe in many things that have not yet proven useful (Maddy, pp. 106–7, pp. 153–60). Maddy instead proposes that we take mathematical considerations and motivations, such as the desire to provide a universal foundation for all of mathematics, as the arbiters of mathematical truth. Quine’s and Maddy’s views do not seem to give us reason to deny the existence of other objects besides those that serve empirical and mathematical goals, but we might worry that scientific usefulness itself implies a strong eligibility that prevents us referring to other things.

But suppose there are indeed irresistible mathematical reference magnets. How might that affect mathematical practice? Can we not still give alternative definitions and theories, and follow out their consequences? Even if we do not succeed in referring to the things we wish to, this has no impact on what we can logically deduce. Hence it again seems that all that is important about a concept or theory, beyond consistency, is its interest and fruitfulness.

7. Conclusions

It has been claimed here that at least some genuinely philosophical problems are solvable by the Method of Conceptual Articulation, and some have already been solved by such means. In particular, Galileo’s Paradox was resolved by the articulation of numerosity into distinct concepts, including those of proper inclusion, Anzahl, and power. Granted, none of our historical figures saw themselves as stipulating extensions of the concept of number with complete freedom. Cantor in particular seemed to recognize that he was presenting genuine extensions, but not arbitrarily; he regarded his multiple conceptions as forced by his mathematical needs, by the determinate iterative process that defined the ordinals, and by considerations of naturalness. But new extensions they were nonetheless.

Power has become the basis of an elegant and useful theory and has proven especially useful in addressing the motivations common to Galileo, Bolzano and Cantor, namely, to grasp the relations between numerosity and geometric magnitude, to defend the analysis of the continuum into points.

Gödel of course has argued from his theorems on the incompleteness of arithmetic that there is more to mathematical truth than mere consistency. But even if we agree with Gödel that there is a unique system of whole numbers — a unique intended model for our axioms of arithmetic — that is no reason to deny the existence of other objects, of non-standard models of arithmetic. Indeed, their existence is implied by the incompleteness theorems and the completeness of first-order predicate logic.
Philosophical Method and Galileo’s Paradox of Infinity

and to explain physical phenomena. It is in virtue of its success in serving such motivations that Cantor’s theory of transfinite numbers constitutes a solution to some of the deeper philosophical problems posed by Galileo’s Paradox.

Nonetheless, to say that power is the only correct notion of numerosity is distorting. Anzahl too can be considered as a notion of numerosity, and Cantor did so conceive it. Furthermore, order extension theorems like Duggan’s give us reason to think that a theory of numerosity satisfying Euclid’s Principle is possible.

In its naïve form, the Method of Conceptual Articulation presupposed that empty questions could be identified and concepts freely refined or modified. These presuppositions face many challenges, perhaps most forcefully from externalist theories of reference. But those challenges do not bear on the most important elements of the method. However reference works, and whether or not we can distinguish between determinate and indeterminate questions, we can still, at least sometimes, identify background motivations for our philosophical puzzles, if perhaps after the fact, and we can articulate concepts, or theories if you prefer, that serve to address those motivations.

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1. Introduction

In the early nineteenth century, mathematical analysis underwent fundamental changes effecting the types of questions asked and the methods employed to answer them. In the process, the attention of mathematicians was gradually led from considering explicit formulae as the fundamental entities of mathematical analysis to focus on a more conceptual approach. This transition can be appreciated by comparing the mathematical styles of Leonhard Euler (1707-1783) in the eighteenth century and Bernhard Riemann (1826-1866) or Richard Dedekind (1831-1916) in the second half of the nineteenth century. Such a comparison highlights that Euler asked questions about formulae and proceeded by extremely skillful manipulations of explicit formulae using the algebraic symbolism to its full extent. This position is here referred to as formula-centred mathematics. Mathematicians of the second half of the nineteenth century began asking questions pertaining to concepts, in the process defining and examining new concepts such as ‘differentiable functions’ or ‘solvable equations’. This position of asking questions about concepts is here referred to as concept-centred mathematics. Within the span of this transition, the works of the Norwegian mathematician Abel shows signs of both styles. Therefore, Abel’s mathematics can be used as a focal point for discussion of this framework of formula-centred
In an effort to philosophically analyse some of the perspectives of the transition from formula-centred to concept-centred mathematics, I here apply a Wittgenstein-inspired framework. Ludwig Wittgenstein (1889-1951) offered the notion of “language-games” that invites a focus on mathematical practices most welcome to my line of enquiry. These language-games encapsulate the activity of doing mathematics in a specific context within a mathematical field and numerous language-games may well be concerned with the same segment of mathematics. The language-games are made up of the textual manifestations of the field, where textual should be understood in a wide sense. It should be emphasised that language-games are not fixed by e.g. syntax or a specific semantics but evolve continually as they are being “played” or practiced. As a particularly feature, this Wittgenstein-inspired analysis allows me to largely leave out of consideration the ontology of mathematics while focusing on ways of philosophically analysing the use of concepts and representations in mathematics.

Most importantly, the framework allows me to make more precise some of the key terms used in my subsequent analysis. For example, in Fig. 2, I describe the question of “representability” in terms of an enlarged language-game in which the original representations become the objects of mathematical investigation. Later, in Fig. 3, I stipulate the notion of “habituation” and characterize it as “the act of pointing out familiarities between different language-games”. However, before that becomes possible a short exposition and interpretation of some of the basic notions of Wittgenstein’s philosophy of language is necessary.

2. Aspects of functions: A Wittgenstein-inspired framework

Before coming to my historical analyses of pieces of Abel’s mathematics, I want to comment upon my conceptual and terminological framework. This framework is inspired by Wittgenstein’s philosophy of language and representations as discussed in his Philosophical Investigations (henceforth PI).\(^7\)

\(^{a}\) See also Sørensen.\(^1\) A similar framework has been used to analyse the mathematics of Riemann in Laugwitz.\(^2\) The scholarship on the mathematics of Riemann and Dedekind is extensive and growing; for some recent additions, see e.g. Ferreiró\(^3\) and Avigad.\(^4\) For another perspective on formula-centred approaches to functions, see also Panza.\(^5\)

\(^{b}\) For a recent analysis of mathematical ontology as part of a naturalist philosophy of mathematics and starting from similar premises as mine, see Muntersbjorn.\(^6\)

\(^{c}\) The following short exposition does not pretend to be a discussion of neither Wittgenstein’s views nor the reception thereof. Instead, it must be thought of as a “Wittgenstein-
Central to Wittgenstein’s philosophy of language is the notion that meaning is constituted through use and mastery of language rather than e.g. through direct reference:

To understand a sentence means to understand a language. To understand a language means to be master of a technique.

Such a view would entail that understanding a piece of mathematics (a theorem, a theory, ...) requires understanding of its mathematical context and the mastery of techniques to employ and manipulate that piece of mathematics. Such a view thus emphasises the practice of mathematics over foundational issues and — as such — it has interesting and fruitful consequences for how we discuss e.g. the use of representations in mathematics. Concerning sensation, Wittgenstein observes that the object can be cancelled out when we focus on the designations (representations):

That is to say: if we construe the grammar of the expression of sensation on the model of 'object and designation' the object drops out of consideration as irrelevant.

In terms of mathematics, this may be interpreted as the ontological viewpoint, that only the representations (in texts, arguments, ...) count — nothing needs to be said of any objects “behind” the representations. However, Wittgenstein discussed further how it is possible to “see” the “same thing” in different representations and — vice versa — to see different representations as the “same thing”:

The concept of a representation of what is seen, like that of a copy, is very elastic, and so together with it is the concept of what is seen. The two are intimately connected. (Which is not to say that they are alike.)

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\(^d\) “Einen Satz verstehen, heißt, eine Sprache verstehen. Eine Sprache verstehen, heißt eine Technik beherrschen” (PI Part I §199, p. 68). All translations into English from Wittgenstein are taken from the bi-lingual edition of the PI. Wittgenstein expressed a similar idea also as a slogan: “Let the use of words teach you their meaning.” (PI Part II §xi, p. 187)

\(^e\) “Das heißt: Wenn man die Grammatik des Ausdrucks der Empfindung nach dem Muster von ‘Gegenstand und Bezeichnung’ konstruiert, dann fällt der Gegenstand als irrelevant aus der Betrachtung heraus.” (PI Part I §293, p. 85)

\(^f\) “Der Begriff der Darstellung des Gesehenen, sowie der Kopie, ist sehr dehnbar, und mit ihm der Begriff des Gesehenen. Die beiden hängen innig zusammen. (Und das heißt nicht, daß sie ähnlich sind.)” (PI Part II §xi, p. 169)
To this end, Wittgenstein’s notions of “aspect” and “changes of aspect” are useful. These may be illustrated using Wittgenstein’s drawing (Fig. 1) which has the potential of multiple aspects.\(^8\) It is possible to look at the picture and see a duck — and nothing but a drawing of a duck with its beak to the left. This duck would be an aspect of the picture, and it may — so to say — force itself upon you. However, the picture could also be seen to depict a rabbit with its ears to the left and looking upwards. This would be another aspect of the same picture, and it is likely that — once you have first seen the duck-aspect — it would have to be shown to you either by a description such as the one given above or by somebody pointing out the various components and naming them. Once you see one particular aspect, it may require an effort, an act of imagination, or an outside influence to be able to see it as only one among multiple potential aspects.

![Figure 1. One drawing with multiple aspects (from Wittgenstein,\(^7\) p. 166)](image)

However, once you realise that the duck-rabbit-picture in Fig. 1 may have at least these two aspects to it, the picture may be seen as one picture with (at least) two aspects, and you may be able to shift rather freely from one aspect to the other. As such, the aspects may be said to have become properties of the picture although nothing has changed in the picture, itself. The only change has taken place in the observer and among observers as a change in their uses of the picture.

\(^8\) Such situations with multiple aspects are the rule rather than the exceptions. The point is that each aspect here corresponds to a language-game (see below).
“Now he’s seeing it like this”, “now like that” would only be said of someone capable of making certain applications of the figure quite freely.

The substratum of this experience is the mastery of a technique.\(^h\)

These ideas of Wittgenstein are now to be applied to a discussion of mathematical analysis in the nineteenth century, in particular to the concept of function and representations of functions. However, first a few general remarks should be made:

With Wittgenstein’s concept of “aspect”, the distinction between “the picture” and its “aspects” may be seen to be a superficial one. The aspects — once realised by an observer — constitute the picture, and there is nothing more to the picture than its various aspects. When this observation is transferred to mathematical functions, it stresses representations as a means of accessing and describing functions — a means appropriate for a specific range of uses and employing a particular technical mastery. Because representations serve these roles, they are central constituents for the language-games of the theory of functions.

When concerned with mathematical functions, one of the most prominent “aspects” or representations is the symbolic one using explicit formulae to express the dependent variable (the function) in terms of the free variable using accepted functions, operations, and constants. Such an explicit representation is a highly efficient way of manipulating and investigating the function and of communicating knowledge about functions.\(^i\) In fact, little can be said of individual functions without some means of representing them. However, representations may serve (at least) two additional purposes concerned with “habituation” and the question of “representability”.

**Representability.** In some circumstances, the aspects can be subjected to mathematical analysis on their own. This happens, for instance, when the question of representability is raised in mathematics. Then it is possible to ask whether certain representations (aspects) are possible under certain conditions. As Wittgenstein noticed, such questions require instances of imagination and are essential to mathematics:

\(^h\) “Nur von dem würde man sagen, er sähe es jetzt so, jetzt so, der imstande ist, mit Geläufigkeit gewisse Anwendungen von der Figur zu machen. Das Substrat dieses Erlebnisses ist das Beherrschen einer Technik.” (*PI* Part II §xi, p. 178)

\(^i\) For the latter purpose, see also Muntersbjoern\(^8\) (p. 197) who argues that representations (in a broad sense) are essential for the intersubjectivity of mathematics.
For many mathematical proofs do lead us to say that we cannot imagine something which we believed we could imagine. (E.g., the construction of the heptagon.) They lead us to revise what counts as the domain of the imaginable.\(^1\)

This revision of the “domain of the imaginable” is intimately tied to the role of representations as questions of representability:

Instead of “imaginability” one can also say here: representability by a particular method of representation. And such a representation may indeed safely point a way to further use of a sentence.\(^2\)

In mathematics, in order to decide whether something is representable in some mathematical language-game, the mathematician has to imagine that the solution is possible only to (perhaps) prove that this leads to a contradiction. As such, an important requirement is that the explicit representation must be viewed as a potential aspect and not as identical with the function, itself.

These remarks lead to the following illustration (Fig. 2) of the interrelations between a concept and its textual representations as aspects. Here, I use the term “representation of a function” as the natural mathematical equivalent of what according to Wittgenstein might also be called “aspect of a function”. The figure illustrates how representations in one, “inner” language-game may become the objects of another language-game. This encompassing language-game is a meta-game to the first one in the sense that it includes reflections upon the first one, but it is at the same time a language-game at the same level as the original one and practiced by mathematicians in much the same way.

Habituation. In situations where more than a single aspect is involved, some of Wittgenstein’s other key concepts can be added to the present framework: the following picture (Fig. 3) may be used to illustrate this situation.

To each aspect, a language-game is associated encompassing the ways

\(^1\) “Denn mancher mathematische Beweis führt uns eben dazu, zu sagen, daß wir uns nicht vorstellen können, was wir glaubten, uns vorstellen zu können. (Z.B. die Konstruktion des Siebenecks.) Er führt uns dann, zu revidieren, was uns als der Bereich des Vorstellbaren galt.” (PI Part I §517, p. 120)

Figure 2. The representations of the “inner” language-game become objects of study of a new, meta-level language-game concerned with the question of “representability”

\[ v = f(r_1, \ldots, r_k, p\sqrt{R}) = p^{-1}\sum_{u=0}^q ru p \]

Figure 3. Different representations give rise to different language-games, with a potential familiarity, and the act of pointing out such familiarities is called the process of habituation of expressing and manipulating the contents of the aspect. Two or more language-games may exhibit what Wittgenstein would call “familiarities” on any one of many different levels: subject-matter, expressions, . . . . Recognizing such familiarities is a matter of mastering the given aspects, but (as such) it may be necessary for practitioners of the language-games to point to them. This act of pointing to a familiarity is what is here called the
process of habituation. Furthermore, the manipulations of representations serve also to add to the familiarity as Muntersbjorn\(^8\) (p. 196) observes: “People are familiarizing themselves with mathematical objects when they engage in symbolic manipulations.” Consequently, in the theory of functions, the aspect of explicit representations may be employed to make new functions ‘familiar’ to mathematicians used to ‘speak the language’ of explicit formulae. As such, working with this aspect may serve as a means of “habituation” for the mathematical community. The aspects then provide ways in which the acknowledged techniques can be applied to new situations and thus constitute an extension of the domain of the language-game. Whereas aspects, language-games, and the notion of familiarity are all taken from Wittgenstein’s philosophical investigations of language, the notion of habituation has a more explicitly social flavour: to point to a familiarity involves pointing it out to someone, mostly to someone else. As such, the notion of habituation is also appropriate for the following historical analysis. There, it will be used to designate and understand Abel’s deliberate efforts to point out familiarities between different aspects of elliptic functions.

3. Roles of representations

The great advances of mathematical analysis (including the discipline we now call algebra) in the Enlightenment owed much to the skillful manipulation of mathematical symbols. Of particular importance was the technical machinery that culminated with Euler’s approach to analysis in the mid-eighteenth century.\(^a\) Guided by fundamental beliefs about functions — such that they should be universally defined and formally manipulated — Euler actually equated the study of any function with the study of any one of its representations:\(^b\)

A function of a variable quantity is an analytic expression composed in any way whatsoever of the variable quantity and numbers or constant quantities.\(^c\)

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\(^a\) Most famously outlined in Euler’s series of textbooks on analysis beginning with the *Introductio*\(^9\) but also impressively put to use in his research papers.

\(^b\) For more detailed analyses of Euler’s concept of function, see e.g. Youschkevitch\(^10\) or Jahnke.\(^11\)

\(^c\) “Functio quantitatis variabils, est expressio analytica quomodocunque composita ex illa quantitate variabili, & numeris seu quantitatis constantibus.” (Euler,\(^9\) chapter 1, §4, p. 4, translated in Euler,\(^12\) p. 3)
Thus, to Euler, there was no difference between a function and its representation and he built his methodology of analysis on the central idea “that infinite formulas served to investigate functions exactly (in the sense that manipulating one is the same as manipulating the other)” (Ferraro, p. 295).

If some function was defined by some other means than an explicit formula (something that Euler explicitly also allowed for), Euler mastered the art of deriving an expression for it as a formula — at least in all the concrete cases at hand. Once such an explicit expression had been found, the machinery of the calculus was at the disposal of the mathematician for manipulating and reasoning about the function. Therefore, it is convenient and quite accurate to say that Euler’s analysis was formula-centred.

However, with the change of focus towards concepts rather than explicit expressions, representations took on new roles. One of the first instances of ‘concepts’ in the form discussed here came with Augustin-Louis Cauchy’s (1789-1857) new approach to analysis dating from the 1820s. Cauchy began the tradition of founding analysis on function classes defined by some of their central properties such as continuity, differentiability, or integrability — an approach that was in strong contrast to Euler’s. With this transition towards a more concept-centred mathematics, the ‘function’ was gradually separated from its explicit representations. In particular, implicit definitions of functions as solutions to e.g. functional or differential equations became more frequent. Such implicitly defined functions had been treated by Euler, but they lacked the grips and handles for formal symbol manipulation that were so prevalent in his approach to analysis. Importantly, the identification of function and formula was invalidated with the concept-centred approach to mathematics.

At this point it is appropriate to notice that with the separation of a function and its representation, the possibility — even desirability — of multiple representations for different needs arose. Indeed, different symbolic representations were often sought — sometimes as goals of their own, sometimes to facilitate different further lines of inquiry. This diversity of representations will be addressed again below, but first we focus on one instance where representations of concepts were subjected to mathematical analysis at a meta-level.

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13 For a thorough analysis of Cauchy’s new approach, see e.g. Bottazzini.

14 Today, we may also include e.g. graphical representations, but such were still uncommon in the early nineteenth century.
4. Representation and representability

In 1826, Abel made his grand entrance on the mathematical stage with a flow of papers that filled a substantial part of the newly founded *Journal für die reine und angewandte Mathematik* during the next 4 years. One of his papers published in 1826 was a proof of the algebraic unsolvability of the general polynomial equation of degree 5.\(^a\) Expressed in terms of representations, the theorem states that a root of the general quintic equation

\[ x^5 + a_4 x^4 + a_3 x^3 + a_2 x^2 + a_1 x + a_0 = 0 \]

cannot be written as an explicit algebraic function

\[ x = f(a_0, a_1, a_2, a_3, a_4) \]

in which \( f \) is built up from the coefficients using only addition, subtraction, multiplication, division, and the extraction of roots.\(^b\) This result is one of the first *impossibility results*, and representations and questions of representability are central to the proof.

As will be described below, the proof first established a workable representation of the sought-after solution and then subjected this representation to the meta-level language-game of representability.

Abel’s proof can be divided into three structural components: First, he characterised the form of the explicit algebraic expressions \( f \) and provided a representation for such expressions that allowed him to reason about them. Specifically, Abel’s classification of algebraic expressions was hierarchic; his means to obtain structure were the two concepts of *order* and *degree*. Thus, if \( f \) was a rational function of expressions of order \( \mu - 1 \) and root extractions of prime degree of such expressions, \( f \) would be an algebraic expression of order \( \mu \). With this idea, Abel could characterise all algebraic expressions of order \( \mu \):

\[ f(g_1, \ldots, g_k; \sqrt[r_1]{r_1}, \ldots, \sqrt[r_m]{r_m}) \]

where \( f \) was a rational expression, the expressions \( g_1, \ldots, g_k \) and \( r_1, \ldots, r_m \) were algebraic expressions of order \( \mu - 1 \), and \( p_1, \ldots, p_m \) were primes. Within each order, Abel described another hierarchy associated with the concept of *degree*. While the order served to denote the number of nested root extractions of prime degree, Abel’s concept of the degree of an algebraic expression counted the number of co-ordinate root extractions at the top level. Thus in Abel (1), the degree was the minimal value of \( m \) which

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\(^a\) Abel.\(^15\) For presentations of the proof, see e.g. Pesic.\(^16\) The present analysis is based on Sørensen,\(^17\) chapter 6.

\(^b\) The assumption that the equation be *general* means that no non-trivial restrictions can be imposed on the coefficients; in particular the result does not rule out algebraic solutions to specific equations with numerical coefficients.
would suffice to write the expression in this form. By further analysing his characterisation, Abel deduced a standard form — a representation — of explicit algebraic expressions of the form

\[ v = f(r_1, \ldots, r_k, \sqrt[\mu]{R}) = \sum_{u=0}^{p-1} q_u R^{\frac{u}{\mu}}, \quad (2) \]

where \( R \) was of order \( \mu - 1 \) and all the coefficients \( q_0, \ldots, q_{p-1} \) were functions of order \( \mu \) and degree at most \( m - 1 \) such that \( R^{\frac{1}{\mu}} \) could not be expressed rationally in the coefficients.

The representation (2) provided Abel with a way of accessing and manipulating the imagined solutions to the equation through the hierarchical structure imposed by “orders” and “degrees”. Thus, without the representation being explicit in the sense of a closed explicit formula, Abel had sufficient information about it to make it the object of his meta-level language-game of representability. Therefore, in the second component of his proof, Abel assumed that an explicit algebraic expression would actually solve the general equation. He then used the representations obtained above (2) to derive important consequences. Based on this central theorem, Abel could refer to available research on permutations to show that only root extractions with exponents 2 and 5 could occur in any supposed algebraic solution formula for the quintic equation.

In his third and final step, Abel investigated each of the cases included in the previous section and arrived at a contradiction in each case. Thus, the assumption that a root of the general quintic equation could be represented by an explicit algebraic function of the coefficients had been reduced ad absurdum, and the theorem was proved.

Abel’s use of the characterisation and resulting representation of any explicit solution formula was the key step in his proof. It allowed him to reason about an object that he would eventually prove was self-contradictory. In the process, he could use it to prove the crucial result that each radical in any supposed solution would depend rationally on the roots of the equation. This result had been missed by Abel’s only predecessor to a proof of the algebraic unsolvability of the quintic equation, the Italian Paolo Ruffini (1765-1822) (see Fig. 4), and it suggests how strong use Abel made of his

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\(^c\) Thus, as indicated, Abel’s concept of order counted the number of nested root extractions of prime degree. For instance, if \( R \) is a rational function (i.e. of order 0), \( \sqrt{R} \) will be of order 1, \( \sqrt[3]{\sqrt{R}} \) of order 2, and similarly \( \sqrt[3]{\sqrt{R}} + \sqrt{R} \) of order 2. Furthermore, \( \sqrt[4]{R} \) is of order 2, since it must be decomposed as two nested square roots, \( \sqrt[4]{R} \).
representation. As this analysis shows, it is perhaps more accurate to speak of the theorem as an unrepresentability result rather than an impossibility result.

<table>
<thead>
<tr>
<th>Step in proof</th>
<th>Ruffini</th>
<th>Abel</th>
</tr>
</thead>
<tbody>
<tr>
<td>Definition (characterisation) of algebraic solution formulae</td>
<td>Yes</td>
<td></td>
</tr>
<tr>
<td>Classification (standard form) of possible solution formulae</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>Each radical in solution formulae is rational in the roots</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>Each radical in solution formulae has 1, 2, or 5 values under permutations</td>
<td>Meticulous enumeration</td>
<td>By reference to Cauchy(^\text{18})</td>
</tr>
<tr>
<td>Reductio ad absurdum</td>
<td>Yes</td>
<td>Yes</td>
</tr>
</tbody>
</table>

Figure 4. Step-by-step comparison of Ruffini’s and Abel’s proofs of the algebraic unsolvability of the general quintic equation (for a presentation of Ruffini’s contribution, see e.g. Cassinet\(^\text{19}\))

Abel was well aware of the conceptual advantage of asking questions about representability rather than dealing with specific representations. In a posthumously published manuscript on a general theory of algebraic solvability of equations, Abel summarised the essentials of the previous approach and compared it to his new plan of attack:

To solve these equations [of the first four degrees], a uniform method was discovered which, it was thought, was applicable to an equation of any degree; but in spite of all the efforts of Lagrange and other distinguished geometers, the proposed goal could not be reached. This led to the assumption that the solution of the general equation was algebraically impossible; but this could not be decided since the adopted method had only been able to lead to reliable conclusions in the case in which the equations were solvable. In fact, one proposed to solve the equations without knowing if that was possible. In this case, one might come to the solution although that was not certain at all; but if by misfortune the solution was impossible, one might search an eternity without finding it. To infallibly reach anything in this matter, it is necessary to follow another route. One should give the problem such a form that it will always be possible to solve it, which can always be done for any
problem. Instead of demanding a relation, of which the existence is unknown, one should ask whether such a relation is possible at all.\footnote{On découvrit pour résoudre ces équations une méthode uniforme et qu’on croyait pouvoir appliquer à une équation d’un degré quelconque; mais malgré tous les efforts d’un Lagrange et d’autres géomètres distingués on ne put parvenir au but proposé. Cela fit présumer que la résolution des équations générales était impossible algébriquement; mais c’est ce qu’on ne pouvait pas décider, attendu que la méthode adoptée n’aurait pu conduire à des conclusions certaines que dans le cas où les équations étaient résolubles. En effet on se proposait de résoudre les équations, sans savoir si cela était possible. Dans ce cas, on pourrait bien parvenir à la résolution, quoique cela ne fût nullement certain; mais si par malheur la résolution était impossible, on aurait pu la chercher une éternité, sans la trouver. Pour parvenir infailliblement à quelque chose dans cette matière, il faut donc prendre une autre route. On doit donner au problème une forme telle qu’il soit toujours possible de le résoudre, ce qu’on peut toujours faire d’une problème quelconque. Au lieu de demander une relation dont on ne sait pas si elle existe ou non, il faut demander si une telle relation est en effet possible” (Abel,\textsuperscript{20} p. 217, emphasis added).} Asking whether “such a relation is possible at all” is what has here been called a question of representability.\footnote{Curiously, in lectures given in Cambridge in 1939, Wittgenstein touched upon the same point regarding the quintic equation. (Wittgenstein,\textsuperscript{21} p. 88)} Abel’s vision of a new approach to asking the right questions in analysis was picked up by some of his most prominent contemporaries. For instance, Carl Gustav Jacob Jacobi (1804-1851) heralded the new programme when he wrote to Legendre upon hearing of Abel’s death:

The vast problems that he proposed to himself — namely to establish the necessary and sufficient criteria for any algebraic equation to be solvable or for any integral to be expressible in finite quantities, or his admirable discovery of the theorem encompassing all the functions which are the integrals of algebraic functions, . . . — characterise a very special type of questions that nobody before him had dared to imagine. He has gone but he has left a grand example to be followed.\footnote{“Les vastes problèmes qu’il s’était proposés, d’établir des critères suffisants et nécessaires pour qu’une équation algébrique quelconque soit résoluble, pour qu’une intégrale quelconque puisse être exprimée en quantités finies, son invention admirable de la propriété générale qui embrasse toutes les fonctions qui sont des intégrales de fonctions algébriques quelconques, etc., etc., marquent un genre de questions tout à fait particulier, et que personne avant lui n’a osé imaginer. Il s’en est allé, mais il a laissé un grand exemple” (Legendre and Jacobi,\textsuperscript{22} p. 265–266; translated into German in the Pieper edition, p. 153).}

Most of the specific examples mentioned by Jacobi transcend the present
discussion, but the main feature of studying representability applies to them as well. In each situation, “one may notice throughout his [Abel’s] works a certain tendency to completely classify classes of mathematical objects by a fitting formalism” (Petri, p. 231). Thus, by finding fitting representations of his concepts, Abel could pose and often solve the questions of representability such as finding the “necessary and sufficient criteria for an arbitrary algebraic equation to be solvable” mentioned by Jacobi and attempted by Abel in his posthumous manuscript. That particular delineation problem was eventually solved with the development of Galois theory that translated it into a question in group theory.  

5. Representations and habituation

In the case of equations, Abel had used representations to define and access the concept of algebraic solvability and had posed the question of representability. As such, the representations had served to both fix the concept and provide a handle for manipulating it. In the following case concerning the introduction of new functions into analysis, the role of the representations were more numerous and more diverse.  

Abel’s most immediately recognised contribution to analysis was his introduction of elliptic functions in 1827 (Abel). In the eighteenth century, Adrien-Marie Legendre (1752-1833) had studied so-called elliptic integrals of the form

$$
\int \frac{x^k \, dx}{\sqrt{a_3 x^4 + a_2 x^2 + a_1 x + a_0}}
$$

that generalised trigonometric functions and provided a natural first additional instance of transcendental functions to be studied in analysis. In his seminal paper of 1827, Abel proposed to study the inverses of elliptic integrals, $\phi(\alpha) = x$, and he gave them the name of elliptic functions:  

$$
\phi(\alpha) = x, \quad \alpha(x) = \int_0^x \frac{dx}{\sqrt{(1-c^2 x^2)}(1+e^2 x^2)} \sim \phi(\alpha) = x. \quad (3)
$$

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See e.g. Kiernan.  

The representation of elliptic functions and the process of habituation is discussed in more detail in Sørensen, upon which this discussion is based.  

Legendre had actually used that name for what is here called ‘elliptic integrals’. In the following, Abel’s chosen factorisation of the polynomial in the integrand as $(1-c^2 x^2)(1+e^2 x^2)$ has been retained.
The inversion made obvious sense for a segment of the real axis, but Abel quickly extended it first to a segment of the imaginary axis and then to any complex variable.\textsuperscript{c}

Here, it is important to stress that although theories of complex integration were in vogue in the 1820s, no well established theory offered an \textit{arithmetical} (as opposed to \textit{formal}) interpretation of the integration between complex limits involved in (3) when \( \phi(\alpha) \) was extended to a function of a complex variable. Therefore, the means of ‘access’ to the function \( \phi \) had to rely on the inversion (3), itself. After deducing basic facts about the new functions, Abel devoted a substantial part of the paper to deriving representations of \( \phi \) in terms of infinite series and products. Abel’s road to these representations consisted of long and formal manipulations starting from a finite formula and letting the number of terms increase to infinity. Among the results obtained, Abel found that elliptic functions could be expressed (represented) in three different categories: The first set of representations consisted of two-fold infinite series or products in which the terms were rational expressions in the known quantities. The second category was based on collecting and grouping terms of the previous representations to obtain (one-fold) infinite series or products of elementary transcendental terms. These two types were supplemented in 1829 by a third category when Abel claimed that elliptic functions could also be represented as quotients of convergent power series.\textsuperscript{d} Surprisingly, Abel never used these representations in subsequent arguments except for one single and peripheral occasion (see below). Therefore, the question arises of why Abel bothered and devoted so much attention and space to obtaining representations in the first place.

One possibility would be that the representations directly provided mathematical insight. Potentially, they could serve to allow numerical calculation and tabulation of the elliptic functions and such had been one great motivation for Legendre. However, Abel never bothered expressing elliptic functions numerically, let alone tabulating them. In particular, Abel never discussed either the numerical convergence of the expressions or the rate of convergence — both would have been important questions if he had been interested in tabulating the new functions.

A second possible insight gained from the representations would be of a structural nature. The fact that elliptic functions could be represented as

\textsuperscript{c} See Houzel\textsuperscript{28} or Sørensen\textsuperscript{17} (chapter 16) for details of this imaginary substitution and the subsequent derivation of the addition formulae.

\textsuperscript{d} Abel.\textsuperscript{29} Such representability as a quotient of convergent power series, i.e. as a meromorphic function, later acquired great structural importance; see below.
two-fold infinite expressions of rational terms or as (one-fold) infinite expressions of elementary transcendental terms would have supported Abel’s understanding of them as the immediate next type of transcendental functions after the elementary ones. However, such comments were never made explicit by Abel, and even such structural information would hardly alone justify the labour he spent on obtaining the representations.

A third role for these representations can be inferred from the one single instance when Abel actually made use of one of them in a subsequent argument. It occurred in another paper devoted to demonstrating how certain of Jacobi’s results in the theory of transformations of elliptic integrals could be easily derived from Abel’s framework. There, Abel reproduced one of the representations and related two elliptic integrals by equating the representations of their inverses. From this, he was able to deduce Jacobi’s criterion of transformation.\textsuperscript{e} Such would be a prototypical use of a representation similar to the ones discussed above in connection with algebraic solvability. However, as mentioned, such use was rare in Abel’s mathematics, and not present in the paper where the representations were derived.\textsuperscript{f}

A fourth — and in my opinion more likely — reason why Abel felt a need to produce these representations is that they may have served an end as ‘tools of habituation’ for the newly defined functions. As previously mentioned, mathematicians had developed a powerful technical machinery for manipulating explicit expressions and had grown used to thinking of functions in terms of their explicit expressions. Such was the case of Euler’s analysis that was the epitome of analysis around 1800 and a major source of motivation and knowledge for Abel. Contrary to such customary thinking about functions, Abel’s newly defined elliptic functions were not expressible as explicit analytical expressions from the outset. As mentioned, mathematicians of the 1820s even lacked a clear theory of complex integration to make sense of the inversion for complex arguments. All in all, it seems likely that the various infinite representations that Abel produced for his new functions could very well serve to render these new objects ‘functions’ in the style of Euler. The fact that Abel could produce more than one representation — e.g. both series and products — then reflected that the more representations were available, the more familiar the new functions would seem to be.

\textsuperscript{e} For the details, the reader is referred to Sørensen.\textsuperscript{26}

\textsuperscript{f} As Abel died at the age of 26, the lack of such uses of representations does not rule out that he could have made them later on, had he lived longer. However, as argued, it does not seem to have been a major concern for him.
Thus, we are led from considerations of the role of these representations to ponder the question of how new functions can be introduced into mathematics and be made ‘known’ to the mathematicians working with them. The notion of being ‘known’ in this context is not primarily a technical or logical one, but must be interpreted more along the lines of ‘familiarity’. One of the few comparable cases to have been studied from a similar historical perspective has to do with the habituation of the exponential and logarithmic curves around 1700 (Bos\textsuperscript{30}). There, a similar situation arose in which implicitly defined functions were in need of some explicit contextualisation within existing methods and theories in order to make the new functions ‘known’ and familiar. In particular, the degree of familiarity can be seen from two of the components of the process of making known: the acceptance of the new functions as results and answers to questions and the technical mastery of the new objects. In the case of Abel’s new elliptic functions, they were met with some reluctance from Legendre, who was the main proponent of the theory of elliptic integrals around 1820. To him, the new inverse functions were hard to grasp, and he continued to rely more on his own approach.\textsuperscript{8} It is my hope that the present discussion and, in particular, the description of the process of habituation given in Fig. 3 add to the historiography of mathematics by following Henk Bos in emphasising how mathematicians came to accept and know new objects in the form of new functions.\textsuperscript{h}

Contrary to Legendre, Jacobi came to master the technique of the new elliptic functions, but he kept searching for other ways to approach them. Most importantly, almost simultaneous with Abel’s death in 1829, Jacobi published his approach to the foundations of elliptic functions in his \textit{Fundamenta nova theoriae functionum ellipticarum} (henceforth \textit{Fundamenta nova}).\textsuperscript{32} In its second part, the \textit{Fundamenta nova} laid out many of the same basic results as Abel had presented in 1827. However, when it came to the role of representations, Jacobi’s emphasis differed from Abel’s.

At the outset of the \textit{Fundamenta nova}, the inversion was not the essential idea. Throughout the first part of the \textit{Fundamenta nova}, Jacobi considered elliptic functions as essentially formal inversions much in the same way as Abel had done in the first section of his \textit{Recherches sur les fonctions elliptiques}.\textsuperscript{27} In particular, nothing like infinite representations were presented by this point of the \textit{Fundamenta nova}. However, that was

\textsuperscript{8} See e.g. Legendre $\rightarrow$ Jacobi, Paris, 4 June 1829 (Legendre and Jacobi,\textsuperscript{22} p. 263).

\textsuperscript{h} Bos\textsuperscript{31} (pp. 1633–4); see also Bos.\textsuperscript{30}
to change in the second part of the *Fundamenta nova* that was devoted to “the theory of developing elliptic functions”, beginning with the expansion into infinite products or into quotients of trigonometric series. Jacobi formulated his central point acutely in one of his section headings: “The elliptic functions are fractional functions of which the functions \(H\) and \(\Theta\) serve as numerator and denominator”.\(^1\) After Jacobi had thus reduced elliptic functions to quotients of the new auxiliary functions \(H\) and \(\Theta\), he was faced with elucidating these new functions. Jacobi provided that elucidation by proving some fundamental relations between these new functions before developing \(H\) and \(\Theta\) into infinite series of trigonometric terms.\(^k\)

In the decades following Jacobi’s *Fundamenta nova*, Jacobi (and other mathematicians) came to pay even more attention to the functions \(H\) and \(\Theta\) and to other functions that could be linked to them and provide a new way into the theory of elliptic functions and integrals. Such re-orientations were undertaken for a combination of goals. The functions \(H\) and \(\Theta\) could serve to provide a new foundation for the original elliptic functions of Abel and Jacobi, but more importantly, they promised a way to generalise these functions to higher transcendental functions — a promise that led to one of the most prominent research programmes of the nineteenth century. Jacobi presented one step in this direction in lecture courses during the 1830s when he generalised the above-mentioned functions into so-called \(\vartheta\)-functions that found a variety of uses.\(^l\) These functions and relations among them were subjected to extensive study in the years following the *Fundamenta nova*.\(^m\)

However, something important had fundamentally changed between the *Fundamenta nova* and Jacobi’s lecture course. In the *Fundamenta nova*, Jacobi had arrived at the functions \(H\) and \(\Theta\) from infinite expansions of elliptic functions. Thus, in the *Fundamenta nova*, the series for \(H\) and \(\Theta\) were secondary to the elliptic functions. In the lecture course, on the

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\(^1\) Theoria evolutionis functionum ellipticarum (Jacobi,\(^{32}\) p. 139).

\(^j\) “Functiones ellipticae sunt functiones fractae. De functionibus \(H, \Theta\), quae numeratori et denominatoris locum tenent” (Jacobi,\(^{32}\) p. 224).

\(^k\) In a sense, Jacobi did the obvious thing suggested by the works of Fourier: When given a periodic function, try to express its Fourier series.

\(^1\) Jacobi’s lectures, however, remained unpublished until 1881; Jacobi.\(^{33}\) The first two \(\vartheta\)-functions were directly linked to the above-mentioned functions of the *Fundamenta nova*:

\[\vartheta(x) = \Theta\left(\frac{2Kx}{\pi}\right) \quad \text{and} \quad \vartheta_1(x) = H\left(\frac{2Kx}{\pi}\right),\]

whereas two additional functions \(\vartheta_2\) and \(\vartheta_3\) could be defined by very similar series.

\(^m\) See e.g. Houzel\(^{34}\) (pp. 41–5).
contrary, the four new $\vartheta$-functions were introduced by their series, all similar to

\[ \vartheta(x) = \sum_{\nu=0}^{\infty} (-1)^{\nu} q^{\nu^2} e^{2\nu x} = \sum_{\nu=0}^{\infty} (-1)^{2\nu} 2q^{\nu^2} \cos 2\nu x. \]

(5)

These series were studied extensively by Jacobi and others, not only because they were at the foundation of the theory of elliptic functions, but also because they were interesting by themselves, and appeared in other contexts. Comparing his approach in 1838 to that of the Fundamenta nova, Jacobi stressed the change of perspective:

Here, I intend to reverse the historical process of discovery of elliptic functions and follow the road in the opposite direction.$^n$

Later in the nineteenth century, the definitions of elliptic functions would change again in the hands of e.g. Joseph Liouville (1809-1882) and Karl Theodor Wilhelm Weierstrass (1815-1897), who each found a different basis for the theory of elliptic functions.$^o$ Liouville’s re-orientation of the theory of elliptic functions is particularly interesting in the present context because it constitutes an astonishing change of aspect that led to a need for habituation. In this and other respects, it was a prototypical concept-centred approach to the subject. In 1844, Liouville took the radical step of basing the concept of elliptic function on concepts of the emerging and developing field of complex analysis.$^p$ According to Liouville’s definition, an elliptic function is a doubly periodic, meromorphic function — nothing more, nothing less. This shifted the central emphasis of the theory of elliptic functions to an aspect that had been thought of as “properties” in e.g. Abel’s approach. Based on the theory of complex analysis, it then became clear that elliptic functions — qua meromorphic functions — admit representations as quotients of power series; something that had also been noticed by Abel and others. As such, this result points out a familiarity between Liouville’s new concept and the theories of Abel and Jacobi, thereby serving as an act of habituation. The most profound strength of Liouville’s ideas, however, was that it opened the theory of elliptic functions to the

$n$ “Im Folgenden beabsichtige ich, den historischen Gang der Entdeckung der elliptischen Functionen umkehrend, den entgegengesetzten Weg einzuschlagen” (Jacobi, 33 p. 499).

$o$ See e.g. Houzel (pp. 21–4, 45–8) and Lützen (pp. 527–57).

$p$ Liouville published little on the subject with only short announcements such as Liouville. For discussions of Liouville’s work on elliptic functions, see Lützen (pp. 527–57) or Peiffer (pp. 221–32).
use of other abstract methods such as relations between the singularities that could be formulated using e.g. Cauchy’s theory of residues."}

6. Conclusion

During the transitions within analysis in the first half of the nineteenth century, “functions” in many ways remained at the centre of the subject. However, in different contexts and for different purposes, functions were treated in a variety of ways. In this paper, I have illustrated and discussed how various “aspects” were emphasised in different situations based on a Wittgenstein-inspired framework.

In the case of the algebraic unsolvability of the general quintic equation, the analysis shows how it was an important part of the result to make the “representation” an object of independent study. Thus, the shift of emphasis from the solution to the question of solvability is closely related to the shift from representation to representability. In particular, Abel’s proof was here discussed in order to illustrate how it differed from the few previous proofs by incorporating this new role for representations as part of the formulation of and solution to this important mathematical problem.

The case concerning the introduction and study of elliptic functions involved even more roles for representations. When Abel originally set out to study the elliptic functions as formal inverses of elliptic integrals, he provided one aspect of these new functions. However, this aspect only gave implicit (indirect) knowledge about some of the key questions concerning the new functions, and Abel’s subsequent infinite expressions for the elliptic functions can be seen as exhibiting additional aspects. Thereby, and because infinite expressions had been a core component of the technical mastery of functions since the time of Euler, Abel’s presentations of these new aspects could be seen as acts of habituation aimed at a mathematical community steeped in a formula-centred (Eulerian) approach to analysis. Thus, this episode resonates with the Lakatos-inspired characterisation that “one of the principal heuristics governing the growth of mathematics [is to] make the implicit explicit” (Muntersbjoern, pp. 161, 167 et passim). Combined and viewed within this Wittgenstein-inspired framework, the two case studies shed new light both on the ways in which mathematicians worked with functions in the nineteenth century and on ways of historically and philosophically thinking about these. This approach has emphasised and addressed practice-based questions of how mathematicians worked with

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34 See e.g. Houzel (pp. 21–3).
representations in addressing important topics of the nineteenth century. In particular, it has pointed to the need for acts of habituation when new concepts were introduced based on new aspects such as was often the case during the transition from a *formula-centred* view of mathematics towards a more *concept-centred* view of mathematics.

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1. The graph metaphor

Consider this schematic attempt to survey all of mathematics. A typical paper in mathematics written at any time in the last 200 years establishes a result, or perhaps several results, here called theorems, that are, in the author’s opinion, of greater significance than some other results established in the paper that are steps on the way to the theorems; let us call these lemmas. If the author is not so obliging as to stratify his results, let us do so for him. We now imagine a directed graph in which all the lemmas and theorems are vertices, and the directed edges are the proofs, and we will pretend that the paper is so carefully argued that the edges are entirely composed of routine, valid arguments. So an edge means something like: ‘the start vertex is used to show the end vertex’.

Of course, the paper is not self-contained; it rests, implicitly and explicitly, on other results in other papers, and so we need to fit this paper together with all the others that have been written, all the books, and so forth. The result would be a monstrous directed graph. Such a thing does not exist, although I suspect it would not be impossible to create given the will to do so. What does exist is the informal awareness among mathematicians of greater or lesser parts of the graph. It is entirely possible to map out a course of study that will take a beginner to their chosen destination at the frontiers of research in much this fashion; that is what books and well-structured lecture courses collectively do.

Let me call this picture the graph of mathematics, or the graph metaphor (for mathematics). My first question is: what, if anything, is
significantly wrong with this picture of mathematics? Of course, I believe there is something wrong with it other than its naïveté. It is not merely over-simplified, it is wrong in ways that would not be remedied by refining the graph; it is the wrong metaphor for mathematics. Certainly we can locate neither the history nor the philosophy of mathematics in it, and, I shall suggest, they cannot be fitted into it in any satisfactory way. But notice that the graph has at least some merit in the eyes of professional mathematicians: it joins up results by chains of routine arguments and it forms a structure to which new results can be joined.

History first. The edges of an ideal graph would all be valid proofs. Those in any actual version of the graph are the written-down proofs, so the graph does not reflect the (let us suppose) timeless logic of implication but the temporal facts of discovery. We could colour code them, display the graph as it evolves every day, week, month or year. For some people that’s all history is — one damn thing after another — but not, I hope, for anyone seriously interested in history. So historians could only accept the graph provided they were allowed access to all the graphs. When I have in mind the graph metaphor seen chronologically I might speak of it as a picture of linear growth: new results are simply joined on to old ones. Occasionally a vertex might have to be cut off, if it is found to have been joined on by a fallacious argument. But the historian of mathematics is likely to reject even this dynamic graph evolving over time as sufficient: it lacks any element of historical analysis, explanation, or structure. There are no mathematicians in it, no motivations, no connections to any society in which this mathematics is done.

Philosophy next. The edges are unproblematic from this standpoint too, so the philosophers’ attention shifts to the vertices (or small collections of vertices) that have no incoming edges. These typically will be the axioms and fundamental definitions in a branch of mathematics. Philosophers may also focus on vertices with few incoming and many outgoing edges, to allow for those cases when a fundamental definition or a key theorem is incorporated in a larger structure. This happens when, for example, real analysis is seen as a part of topology, or when topology is seen as an aspect of set theory. Insofar as these assumptions are extra-mathematical they seem like the best target for philosophical enquiry. But if the graph suggests certain philosophical tasks, notably those in foundations, it does not suggest all of them: it lacks enough structure to highlight significant structural features of the mathematical enterprise.

Philosophers of mathematics have, it seems, concentrated well and fruit-
fully on topics in the foundations of mathematics and the related field of mathematical logic, but this conference addresses a feeling that there is work to be done elsewhere in what can be called the philosophy of mathematics. My tribe, the historians of mathematics, are an easier group for me to criticise, and I think we must agree that too much history of mathematics is a walk along part of the graph, bulked out with matters the graph does not try to reflect (biographical, social, motivational, etc.).

I offer the graph as a challenge. It is a picture of mathematics superficially attractive to mathematicians, it allows for a routine kind of history, and it suggests that philosophy of mathematics should be a study of the foundations of mathematics. We are out of business if our call to action does not result in something better than what this graph allows.

2. Are we there yet? (And why we aren’t)

We need to think what the defects of the graph are, and why the problems they suggest in the history and philosophy of mathematics weren’t raised and solved a generation or two ago. There are many answers.

(1) Mathematical logic is not philosophy of mathematics, but it is close, and it is powerful and precise. Analysis of fundamental presuppositions in domain X is rightly regarded as a prime focus in philosophy of X, but somehow, those questions ceased to engage mathematicians, who lost interest in the philosophy of mathematics and often (but not always) kept mathematical logic at a considerable distance.

(2) There is a division of labour that assigns to the mathematician the task of pronouncing on the validity of a proof. This plays to a view that philosophers need not concern themselves with mathematical validity but can accept the verdict of the mathematician. The historian of mathematics can also subscribe to that point of view, and when these groups do so subscribe they elect not to study the historical and philosophical issues hidden in the edges of the graph.

(3) The nature of mathematical knowledge has an awkward place in theories of knowledge, and not all philosophers engage with it even when epistemology is their subject.

(4) An overweening pride among philosophers of science gave their work a disagreeably prescriptive or normative edge which did not endear them widely.

(5) History of science as practised in Britain or America is done these days with almost no attention to the issues that engage historians of
mathematics or, all too often, philosophers of science.

Lakatos was one of the few philosophers of mathematics to contest this image. His discussion in Lakatos\(^1\) of a relatively trivial topic in mathematics, the classification of surfaces, was intended as a metaphor that displayed other features of mathematical life, chiefly centred on the analysis of purported proofs. He was not interested in the quality of routine arguments, but pointed out two more global processes that are at work. Monster-barring concerns counter-examples, concept-stretching the way in which the fundamental concept can seemingly change. A monster might be excluded — ‘the theorem is not intended to cover that’ — and it might be included later on when the concept has been suitably stretched.

There are two criticisms of the graph metaphor here. One says that the graph does not adequately deal with discovery but is too focussed on its aftermath. The other says that the process of drawing edges from existing vertices to a new one is subject to an analysis that goes beyond the quality of those edges, which is why I called it global earlier on. It is reasonable, I believe, to think of these points as concerning the meaning of the key mathematical terms; one did or did not intend by, say, ‘polyhedron’ to mean, refer to, include something ‘like that’. These observations of Lakatos are entirely sound, but they do not have the force in history and philosophy of mathematics that he must have hoped for, and one reason for that is that they are simultaneously too radical and too timid. Too radical in their opposition to foundationalist reductionism (the idea that all philosophical questions in mathematics concern the foundations of mathematics as laid down in various ways in the twentieth century). Too timid because I think it came to be felt that there might be many ways in which mathematical practise departs or has departed from the linear growth model implied by the graph metaphor, so many that the philosopher of mathematics would do well to regard the actual growth of mathematics as too ad hoc, culture specific, or downright psychological to be grist to any philosophical mill; better to stick with the foundations of mathematics where honest philosophical work is to be done.

3. A look at the history of analysis

3.1. Dirichlet on Fourier series

I want to look at the history of analysis in the nineteenth century again, and suggest that there are ways of thinking about it that on the one hand show clearly what is wrong with the graph metaphor for mathematics, and that
on the other hand show clearly how history and philosophy of mathematics can proceed together to their mutual advantage.

I will start not with Cauchy but with Dirichlet, because I think Dirichlet was the first to see clearly the philosophical implications for mathematics of the new proof methods proposed by Cauchy for the treatment of real analysis. In 1829 Dirichlet published one of the best-known and widely appreciated papers in mathematics, in which he set down his ideas about the convergence of Fourier series. Fourier had claimed, most evidently in his *Théorie analytique de la chaleur* of 1822, that any function $f(x)$ on the interval $[0, 2\pi]$ can be written as an infinite series of the form

$$\frac{1}{2} + \sum_{n=1}^{\infty} a_n \cos(nx) + b_n \sin(nx),$$

where

$$a_n = \frac{1}{2\pi} \int_{0}^{2\pi} f(x) \cos(nx) \, dx$$

and

$$b_n = \frac{1}{2\pi} \int_{0}^{2\pi} f(x) \sin(nx) \, dx.$$

His argument was little more than a naive calculation; his claim was entirely general.

As put, this provoked Poisson and Cauchy into attempting proofs, but Dirichlet's intervention proved decisive. What attracts me to this proof in the present context is Dirichlet's willingness to restrict the claim to a class of functions for which he can offer a proof. These are the functions which are monotonic increasing or decreasing except at a finite number of points in the interval $[0, 2\pi]$ and are continuous in that interval except again at a finite number of points. For these functions the apparatus of Cauchy's calculus can be used to prove convergence. On the other hand, as Dirichlet noted with the 'toy' example of the function that takes one value at the rational numbers and another value at the irrational numbers, the theorem cannot be true for every function on the interval (because this one cannot be integrated, so there is no way of evaluating $a_n$ and $b_n$). He could have offered a meatier example, $\sin\left(\frac{1}{x}\right)$, which is not piecewise monotonic in the interval $[-\pi, \pi]$.

With this theorem, Dirichlet abandoned the idea that a function is something we know about, and which has automatically a number of properties. You might argue that Cauchy had earlier reached this position, but I would
rather interpret Cauchy as a sophisticated naturalist: there are functions out there in the same way that there are snakes. Some snakes may be poisonous while others are not, just as some functions may be analytic, differentiable, continuous, while others are not. And of course there will be cases where it is too hard to say. Dirichlet would not have disagreed, but the ‘toy’ function shows that Dirichlet knew that Fourier’s claim would not cover all the range of functions, because some eluded the fundamental techniques of the calculus. He did envisage extending the range of functions for which Fourier’s claim could be proved, although he never succeeded, but the realisation that the calculus was not adequate to describe the full range of functions was first made by Dirichlet.

3.2. Riemann on Fourier series

This raised another question: what precisely were the functions that the calculus applied to, and what were out of range? Until that question was answered, mathematicians could not claim to understand the calculus after all, despite all Cauchy’s fine work. This was Riemann’s opinion, in the magnificent paper he wrote for his Habilitation in 1854 (first published in 1868). He was willing to agree, he said, that Dirichlet’s examples covered all the functions that arise in nature — that is, in the study of natural phenomena. But there were applications of Fourier series methods outside the physical sciences, for example, to number theory, where precisely the cases omitted by Dirichlet ‘seem to be important’. And there was, in any case, the need for clarity and rigour in the principles of the infinitesimal calculus, and that plainly could not be fully met until this whole question was fully resolved.

Riemann then proceeded to open the subject up completely by giving a method for finding explicit examples of functions that failed in various ways to agree with their Fourier series, or failed to have a Fourier series at all. The details need not concern us, but we should note that in exploring the range of functions to which the differential calculus applied Riemann spoke of investigating functions without making any particular assumptions on their nature. Later workers were going to spend some decades talking about what they called ‘assumptionless’ functions. At least in Riemann’s opinion, these functions were not only entirely arbitrary, they were the exclusive property of the mathematicians because they do not occur in nature. Of course nature was to reject these constraints, but it is clear that at this stage Riemann was quite clear that mathematics did not stop with the objects one might say were (possibly idealised) abstractions from things
in the ‘real’ world.a

Mathematics then, for Riemann, is about concepts created by the mathematician, and one important problem here is to evaluate the methodological tools by which such functions are created. May I use the word non-linearity here in a metaphorical sense? It is not the case that there are functions out there which are brought to the laboratory where they may baffle existing techniques. The functions cannot be defined without these techniques; they are a product of the techniques used to analyse them.

3.3. Cantor

I can only look briefly here at another well-known part of the story of nineteenth century analysis that bears retelling: the pre-history of Cantor’s transfinite ordinals. The original, and significant, question that Cantor took over from his colleague Heine and answered very much more profoundly was a question about the limits of mathematics. Cantor wanted to know how, or at what point, does the theory of Fourier series break down and a function have more than one Fourier series, which would surely spell trouble. It was to that end, via his theory of derived sets, that he constructed not only strange subsets of the real numbers (at that time nowhere properly defined) but also the process that was to lead him to the transfinite ordinals a decade later.

3.4. Paul du Bois-Reymond

The first volume of Paul du Bois-Reymond’s Allgemeine Functionentheorie, but the only one to appear, was published in 1882.5 It provoked at least one sharp rebuttal, by Benno Kerry,6 which shows how divergent views had become in the philosophy of mathematics.b Much of his book was motivated by the following question. One often asks in mathematics if a certain expression has a meaning and, in particular, a numerical value. Typically, the expression is obtained from a sequence of meaningful expressions, so the question is whether this sequence has a limiting value. Du Bois-Reymond’s Allgemeine Functionentheorie deals with two themes: certain types of infinite sets of numbers, and philosophical questions in the foundations of the

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a Note here that when a mathematician speaks of a function disagreeing with its Fourier series this already involves a distinction between what the function is and how it is represented; Fourier had only imagined that a Fourier series would equal the function of which it was the Fourier series.

b I deal with these and related issues at greater length in my forthcoming book Plato’s Ghost.7
calculus. They are themes on which he could speak with some authority. Du Bois-Reymond considered the sorts of infinite subsets of the real line that can arise, alerted by his study of Fourier series, and he wished to give some kind of scientific or even philosophical account of what his infinite sets are. Here he continued a dispute between two groups he called idealists and empiricists that he had raised in the first half of his book. Roughly speaking, the idealist is able to go to the limit, the empiricist is not.

By a simple construction he cooked up a sequence of complicated infinite sets of points, $B_n$, that exemplified Cantor’s iterative treatment of sets. For each value of $n$ the point set $B_n$ is a subset of the point set $B_{n+1}$, and the nature (some might say, even the existence) of the union of the sets $B_n$ is at issue; it is a subset of the interval $[0, 1]$, but not the whole thing. Du Bois-Reymond concluded that the infinite point set of all numbers between 0 and 1 existed only for his idealist. His empiricist, he believed, could accept only countably infinite sets, whereas what remains when the union of the sets $B_n$ is removed is uncountable.

The idealist and empiricist positions are irreconcilable. To do analysis, du Bois-Reymond therefore proposed a neutral language, set out under the motto of ‘Empiricist language, idealist proofs’ (p. 156). Although the idealist may say a limit exists, the empiricist may say that the existence is nothing more than the existence of the sequence — it matters not. What matters is that the talk of limits and limiting values has been made precise in a way each can, on different grounds, accept. This neutral approach resolved not only the question of the meaning of infinite sets, but, as du Bois-Reymond put it, broke the spell and allowed analysis to be the mistress of the house (p. 167).

In 1890 the philosopher Benno Kerry’s posthumous book *System einer Theorie der Grenzbegriffe* appeared. It represents his definitive criticism of du Bois-Reymond’s ideas. It did not seem to Kerry that the existence of a limit in either the idealist or the empiricist senses that du Bois-Reymond had invoked mattered in mathematics, unless one wanted it to be true and applicable. Rather, all that was required was that a concept, such as limit, was clear and fruitful for making hypothetical judgements. Du Bois-Reymond, Kerry noted, had an exclusively geometrical sense of existence in mind, but the way in which geometric points exist should not be confused with the way in which limits exist. Things exist in different ways, and failure

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$c$ The union of the sets $B_n$ is the union of a countable union of countable sets, so it is countable, but the set of all real numbers is not countable, a fact Cantor had proved in 1874.
to exist in one sense does not preclude it in another.

As for limits, said Kerry, often their existence is undoubted: the sequence $1/n$ tends to 0 with increasing $n$, $1/3$ is the limit of 0.3333..., and so on. Some limits are taken to exist even when they are never seen — ‘a pure black colour’, for example. In the case of the physicist’s perfect fluid, what matters is the applicability of the deductions, not their truth. Such limits are different from the ‘limit’ of the sequence $1, 1 - 1, 1 - 1 + 1, \ldots$ because that ‘limit’ is a self-contradictory idea.

Du Bois-Reymond’s book bears witness to a deeply held belief that mathematics must be about something, its objects should exist, and should do so in a way closely akin to the way physical objects do. Existence should mean something like existence in space and time. Kerry’s alternative was much more radical. Existence is freedom from contradiction, mathematical objects may exist in many ways and have merely to imply coherent conclusions. Du Bois-Reymond’s approach can seem remarkably limited, but it is not, ultimately, that different from Frege’s.

4. A second look at the histories and philosophies of mathematics

If you look at even the best histories of mathematics — and this is a topic with very good coverage — the above account is not what you see. There is much more emphasis on the difficult mathematics, the discovery of point sets of various kinds (such as the famous Cantor set), the failure to distinguish topological properties of sets from measure-theoretic ones, and so on. Increasing rigour is a prominent theme. I have no quarrel with that, except that a dimension is missing.

If, on the other hand, you look at philosophy of mathematics at any time in the last fifty years for a discussion of this, you also don’t see this part of the story. There is considerable attention to the reductionist narrative, which reduces the real numbers ultimately to sets. It picks up foundationalist questions, it looks at increasing rigour and follows them until the reduction is done. On one account the process ends with Weierstrass and Frege and the transition form reductionism to logicism. On another, it ends with Weierstrass, and Frege has failed to appreciate what the mathematicians did. Recently, Tappenden has contested the accuracy of each version on the grounds that it neglects the arguably more important developments in complex analysis, and argued that this has contributed to a failure to appreciate Frege’s mathematical concerns. What is missing in each account, the historians’ and the philosophers’
of mathematics, is exactly the overlap: the fact that these mathematicians were behaving like philosophers of mathematics, and the fact that we need to be both historians and philosophers to see it.

5. Mathematicians as philosophers of mathematics

The change from mathematics as the study of the idealised objects of nature to mathematics as an abstract enquiry with its own rules, independent of nature, occupied the whole of the nineteenth century and the early decades of the twentieth. On the first description, existence questions in mathematics are merely disguised versions of existence questions of the usual sort. Rules for correct reasoning are guided by their correctness when applied to the objects under discussion. Mathematics is true, because it is about (idealised) objects and its statements are meaningful. Rigour is a matter of reasoning more carefully about things we find it harder to access epistemologically.

On the second description, all this is changed. Existence questions cannot be settled by appeals to nature but require new criteria. Rules for correct reasoning must, ultimately, be adapted to the new, abstract, conceptual objects, even if they are grounded in, as it might be, day-to-day logic. Mathematics is not in any simple sense true, its statements are not meaningful. It may be useful, correct in some sense, but its applicability becomes both mysterious and possible in new, more indirect ways. Rigour is involved in the very creation of the objects.

Very little of these changes would have been apparent at the outset. Agreement about them was not to be expected. Some quite fundamental questions would seem absurd; the old certainties might be recaptured in an altered form.

The fundamental error implicit in the graph metaphor, I suggest, is the way it presumes a false definition of mathematics. On this definition, we know what mathematics is, if only in the sense that we can recognise it when we see it. We can recognise putative mathematical claims and their purported proofs, and attach vertices (theorems) to other vertices by the right edges (proofs). And so, indeed, we can, much of the time. All of the time for routine mathematics, much of the time even for the harder stuff. Just as, in the much-quoted phrase, we know what time is — until we think about it. I want to argue that there are times when mathematicians do not know what mathematics is, and to find out they have to behave like philosophers, in that they must analyse the very foundations of their subject.
The foundations need not be the past axioms of set theory. These days they are quite likely not to be, and certainly they were not in the nineteenth century. They may, for example, concern the way mathematical terms acquire meaning and mathematics can be used, or understood. They may, as here, concern the nature of mathematical objects: by what legitimate processes can they be constructed? And they may concern the processes themselves: what processes are in themselves legitimate?

Riemann took up a question that Dirichlet had only felt able to hint at when he (Dirichlet) limited his arguments and hinted at the existence of counter-examples to the original claim. Riemann made this into a question about the limits of arguably the most powerful branch of mathematics, the differential and integral calculus. If the philosopher and the historian see only technical mathematics here, if they just see the creation of a chain of definitions, theorems, and proofs, they miss a significant dimension of the original enterprise. This I believe was a profound investigation into the nature of mathematical objects. It was clear to several people in the field, I believe, that what had to be decided was what the calculus could do. If this was less than it had been thought (which was to answer any mathematical question about any function) then how could that be shown? What objects could serve as counter-examples, what would delineate the boundary between the knowable and the unknowable, and how could the mathematician be sure?

We know about similar questions in the early days of set theory. I suggest they were around before (and since, but I haven’t argued for that). I suggest that if we look at the history of mathematics with philosophers’ eyes and the philosophy of mathematics with historians’ eyes we will find common ground, and we will find that some of the mathematicians of the past were there before us.

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DEDEKIND, STRUCTURAL REASONING, AND MATHEMATICAL UNDERSTANDING

ERICH H. RECK
Department of Philosophy
University of California, Riverside
* E-mail: erich.reck@ucr.edu
http://faculty.ucr.edu/~reck/

1. Introduction

The last few decades have witnessed a broadening of the philosophy of mathematics, beyond narrowly foundational and metaphysical issues, and towards the inclusion of more general questions concerning “mathematical methodology” and “mathematical practice” (a development parallel to an earlier broadening of the philosophy of science). There is now widespread, and growing, interest in topics such as: concept formation and conceptual change in mathematics, the role of ambiguity and inconsistency in mathematical research, the applicability of mathematics, and even sociological or anthropological questions concerning the mathematical community. Part of this broadening, although a part that remains relatively close to foundational and metaphysical issues, is the turn towards a “new epistemology” for mathematics. The latter includes the study of topics such as: the role of visualization in mathematics, the use of computers in proving mathematical theorems, and the notion of explanation as applied to mathematics.

The present paper is a contribution to this new epistemology. More particularly, it is an attempt to bring into sharper focus, and to argue for the relevance of, two related themes: “structural reasoning” and “mathematical understanding”. As the notion of understanding is vague and slippery in general, as well as very loaded in philosophical discussions of the sciences, the latter label has to be handled with care, though. It will have to be clar-

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1 Compare, e.g., Mancosu, Jørgensen and Pedersen, 1 Ferreirós and Gray, 2 Van Kerkhove and Van Bendegem, 3 and Mancosu. 4
ified what, if anything (or anything reasonably precise), is to be meant by “understanding” in connection with mathematics. Similarly, while talking about “structural” reasoning in mathematics may be suggestive, that term too requires further elaboration. My clarifications and elaborations will be tied to a specific historical figure and period: Richard Dedekind and his contributions to algebraic number theory in the nineteenth century. This is not an incidental choice; Dedekind’s case is particularly pertinent in this context, as I also hope to establish.

I will proceed as follows: In Section 2, I will provide a brief summary of Dedekind’s work on the foundations of mathematics, as well as of its usual perception in the philosophy of mathematics. In Section 3, I will turn to his more mainstream mathematical work, especially in algebraic number theory, including its usual perception by historians of mathematics. In the next few sections, the epistemological significance of this mathematical work will be explored further. In Section 4, I will review corresponding analyses in three pieces of secondary literature: Stein,5 Ferreirós,6 and Avigad.7 In Section 5, I will introduce the notions of style of reasoning and explanation to deepen their analyses. In Section 6, my views on mathematical explanation and, correspondingly, on mathematical understanding will be clarified further. Finally, in Section 7, I will indicate how the epistemological issues at the core of this paper can be seen as being of a piece with foundational and metaphysical issues.

2. Perceptions of Dedekind by philosophers of mathematics

While Dedekind did not publish any primarily philosophical writings, his foundational work is familiar to most contemporary philosophers of mathematics. His contributions in three areas, in particular, are well known: the foundations of analysis, the foundations of arithmetic, and the rise of modern set theory. Let me remind the reader briefly of those contributions, as well as of their typical characterizations by philosophers.

Dedekind is probably best known for his introduction and treatment of the real numbers in terms of “Dedekind cuts” (first presented in Dedekind8). This treatment is usually seen as a contribution to the “arithmetization of analysis” in the nineteenth century. In the twentieth century, it became part of the standard account of the real numbers within axiomatic set theory. The treatment is closely related to, indeed was based on, Dedekind’s anal-

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5 For further details concerning these publications, see the bibliography. For references to other relevant literature, compare the following footnotes.
ysis of the notion of continuity (in the sense of line-completeness). That analysis was later codified as one of the axioms for a complete ordered field — one of the “Dedekind-Hilbert Axioms”, as they should perhaps be called — and is, as such, definitive for the classical conception of the real numbers.

Dedekind’s investigations into the foundations of arithmetic, in Dedekind, are known almost as well. In that case he was, in effect, led to the “Peano Axioms” — or the “Dedekind-Peano Axioms” — for the natural numbers. He also proved what we now call the categoricity of this system of axioms; he constructed a standard model for it, in the form of a “simply infinite system”; and the whole account was grounded in an analysis of the methods of proof by mathematical induction and definition by recursion. Dedekind’s account of the natural numbers became, again, a standard part of set theory in the twentieth century, especially after it was made clear, by Zermelo, that it could be extended to ordinal numbers, induction, and recursion in the transfinite case.

As mentioning the notion of a simple infinity already flags, the approach taken in Dedekind includes a systematic reflection on the notion of infinity, as well as on those of set and function. Especially important in this connection are: Dedekind’s explicit adoption of a general, extensional notion of set; his parallel adoption of a general, extensional notion of function (without reducing functions to sets); and his definition of infinity in terms of what is now called being Dedekind-infinite. Dedekind made other contributions to the early development of set theory as well, partly in correspondence with Cantor, such as his proof of the Cantor-Bernstein theorem.

Standard accounts of Dedekind’s foundational work, such as the one just given, lead naturally to three views about him: a) that he was a strong proponent, indeed one of the founding fathers, of “classical mathematics” (with his acceptance of the actual infinite, his adoption of generalized notions of set and function, his rejection of constructivist restrictions, etc.); b) that he was a main contributor to set theory, indeed to set theory con-

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*a* For Dedekind’s role in the arithmetization of analysis, see Boyer and Merzbach (ch. 25, pp. 563–66), and Cooke. *b* For more on the “Dedekind-Hilbert Axioms”, see Awodey and Reck. *c* For a historically and philosophically rich discussion of Dedekind’s role in the development of set theory, see Ferreirós, especially chs. 3, 4, and 7.
ceived of as a foundation for all, or at least large parts, of mathematics (with his set-theoretic treatments of the natural and real numbers, his analyses of continuity, induction, etc.); sometimes also, c) that he was as a direct precursor of, and a strong influence on, the “formal axiomatic” approach championed by Hilbert and Bernays later (with his implicit formulation of the axiom systems for the natural and real numbers, his attention to questions about categoricity, consistency, etc.).

There is a lot of truth in these standard views about Dedekind, and they do bring out important aspects of his work. In what follows I will attempt to show, however, that in some ways they do not go far enough — they neglect or underemphasize a philosophically significant dimension of Dedekind’s work. To prepare the corresponding arguments, it helps to turn to his less foundational and more straightforwardly mathematical works, starting with their standard perceptions by historians of mathematics.

3. Perceptions of Dedekind in the history of mathematics

In addition to his foundational work, Dedekind made several well-known contributions to other parts of mathematics, especially to algebra and related fields. For instance, his work contains one of the first, probably even the first, modern presentation of Galois theory. He also published important papers on what were later called “lattices”; in fact, he characterized lattices for the first time in an explicit, general, and conceptually clear manner. And in collaboration with Heinrich Weber, he introduced a novel approach to the study of algebraic functions, connected with a new treatment of Riemann surfaces, and leading to a purely algebraic proof of the celebrated Riemann-Roch theorem.

Dedekind’s most important and most influential mathematical work, however, was in algebraic number theory. In historical accounts of nineteenth-century mathematics he is, thus, routinely mentioned for two things: his invention of the theory of ideals, seen as a crucial new tool in the study of algebraic integers and algebraic number fields; and his introduction, in that context, of the abstract mathematical notion of a field, applied by him to all subfields of the complex numbers and including adopting the word “field” for this purpose (or rather the corresponding German word,

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\[^{4}\text{Seeing Dedekind as a proponent of classical mathematics is standard wisdom, I believe. For Dedekind’s role in the development of modern set theory, see again Ferreirós.}^{6} \text{For a sophisticated interpretation of Dedekind as a precursor of Hilbert and Bernays, see Sieg and Schlimm.}^{15}\]
“Körper”). Both contributions occur in supplements to lectures notes on number theory, based on lectures by his teacher Dirichlet but edited by Dedekind, and most maturely, in the fourth edition of that work (Dirichlet and Dedekind; compare also Dedekind). Two general aspects of this mathematical work and of its impact are typically emphasized in historical accounts. First, Dedekind’s novel approach to algebraic number theory, as embodied in his ideal theory, did not go unchallenged and unopposed. Kronecker’s parallel work in this area, culminating in his divisor theory, was seen as a significantly different alternative to Dedekind’s from early on. Kronecker himself kept emphasizing the more concrete, finitary, and constructive aspects of his theory, while being critical of the abstract, infinitary, and non-constructive aspects of Dedekind’s. Second and in spite of such criticisms, Dedekind’s approach had a strong influence on twentieth-century mathematics, through the works of Hilbert, Noether, van der Waerden, Bourbaki, and others. This influence was often acknowledged explicitly, e.g. by Emmy Noether. Reflecting on the basic methodological orientation of her own, itself very influential, work in algebra and topology, she stated: “It’s all already in Dedekind!”

As Dedekind’s mathematical works tend to be much less well known to philosophers than his foundational contributions, let me add a bit to this brief summary (before proceeding to a deeper analysis in later sections). In particular, what are the main goals and challenges in Dedekind’s and Kronecker’s theories in algebraic number theory?

Both theories were heavily indebted to earlier works by Gauss, Dirichlet, and Kummer. For all of these mathematicians the basic goal was the solution of various algebraic equations. A famous example is provided by Fermat’s Last Theorem, which concerns the existence, or lack of existence, of integer solutions to the equation \( x^n + y^n = z^n \), for various exponents \( n \). Gauss and Kummer approached this (very difficult) issue by studying certain extensions of the ordinary integers, as well as of the fields of numbers that contain them. Gauss considered what happens when you add the “Gaussian integers” \( a + bi \), with \( a \) and \( b \) regular integers and \( i = \sqrt{-1} \); Kummer investigated more complex “cyclotomic integers”. Along the way, it became clear that a crucial issue, and a major stumbling block, was the following: In some such extensions of the ordinary integers — in some “in-

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\(^a\) See again Boyer and Merzbach (now ch. 26, pp. 594–6), as well as Stillwell (ch. 21).  
\(^b\) See McLarty for the source of the quotation (p. 188), and more generally, for Dedekind’s influence on Noether. Edwards provides a comparative discussion of Dedekind and Kronecker.
tegral domains”, as Dedekind called them— the familiar theorem about unique factorization into powers of primes fails. A crucial question became, then, whether a suitable alternative for such factorization could be found.

Kummer attempted to recover unique factorization by introducing “ideal numbers”. While this led to striking progress, some basic questions remained. In particular, how exactly was one to think about the nature of these new “numbers”; and what was the best way to generalize Kummer’s approach, if this was possible at all? As a consequence, the range of applicability of his ideas remained unclear, to some degree even the validity of his results. Both Kronecker and Dedekind tried to justify and extend Kummer’s work. Kronecker did so by considering in depth — and as part of an essentially computational task (starting from a finitary basis and preserving decidability) — a range of constructible domain extensions. Dedekind investigated — in a more general, abstract, and non-constructive way — arbitrary algebraic number fields and the integral domains they contain. He also replaced Kummer’s “ideal numbers” by his “ideals”, defined in an explicitly set-theoretic way (as certain infinite sub-sets of the complex numbers), and he recovered unique factorization that way. Both Kronecker’s and Dedekind’s approaches led to further results right away, as well as to important developments later on.

Kronecker’s and Dedekind’s works are similar insofar as both constitute “arithmetic” approaches to algebra. (They are two instances of the “arithmetization of algebra” in the nineteenth century, parallel to the more familiar “arithmetization of analysis”.) Apart from that, they differ markedly. Comparing the two mathematicians and their lasting impacts, the historian of mathematics Harold G. Edwards comments:

Kronecker’s brilliance cannot be doubted. Had he had a tenth of Dedekind’s ability to formulate and express his ideas clearly, his contributions to mathematics might have been even greater than Dedekind’s. As it is, however, his brilliance, for the most part, died with him. Dedekind’s legacy, on the other hand, consisted not only of important theorems, examples, and concepts, but of a whole

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In current terminology, an integral domain is a ring that is commutative under multiplication, has a unit element, and has no divisors of zero.

An ideal $I$ in an integral domain (or, more generally, a ring) $R$ is a subset that forms an additive group such that, for all $x \in I$ and $y \in R$, $xy \in I$. The crucial theorem is: In a domain $R$ of algebraic integers, any ideal $I$ of $R$ can be represented uniquely (except for the order of the factors) as a product of prime ideals.

Besides Edwards and McLarty, compare Reed (ch. 4) and Corfield (ch. 8).
style of mathematics that has been an inspiration to each successive generation. (Edwards,\textsuperscript{20} p. 20)

On the surface, this passage is complimentary of Dedekind’s work, highlighting his “great contributions” and his corresponding “legacy”. However, the way in which Kronecker’s “brilliance” is juxtaposed to Dedekind’s “ability to formulate ideas clearly” may give one pause. Kronecker certainly was a brilliant mathematician; and Dedekind had that ability. But is the latter all that is noteworthy about Dedekind in this connection; doesn’t his work exemplify other, equally or more significant, virtues as well?\footnote{In the background of such remarks are Edwards’ strong and well-known sympathies for a Kroneckerian approach to mathematics; compare Edwards.\textsuperscript{23,24} To be fair, he does have more to say about Dedekind, also in Edwards;\textsuperscript{25} but the general perspective assumed is always Kronecker’s.}

Edwards also attributes “important theorems, examples, and concepts” to Dedekind. More intriguingly, he mentions a Dedekindian “style of mathematics” that inspired later generations of mathematicians. The latter raises another question, however: How is the word “style” to be understood here; in particular, is it used in a merely psychological or sociological sense, or is more at issue?\footnote{Other historians of mathematics, such as Ivor Grattan-Guinness, have written about the “fashion” Dedekind’s work inspired, as well as the “popularity” set theory gained later on (Grattan-Guinness,\textsuperscript{26} p. 535). Elsewhere Edwards writes about a “new orthodoxy” in this connection, one that was “consolidated by Hilbert” and that “has reigned ever since” (Edwards,\textsuperscript{24} p. ix).} Raising this question is also meant to lead us beyond Edwards’ remark. The further, more important issue, for present purposes, is whether “style” could be used in a philosophically more substantive sense in this context. Or more generally, is there anything else to be said about the epistemological significance of Dedekind’s approach, compared to Kronecker’s and in itself? These are the questions I want to turn to now. Actually, a few other philosophers of mathematics have already started to move in that direction, and I want to follow their lead.

4. Philosophical analyses of Dedekind’s mathematical work

There is a relatively small, but illuminating and suggestive, series of commentaries in the literature in which Dedekind’s approach to algebraic number theory, and with it, his methodology in general, is analyzed with an eye towards its epistemological significance. It would be worth reviewing, and then building on, all of them; but I will have to restrict myself to three
here: Stein, Stein, Ferreirós, and Avigad.

The focus in Stein’s paper is not so much on Dedekind, but on a more general change that took place in nineteenth-century mathematics, especially in its methodology. Stein associates this change with a group of mathematicians who influenced each other directly: from Gauss and Dirichlet through Riemann, Cantor, and Dedekind, on to Hilbert and Minkowski. Dirichlet is seen as the “poet’s poet” in this group. Dedekind, although not the primary focus of Stein’s study, is very important as well, especially because of his close association with Dirichlet. In a retrospective tribute to Dirichlet at the beginning of the twentieth century, Minkowski captured the transformation at issue in a nutshell; in Minkowski’s memorable phrase, it consisted of adopting Dirichlet’s guiding principle “to conquer the problems with a minimum amount of blind calculation, a maximum of clear-seeing thought” (quoted in Stein, p. 241).

According to Stein, this principle shapes Dedekind’s approach quite generally. It guides his reconceptualization of the foundations of arithmetic and analysis in terms of abstract sets and functions, instead of concrete numerals, formulas, and their intuitive applications, as was usual before; it underlies his innovative presentation of Galois theory in terms of field extensions and their automorphisms, rather than substitutions in formulas and functions; and it is perhaps most evident in his algebraic number theory. Concerning the latter, the contrast between “clear-seeing thought” and “blind calculation” amounts to this: While Kronecker, like Kummer and Gauss before him, works with a restricted number of constructible cases, trying to extract computational information, Dedekind considers an enlarged class of algebraic structures, here number fields, always searching for general, not necessarily decidable, and characteristic concepts.

Quoting related remarks by Cantor, Stein characterizes the shift towards the more “conceptual” mathematics to be found in Dirichlet, Dedekind, Riemann, and others also as a “freeing” of mathematics. Previous mathematics tended to be confined to the physically applicable and intuitive, especially the geometric, on the one hand, and to the calculable and constructive, on the other. It is not that such a narrower conception — with its emphasis on “the applied force of the formula”, to quote Minkowski again — can’t lead to new results. Indeed, Kronecker’s brilliant extension of Kummer’s work...
shows that it can. But the reorientation advocated by the champions of “mathematical freedom” can, and did, lead to an important broadening of mathematics, including the formation of various new and fruitful concepts. To use one of Stein’s own happy phrases, it leads, indeed it gives pride of place, to the “free exploration of conceptual possibilities”.

As Stein notes, Dedekind’s allegiance to such a more freely exploratory mathematics went hand in hand with, indeed involved centrally, the use of set-theoretic techniques and proofs, instead of the earlier reliance on intuitive constructions and calculations. Stein elaborates on that aspect to some degree. In Ferreirós’ book does again not focus on Dedekind alone. It discusses his employment of set-theoretic techniques in the context of a more general account of the rise of modern set theory, from the nineteenth into the twentieth century. According to Ferreirós’ account, Dedekind is one of the central figures in the early history of set theory. This is so, among others, because he explicitly adopted two ideas: to treat sets as mathematical objects in themselves; and to allow for the use of infinite sets, indeed to use them as a central tool for concept formation in mathematics.

Besides shedding new light on Dedekind’s role in the rise of modern set theory, Ferreirós’s discussion of him also confirms, and extends, some of Stein’s insights into the significance of his work in algebraic number theory, and of his mathematical methodology more generally. As analyzed by Ferreirós, this methodology involves: the consideration of whole classes of systems, e.g., of the class of arbitrary sub-fields of the complex numbers; their abstract treatment in terms of general laws, such as the laws that characterize number fields, integral domains, or simple infinities; and more specifically, the definition of operations on mathematical systems in terms of their behavior as sets, thus independently of particular formalisms and calculations based on them.

The last point — the preference for general, abstract, and representation-invariant specification of mathematical operations and objects — can be exemplified well by a particular aspect of Dedekind’s theory of ideals. Dedekind labored for quite a while — through various supplements to Dirichlet’s “Lectures on Number Theory”, published over three decades — to find a good, perhaps even the best, way to define an ex-

\[\text{In Tait, the emphasis on “free mathematics” is discussed further, in connection with Cantor and Dedekind; compare also Reck.}\]

\[\text{The search for “characteristic concepts”, by Riemann, Dedekind, and Frege, is discussed more in Tappenden, and the relevant parts of Mancosu.}\]
tended notion of “integer”, applicable to number fields in general; similarly for “ideal divisor” and, especially, for “prime divisor” (as needed to ensure unique factorization). Now, Kronecker worked on solving a parallel problem. But characteristically, Kronecker’s solution was not meant to apply as generally; it did not employ set-theoretic techniques, especially not the use of infinite sets; and it was tied to specific formalisms and representations (as needed by Kronecker to ensure computability). In the end, their respective solutions had different advantages and disadvantages.

In the present paper, I am mainly concerned with the philosophically significant advantages of Dedekind’s approach, in this case and more generally. It is standard to assume that Dedekind constructed his ideals and related mathematical objects in an explicitly set-theoretic way because the “ideal numbers” appealed to by Kummer had provoked mistrust or doubt, since they lacked an explicit, secure foundation. But a standard rejoinder, especially by constructivists, is this: As the “foundational crisis” in the early twentieth century has shown, the use of sets, especially infinite sets, is not necessarily more secure; but then, their use should be seen with mistrust too, shouldn’t it? Whether or not one agrees with this rejoinder, I do not think — and this should have become apparent by now — that providing a secure foundation for certain parts of mathematics was the only objective for Dedekind. Arguably it was not even his main objective, particularly in algebraic number theory; nor was it his philosophically most significant achievement, at least from a methodological point of view.

An additional, more recent attempt to get at Dedekind’s main methodological achievements — focused squarely on his approach to algebraic number theory — is Avigad. In this paper, several of Stein’s and Ferreirós’ observations are confirmed yet again, while many number-theoretic details are added and the analysis of their significance is deepened. Like Stein and Ferreirós, Avigad mentions the use of set-theoretic techniques by Dedekind, including his acceptance of the actual infinite. Once more, he emphasizes the contrast between Dedekind’s abstract, conceptual, or structural approach, on the one hand, and Kronecker’s focus on algorithmic tractability and decidability, on the other. And once again, he puts his finger on Dedekind’s aim to find general, mathematically fruitful concepts or characteristics.

A related aspect, mentioned already in connection with Ferreirós, is discussed in considerable detail by Avigad as well: the fact that, according to Dedekind, mathematical objects and operations should be defined in a representation-invariant way. Avigad also sheds further light on two related ways, touched on by Stein, in which Dedekind characterizes his own pro-
procedure: as an attempt to distinguish “internal” from “external” properties of mathematical entities; and as an attempt to keep mathematical theories as “pure” as possible, i.e., to avoid the admixture of “foreign” elements. Examples are: Dedekind’s resistance to taking particular formalisms, typically tied to individual cases, to be more important than the underlying mathematical structure; and his rejection of appeals to geometric ideas in his treatment of the foundations of arithmetic and analysis.

At several points, Avigad summarizes what is crucial about Dedekind’s approach, and what distinguishes it most significantly from Kronecker’s, in terms of the adoption of certain methodological values and principles. Central among them are: the preference for conceptual reasoning, as opposed to algorithmic calculation; the preference for abstraction, generalization, and the uniform treatment of as large a number of cases as possible, as opposed to gaining computational information by focusing on a restricted number of constructible instances; and the preference for identifying characteristic internal properties, thus for a certain kind of simplicity and purity, in the sense just indicated. It should be emphasized that these values are in addition to, and that they complement, Dedekind’s use of set-theoretic and infinitary tools and techniques. Perhaps they are even what motivates and what ultimately justifies his adoption of them?

I hope it is plausible by now that Dedekind’s work does not just stand out because of his exceptional expository ability. Nor is it entirely satisfactory, in the end, to characterize the difference between his approach and Kronecker’s simply by pointing to Dedekind’s adoption of set-theoretic and infinitary techniques, in contrast to Kronecker’s constructivist and finitist procedures, although that is again part of the story. Various additional, perhaps more basic, and arguably more important features have come to the fore, in Stein’s, Ferreirós’, and Avigad’s studies. Now, can these additional features be analyzed in any further, deeper way? More specifically again, can the sense in which they are significant epistemologically be brought out more sharply, after having identified them? I will attempt to do so in the next two sections.

To prevent a possible misunderstanding, I do not mean to discount this aspect completely. Being able to present ideas clearly is crucial, e.g., in a pedagogical context; and some of Dedekind’s most important ideas arose in just such a context; see Dedekind, p. 1. But there is quite a bit more to be said about Dedekind’s approach, especially from an epistemological point of view.
5. Styles of reasoning and mathematical explanation

At this point, I want to come back to Edwards’ remark about a Dedekindian “style of mathematics”. One question I raised earlier was whether “style” is used here in a merely psychological or sociological sense, or whether more is at issue. More importantly, could “style” be employed in a philosophically more substantive sense in this connection, whether or not Edwards does? Let us consider the latter issue in some detail now.

In contrast to using “style” in a psychological, sociological, or anthropological sense (including the idea of “national style” in mathematics), also in contrast to using it in a personal, aesthetic, or art-historical sense (the “style” of a writer or painter), there is the way in which the notion has been employed, and codified, by Ian Hacking. What Hacking talks about specifically, in the context of the history and philosophy of science, are “styles of reasoning”. While he is reluctant to define what a style in his sense is, at least in any reductive or formulaic way, he provides various examples, including: the postulational style that characterized the mathematical sciences in Ancient Greece; the experimental style that arose in early modern science; and the statistical and probabilistic style that, in the nineteenth century, began to shape the social sciences. And he elaborates on what is significant about such styles, namely:

Every style of reasoning introduces a great many novelties, including new types of: objects; evidence; sentences, new ways of being a candidate for truth and falsehood; laws, or at any rate modalities; possibilities. One should also notice, on occasion, new types of classification and new types of explanation (Hacking, p. 189).

As one may also put it, Hacking is talking about different kinds of “cognitive style”.

An aspect that makes the notion of cognitive style, in Hacking’s sense, useful is that it foregrounds philosophical issues, including epistemological issues (also related metaphysical ones). Thus, what matters are “ways of being a candidate for truth and falsity”; equally crucial are new types of “evidence”, “laws”, “classification”, and “explanation”. Along such lines, the focus is on general, and often novel, ways in which scientists conceptual-
ize issues, formulate problems, evaluate solutions, systematize results, etc. Basically, a style of reasoning is a distinctive, integrated manner of doing those kinds of things. Understood as such, a reasoning style can crystallize in the work of one or more thinkers, so as then to shape the direction of a discipline for a while.

Most of the examples of reasoning styles considered by Hacking come from the natural and social sciences. They are also very broad and general. He mentions only a few mathematical examples, such as the postulational style of the mathematical sciences in Ancient Greece. But nothing rules out the application of this notion to other cases in mathematics, as one may argue, including more recent ones. In fact, what we considered above — the distinctively “conceptual” or “structural” approach to mathematics championed by Dedekind — appears to be a very good example. It involves all the features highlighted by Hacking: new types of evidence (definitions and constructions involving set-theoretic techniques, including uses of the actual infinite, non-constructive and non-computational proofs, etc.); new laws (laws for generalized classes of structures, appeals to the general notions of set, function, etc.); new types of classification (simple infinities, number fields, groups, lattices, etc.); and new types of explanation (based on characteristic concepts, on novel ways of relating phenomena, etc.). To have a slogan, we may talk about a “structural style of reasoning” as exemplified by Dedekind’s work.

A general way in which talking about a style of reasoning in this connection is helpful is by drawing attention to the epistemological dimension of Dedekind’s works. In addition, the specifics of Hacking’s proposal — concerning the introduction of novel kinds of evidence, law, classification, explanation, etc. — provide us with conceptual tools for deepening the analysis. It would be interesting to consider, in detail, each of these tools

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\[a\] For a related account of what is important epistemologically in this context, as well as a comparison to how the notion of “style” is used in art history, see Davidson. As Davidson emphasizes, “styles of reasoning give systematic structure and identity to our thought” (p. 141).

\[b\] Alternatively, one could try to analyze Dedekind’s approach as an example of a Kuhnian “paradigm” (both in the sense of “exemplar” and “disciplinary matrix”) or as an example of a Lakatosian “research programme”. Ferreirós contains remarks about Dedekind along Kuhnian lines, while Corfield pursues a Lakatosian direction. In what follows, I will indicate what is particularly helpful about the notion of style or reasoning in our context. (More on “research programmes” in footnote d, Section 6.)

\[c\] Dedekind’s approach and position have also been called “logicist”, e.g. by his contemporaries C. S. Peirce and Ernst Schröder; compare the corresponding discussion in Ferreirós.
and what can be done with it. Let me single out the last one here: the notion of explanation. I think it is correct, and to the point, to see Dedekind’s structural style of reasoning as involving a new type of mathematical explanation. However, it then needs to be clarified what that implies. Also, how does talking about explanation in the context of mathematics relate to discussions of that notion in the philosophy of science, if at all?

In general philosophy of science, the notion of explanation is often discussed in connection with the notion of causation. Along such lines, what philosophers of science — philosophers of physics, biology, sociology, economics, etc. — are interested in is to get at the sense, and the precise forms, in which appealing to the cause of an event or phenomenon is, or can be, explanatory. Now, it may seem that causation has no role to play in mathematics, which may also make talk about mathematical explanation seem dubious. In one sense this is surely correct, namely if “cause” is used in the narrow sense of “efficient cause” (analyzed in terms of natural laws, capacities, counterfactual dependence, etc.). Then again, one can use “cause” in a more general sense as well. In that sense, anything that is given as the answer to a why-question counts, especially if the answer takes the form of “because…” In mathematics we can, and sometimes do, ask why-questions. As an example, consider: “Why are certain kinds of algebraic equations solvable by integers while others aren’t?” Answers to such questions may then be taken to provide “explanations”, perhaps even “causal explanations”.

One might also want to compare the corresponding explanatory power of mathematical theories.

This last remark calls for further clarification, or for a distinction that will be helpful. The distinction is between a “local” and a “global” sense of “explanatoriness”, both in the sciences and in mathematics. In the case of mathematics, the local sense concerns the manner in which a particular proof of a theorem can be seen as explanatory, or as more or less explanatory than other proofs of the same result. (The literature in philosophy of mathematics contains some proposals for how to think about being explanatory in this local sense, e.g., in Steiner; more on this in the next section.) The global sense of explanatoriness, in contrast, concerns the

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\(^e\) Not always; Hempel’s and Kitcher’s discussions of explanation are exceptions. But compare Salmon\(^f\) and subsequent works by Cartwright, Humphries, Salmon, and Woodward, among others.

\(^f\) To be more careful, one should distinguish between explanatory and other why-questions in this context. In making answers to why-questions central to the issue of explanation, I follow van Fraassen\(^g\) and the erotetic literature on which he relies. In addition, I am heavily indebted to Wright.\(^h\)
way in which a whole theory or a general approach to a subject matter is explanatory, or can be evaluated as more or less explanatory than other theories or approaches. (Here too the literature contains proposals, e.g., in Kitcher; again, more on this below.)

With these clarifications and distinctions in place, I can now state a general claim. The claim is that an important aspect, perhaps even the main aspect, of what makes Dedekind’s structural style of reasoning significant epistemologically is its characteristic explanatory power in the global sense. A lot more would have to be said to substantiate and defend this claim fully. I will only have space for a few additional remarks in the next section, before wrapping things up more generally.

6. Explanations, background assumptions, and understanding

While it will be crucial for us to get clear about the explanatory power of Dedekind’s approach in the global sense, as just suggested, it helps to start with the local sense, especially since the two senses are not unrelated. In particular, it helps to examine a specific proposal for how to think about explanatoriness at the local level.

The proposal I have in mind is due to Mark Steiner; in his own words:

\[ \text{[A]} \text{n explanatory proof makes reference to a characteristic property of an entity or structure mentioned in the theorem. It must be evident, that is, that if we substitute in the proof a different object of the same domain, the theorem collapses; more, we should be able to see as we vary the object how the theorem changes in response.} \]

(Steiner, 40 p. 143)

It is not obvious whether this criterion for being an explanatory proof applies generally, nor whether it characterizes being explanatory fully. It has been criticized seriously in connection with other examples. Nevertheless, Steiner’s criterion looks promising in connection with Dedekind’s work, especially in algebraic number theory. As we saw, it was a main goal for Dedekind to come up with “the right concepts”, and thus to identify “characteristic properties” of various entities and operations. Moreover, the right concepts for him were exactly those that apply to a wide range of cases, or even, allow us to distinguish those cases for which a certain proof worked from others.

\(^a\) For references and further discussion, see Hafner and Mancosu and Mancosu.
Let me connect this point with an insight gained by looking at explanations as answers to why-questions. A main advantage of approaching the notion of explanation that way — besides the fact that it makes its applicability in mathematics plausible — is that we are directly led to the crucial role of background assumptions. Consider asking a why-question, in the form “why p?”, and answering it, with “because q”. Evidently, the whole exchange can only be successful if a number of presuppositions are in place. Two of these presuppositions are especially noteworthy. The first is the availability and determinate nature of what is often called the “contrast class” for p.\(^b\) It has to be clear, that is, what the alternatives to p are in the context at hand: p as opposed to p’, p”, etc.; otherwise it is not even clear what question is being asked by using the phrase “why p?” Second, it has to be clear, again in context, what kinds of explanatory factors or “causes” are relevant.\(^c\) These two presuppositions are closely related. Distinguishing p from p’, p”, etc. will have to be in terms of specific features; and those features will be identical with, or intimately related to, the explanatory factors relevant in the context at hand.

For illustration, consider again our mathematical example from above: “Why are certain kinds of algebraic equations solvable by integers while others aren’t?” In formulating the question in this way, the appeal to a contrast class is readily apparent, at least in a general way. The fact that certain explanatory factors are presupposed is more hidden. Now, compare Dedekind’s approach again with Kronecker’s. For Kronecker, the contrast class consists of a tightly circumscribed range of equations, corresponding to number fields constructed finitistically; and the presupposed explanatory factors are computational ones. For Dedekind, the contrast class is determined by an enlarged class of number fields, thus consisting of a larger number of equations; and the relevant explanatory factors involve entities defined set-theoretically and considered structurally. Altogether, the most radical differences between Dedekind’s and Kronecker’s approaches can be located at this level, I would suggest. They consist of differences in the general background assumptions for their respective explanatory enterprises.

We are now also in a position to relate the global and local senses of explanatoriness to each other, at least in our case. Consider again Steiner’s

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\(^b\) Van Fraassen\(^38\) contains an illuminating discussion of the notion of contrast class.

\(^c\) In Wright,\(^39\) the author talks about a presupposed “causal matrix” in this connection. I should add that, with respect to this second aspect, Wright’s and my approach differs significantly from van Fraassen’s (which appeals to a somewhat mysterious, and often criticized, “relevance relation” at this point).
criterion: that at the local level, i.e., the level of particular theorems and proofs, what is crucial is to identify the “characteristic property of an entity or structure”; also, to show that “if we substitute in the proof a different object of the same domain, the theorem collapses”; or even, to establish how “as we vary the object the theorem changes in response”. Notice that such a criterion only has a chance of applying if it is clear, first, what the relevant “objects of the domain” are and, second, which kinds of “characteristic properties” count. And again, those are exactly the two ways in which Kronecker’s and Dedekind’s approaches differ radically. We get the following consequence: Considered just locally, the two approaches are very hard to compare, if not incommensurable, since they differ so much in their respective background assumptions (thus the continuing disagreement between Dedekindians and Kroneckerians). At the global level, there may be more room for comparative evaluation. In particular, we can ask in which way, and to what degree, various background assumptions have proved fruitful mathematically.

A comparative assessment at the global level will still not be straightforward, partly because fruitfulness is hard to quantify, partly because it is relative to the goals one is pursuing (e.g., computational versus structural goals), and partly because it becomes manifest only over time. It may also go through varying phases; e.g., while an approach may not be fruitful for a while, it may pick up again later. And in any case, assessing degrees of fruitfulness is not the same as providing criteria for explanatoriness, neither at the local level (where something like “getting at the core of things” seems to play a role, together with values such as simplicity and purity, in ways that are hard to capture) nor at the global level (where unification, systematicity, etc. seem to play some role, although it is not clear

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d Here Lakatos’ suggestions for how to evaluate “research programmes” may be of help. Compare again Corfield22 (and footnote c, Section 5), earlier also Hallett,43 among others. As to incommensurability, I do not mean to push this issue too far. It may be possible to find background assumptions and a framework within which comparisons can be made, although the difficulty will be finding ones acceptable to both sides.

e Kronecker’s approach was quite fruitful initially; it was then overshadowed, for some time, by work along Dedekindian lines; but it became fruitful again in the middle of the twentieth-century, in research by Grothendieck and others. Compare again Reed21 (ch. 4) and Corfield22 (ch. 8).

f Explanatory power at the local level, like mathematical understanding in general (see below), may be too vague or multi-faceted a notion to be captured in any simple formula. However, there is some recent research in automated theorem proving, as discussed in Avigad44 and Vervloesem,45 which contains potentially fruitful, and very application-oriented, reflections on related issues.
which one exactly\(^6\)). Still, I hope that some new light has been shed on how to analyze the differences between Dedekind’s and Kronecker’s methodologies more deeply, and especially, on what makes Dedekind’s distinctive and noteworthy.

Let me add two further clarifications, now of the sense in which this all concerns epistemology. Both clarifications will involve the notion of understanding, which first needs to be clarified itself. For present purposes, I want to use “understanding” not in contrast to “explanation” (as was common in nineteenth- and early-twentieth-century debates about the unity of science, i.e., about whether the methodologies of the human and the natural sciences are fundamentally different or not). Rather, “understanding” and “explanation” are taken to be correlative terms, along the following lines: What a successful explanation does is to improve our understanding of things; and an explanation is better the more it does so.\(^h\) The main claim I argued for above can then be put thus: The most characteristic, and perhaps the most valuable, aspect of Dedekind’s approach is the specific way in which it allows us to understand mathematical phenomena.\(^i\)

Second, epistemology is often understood to be the philosophical study of human knowledge, and specifically, of its forms of justification and its connections to truth, also its means, conditions, limits, etc. However, for our purposes this is too narrow — it tends to exclude the topic of understanding from epistemology. Insofar as that is the case, “understanding” stands opposed to “knowledge”. In a recent paper, Howard Stein makes a related point, by distinguishing between the “enterprise of understanding” and the “enterprise of knowledge” (Stein, \(^{47}\), p. 135). Both enterprises are important, as he emphasizes; they are also often intricately intertwined. Nevertheless, one can distinguish them conceptually. The enterprise of knowledge concerns what epistemologists typically focus on: justification, truth, etc. The enterprise of understanding, in contrast, has to do with our “grasp of ideas or concepts” and their “clarification” (ibid.). I would add that much of what we discussed above — distinguishing different styles of reasoning (based on different sets of concepts), considering their explanatory power (partly in

\(^6\) Compare Kitcher\(^{44}\) and the ensuing debate. Very briefly, I doubt that either unification or systematicity, in themselves, account for explanatoriness, especially at the local level; but they seem to play some role, perhaps of a supplementary kind, in connection with global explanatory power.

\(^h\) Here I again follow Wright, \(^{39}\) who in turn builds on Scriven, Austin, and Wittgenstein.

\(^i\) Related discussions of mathematical understanding, including its connection to proof, can be found in Tappenden, \(^{46}\) Avigad, \(^{44}\) Vervloesem, \(^{45}\) and the corresponding parts of Mancosu.\(^4\)
terms of “finding the right concepts”), etc. — falls within the latter as well.

Put in these terms, another main goal in this paper has been to make evident that Dedekind’s approach to mathematics is worth studying as part of the “enterprise of understanding”, and thus as part of epistemology understood in a broad sense.

7. Connections to foundational and metaphysical issues

My focus in this paper has been on epistemologically significant aspects of Dedekind’s work, including clarifying the sense of epistemology involved. A striking feature of his approach is, however, that it forms a tightly integrated whole. More specifically, epistemological aspects are tied closely to foundational and metaphysical aspects.

What I have in mind here is the following: As argued above, Dedekind’s characteristic way of understanding mathematical phenomena — in terms of his abstract, conceptual, or structural style of reasoning and explanation — involves corresponding background assumptions. Among them are assumptions about the factors one can appeal to in definitions, constructions, and proofs. For Dedekind, unlike for Kronecker, infinite sets, a generalized notion of function, etc., are available; thus, he gives definitions of various operations in terms of their set- and function-theoretic behaviors, independently of particular forms of representation and methods of calculation. With respect to the contrast classes assumed, we noted his use of enlarged classes of objects and structures, the preference for finding uniform treatments for them, etc. Overall, mathematical phenomena are treated in abstract relational and functional, thus structural, terms.

These aspects are crucial for Dedekind’s work in algebraic number theory, as I argued above. But not only that; the same aspects are also characteristic for his other mathematical works: his contributions to Galois theory, to the theory of algebraic functions, to lattice theory, etc. In fact, they even shape Dedekind’s foundational works, including his treatments of the natural and real numbers. Here too, we find the use of set-theoretic constructions, the acceptance of the actual infinite, the employment of a general notion of function, the consideration of generalized classes of cases, their treatment in abstract relational and functional terms, the search for internal, characteristic properties, etc. In other words, the same conceptual tools are employed throughout.

It is tempting to think, again, that it is Dedekind’s recognition of their fruitfulness in his mathematical work — his realization of how instrumental they are in increasing our understanding of, say, the solubility of algebraic
equations — that underwrites the use of these tools also in his foundational work. More likely, perhaps, is that he realized their explanatory power in both cases together, so that there was mutual reinforcement.\textsuperscript{a} In either case, we can see that mathematical and foundational concerns need not be as separate as is often assumed. If I am right, they are of a piece in Dedekind’s work.\textsuperscript{b}

Finally, the same point can be made about the close connection between epistemological and metaphysical aspects in Dedekind’s work. A central feature of his methodology is to study mathematical objects and operations not in terms of particular formalisms or symbolic representations. Dedekind recognized that it is epistemologically fruitful — that it increases our understanding in mathematics — if we investigate them, instead, in set-theoretic, abstract relational, and generalized functional terms. But making this shift also leads away from conceiving of the nature of mathematical entities and phenomena in two traditional ways: along narrowly formalist lines, so that all we are dealing with are empty symbols, mere formulas, etc.; in broadly physicalist terms, i.e., by making empirical applications of mathematics essential, so that numbers, e.g., are conceived of in terms of concrete quantities. In other words, Dedekind’s epistemological shift calls into question formalist, physicalist, and similar metaphysical views.\textsuperscript{c}

What Dedekind’s methodology suggests, instead, is to think of mathematical objects, concepts, and functions in structuralist terms. The resulting metaphysical position — Dedekind’s “logical structuralism” — has already been analyzed in Reck,\textsuperscript{13} but without paying much attention to the epistemological side, as elaborated in the present paper. In the end, metaphysical and epistemological aspects are flip sides of the same coin, in

\textsuperscript{a} To be more definite here, the precise chronology of Dedekind’s main ideas and results would have to be established (by studying his Nachlass, correspondence, etc.). I intend to do so in future work.

\textsuperscript{b} In Tappenden,\textsuperscript{31} the same point is made about Riemann and Frege. The widespread separation of mathematical and foundational concerns, by philosophers and mathematicians, is illustrated in it as well.

\textsuperscript{c} Whether or not formalist, physicalist, and related metaphysical positions should be rejected completely, and Dedekind’s simply adopted, is another question. His “logical structuralist” position is not without its weaknesses; other alternatives have come up since Dedekind’s time; and even a position such as narrow formalism has led to important insights, as Kronecker’s case illustrates. Moreover, there may not be one metaphysical position that does justice to mathematical facts and phenomena in all their richness, especially since the practice of mathematics keeps evolving. Still, Dedekind’s approach has led to novel and deep insights as well, as seems indubitable, at least for those who do not reject its background assumptions.
Dedekind’s case and more generally. If this is correct, only a joint treatment can do full justice to either side. With respect to this conclusion too, much more would have to be said to make it fully convincing; a single, short article can only scratch the surface. Indeed, a whole book would seem to be needed to do an adequate job. The present paper is perhaps best seen as motivating a corresponding book project. I hope I will have a chance to pursue such a project further in the near future.

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on the penultimate draft. Needless to say, all the remaining mistakes and other weaknesses are my own.
A MATHEMATICIAN AND A PHILOSOPHER ON THE
SCIENCE-LIKENESS OF MATHEMATICS: KLEIN’S AND
LAKATOS’ METHODOLOGIES COMPARED

EDUARD GLAS

Department of Mathematical Analysis
Delft University of Technology
∗E-mail: e.glas@tudelft.nl
http://fa.its.tudelft.nl/people/glas.htm

1. Introduction

As in all scientific research, there are two kinds of tendencies in mathematics: on the one hand the tendency towards abstraction, which seeks to bring out the logical aspects of various subject matters and to interconnect them systematically, and on the other hand the tendency to start from a vivid mental grasp of the objects and their material (inhaltliche) relationships. With regard to geometry, the abstract tendency has led to the magnificent systematic subjects of algebraic geometry, Riemannian geometry and topology, in which the methods of conceptual analysis, symbolism and calculus have been applied abundantly. Still, intuitive comprehension occupies even today a prominent place in geometry, not only on account of its superior heuristic power (Kraft des Forschens), but also with regard to the interpretation and appraisal of the results of inquiry ...Because of the many-sidedness of geometry and its relations to the most divergent branches of mathematics, we also acquire in this manner an overview of mathematics as a whole and an impression of the plenitude of its problems and the richness of its ideas. (Hilbert and Cohn-Vossen, p. v)

Rather surprisingly, the passage just quoted, written in the early 1930s, comes from a book by David Hilbert, who is often (wrongly) regarded as an archformalist and archenemy of intuitionism. Yet he voices here almost
exactly the same opinion as his former teacher Felix Klein (1849-1925), who presented himself as ‘an intuitionist and also a logician’. Klein distinguished between logicians, formalists and intuitionists. The logicians’ main strength, he wrote,

resides in their logical and critical power, in their skill to devise strict definitions, and to derive rigid deductions from them. The great and wholesome influence exerted in Germany by Weierstrass in this direction is well known . . . The formalists excel mainly in the skilful formal treatment of a given question, in devising for it an algorithm (Cayley, Sylvester) . . . To the intuitionists belong those who lay particular stress on geometrical intuition (Anschauung), not only in pure geometry, but in all branches of mathematics. What Peirce has called ‘geometrizing a mathematical question’ seems to express the same idea. (Klein,\textsuperscript{2} p. 2)

Klein classed himself with the intuitionists and also the logicians, laying stress both on intuitive perspicuity and on mathematical rigor. Clearly, Klein’s distinction does not coincide with the more modern distinction between the foundational schools of logicism, formalism and intuitionism. Rather than foundations, it is the use of vivid representations of the abstract structures under consideration that Klein emphasises, logical (and, less prominently, formal) methods serving to derive conclusions in a perfectly rigorous fashion. As such Klein’s views seem to foreshadow the very similar views of Imre Lakatos (1922-1974).

Klein is perhaps the most outstanding example of an eminently fruitful mathematician who opposed the one-sided obsession of most mathematicians of his generation with purity and rigor, through which the discipline increasingly tended to fall apart into disparate, self-contained specialties. His perception of mathematics contrasted sharply with the views of the leading schools, which shunned meddling with the grey areas between specialties, where the appropriate rigor was not to be had and purity was not attainable. Against this trend, Klein’s life-long commitment was “to comprehend under an all-embracing unitary idea the opposing views of different schools of thought” (Klein,\textsuperscript{3} p. 52). Whereas the adepts of rigor and purity eschewed reliance on intuitive or quasi-empirical insights — the general trend was directed at the so-called arithmetisation’ of mathematics — Klein’s methodology was based on the use of geometric and even physical models and thought experiments, a methodology which certainly qualifies as ‘quasi-empirical’.
Klein’s successes depended in large measure on his exceptional versatility in the mental visualisation even of the most abstract mathematical objects and relations. He used his imaginative powers as forceful heuristic means of inquiry into intricate mathematical questions, aiming especially at the exploration of interconnections between seemingly disparate branches of mathematics. The ultimate unity of mathematics (and science) was the leading idea in all his intellectual pursuits. Throughout his career, Klein kept insisting that intuition, especially spatial intuition, is indispensable in all mathematical endeavours. It is also through this intuition that mathematics is firmly rooted in concrete experiences.

Klein was as much a maverick in the eyes of ‘pure’ mathematicians (especially those of the Berlin School\textsuperscript{a}) as Imre Lakatos would become in the eyes of mainstream philosophers of mathematics. Like Lakatos, Klein insisted that progress in mathematics relies on methods that are very much akin to those of natural science, especially as concerns the use of models and (thought) experiments. He in fact practised a model-based, quasi-empirical method of investigation that tallies nicely indeed with Lakatos’ quasi-empiricist methodology.

I will now proceed by first giving a brief characterisation of Lakatos’ version of quasi-empiricism, and subsequently show how some of Klein’s major achievements fit into it.

2. Quasi-empiricism

Lakatos characterised quasi-empiricism in terms of the characteristic direction of the logical flow (Lakatos,\textsuperscript{5} pp. 187–94). In quasi-empirical science, the characteristic truth value flow is the bottom-up retransmission of falsity, not the top-down transmission of axiomatic truth. The claim that mathematics is quasi-empirical therefore boils down to the claim that mathematical knowledge is not generated and established by rigorous deduction from a rock bottom of unshakable axioms, but is developed in a science-like fashion through ‘trials and tests’. Axioms should not be regarded as a basis of absolute certainty, but as ‘hypotheses’ whose acceptability depends (under the side condition of consistency) on how well they capture and explain the

\textsuperscript{a} In the report of the Advisory Committee for the succession of Weierstrass at Berlin (1897), we read: “Candidates must be able to lead students into serious and disinterested immersion in the problems of mathematics. For this reason we had to discard Professor Klein … whose scientific merits are much disputed among scholars, and whose activities in writing and in teaching contradict the said tradition of our University” (Biermann,\textsuperscript{4} p. 207).
mathematical knowledge that they are intended to consolidate.

In natural science, tests are experimental trials of a theory or hypothesis that create conditions under which disconfirmation of the operational consequences of the hypothesis or theory may be detected. In mathematics, the most nearly analogous sorts of tests are thought experiments (perhaps apart from computational tests, which Lakatos did not consider). It should be noted that in natural science, too, thought experiments are powerful means to tackle conceptual problems, especially in periods of conceptual reform, when the results of real experiments tend to be interpreted through different theoretical categories and hence to be conceptualised in different ways (cf. Kuhn,\textsuperscript{6} pp. 240–65). In \emph{Proofs and refutations},\textsuperscript{7} Lakatos identified informal proofs in mathematics with thought experiments, construed as attempts \emph{at once} to anchor a conjectured theorem to already accepted or trivial lemmas \emph{and} to detect counterexamples, either to the theorem or to one or more of the said lemmas (global and local counterexamples, respectively).

Such informal proofs, which depend on an intuitively persuasive — 'quasi-experimental', not strictly deductive — mode of reasoning, are not just logically deficient proofs, but are different in \emph{kind} from formal proofs. Although there is no method for verifying them with certainty, there is a method for \emph{testing} them, viz., the construction of counterexamples, which may show, for instance, that a theorem was 'stated in a false generality' (Lakatos,\textsuperscript{8} p. 65) This analogy with science enabled him to adapt Popper's method of conjectures and refutations to mathematics, and at the same time to surpass it by providing it with a dynamic and heuristic dimension (fully developed only in the methodology of research programmes, Lakatos\textsuperscript{9}). Whereas Popperian refutations are logical refutations, which confront a statement with evidence with which it is logically incompatible, Lakatosian 'refutations' are merely heuristic: "Testability in mathematics rests on the slippery concept of a heuristic falsifier. A heuristic falsifier after all is a falsifier only in a Pickwickian sense: it does not falsify the hypothesis, it only suggests a falsification — and suggestions can be ignored" (Lakatos,\textsuperscript{9} p. 40). However, ignoring (for instance by monster barring, exception barring, monster adjustment) implies neglecting opportunities to improve the generality and scope of the conjectured theorem, its unifying, explanatory and problem-solving potential, etc. — this is just the lesson of \emph{Proofs and refutations}.

Much the same applies to the testability of a fully formalised theory. If an informal counterexample is found to a formal theory, for instance a for-
mal theory of arithmetic, this does not show that the theory is inconsistent, but it does show that it is not a correct formalisation of arithmetic, while it may still be a true theory of some mathematical structure that is not isomorphic to arithmetic. So logic does not compel us to accept any informal counterexample, either to a formal or to an informal mathematical theory: we can retain it ‘come what may’, but always at the price of losing — or in any case not gaining — interesting content. ‘Content’ and ‘growth of content’ are the all-important notions that Lakatos inherited from Popper and tried to apply to mathematics.

Lakatos opposed in particular the utterly unhistorical conception of mathematics as ‘the set of all formal systems’. Rather than to introduce consistent formal systems in an arbitrary way and next to look for criteria to sift respectable mathematical theories from a mass of uninteresting formal ‘games’, he would reverse the order: “we should speak of formal systems only if they are formalisations of established informal mathematical theories. No further criteria are needed. There is indeed no respectable formal theory which does not have in some way or another a respectable informal ancestor” (Lakatos, 8 p. 62).

Both Lakatos and Klein were primarily concerned with the process of conceptual development, the growth of the informative contents of theories. They accordingly conferred a central role in their methodology on informal, heuristic reasoning, that is, on the situational and topical logic of inquiry rather than the formal logic of justification. Justificationist mathematicians do not regard model-based considerations, thought experiments and other informal practices as significant forms of mathematical reasoning, because if they cannot be reduced to a deductive argument, they cannot warrant the correctness of the conclusions. The fallibility of these methods denied them a place among the logical precepts of the justificationist philosophy of mathematics. However, such modes of reasoning were precisely at the heart of Klein’s most impressive mathematical achievements.

3. Klein’s quasi-empirical method

I now want to briefly consider some examples of Klein’s characteristic mode of proceeding in order to pinpoint more precisely the quasi-empirical element in his mathematics.

When Klein was appointed professor of geometry at Leipzig (1880), he immediately began lecturing on Riemann’s analytical function theory. This resulted in a publication, which was presented as a ‘completion’ (Ergänzung) of the theory (Klein10). He took the standard formal expo-
osition of the theory for granted, and set himself the task of reconstructing the heuristic reasoning underlying Riemann’s results, “attending with the utmost care to the reconstitution of the actual line of argumentation, and aiming to bring out clearly the range and power of the method” (Klein, p. 501). Klein showed how lamellar currents (of fluid, heat, electricity) along a surface can be described by complex functions, and how the properties of these functions can be clarified, inversely, by studying such currents as an appropriate model. The formulas (a pair of partial differential equations) from which Riemann started his exposition here arose as the end results of the physical thought experiments. As Klein pointed out, the formal exposition was excellently suited to deliver straightforward and rigorous proofs of the propositions of the theory, but it was entirely unsuited to convey its true meaning and scope. For Klein the physical analogy was not merely a dispensable aid to discovery: it represented the very essence of Riemann’s mode of proceeding.

The shape of the surface on which complex functions are pictured is not essential: it may be expedient to shift through a conformal mapping to a spherical surface (by placing the south pole of the sphere on the origin of the complex plane and projecting it with the north pole as centre of projection). Klein tackled the problem of the solvability of algebraic equations of the fifth degree by considering the symmetry group of a regular icosahedron inscribed within the said spherical ‘compactification’ of the complex plane. Very typically, he thus succeeded in connecting the abstract algebraic problem of the general fifth-degree equation with the ‘anschaulich’ geometrical problem of the rotations of the icosahedron, with the help of the analytical theory of functions of a complex variable (Klein).

Here we come across the very essence of Klein’s successful way of proceeding: his intuitive versatility in the creation of geometrical models and interpretations of mathematical structures. At the root of Klein’s success lay his use of geometrical imagination and visualisation, which ultimately are based on experiences in the real world.

In his *Erlanger Programm* (Klein), he had characterised the various geometries, Euclidean as well as non-Euclidean, metric as well as projective geometries, as the invariant theories of certain groups of transformations. Classical geometry, for instance, is the invariant theory of the group of translations, rotations and reflexions, that is, it studies those properties of figures that are retained upon rigid movement and mirroring. Projective geometry studies those properties of figures that are retained upon projection; at the top we find topology, which is the invariant theory of all continuous point
transformations. This group-theoretical approach generates a taxonomical
tree, which visualizes the interconnections between the various geometries
in a most elucidating fashion. Geometries higher up in the taxonomy are,
in Klein’s words, “sifted step by step from ordinary geometry, comparable
to how a chemist, by applying stronger and stronger reagents, isolates in-
creasingly more valuable substances from his original compound” (Klein,14
p. 132). Klein’s approach provided what were in essence rules of translation
from one geometrical system to another, showing, among other things, that
the non-Euclidean versions were no less ‘real’ than the classical version. It
all depended merely on the perspective applied. Klein’s group-theoretical
approach explained the nature of the various forms of geometry, their re-
lations with each other and with the real world, in a way that could not
simply be reduced to a deductive, readily formalisable chain of reasoning.
Its significance lay in its explanatory, unifying and heuristic power.

The most familiar geometry undoubtedly is classical Euclidean geo-
metry. Its transformation group of rigid displacements and reflections is
the group under which all classical geometric properties of figures are pre-
served. As such it represents the natural way in which we experience the
world, namely as composed of objects that maintain their shape when they
are moved about or seen in a mirror. In contradistinction to the more ab-
stract geometries, this geometry (says Klein in a footnote to the Erlanger
Programm) “is not merely a representational form of abstract relations. A
model, either actually constructed or merely vividly imagined, is for this
geometry not a means to the end but its very object (die Sache selbst)"
(Klein,15 p. 75) — that is: not just a vehicle for research, but what this
geometry is ultimately all about. Nevertheless, Klein consistently spoke of
classical geometry as being itself a model, on a par with other forms of
geometry and not in any way epistemologically privileged. It is the ‘gen-
quine’ geometry (die eigentliche Geometrie) only in the pragmatic sense
that it represents in the most natural way the appearance of things in the
real world. Our experiences of natural phenomena as such are no part of
mathematics, but a mathematical theory that would not even in princi-
ple allow of application to the world of empirical and practical experience
would be utterly useless. Klein’s generic taxonomy pictured in a most elu-
cidating manner how the various branches of geometry grow out of other
branches and ultimately out of a common stem — die eigentliche Geome-
trie — through which the tree as a whole is firmly rooted in the real world
and its empirical relevance is ensured.
4. The status of axioms

Klein fully recognised the merits — indeed, the indispensability — of the axiomatic method when it comes to expounding the logical core underlying an already developed piece of mathematics. However, he did not think it was a fruitful method of discovery. What he vehemently rejected, though, was the idea that axioms are just arbitrary statements which we may choose at will, and that basic concepts are only arbitrary symbols which refer to nothing but their own rules of operation (the view ascribed to contemporary formalists). Axioms derive their substance and weight from the intuitive evidence of their logical consequences, which in a sense are to be explained by them.

Fundamental concepts and axioms are not immediate facts of perception, but they are appropriately selected idealisations of these facts (Klein, pp. 186–7). Although individual axioms do not express facts of immediate experience, the axiom systems are ‘occasioned’ by it, in the sense that experience induces our intuitive faculty to form idealisations, and to select from these idealisations such axioms as are appropriate for the design of models that apply to the empirical world. The criterion for the selection of a set of axioms is expediency in deriving and proving the content of informally developed theories which fit the appearances. Klein insisted that “mathematical intuition . . . everywhere in its domain runs ahead of logical thinking, and therefore has at every moment a wider scope than the latter” (Klein, p. 237). This statement amounts to very much the same as Lakatos’ above-mentioned claim that “there is no respectable formal theory which does not have in one way or another a respectable informal ancestor”. Theories are not developed on the basis of a priori axioms; the axioms are introduced afterwards, in order to furnish gapless proofs to informally developed theories and to render them more precise, complete, general, etc.

For instance, as Klein reported, he and Lie had originally defined groups as “systems of operations of which any combination also belongs to the system” (Klein, pp. 335–6). In his further research on infinite groups, Lie had to stipulate also the presence of all the inverses of the operations in the group. The other two ‘criteria’ (presence of a neutral element and associativity) were added when group theory finally became axiomatised, which implied also that the concept of a group was no longer defined explicitly but implicitly, in terms of unspecified ‘elements’ that satisfy the group axioms. We thus have here a typical example of intuition leading the way in the process of abstraction, idealization and modelling of relationships, and axiomatisation following in order to consolidate the new territory. As Klein
pointed out, the implicit definition of a group in terms of four axioms was
more precise than the one he and Lie had originally used, and the axiomatisation was excellently suited to furnish direct and complete proofs for already established propositions, without further appeals to imagination. As such it represented the closure of a preceding development, allowing the rigorous deduction of the full content of the informally developed theories. Intuition having fulfilled its explorative task, the formal reformulation was justified in making definitions more exact and proofs more straightforward and ‘gapless’ (lückenlos).

There is a movement back and forth between quasi-empirical insights and axiomatic principles. Naive intuitions are refined deductively by logical development from axioms, which, however, were themselves first inferred, retroductively or abductively, from spatial intuitions. The abductive movement goes up from quasi-empirical insights to such principles as allow their deduction in a perfectly logical fashion (what nowadays would be called ‘abduction to the best explanation’). Ultimately, the truth and certainty of axioms derive from the quasi-empirical evidence of their logical consequences, which could not be deduced without them. Logic provides a rigorous skeleton, but it is the imaginative element that brings a theory to life and enables us to pose questions that fall across the boundaries between self-contained specialties, and to discern previously unsuspected connections between them. Mathematical discovery depends on inspired intuitions, which are refined through logical development from axioms, but which are not reducible to logic alone and cannot be discarded once the axiomatisation is complete.

5. Conclusion

Klein’s conception of mathematics was thoroughly holistic. His views and his practices were centred on mutual relationships between structured wholes, especially models, rather than on unidirectional chains of inferences from proposition to proposition. He basically construed mathematics as a set of interconnected models, some of which are isomorphic to the appearances and thereby furnish the indispensable links with physical reality. Besides many important similarities, there is in this respect also a difference in emphasis between Klein’s and Lakatos’ views. As a methodologist, Lakatos focussed primarily on proofs and refutations, that is: on trials and tests of mathematical propositions. In contrast to Klein’s ‘model’ view of mathematics, Lakatos entertained a propositional, theory-centred view, in which models and thought experiments serve primarily to test and
thereby to improve mathematical propositions. But, as is clear from the
examples adduced, the role of quasi-empirical methods is not confined to
the testing of propositional conjectures or delivering the informal proofs
and refutations upon which Lakatos’ methodology relies.

Mathematics does grow through ‘trying and testing’, but in a somewhat
wider sense than Lakatos envisioned. The growth of mathematical knowl-
edge has more than one dimension. One important dimension is growth in
breadth, by expanding and improving the body of propositions so as to
capture wider and wider ranges of problems and questions. Here Lakatos’
methodology may serve well. But mathematics also grows in depth, proceed-
ing to more comprehensive, integrated and unified theories at a more ‘pro-
found’ level. It is with regard to the latter sort of progress that Klein’s quasi-
empirical methods showed to full advantage. Klein’s modelling practice typ-
ically drew on resources from different scenes of inquiry (algebra, analysis,
geometry), melding them so as to tackle problem situations that fall across
the boundaries between them. He thus established ‘deeper’ connections be-
tween fields that were previously considered entirely unconnected, thereby
 superseding old dichotomies such as between pure and applied mathematics,
analytical and geometrical methods, and especially between mathematics
and empirical science.

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1. Introduction

The idea that geometry derives from innate knowledge has an ancient history in philosophy. As chronicled by Plato in *Meno*, ca. 380 B.C., Socrates probed the geometric intuitions of an uneducated slave boy in a Greek household, leading him, through a series of questions, to discover relationships between the areas of squares drawn in the sand. He concluded that the slave’s soul must have always possessed this knowledge, and that our learning of geometric concepts is actually recollecting (*anamnesis*) what we have always known as immortal souls. Descartes (AT VI 135–137) wondered how a blind person is able to learn spatial relationships by touch, and extended this problem to sighted people who learn to orient themselves in space by sight. He suggested that this is possible through *une géométrie naturelle*, an innate geometry (Descartes, AT VI 137–138). The most detailed philosophical argument for the innateness of geometric intuitions comes from Kant’s argument from geometry, which argues that our geometric knowledge derives from an a priori intuition of space.

This paper aims to look at this longstanding philosophical issue through the lens of cognitive science. I will investigate whether empirical data from studies on animal cognition, developmental psychology and anthropology warrant an enhanced version of the argument from geometry. In Section 2, I briefly discuss the argument from geometry and propose an interpretation of this argument which can be pitted against the evidence from cognitive science and the history of mathematics. Section 3 considers evidence for
spatial intuitions in animals, young children and toddlers, and looks at the neurobiological underpinnings of this ability. Section 4 examines anthropological and historical data for continuities and discontinuities between our spatial intuitions and formal geometry. Section 5 assesses whether these data support the enhanced argument from geometry and provides some suggestions for future research.

2. The argument from geometry

2.1. Kant’s original argument from geometry

The argument from geometry appears in Kant’s *Critique of pure reason* (1781). First, in the section *On space, metaphysical exposition of this conception*, Kant stresses that the original representation of space is an a priori intuition (Kant, A25/B40). He specifies that our intuition of space does not and cannot be derived from outward experience:

Space is not an empirical concept that has been drawn from outer experiences. For in order for certain sensations to be related to something outside me (i.e., to something in another place in space from that in which I find myself), thus in order for me to represent them as outside one another, thus not merely as different but as in different places, the representation of space must already be their ground. Thus the representation of space cannot be obtained from the relations of outer appearance through experience, but this outer experience is itself first possible only through this representation (Kant, B38).

Then, in the paragraph entitled *Transcendental exposition of the concept of space*, Kant argues that it is precisely this intuition of space that enables us to develop geometry as a scientific discipline:

Geometry is a science that determines the properties of space synthetically and yet a priori. What then must the representation of space be for such a cognition of it to be possible? It must originally be intuition; for from a mere concept no propositions can be drawn that go beyond the concept, which, however, happens in geometry. But this intuition must be encountered in us a priori, i.e., prior to all perception of an object, thus it must be pure, not empirical intuition. For geometrical propositions are all apodictic, i.e., combined with consciousness of their necessity, e.g., space has only three dimensions; but such propositions cannot be empirical
or judgments of experience, nor inferred from them. Now how can an outer intuition inhabit the mind that precedes the objects themselves, and in which the concept of the latter can be determined a priori? Obviously not otherwise than insofar as it has its seat merely in the subject, as its formal constitution for being affected by objects and thereby acquiring immediate representation, i.e., intuition, of them, thus only as the form of outer sense in general. Thus our explanation alone makes the possibility of geometry as a synthetic a priori cognition comprehensible (Kant, $^3$ B41, emphasis added).

Philosophers of mathematics have generally taken this passage as the unreasonable claim that geometry necessarily reflects eternal, unchanging and universal intuitions of space. Indeed, many agree that the development of non-Euclidean geometries refuted Kant’s argument altogether (e.g., Hersh, $^4$ pp. 131–2). However, Shabel$^5$ has persuasively argued for an alternative weaker interpretation for the argument from geometry: it does not analyze geometric cognition in order to establish that we have an a priori intuition of space. Rather, it establishes that geometric cognition itself develops out of a pure intuition of space. This intuition is offered both as the actual source of our cognition of the first principles of geometry and as a basis for culturally transmitted geometric concepts. This does not exclude the cultural evolution of geometric principles that do not correspond to, or even violate, these intuitions. Rather, it amounts to the modest claim that without a priori intuitions of space, geometry of any kind would be impossible. They admit the possibility of geometry, as Kant puts it, rather than any particular formal geometric system. The a priori concept of space is underdetermined, but it governs our capacity to form intuitions of particular finite spatial regions (Shabel$^6$).

2.2. An enhanced argument from geometry

It is not my intention of reformulating or explicating Kant’s argument, as this has already been aptly done by other scholars. However, I use Kant as my starting point for gauging the role of spatial intuitions in geometrical knowledge as he provides the most detailed formulation of the a priori basis of spatial cognition and geometry. Based on this careful re-examination of the argument from geometry, I can now formulate an enhanced version of this argument:

Humans possess an innate intuition of space.
This pure intuition of space constrains and governs the development of formal geometry.

In recent years, a growing number of philosophers of mind have turned to cognitive science (e.g., De Cruz, Decock, Margolis and Laurence) to examine how people acquire concepts and other forms of abstract knowledge. Working within the framework of naturalized epistemology (Quine), they use findings from empirical science to examine the real-world conditions under which epistemic agents acquire knowledge within the scope and limits of their cognitive capacities. Within the framework of naturalized epistemology, the argument from geometry thus becomes a statement whose truth value depends to an important extent on cognitive science for the answer to the following questions: is there any evidence for innate intuitions of space, and do these intuitions play a role in geometry? Fortunately, we are in an excellent evidentiary position to do this. Investigations of the cognitive capacities that underlie our knowledge of space have a rich and diverse history, especially given the fact that cognitive science is such a young discipline. Since the 1960s, a large body of evidence has accumulated on how animals navigate and how they represent space. Since the 1970s, neuroscientists have been investigating how space is represented at the neural level. About a decade later, developmental psychologists began to investigate how toddlers and young children represent space. Recent work has focused on the question in how far human spatial cognition is continuous with nonhuman animal representations of space, which can help us evaluate the claim that humans possess an innate intuition of space. The second part of the enhanced argument from geometry can be evaluated as follows: is there evidence that geometry depends on this innate capacity for reasoning about space? Evidence in favor of this claim should come from continuities between intuitive notions of space and formal geometry observed in the history of mathematics and from other cultures.

3. The cognitive basis of spatial cognition

3.1. Evidence from studies of animal spatial cognition

From an evolutionary perspective, there are good reasons to expect that animals possess a priori intuitions of space. All animals must be able to find their way back to particular locations like food-sources, hives, nests and caches. Over the past 50 years, studies on animal navigation indicate that animals possess a wide variety of specialized mechanisms that help them to navigate. Some of these abilities are highly specialized, such as
the homing pigeon’s (*Columba livia*) sensitivity to the Earth’s magnetic field (Mora et al.\(^{11}\)), or the indigo bunting’s (*Passerina cyanea*) ability to use trigonometric relationships between clusters of stars to orient itself during migration (Emlen\(^{12}\)). Of course, as one reviewer to this paper has aptly remarked, cognitive adaptations for navigation do not automatically entail sophisticated spatial skills. Some species’ navigational skills can be explained by an efficient interaction between their sensori-motor capacities and properties of the environment, such as Pharaoh’s ants’ (*Monomorium pharaonis*) ability to use pheromone trails to find their way back from a food source to the nest (Jackson et al.\(^{13}\)). Another simple solution to the problem of navigation, found in insects such as ants and bees, is to store mental snapshots of landmarks encountered along paths from the nest to a food-source. However, this solution is relatively inflexible and demands much memory resources — thousands of images need to be stored as animals need to make multiple snapshots of a particular landmark from different vantage points so as to recognize it from multiple angles (Judd and Collett\(^ {14}\)). For animals with a wide and variable home range, it may be useful to represent spatial properties on a more abstract level. Because geometric relationships are salient, we can expect that many species have evolved sensitivity to such cues.

Carefully controlled experiments have assessed whether animals can make high-level geometric representations. One research program has focused on Clark’s nutcrackers (*Nucifraga columbiana*, a species of corvid). These food-caching birds can conceptualize the halfway point between two landmarks. Kamil and Jones\(^{15}\) trained Clark’s nutcrackers to find a partially buried seed halfway between two plastic pipes. On the test trials, the seeds were entirely buried. The birds were presented with new distances between the pipes to test their ability to generalize the geometric relationship between the seed and the two landmarks. They readily learned to bisect the inter landmark distance: they correctly found the halfway point when the landmarks were presented with new distances between them. The geometric sense of the birds is quite abstract, since alteration of the height of one of the landmarks did not affect their success at finding the seed. Simpler explanations were experimentally ruled out, e.g., finding the seeds by smell or by cues other than geometry. Moreover, the birds were more accurate in finding the line connecting the landmarks than in locating the correct position on the line. This suggests that they make two separate decisions (Kamil and Jones\(^{16}\)): finding the line connecting the landmarks, and subsequently determining the halfway point on it. Clark’s nutcrackers
store up to 33,000 pine seeds in thousands of cache sites during the autum

months. The seeds constitute the bulk of their winter diet, and are fed to their nestlings in spring (Gould-Beierle and Kamil). Considering these selective pressures, it is not surprising that these corvids have evolved geometric abilities to retrieve previously stored seeds.

Cheng has experimentally shown that rats rely exclusively on geometric cues (the shape of a room) to reorient themselves in order to find food. Hungry rats explored a room with partially buried bits of food. After the food was fully buried, they were reintroduced. Although the animals were provided with a wealth of nongeometric information (here termed featural cues), such as distinctive odors and relative brightness of the walls, they apparently exclusively relied on one clue only, the shape of the room. The rats betrayed their search methods by looking with high frequency for the food at its true location as well as its geometric equivalent (a mistake termed the rotational error). For example, the rats would search in the two corners that are located to the left of the short walls, which are geometrically indistinguishable, as shown in Fig. 1. Subsequent studies revealed that the rats were indeed able to discriminate nongeometric cues, but that they were somehow unable to use them in remembering locations. These findings led Gallistel (p. 172) to propose that rats have an innate geometry module, which is informationally encapsulated, meaning that it does not have access to nongeometric information. While the claim that intuitive geometry is informationally encapsulated remains controversial (see Cheng and Newcombe, for a review), the idea that many species have specialized cognitive mechanisms to deal with the geometric properties of their environment has become widely accepted.

Not all species place a similar premium on geometry. Some species, such as chickens, goldfish and lizards can use both featural cues and geometry to orient themselves. Chicks (Gallus gallus domesticus) can use geometric cues to find food in a rectangular room; they betray their search method by the rotational error. Once the walls are visually distinguishable, they can solve the task almost perfectly. When featural and geometric information provide contradictory cues, they rely mostly on features (Vallortigara et al.). Goldfish (Carassius auratus), too, encode both featural and geometric information; they even use geometry when it is not strictly necessary to solve the task (Vargas et al.). The whiptail lizard (Cnemidophorus inornatus) uses both featural cues and geometry, but it is more accurate when it relies on geometry (Day et al.). In sum, there is a considerable variation between species in the relative importance of featural and geometric
An Enhanced Argument for Innate Elementary Geometric Knowledge

3.2. Evidence from developmental psychology

Replicating Cheng’s experiment, Hermer and Spelke\textsuperscript{24,25} demonstrated that 18- to 24-month-olds use geometric cues to look for a hidden toy: disoriented toddlers reliably looked in the geometrically appropriate corners of a rectangular room. Like rats, they were utterly incapable to orient themselves by use of nongeometric cues, such as the color of a wall or colorful boxes close to the hidden toy. Even when the experimenter pointed out these useful landmarks, the toddlers failed to make use of them (Hermer and Spelke,\textsuperscript{24} p. 58). Between five to seven years of age, children gradually exhibit more flexible reorientation behavior, paying attention to both landmarks and geometric relationships (Hermer-Vazquez et al.\textsuperscript{26}). In another study, Haun et al.\textsuperscript{27} found that prelinguistic infants of 12 months prefer geometric over featural cues, and that this preference reverses at about three years of age. They presented infants, three-year-olds and four species of nonhuman apes (chimpanzee, bonobo, orang-utan and gorilla) with a simple object-search task, in which a reward was hidden under one of three containers, each of which looked distinctly different. The setup was then occluded, and two of the containers were switched. In the feature condi-
tion, the reward moved with its distinctive container, whereas in the place condition, the reward remained in its original place but came under a different container. Both infants and nonhuman apes performed better in the place condition, indicating that they exclusively relied on geometric cues to obtain the reward. In contrast, three-year-olds did better in the feature condition. This research indicates that geometric cognition arises earlier in child development than featural spatial cognition. Since the spontaneous use of geometric cues emerges prior to language acquisition and is similar to that of our closest living relatives, it seems very plausible that geometric cognition arises prior to experience.

A detailed case-study of a two and a half-year-old blind child (Landau et al.\textsuperscript{28}) shows that these early intuitions of space do not even require vision for their development. The girl was taken along several paths connecting four landmarks in a room, as is shown in Fig. 2 (dashed lines). The landmarks were easily distinguishable by touch, such as a table and a heap of pillows. During the test, she was asked to move directly between objects on paths she had never taken (solid lines). For example, during training, the child was guided to walk from her mother (M) to the heap of pillows (P), and back, and next from her mother (M) to the basket of toys (B). From there, she was asked “can you go to the pillows”, a path she had not walked before. She spontaneously took the correct path (B)-(P). The girl's accuracy in approaching the targets was striking; she even adjusted her movements as she went towards the targets, with 11 successful trials out of 12. A control experiment with blindfolded three-year-olds and adults revealed similar levels of success. To be able to deduce the correct path between two objects after moving to each of them from a third point, one must be able to derive angular relationships between previous routes, and the length of both routes. From this information, one can derive new angular relationships: the angular direction from one object to the other. Angle and distance are properties that are both conserved in metric geometries such as Euclidean geometry, but not in non-metric geometries, such as topology and projective geometry (the latter conserves only angles). Interestingly, the axioms of Euclidean geometry suffice to solve the aforementioned task. Landau et al.\textsuperscript{28} therefore concluded that some principles of Euclidean geometry are known from an early age on, and that sight is not required to learn about them.
3.3. Evidence from neuroscience

Does the ability of humans and other animals to derive geometric relationships from the environment depend on a specialized neural circuitry? In the 1970s, neurobiologists O’Keefe and Dostrovsky\textsuperscript{29} discovered that individual neurons in the rodent hippocampus increase their firing rates when the animal traverses specific regions of its surroundings. Each of these place cells has its own place field, typically covering about 10 to 20 percent of the current environment, only firing when the animal is located somewhere within its place field, and being silent when it is outside of it. In this way, place cells together constitute a kind of cognitive map. Lever et al.\textsuperscript{30} found that the firing patterns of place cells of a rat placed in rooms in which the walls
form a square or a circle increasingly diverged as the animal got familiar with the two geometric configurations. In other words, there were individual cells that fired preferentially responding to the circle, and those that fired only when the rat was in a square-shaped room. When the rats were placed in novel enclosures, the distinct firing patterns were immediately transferred to novel enclosures of the same shape. These patterns persisted after a delay of several weeks. This work suggests that place cells can represent the geometry of an environment at an abstract level, and possibly, that some place cells may be specifically attuned to geometry. Place cells do not simply code visual properties: the place cells of early blind rats, which have never seen their environment, respond in the same way as place cells of sighted rats (Save et al.\textsuperscript{31}).

Hafting et al.\textsuperscript{32} found that neurons in the entorhinal cortex of freely roaming rats also respond to spatial properties: if plotted on a map of the environment, the firing locations of the neurons form a triangular grid, resulting in a triangular lattice. Like place cells, these grid cells do not depend on visual input: once the rat is familiar with a particular enclosure, they keep firing in the same pattern when the animal roams in total darkness (Hafting et al.,\textsuperscript{32} p. 803). While place cells can be conceptualized as a map, the grid cells’ role is to provide a coordinate system, not unlike the overlaid grids of a map — except that the unit of the grid is not a square but an equilateral triangle.\textsuperscript{a} When a rat occupies a particular location, the grid cell that represents this position will be very active, together with its neighboring cells. As the rat moves, so does this field of activation, comparable to a moving ‘you are here’ sign on a map. To maintain this mapping between the rat’s real position and the cognitive map provided by the place cells, specialized grid cells encode information about head-direction, position, and even the speed with which the animal moves (Sargolini et al.\textsuperscript{33}).

\textsuperscript{a} There may be good reasons why the lattice is triangular rather than simply square. Mathematical models of percolation can model the connectivity of a lattice. Each site in a lattice (in this case, each triangle) can be part of a cluster (i.e., a group of neighboring sites that have the same state, namely occupied or empty) with probability $p$. If $p$ is small, there are many disconnected clusters; if $p$ is large, however, there are fewer large clusters. If $p \approx 1$, most of the occupied cells will form one large cluster, extending from one end of the lattice to the other. If grid cells are to be useful in navigation, connectivity between them may not be too high (as too many cells would fire and the animal would become confused about its actual location), nor too low (as the animal would be unable to figure out its trajectory). The $p$ value for an individual site to be part of a cluster in a triangular lattice is exactly $\frac{1}{2}$ (I thank Lesley De Cruz for pointing this out to me). It would be interesting to examine whether this connectivity is optimal for navigational purposes.
The neurobiology of navigation reflects the two main strategies for keeping track seen in animals: a geometric strategy, which depends heavily on relationships between features of the environment, and path integration, in which animals use self-generated motion cues to update a vector-based representation of distance and direction from a fixed reference point (Witter and Moser).34

![Figure 3. Areas in the rat brain (left) and the human brain (right) dealing with the representation of space (only left hemisphere shown; brains not to scale)](image)

Figure 3. Areas in the rat brain (left) and the human brain (right) dealing with the representation of space (only left hemisphere shown; brains not to scale)

Because recording individual neurons requires invasive brain surgery, it has long been unclear whether the human brain contains similar space-sensitive neurons. Ekstrom et al.35 recorded single neurons in seven epileptic patients prior to brain surgery, while they navigated in a virtual world. They found neurons in the hippocampus and parahippocampal region (which encompasses the entorhinal cortex) that responded to spatial properties (Fig. 3). Many cells preferentially responded to a single landmark. Most neurons in the parahippocampus were view-independent: they fired regardless of the angle or view from which the subject saw the landmark. In this way, the parahippocampus extracts allocentric spatial information from the salient visual landmarks. It provides us with the basic geometric layout of the environment. This study is in accordance with earlier neuroimaging studies (e.g., Epstein and Kanwisher36) which indicate that the geometric layout of an environment is constructed in the parahippocampal place area. Patients with focal brain damage to this area have an impaired ability to process geometric information: though they are able to produce accurate maps of places they knew before the brain lesion occurred, they are unable
to produce accurate maps of places they have encountered since. However, their perceptual abilities for nongeometric objects remained largely intact (Epstein et al. 37). This indicates a double dissociation between vision and geometry. In sum, neurophysiological recordings suggest that human reasoning about geometric relationships rests on specialized neural circuitry, which deals with a higher-order representation of space.

3.4. Evolutionary origins of human spatial cognition

Given that navigation is a universal problem for animals, it is unsurprising that all species examined for it possess the ability to attend to geometric relationships between landmarks. Next to this, human adults and a variety of other species also attend to nongeometric (featural) information when they navigate. Given that toddlers, young children and nonhuman apes attend primarily to geometry when dealing with search tasks, it seems likely that geometric intuitions are a stable and universal part of human cognition. Taken together, this evidence is in favor of the first part of my reformulation of Kant’s argument from geometry, namely that humans possess an innate intuition of space.

4. Does geometry develop out of a pure intuition of space?

This does not automatically entail the second part of the enhanced argument from geometry, namely that geometry develops from this pure intuition of space. After all, the gap between our geometric intuitions, impressive as they are, and geometry as a formal science is immense. To examine whether our innate intuition of space constrains and governs geometry, we need not examine geometry in its current form, but rather its historical roots. Historically, geometry has developed to provide formal and standardized solutions to problems frequently encountered in land surveying, navigation, engineering and architecture, such as how to square surface areas. This presupposes the ability to reason about abstract spatial relationships between objects, regardless of their features. Without this a priori intuition, it might never occur to us to calculate the ratio of a diagonal of a square to its side as $\sqrt{2}$ — any square, regardless of its actual size or features.

4.1. Evidence from anthropology

Cultures display a considerable variation in how space is represented, and this variation is expressed in linguistic differences. For example, Dutch and
English speakers use mainly relative, viewpoint-dependent (and thus egocentric) spatial terms, such as ‘the ball lies to the left of the tree’. Speakers of several indigenous Australian, Papua, and !Kung languages use absolute terms, e.g., ‘the water is in the northern well’. Experimental evidence indicates that spatial strategies are influenced by these cultural or linguistic preferences: !Kung children do better in search tasks where objects maintain their position in relation to the surrounding area, whereas Dutch children perform better in tasks where objects maintain their position in relation to themselves (Haun et al. 38). This cultural variation indicates that our intuitive notions of space are underdetermined; their development is subject to cultural variation.

How much, then, of our geometric intuitions are prespecified? Although the empirical evidence on this point needs to be further developed, evidence from anthropology and the history of mathematics provides tentative support for the second part of the enhanced argument from geometry, namely that we do not just have an innate intuition of space, but that this intuition can also serve as a basis for the development of formal geometry. Through an anthropological case study, Dehaene et al. 39 argue that some properties of Euclidean geometry may be part of our intuitive knowledge. The Mundurukú, an Amazonian culture, do not possess maps, little (if any) schooling, no compasses, nor do they possess an intricate geometric vocabulary. In their study, Dehaene et al. 39 asked Mundurukú participants to choose among arrays of six images the ‘ugly’ or ‘weird’ one. In each array, five pictures instantiated a concept from Euclidean geometry (e.g., parallelism, points, lines and symmetry), while one violated it. The figures were designed to minimize cues other than the desired conceptual relation that could identify the target figure (i.e., the odd one out). All participants performed significantly above chance level; there was no difference between children and adults. Fig. 4 shows samples of the test arrays, with the deviant figure marked in grey. The percentage of participants choosing the correct intruder is shown above each array; results higher than 35 percent are statistically significant. As can be seen in Fig. 4a, subjects were able to discriminate curves from straight lines, see Euclid’s fourth definition. They could see whether a point was aligned with a straight line or not, an assumption implicit in Euclid’s Elements, often used in his propositions (Fig. 4b). They detected parallel lines in a collection of secant lines and vice versa, see Euclid’s fifth postulate (Fig. 4c), as well as a right angle in a collection of non-right angles and vice versa, see Euclid’s fourth postulate (Fig. 4d). The Mundurukú provide an interesting example because,
like many other cultures, they do not possess a formalized geometry. Nevertheless, they spontaneously detected Euclidean-like geometric principles.

![Figure 4](image.png)

To claim that these principles are universal human cognitive features needs further support. For example, one could replicate this study in different cultures or with young children. In this way, one could examine whether ‘folk geometry’ has features that are invariant across culture, in a similar way as the investigations on folk biology (e.g., Atran) or folk psychology (e.g., Callaghan et al.). If the Mundurukú results can be replicated in other cultures, this could provide compelling support for the intuitiveness of Euclidean geometry.

### 4.2. The success and persistence of Euclidean geometry

For centuries, Euclid’s parallel postulate was taken as intuitively true, albeit less self-evident than the other four. Mathematicians felt it had to be proved as a theorem from the other four. Although many tried, none succeeded. In 1733, the Italian Giovanni Gerolamo Saccheri unintentionally invented a new viable hyperbolic geometry while trying to prove Euclidean

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If two lines intersect a third in such a way that the sum of the inner angles on one side is less than two right angles, then the two lines must inevitably intersect each other on that side if extended far enough.
geometry in his *Euclides ab omni naevo vindicatus* (Euclid freed from all flaws). Less known is that Muslim mathematicians, more than six centuries earlier, also wrote commentaries on the *Elements*, and that they also devoted special attention to the parallel postulate (Kanani, 42, pp. 316–9). No one doubted its validity, but many felt that it could not be accepted without proof. Ibn al-Haytham (865–901) introduced the concept of motion into geometry. His proof of the parallel postulate goes as follows: given point $P$ not on line $L_1$, the shortest distance between them is the line $PS$, which goes through point $P$ and perpendicular to line $L_1$. If we start moving $PS$ along the line $L_1$, a new line $L_2$ will be constructed. $L_2$ is parallel to $L_1$ since the distance between $P$ and $L_1$ remains unchanged. Omar Khayyám (1048–1131) objected to this introduction of motion in geometry. To him, motion was an attribute of matter; it should therefore not be introduced into the immaterial world of mathematics. In his *Explanation of the difficulties in Euclid’s postulates*, written around 1100, he used an indirect proof, arguing that the assumption that the fifth postulate was false would lead to a contradiction. His proof is summarized in Fig. 5. In the course of his attempt to prove the parallel postulate, Khayyám was led to posit some non-Euclidean theorems. He constructed two line segments $AC$ and $BD$, perpendicular to $AB$, to construct $ABCD$. The angle $ACD$ is equal to angle $BDC$. He then examined three possible cases: (a) angles $ACD$ and $BDC$ are each less than $90^\circ$; (b) angles $ACD$ and $BDC$ are each larger than $90^\circ$; (c) angles $ACD$ and $BDC$ are each equal to $90^\circ$. The Khayyám-Saccheri quadrilateral (the grey area in Fig. 5), composed of line segment $CD$, and two equal legs standing at right angles to it, $CK$ and $DH$, has oblique angles at $KH$ in cases (a) and (b). However, he never envisaged the possibility of a non-Euclidean geometry, because he was convinced that Euclidian geometry was the only true one (Kanani, 42, p. 319). For the same reason, Saccheri failed to recognize the significance of his discoveries.

Around 1830, attempts to prove the parallel postulate culminated in the purportedly independent invention of hyperbolic geometry by Nicolai Lobatchevsky, János Bolyai and Karl Friedrich Gauss. Hyperbolic geometry fulfils all of Euclid’s postulates and common notions, except that the fifth postulate does not hold, as there are at least two distinct lines through point $P$ which do not intersect line $L$. The realization that Euclidean geometry was not the only possible way to look at spatial relationships has enabled mathematicians to disregard the semantic content that was traditionally associated with some mathematical concepts. Traditionally, geometry had an obvious semantic component: spatial relationships. But today, primitive
terms such as ‘point’, ‘line’ and ‘plane’ are undefined. David Hilbert once famously remarked that in a system of geometrical axioms one must be able to substitute ‘point’, ‘line’ and ‘plane’ by ‘table’, ‘chair’ and ‘beer mug’. The increasing formalization of mathematics as a discipline in the nineteenth century was an important first step in the distancing of geometry from its ancient historical roots, as epitomized in Hilbert’s set of 20 assumptions which constituted a foundation for a modern treatment of Euclidean geometry. Although this program proved untenable in the face of Gödel’s incompleteness theorems, it is still maintained with some modifications. While it is impossible to formalize all of mathematics, mathematicians still aim to formalize most of it. Interestingly, though, Hilbert at the same time believed Euclidean geometry to be the closest approximation of our intuitions of space. As Majer’s analysis of his view on geometry shows, Hilbert believed Euclidean geometry to reflect our everyday conception of space, while he regarded non-Euclidean geometries as more applicable to express

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\[\text{Euclid did provide definitions for these terms, for instance, a point is defined as that which has no part. It is impossible to define every term we use, because a definition requires terms that in turn need to be defined and so on (what does it mean to have 'no part'?') — this would quickly lead to infinite regress.}\]
scientific concepts of space-time. Indeed, the invention of non-Euclidean geometries did not automatically undermine the Kantian view of an a priori Euclidean notion of space. However, foundational approaches to geometry did lead to the growing realization that one cannot simply equate geometrical intuitions with geometry as a formal science or as a description of physical space. Poincaré’s conventionalism, which holds that some mathematical theorems cannot be preferred above others on a priori intuitive grounds, serves as a good illustration. When asked which geometry was true, Poincaré**4** (p. 50) famously responded:

> The geometrical axioms are therefore neither synthetic a priori intuitions nor experimental facts. They are conventions [...] What then are we to think of the question: Is Euclidean geometry true? It has no meaning. We might as well ask if the metric system is true and if the old weights and measures are false [...] One geometry cannot be more true than another: it can only be more convenient.

More decisively, in the early decades of the twentieth century, Einstein’s theory of relativity showed that physical time-space could be more adequately captured by non-Euclidean (Riemannian) geometry than by Euclidean geometry. Yet even then the Kantian notion of intuitive Euclidean geometry was not immediately abandoned. In his doctoral dissertation of 1922, Rudolf Carnap attempted to resolve the conflicts in foundational geometry by stating that intuitive space, geometry and physical space in fact referred to different types of space and that there was thus no contradiction. His view was strongly influenced by Husserl’s concept of ‘essential insight’ (*Wesenserschauung*), which is directed at universal features of space, rather than at particular geometrical properties of the environment. For Carnap, intuitive space was synthetic a priori and experience-constituting in Kant’s original sense. However, in later writings he progressively abandoned this notion (Friedman**43**). In the course of the twentieth century, Kant’s argument from geometry has fallen from favor. The intellectual climates of foundational mathematics and theoretical advances in physics have together led to an increasing divergence between our intuitive notions of space and formal geometry. The same happened in other sciences, such as physics and biology which diverged increasingly from their evolved counterparts, such as intuitive physics and folk biology. Newtonian physics denied the notion of impetus, an important principle of intuitive physics, while Darwinian evolutionary theory made untenable the notion that species possess an unchanging essence, a central belief in intuitive biology (De Cruz and De
Up to the eighteenth century, intuitions of space did play an important role in the development of geometry. The metatheoretical research programs in nineteenth century mathematics, which explicitly ignored any semantic content of mathematical objects, made non-intuitive ideas such as non-Euclidean geometries acceptable. Although non-Euclidean geometries may be better suited to describe the spatial relationships within space-time than Euclidean geometry, natural selection does not take observations on such a grand scale into account, because they are unimportant for an animal’s day to day survival and reproduction. It is therefore not unlikely that natural selection could have produced Euclidean spatial intuitions in the nervous systems of humans and other animals. Since physical space and intuitions of space do not need to coincide, the argument from geometry can become an empirical question that cognitive science can address.

5. Concluding remarks

Kant’s argument from geometry has often been interpreted as the far-reaching claim that geometry reflects eternal, unchanging and universal intuitions of space. Here I have argued for a weaker interpretation of this argument, which states (1) that humans have innate, evolved and species-universal cognitive adaptations to deal with space, and (2) that these intuitions constrain and govern the development of formal geometry. Geometry as a science has its roots in problems of land surveying, engineering, navigation and architecture, which require a formal solution to represent spatial relationships. Were it not for our intuitive ability to represent spatial relationships, it seems unlikely that geometry would have arisen in the first place. In this paper, I have reviewed studies that indicate that human spatial cognition emerges early in development, that it is continuous with that of nonhuman primates, and that it is subserved by specialized neural circuitry that we share with other mammals. Further empirical investigations are needed to evaluate the second part of the enhanced argument from geometry. More cross-cultural experiments are needed to test whether principles of Euclidean geometry, such as parallelism and right angles, are a stable and universal feature of human cognition. It is interesting to consider Kant’s following remarks in the light of the evidence from cognitive science:

We can […] speak of space, extended beings, and so on, only from the human standpoint. If we depart from the subjective condition under which alone we can acquire outer intuition […] then the representation of space signifies nothing at all (Kant, A26/B42).
Our expositions accordingly teach the reality (i.e., objective validity) of space in regard to everything that can come before us externally as an object, but at the same time the ideality of space in regard to things when they are considered in themselves through reason, i.e., without taking account of the constitution of our sensibility (Kant,\textsuperscript{3} A28/B44).

Kant argues here that it is only from our human standpoint that we can speak of space: although it exists independently from our experience, we cannot cognize space as it is in itself, but only through our a priori intuitions. Like other animals, humans can only intuitively think about space through the lens of cognitive adaptations. We can only overcome these intuitions through a rigorous formalization of geometry as a science, which as a consequence becomes less intelligible to us.

Bibliography


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1. Introduction
This paper focuses on mathematical practices instead of mathematical products. It is indeed part of the growing awareness that historical, social and psychophysical processes precede the cut and dried results of mathematics, even those which have been presented as the obvious starting points of all pure mathematics. As all fashionable currents, the shift from foundations to practices in the philosophy of mathematics has its heroes. Lakatos immediately comes to mind, as does Wittgenstein. So, if I were to say that, given their mutual influence, Wittgenstein’s view on mathematics can be identified with Russell’s, you would correctly blow the whistle on me. However, if I were to say that, given their collaboration and Prinicia mathe- matica co-authorship, Whitehead’s view on mathematics can be identified with Russell’s, this claim would pass without a shred of protest. But in the latter case, I would firmly disagree. In fact, this paper is a first expression of my conviction that Whitehead, like Wittgenstein, should be differentiated from Russell, and given his own niche in the gallery of major philosophers of mathematics. Furthermore, Whitehead’s writings, based on his own mathematical experience, offer a perspective on mathematical practices which equals, or even surpasses, the perspective offered by Wittgenstein.

2. Whitehead’s philosophical urge
Two prime factors to differentiate philosophers are their ultimate philosophical drive and their dominant philosophical style. In Russell’s case, the emotional insecurity of a childhood marked by death and increasing solitude
grew into an intense longing for certainty, and into a lifelong succession of philosophical conversions. When Whitehead — in 1903 — abandoned the writing of the second volume of his *A treatise on universal algebra with applications*\(^2\) to team up with Russell in writing a second volume of *The principles of mathematics*,\(^3\) the search for certainty had taken Russell to a project to reduce all mathematical concepts and propositions to a secure base of fundamental logical concepts and principles, and his conversion from an idealist monism to a realist pluralism had taken him to a mathematical Platonism and a material atomism. However, it would be wrong to deduce that — at least during their decade-long collaboration on *Principia mathematica* — Whitehead and Russell were two of a kind. Contrary to Russell, Whitehead was not reborn at the beginning of the twentieth century. Whitehead did not shake off his personal prehistory. The later phases of his thinking always integrated the earlier phases, including the nineteenth century legacy of his education in Victorian England. Consequently, and to give but one example, Joan Richards’ identification of Whitehead with Russell in her otherwise superb *Mathematical visions: The pursuit of geometry in Victorian England* is inappropriate.\(^4\)

In *Mathematical visions*, Richards shows that the discussions on mathematics in Victorian England “culminated in a radical change in the perception of the nature of geometry. Whereas in the nineteenth century, geometrical results were perceived descriptively, as binding truths about real space, in the twentieth they are more commonly seen formally, as deductions drawn from an abstract axiom system only more or less applicable to the real world” (p. 4). According to Richards, “Russell and Whitehead both abandoned the descriptive view of geometry […] and together moved wholeheartedly into the formal development of logistics” (p. 229). Taking her identification of Whitehead with Russell for granted, she not only writes that “the basic outlines of this new perspective were clearly laid out by Whitehead” (p. 241), but even holds that Whitehead “represented a radical break from the nineteenth-century tradition […] which tried to bring all of knowledge under a single philosophical umbrella” (p. 243).

Richards’ identification of Whitehead’s view on mathematics with a univocal stress on its formal, pure and abstract character, at the expense of its descriptive, applied and concrete character, and even at the expense of the unitary view of truth prevailing in Victorian England, is inappropriate. Richards’ one-sided emphasis on Whitehead as a formalist causes her to overlook the apparent paradox that Whitehead only tried to free mathematics from the obligation to be descriptive because he was convinced that
its formal growth would ultimately best serve its descriptive aim. In order to understand this apparent paradox, we need to take Whitehead’s roots into account. Considering Whitehead’s collaboration with Russell during the first decade of the twentieth century as a justification to abstract from his Victorian roots is as misleading as considering Wittgenstein’s interaction with Russell during its second decade as an excuse to study Wittgenstein’s writings in abstraction from his Viennese roots, an error corrected — thirty five years ago — by Allan Janik and Stephen Toulmin.\footnote{Wittgenstein wrote: “I don’t believe that I have ever invented a way of thinking. I have always taken one over from someone else. I have simply straight-away seized upon it with enthusiasm for my work of clarification […]. Boltzmann, Hertz, Schopenhauer, Frege, Russell, Kraus, Loos, Weiniger, Spengler, Straffa have influenced me” (Wittgenstein, p. 19e). In \textit{Wittgenstein’s Vienna}, Janik and Toulmin took Wittgenstein’s remark seriously, and invited us to consider Wittgenstein’s \textit{Tractatus logico-philosophicus}, not exclusively as his response to the endeavours of Frege and Russell to provide mathematics with logical foundations, but also as Wittgenstein’s reform of Fritz Mauthner’s critique of language, inspired by Heinrich Hertz’s reform of Ernst Mach’s critique of mechanics. With their presentation of the subject of Wittgenstein’s \textit{Tractatus} as a calculus of language, functioning as a Hertzian model of reality which makes it possible to clarify the nature and the limits of language from within, and ultimately to establish a saying/showing, fact/value and science/ethics dichotomy, Janik and Toulmin revealed that Wittgenstein’s ultimate motivation was different from Russell’s. Whereas Russell was in search of mathematical certainty, Wittgenstein was in search of linguistic purification.}

Whitehead wrote: “I can have no claim whatsoever to standing above, or beyond, or in any way outside of my age. I am exactly an ordinary example of the general tone of the Victorian Englishman, merely one of a group”\footnote{Whitehead’s reform of Ernst Mach’s critique of mechanics. With their presentation of the subject of Wittgenstein’s \textit{Tractatus} as a calculus of language, functioning as a Hertzian model of reality which makes it possible to clarify the nature and the limits of language from within, and ultimately to establish a saying/showing, fact/value and science/ethics dichotomy, Janik and Toulmin revealed that Wittgenstein’s ultimate motivation was different from Russell’s. Whereas Russell was in search of mathematical certainty, Wittgenstein was in search of linguistic purification.} (Whitehead, p. 115.) But Richards was no Janik or Toulmin for Whitehead. So, who takes Whitehead’s remark seriously? E.g., who listens when Russell points out that “Maxwell’s great book on electricity and magnetism [was] the subject of Whitehead’s Fellowship dissertation,” and that “on this ground, Whitehead was always regarded at Cambridge as an applied, rather than a pure, mathematician”\footnote{Or, to give another example, who compares Victor Lowe’s Whitehead biography with Andrew Warwick’s study on the education of J. H. Poynting, Joseph Larmor and J. J. Thomson in \textit{Masters of theory}, and draws the conclusion that Whitehead was a similar Cambridge product, that is to say: a sec-}
ond generation Maxwellian? Well, I do, and I think no proper account of Whitehead’s philosophy of mathematics can be given, unless it starts from the impact the success of Maxwell’s mathematical models of electric and magnetic phenomena had on Whitehead. Hamilton’s formal generalisation of complex numbers into quaternions ultimately gave rise to a calculus that led Maxwell to the descriptive unification of electricity and magnetism, and that struck home to Whitehead.

That Maxwell, “guided by the most abstract of pure mathematical considerations,” was “led back to the most fundamental [. . .] laws of nature,” inspired Whitehead to write: “It is no paradox to say that in our most theoretical moods we are nearest to our most practical applications” (Whitehead,\textsuperscript{11} p. 71). This 1911 statement is rephrased in 1925: “Nothing is more impressive than the fact that as mathematics withdrew increasingly into the upper regions of ever greater extremes of abstract thought, it returned back to earth with a corresponding growth of the importance for the analysis of concrete fact. [. . .] The paradox is now fully established that the utmost abstractions are the true weapons with which to control our thought of concrete fact” (Whitehead,\textsuperscript{12} p. 32). And again, in 1929, Whitehead writes: “It is a remarkable characteristic of the history of thought that branches of mathematics, developed under the pure imaginative impulse, [. . .] finally receive their important application” (Whitehead,\textsuperscript{13} p. 6).

Given these quotes, the following statement of Whitehead will come as no surprise: “Every branch of physics gives rise to an application of mathematics. A prophecy may be hazarded that in the future these applications will unify themselves into a mathematical theory of a hypothetical substructure of the universe, uniform under all the diverse phenomena” (Whitehead,\textsuperscript{9} p. 285). Whitehead tried to fulfil this prophecy himself, and consequently, I hold that Whitehead’s utilization of Grassmann’s calculus of extensions to arrive — in 1898 — at \textit{A treatise on universal algebra with applications}\textsuperscript{2} is inspired by, and tries to improve upon, Maxwell’s utilization of Hamilton’s calculus of quaternions to arrive at \textit{A treatise on electricity and magnetism}. Whereas the subject of Wittgenstein’s \textit{Tractatus} was a calculus of language, functioning as a Hertzian model of reality, the subject of Whitehead’s \textit{Universal algebra} was a calculus of physics, functioning as a Maxwellian model of reality.

It is well known that Hertz developed his views by studying Maxwell, and this knowledge can be taken as an invitation to initiate a Wittgenstein/Whitehead comparison. I would welcome such an initiative. However, from the start it should be clear that the unitary view of truth of the Victo-
rian Whitehead is fundamentally at odds with Wittgenstein’s firmly rooted fact/value dichotomy.

Anyway, what I said about Whitehead’s *Universal algebra* can be repeated for his memoir “On mathematical concepts of the material world”.

This memoir was written in 1905, and published by the Royal Society in 1906. In other words, it was written and published while Whitehead and Russell were fully immersed in the *Principia mathematica* project. In this memoir Whitehead utilizes Russell’s logic of relations to arrive at a general and unifying mathematical concept of the material world. Whitehead’s 1905/1906 mathematical concept of reality has to be compared with Faraday and Maxwell’s concept of the electromagnetic field, and was intended by Whitehead to overcome Russell’s concept of the material world in Chapter LIII of *The principles of mathematics* — a classical atomist picture in which bits of matter occupy points of space and instants of time — as well as all mechanistic ether models. All this explains why Whitehead, looking back in 1912 on the two decennia of research which cover both the individual *Universal algebra* project, and the joint *Principia mathematica* project, could write: “During the last twenty-two years I have been engaged in a large scheme of work, involving the logical scrutiny of mathematical symbolism and mathematical ideas. This work had its origins in the study of the mathematical theory of electromagnetism, and has always had as its ultimate aim the general scrutiny of the relations of matter and space” (Lowe, pp. 155–6).

By now it will be clear that Whitehead’s ultimate motivation does not coincide with Russell’s urge for mathematical certainty. Whitehead was striving for generalisation and unification. At the root of his enthusiasm, first for Grassmann’s algebra of extensions, then for Russell’s logic of relations, lay their potential to develop into unification-tools. The things Whitehead wanted to think together are, to start with, various mathematical and physical disciplines as diverse as non-metrical and metrical geometry, Euclidean and non-Euclidean geometry, the arithmetic of natural numbers and Cantor’s theory of the transfinite, hydrodynamics and electromagnetism. However, Whitehead’s ultimate scope includes all mathematical, physical, esthetical, ethical and religious experience.

So instead of accepting only two possible functions for one and the same logical apparatus — the Russellian reduction to atoms of certainty, and the Wittgensteinian purification of language — we should consider a third option: the Whiteheadian unification of experience. Instead of identifying Whitehead’s approach with Russell’s search for the indubitable logical
concepts and principles to which the entire mathematical universe can be reduced, we should re-evaluate Whitehead’s continual attempts to generalise mathematics in order for it to become a unifying science of all patterns of experience. Instead of conceiving Whitehead as a happy inhabitant of the early twentieth-century paradise for mathematicians, created by the vigour of Russell’s anti-idealism, we should listen to Russell’s statement that “it was Whitehead who was the serpent in this paradise of Mediterranean clarity” (Russell, p. 41).

3. Grattan-Guinness’ failure

The failure to recognise Whitehead as the serpent in Russell’s paradise prevents a proper understanding of Whitehead’s oeuvre, including the joint *Principia mathematica*. The failure to value both Whitehead and Russell as mutually influenced, but distinct, philosophers of mathematics can lead historians and philosophers of mathematics to misjudge both preface and introduction to the first edition of *Principia mathematica*, as well as Whitehead’s later writings on mathematics. For example, in “Mathematics in and behind Russell’s logicism” — his contribution to *The Cambridge companion to Bertrand Russell* — Ivor Grattan-Guinness writes on the *Principia mathematica*: “The book started poorly, with a short introduction in which the authors failed to state logicism explicitly” (Grattan-Guinness, p. 68). And in “Mathematics and philosophy” — a paper on the mathematics-chapter in Whitehead’s *Science and the modern world* — Grattan-Guinness arrives at the conclusion that “it is a disappointing chapter, both as history and philosophy” (Grattan-Guinness, p. 80). Only a failure to discern and appreciate Whitehead’s philosophy of generalisation and unification can lead to these negative judgements. I limit myself to a critique of the first of the two Grattan-Guinness statements.

Whitehead and Russell did not state logicism explicitly in the *Principia mathematica*, because, in a sense, the logicism explicit in Russell’s *The principles of mathematics* was dead by the time the first volume was published in 1910. Initially — ten years prior to this publication — Russell thought that the logicist reduction might add certainty to the arithmetical base of mathematics. In other words, Russell thought that, based on logic, he would “arrive at a perfected mathematics which would leave no room for doubt”, and that he would even be able “bit by bit to extend the sphere of certainty from mathematics to other sciences” (Russell, p. 28). However, soon, and increasingly, Russell came to appreciate that the system of logic — completed with extra-logical additions such as a theory of types in order
to prevent paradox, and an axiom of infinity in order to be able to generate all possible natural numbers — was subject to a greater degree of doubt than the system of arithmetic it intended to secure. By the time he lectured on the merits of the search for logical premises of mathematics before the Cambridge Mathematical Club in March 1907 he no longer held that this search might lead to an increase of certainty. Instead, Russell highlighted three other merits. The search for logical premises of mathematics leads 1. to “an organisation of our knowledge, making it more manageable and more interesting” (Russell,\textsuperscript{19} p. 282); 2. to “a number of new results” (p. 282), such as the deduction of “Cantor’s theory of the transfinite” (p. 275); and 3. to “a new philosophy” (p. 283), one which takes into account a new mathematical method, “The regressive method of discovering the premises of mathematics” (title), which “is substantially the same as the method of discovering general laws in any other science” (p. 274). Russell’s March 1907 lecture was all about generalisation and unification and, in my view, was a clear manifestation of the impact of Whitehead’s philosophy of mathematics on Russell. It is no coincidence 1. that one of the finest summaries of the \textit{Principia mathematica} was given by Whitehead in an essay called “The organisation of thought” (Whitehead\textsuperscript{20}); 2. that Cantor’s theories played a major role in the early Whitehead-Russell collaboration; and 3. that in \textit{Order and organism}, one of the few monographs dealing with Whitehead’s philosophy of mathematics, Murray Code — independently from Russell’s lecture — calls Whitehead’s quasi-empirical method of discovering the premises of mathematics “The method of retroduction” (Code,\textsuperscript{21} p. 29), where ‘retroduction’ — a synthesis of ‘induction’ and ‘deduction’ — clearly coincides with Russell’s ‘regression’.

In \textit{The search for mathematical roots: 1870-1940}, Grattan-Guinness did actually discuss Russell’s “The regressive method of discovering the premises of mathematics” (Grattan-Guinness,\textsuperscript{22} §7.7.2), but he failed to notice the Whiteheadian character of Russell’s lecture, and did not link it to the preface of \textit{Principia mathematica}. If Grattan-Guinness had better understood that there actually exists such a thing as Whitehead’s own philosophy of mathematics, that it is different from the one Russell made explicit in \textit{The principles of mathematics}, that it seriously impacted Russell, and that the 1910 preface of \textit{Principia mathematica} echoed — sometimes almost verbatim — passages from Russell’s March 1907 lecture, I doubt he would have written that the book started poorly.
4. Whitehead’s philosophical style

In 1902, in his essay “The study of mathematics”, Russell gave the following picture of what the early twentieth-century logicist ideal meant to him: “The discovery that all mathematics follows inevitably from a small collection of fundamental laws [...] comes with all the overwhelming force of a revolution; like a palace emerging from the autumn mist as the traveller ascends an Italian hill-side, the stately storeys of the mathematical edifice appear in their due order and proportion, with a new perfection in every part.” (Russell, p. 68.) This picture of Mediterranean clarity emerging from the mist has the power to seduce us into thinking that it applies to all logicians, and into overlooking the importance of the following statement in Russell’s *Portraits from memory*:

> It was Whitehead who was the serpent in this paradise of Mediterranean clarity. He said to me once: “You think the world is what it looks like in fine weather at noon day: I think it is what it seems like in the early morning when one first wakes from deep sleep”. I thought his remark horrid, but could not see how to prove that my bias was any better than his. (Russell, p. 41)

The last two quotes prove that differences of philosophical urge — Russell’s urge to reduce mathematics to simple atoms of certainty, Whitehead’s urge to generalize mathematics to a unified field of experience, and Wittgenstein’s urge to purify language to reach the mystical silence beyond what can be said — are closely linked to differences in philosophical style. Russell “wanted certainty in the kind of way in which people want religious fate” (Russell, p. 53). Hence, he could not stand the mist of uncertainty, and repeatedly converted to philosophical views which seemed to reveal sun-drenched truths. Within the realm of mathematics Platonism was such a view, and within the realm of physics atomism was. In the early twentieth century, Russell adhered to both these view. So we can appreciate why, in response to Whitehead’s gradual replacement of Platonic foundations with quasi-empirical principles, and of atomic discernment with ethereal relatedness, Russell called Whitehead “the serpent in this paradise of Mediterranean clarity”. Also, given Russell’s conviction that all mathematical propositions can be reduced to a small collection of logical laws, we can appreciate why, in response to Wittgenstein’s verdict that all logical laws are tautologies (Wittgenstein, §6.1), Russell wrote: “When I was young I hoped to find religious satisfaction [and] the eternal Platonic world gave me something non-human to admire. I thought of mathematics with rev-
ference, and suffered when Wittgenstein led me to regard it as nothing but tautologies” (Russell, p. 19).

Whitehead, contrary to Russell, did not want to exclude any experience because of the feeling of certainty entailed by the tentative truths arrived at. Hence, he did not hesitate to take into consideration the early-morning mist preceding the noonday sun, the awakening preceding the clear consciousness, the vague experiences preceding each feeling of certainty, the processes preceding all products, — including our linguistic, mathematical and logical propositions. Whereas Russell’s philosophical style is the style of the convert who saw the divine light, Whitehead’s style of thinking has correctly been called ‘process philosophy’. Introducing Russell to a Harvard audience in 1940, Whitehead humorously summarized the difference by saying: “Bertie thinks that I am muddleheaded; but I think that he is simpleminded” (Lucas, p. 109).

In January 1914 Russell received a letter from a recluse in Norway, advising him to change his philosophical style. It was a letter from Wittgenstein who — after a relatively limited exposure to Russell’s way of doing philosophy — had the nerve to give Russell some good advise. He wrote: “All best wishes for your lecture-course in America! Perhaps it will give you at any rate a more favourable opportunity than usual to tell them your thoughts and not just cut and dried results. THAT is what would be of the greatest imaginable value for your audience — to get to know the value of thought and not that of a cut and dried result” (Wittgenstein, p. 48). Even though Wittgenstein’s interaction with Russell had been limited compared to Whitehead’s, his remark is amazingly similar to what Whitehead told Russell on his own early-morning muddleheadedness and Russell’s noonday simplemindedness. Both Whitehead and Wittgenstein focus on the thought-process prior to the thought-product, on the activity preceding all propositions. However, whereas Whitehead does not exclude the tentative propositions of philosophy from the realms of meaning and value, Wittgenstein limits the value of philosophy to the purifying thought-process, and holds philosophical propositions to be as nonsensical as is the slimy trace left behind on the pavement by a moving snail, or, in his own words, as the ladder left behind after the climbing (Wittgenstein, §6.54).

This explains Wittgenstein’s aphoristic style. Indeed: Why would the snail construct a sedentary shell out of the mucus that merely facilitates the nomadic movements of the shell it bears? Why would Wittgenstein have built a philosophical system out of the *Tractatus* nonsense that only fueled his ascend to years of philosophical silence?
5. The parting of the ways

Russell’s awareness of the differences with regard to ultimate philosophical urge and dominant philosophical style, which separated him from Whitehead and Wittgenstein, only grew gradually. Given Whitehead’s interest and expertise in geometry, Russell wanted Whitehead to derive all of geometry from logical premises; this work was supposed to culminate into a fourth volume of *Principia mathematica*. Also, impressed with Wittgenstein’s genius, and with his critique on *Principia mathematica* approach, Russell wanted Wittgenstein to join the logicist research program, in order to rewrite the first part of the *Principia*. But when Whitehead and Wittgenstein left Cambridge — in 1910 the first moved to London in search for a more varied life; in 1913 the latter moved to Norway in search for a more solitary life — the philosophical divergences became manifest. Inspired by Einstein’s theories of relativity, and by the multifaceted critique of the bifurcation of nature into the abstract world of science and the concrete world of human experience, Whitehead turned the project of deducing pure geometry from logical premises into the project of constructing both a Minkowskian space-time geometry and a Maxwellian unified field theory from experiential events by means of the logic of relations. So the project to write a fourth volume of *Principia mathematica*, culminated in Whitehead’s first triad of books, consisting of *An enquiry concerning the principles of natural knowledge*,27 *The concept of nature*,28 and *The principle of relativity*.29 On the other hand, Wittgenstein’s critique of Russell’s theory of types led him to a theory of symbolism (Wittgenstein,26 p. 19), which subsequently matured into a Hertzian picture theory of language, not only fit to overcome Russell’s theory of types (Wittgenstein,7 §3.331-3.334), but also to separate saying from showing in general, and science from ethics. So the project to rewrite the first part of *Principia mathematica* culminated in Wittgenstein’s *Tractatus logico-philosophicus*.

This parting of the ways was a big disappointment to Russell, and it contributed to his feeling of “inner failure” which made his “mental life into a perpetual battle” (Russell,16 p. 56). I think of Russell’s failure as tragic, for it was the reverse side of his urge for certainty. It was Russell’s urge for certainty which prevented him to progress smoothly from his initial awareness of the divergences with Whitehead and Wittgenstein to a full understanding of their alternatives, and this failure strained his friendships with both Whitehead and Wittgenstein to the point of breaking. The story of Russell’s inability to fully appreciate what the *Tractatus* was all about, and of Wittgenstein’s subsequent reproaches, is well known. Less known is
the story of Whitehead’s critique of Russell’s inability to fully comprehend the novel approach that emerged from Whitehead’s writing of the fourth volume of *Principia mathematica*. While Whitehead was still in the process of developing his method of extensive abstraction, a logical method to abstract points of space and instants of time from the relatedness of experiential events, Russell — who was kept up to date by Whitehead on the volume four progress — published *Our knowledge of the external world* in 1914. In it, Russell gave “a rough preliminary account of the more precise results which he [Whitehead] is giving in the fourth volume of our *Principia mathematica*” (Russell, p. 11). So, *Our knowledge of the external world* contains the first published presentation of the method invented and employed by Whitehead, except for the tentative version in his 1905/1906 memoir, “On mathematical concepts of the material world”. However, Whitehead was displeased with Russell’s presentation, and when Russell asked for his volume four notes again in 1917, Whitehead refused to give them, because he did not want Russell to propagate another “incomplete misleading exposition”, and in his letter of refusal he added: “My ideas and methods grow in a different way to yours and the period of incubation is long and the result attains its intelligible form in the final stage, — I do not want you to have my notes which in chapters are lucid, to precipitate them into what I should consider as a series of half-truths”. Russell, who reproduced Whitehead’s letter of January 8, 1917, in his *Autobiography*, added the comment that this letter “put an end to our collaboration” (Russell, p. 78).

According to me, Russell could not stand the uncertainty of a long incubation process. He wanted immediate clarity to emerge from the vagueness of Whitehead’s philosophical struggle, and consequently hardened the soft notion of ‘experiential events’. His longing for atoms of certainty led him in *Our knowledge of the external world* to shortcut Whitehead’s research, and to replace Whitehead’s events with “the hardest of hard data” of the first sort, namely: “our own sense-data” (Russell, pp. 78–9). But Russell’s method of constructing space and time out of the external relatedness of internally isolated sense-data was no more than a half-truth compared to Whitehead’s ultimate method of extensive abstraction of space-time from the internal relatedness of experiential events. In fact, Whitehead’s process thinking was to lead to a completely new theory of perception, in which Russell’s cut and clear sense-data are non-fundamental results of a complex perception process, involving causal efficacy, presentational immediacy and symbolic reference.
Ultimately, according to Whitehead, Russell’s apparently foundational and isolated sense-data are items of finite knowledge emerging from the largely unconscious process of our interaction with the infinite universe, and he wrote:

The notion of the complete self-sufficiency of any item of finite knowledge is the fundamental error of dogmatism. Every such item derives its truth, and its very meaning, from its unanalyzed relevance to the background which is the unbounded Universe. [...] Every scrap of our knowledge derives its meaning from the fact that we are factors in the universe, and are dependent on the universe for every detail of our experience. [...] There is no entity which enjoys an isolated self-sufficiency of existence. (Whitehead, p. 101–2)

This brings me back to my initial claim on the importance of Whitehead’s writings for the philosophy of mathematical practices. Indeed, in the context of the last quote, Whitehead adds: “Not even the simplest notion of arithmetic escapes this inescapable condition for existence” (p. 101). Whitehead ultimately put Russell’s cut and dried “general truths of logic” — Russell’s “hardest of hard data” of the second sort (Russell, p. 78), the data out of which he wanted to construct all of arithmetic and all of pure mathematics — on a par with Russell’s sense-data and all other knowledge. Consequently, Russell’s apparently foundational and isolated bricks of the mathematical building also emerge from the largely unconscious process of our interaction with the universe. In this sense, Whitehead’s process thinking led him to the conclusion that “logic, conceived as an adequate analysis of [...] thought, is a fake” (Whitehead, p. 96). With this conclusion, Whitehead actually approached Poincaré’s standpoint in the famous Poincaré-Russell controversy on logicism. And we might add that with his rejection of Russell’s reduction to, and dualism of, sense data and logical truths, Whitehead also prefigured Quine’s critique of the two dogmas of logical positivism. But the most important lesson I want to draw in the context of this edited volume is the fact that Whitehead — like Wittgenstein — can be considered as a philosopher of mathematics who is essentially different from Russell, and who is worth studying in the context of the contemporary currents of the philosophy of mathematics, especially those which focus on mathematical practices.
6. Conclusion

The main theme of this paper was that Whitehead — like Wittgenstein — is different from Russell. I want to close this discussion with a Whitehead quote that I consider to be his ultimate warning against any Russellian quest for absolutely certain foundations:

The besetting sin of philosophers is that, being merely men, they endeavour to survey the universe from the standpoint of gods. There is a pretense at adequate clarity of fundamental ideas. We can never disengage our measure of clarity from a pragmatic sufficiency within occasions of ill-defined limitations. Clarity always means ‘clear enough’. (Whitehead, p. 123)

That Whitehead is worth comparing with contemporary philosophers of mathematics (humanists, structuralists, etc.), is a claim the substantiation of which needs several more papers. So let me make a shortcut, and end this one with a conjecture: Whitehead is the philosopher par excellence who fulfils all the essential criteria — as listed by Reuben Hersh in What is mathematics, really? (Hersh, ch. 2) — to identify a contemporary philosopher of mathematics.

Bibliography

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BRIDGING THEORIES WITH AXIOMS:
BOOLE, STONE, AND TARSKI

DIRK SCHLIMM

Department of Philosophy
McGill University, Montréal
* E-mail: dirk.schlimm@mcgill.ca
http://www.cs.mcgill.ca/~dirk

1. Introduction

In discussions of mathematical practice the role axiomatics has often been confined to providing the starting points for formal proofs, with little or no effect on the discovery or creation of new mathematics. For example, quite recently Patras wrote that the axiomatic method “never allows for authentic creation” (Patras,1 p. 159), and similar views have been popular with philosophers of science and mathematics throughout the twentieth century. Nevertheless, it is undeniable that axiomatic systems have played an essential role in a number of mathematical innovations, most famously in the discovery of non-Euclidean geometries. It was Euclid’s axiomatization of geometry that motivated the investigations of Bolyai and Lobachevsky, and the later construction of models by Beltrami and Klein.b Moreover, it was not only through the investigation and modification of given systems of axioms that new mathematical notions were introduced, but also by using axiomatic characterizations to express analogies and to discover new ones, as can be seen, for example, in the developments that led to the formation of algebraic structures, like groups and lattices.c In the present paper I would like to draw attention to a different use of axiomatics in mathematical practice, namely that of being a vehicle for bridging theories belonging to

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a See Schlimm2 for an overview of these discussions.
b See, e.g., Bonola3 and Gray.4
c See Schlimm,5,6 See also Schlimm7 for a more general discussion of the use of axiomatics for characterizing analogies.
previously unrelated areas. How axioms have been instrumental in linking mathematical theories is illustrated by the investigations of Boole, Stone, and Tarski, all of which revolve around the notion of Boolean algebra. An interesting aspect of Boole’s and Stone’s work is that it also shows how too heavy reliance on formal similarities between theories can lead to developments that, with hindsight, appear as detours.

2. Boole’s algebraization of logic

In the first half of the nineteenth century, through the work of Peacock, Gregory, De Morgan, and Hamilton an understanding of algebra had developed in Britain, according to which different meanings can be given to the symbols of algebra and where inferences should be independent from these interpretations. George Boole (1815–1864), who was personally acquainted with some of these authors and was certainly familiar with their work, developed a calculus of logic in *The mathematical analysis of logic* (1847), and presented essentially the same calculus in his more famous *An investigation of the laws of thought* (1854). In the earlier book the symbols of the calculus are interpreted primarily as choice operators (“elective symbols”) on a class of things, later as classes of things. Other interpretations that are discussed by Boole are propositions, probabilities, and a two-element domain containing only the numbers 0 and 1.

According to Boole, his investigations were motivated by the realization of a certain similarity between the use of conjunction and disjunction of concepts in everyday language and addition and multiplication of numbers. This similarity had been noted much earlier by Leibniz, but “he did not find it easy to formulate the resemblance precisely” (Kneale and Kneale, p. 404). Influenced by the aforementioned tradition in algebra, Boole employed algebraic formulas to articulate logical relationships, which allowed him to show how the analogy between logic and the algebra of numbers can be expressed by a set of common underlying formulas.

However, Boole was not only interested in expressing this analogy, but,

\[ A \text{ Boolean algebra} \langle A, \cup, \cap, 0, 1, \prime \rangle \text{ is a set } A \text{ of elements, which has two binary operations } (\cup \text{ and } \cap; \text{ sometimes they are also symbolized by } + \text{ and } \times) \text{ that are commutative, associative, and for which the absorption and distributive laws hold. It has unique zero and one elements, and one unary operation of complementarity. Examples of Boolean algebras are fields of sets with union and intersection, the binary truth values of sentential classical logic, and probability events. See Sikorski.}\]

\[ a \text{ See Peckhaus for an account of these developments.}\]

\[ b \text{ In the terminology introduced in Schlimm, these are mostly object-rich domains.}\]
together with an analysis of language, it also guided the construction of his
calculus, since he had “the desire to retain as much as possible of the nor-
mal algebraic formalism in his new calculus of logic” (Kneale and Kneale,12
p. 408). In a similar vein, Hailperin describes Boole as “hewing closely to
‘common’ algebra” in order to be able to use familiar techniques and proce-
dures (Hailperin,13 p. 77). This strategy is clearly reflected in Boole’s choice
of terminology for the logical operations and in the frequent references to
this analogy in his presentations. A passage on pp. 36–7 of Boole11 provides
a telling example. Here, Boole dramatically describes a situation in which
the analogy seems to break down. First, he argues that the inference from
\[ x = y \] to \[ zx = zy \] holds both in algebra and for classes, if the variables
are interpreted by classes and the multiplication symbol with intersection.
But, with regard to the reverse inference, i.e., from \[ zx = zy \] to \[ x = y \], he notes that “the analogy of the present system with that of algebra, as
commonly stated, appears to stop” (Boole,11 p. 36), since this inference is
not generally valid for classes, and explains: “In other words, the axiom of
the algebraists, that both sides of an equation may be divided by the same
quantity, has no formal equivalent here” (Boole,11 pp. 36–37). Luckily, fur-
ther inspection reveals that in the case of \( z = 0 \) this ‘axiom’ is violated
in algebra, too. Therefore, Boole can assert with relief that “the analogy
before exemplified remains at least unbroken” (Boole,11 p. 37).

Nonetheless, Boole did notice a genuine negative analogy: \( x = x^2 \) holds
if the variables are interpreted as classes, but not if they are interpreted by
natural numbers.\(^c\) In fact, this equality only holds for \( x = 0 \) and \( x = 1 \),
which leads Boole to consider a new universe of discourse that consists of
only the two numbers 0 and 1.\(^d\) He writes:

\[ \text{Hence, instead of determining the measure of formal agreement of} \]
\[ \text{the symbols of Logic and those of Number generally, it is more im-} \]
\[ \text{mediately suggested to us to compare them with symbols of quan-} \]
\[ \text{tity admitting only of the values 0 and 1.} \]

\(^c\) Later Boole says that ‘\( x = x^2 \)’ expresses “the fundamental law of thought” (Boole,11
p. 49).
\(^d\) In Boole,10 the symbols 0 and 1 were used to represent the empty class and the entire
universe of discourse.
of an Algebra of Logic. Difference of Interpretation will alone divide them. Upon this principle the method of the following work is established. (Boole,\textsuperscript{11} pp. 37–8, original emphasis)

Boole’s strong emphasis on the formal similarities between algebra and logic may also account for a peculiarity of his system, a “defect of elegance,” according to Kneale and Kneale\textsuperscript{12} (p. 408). What I am referring to is Boole’s restriction of $x + y$ (denoting the class that contains elements from either $x$ or $y$) to the case where $xy = 0$, which expresses that the intersection of $x$ and $y$ is empty.\textsuperscript{6} As a consequence, the equation $x + x = x$ is barred from Boole’s system, which prevents the possibility of formulating De Morgan’s rules of distributivity, which express the duality of addition and multiplication, or union and intersection, respectively. Kneale and Kneale speculate that Boole’s interpretation of $+$ was linked to his desire to be able to derive $x - y = z$ from $x = y + z$, since this inference is only valid if $yz = 0$. Another possible reason for Boole’s choice, however, is based on the analogy between the rules of algebra and logic. Admitting the symbol $x + y$ without Boole’s restriction would also make ‘$1 + 1$’ a meaningful term. The modern reader might have no objection to admitting $1 + 1 = 1$ in the system, but this clearly constitutes another negative analogy to the common algebra of natural numbers.\textsuperscript{7} As such, it would certainly be undesirable from Boole’s point of view. Later logicians, like Jevons, Peirce, and Schröder, valued the duality of addition and multiplication higher than the analogy to algebra, and thus “corrected” Boole’s system (only Venn sided with Boole on this issue).\textsuperscript{8} In fact, it is leaving Boole’s “close analogy to mathematical notation” that Peckhaus considers as one of the characteristics of Jevons’ logic, which he regards as “a considerable step forward in Boolean logic” (Peckhaus,\textsuperscript{17} p. 279).

To summarize, the analogy between algebra and logic that is explicated by common formulas proved to be a powerful motivation for Boole’s formulation of a calculus of classes and his introduction of a new suitable model.

\textsuperscript{6} Note that this is not equivalent to interpreting $x + y$ as disjoint union, since the latter has a definite meaning also when the intersection of $x$ and $y$ is not empty. Exceptions that render expressions meaningless are not unknown in mathematics, e.g., in ordinary algebra the term ‘$x/y$’ is only meaningful if $y \neq 0$.

\textsuperscript{7} For a discussion of various notions of disjunctions and their relation to natural language, see Jennings,\textsuperscript{14} in particular pp. 70–7, where Boole’s approach is discussed.

\textsuperscript{8} See, e.g., Schröder,\textsuperscript{15} I, p. 263. We see here how mathematicians can make different choices regarding the development of theories that depend upon their general views on the nature and the aims of mathematics, or, in other words, on their ‘image’ of mathematics (Corry\textsuperscript{16}).
for it. However, clinging too strongly to this analogy prevented him from developing a system that satisfied other general desiderata, like those of symmetry and duality.

3. Stone’s unification of algebra and topology

In connection with the preparation of his book on *Linear transformations in Hilbert Space* (1932), the young American mathematician Marshall Stone (1903–1989) developed an interest in Boolean algebras. In his first announcement and summary of his results, Stone reports that he perceived an analogy between Boolean algebras, as presented by the axiomatizations of Huntington and others, and the theory of rings, as presented in the newly published textbook *Moderne algebra* by van der Waerden. In the latter, rings are defined axiomatically as systems of double composition with the operations of addition (associative and commutative) and multiplication (associative and both left- and right-distributive with respect to addition). Furthermore, rings have unique additive inverses and a unique neutral element 0. On the basis of the formal similarity between the definitions of Boolean algebras and rings, Stone was able to transfer the central notions of ideals and homomorphisms from the theory of rings to the theory of Boolean algebras (Stone, 1935), wherein he explains that he chose the postulates

with the intention of emphasizing the analogy between Boolean algebras and abstract rings, the latter being systems which have

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a See Mehrtens, pp. 260–1.
b See Huntington and Van der Waerden (Ch. 3).
c “By a system of double composition we shall mean a set of elements in which, for any two elements 0, a, b, . . ., a sum a + b and a product a · b belonging to the set are uniquely defined” (Van der Waerden, p. 37; quoted from the translation of the second edition, p. 32).
d Alternatively, a ring \( (A, +, \times, 0) \) may be characterized as consisting of an additive group \( (A, +) \) with neutral element 0 and a monoid (like a group, but without inverses) \( (A, \times) \), that are connected by distributive laws. To guarantee the existence of additive inverses Stone postulates that the equation \( x + a = b \) has a unique solution in \( A \) for arbitrary elements \( a \) and \( b \) of the ring (Stone, 1935).
already undergone extensive analysis. From this point of view our postulates appear to be as satisfactory as possible, so long as logical addition and multiplication are to be treated as the analogues of ring addition and multiplication. (Stone, p. 703)

In this context Stone mentions three principles that guided his quest for axioms:

in the first place, they shall embody known properties of the operations of forming finite unions and intersections of classes [i.e., one of the most important instances of Boolean algebras]; in the second place, they shall embody as far as possible only such properties as are valid of the operations of addition and multiplication in a ring; and, in the third place, they shall be independent. (Stone, p. 704)

It seems that his first principle put him on a wrong track initially. Since, in general classes do not have inverses with respect to union and to the empty class, which are the straightforward candidates for being interpretations of addition and zero in a ring. As Stone points out, for classes $a$ and $b$, and union $\cup$, the equation $x \cup a = b$ has only solutions for $x$, if $a$ is contained in $b$, and the solution is unique only if the intersection of $x$ and $a$ is empty. Thus, under this interpretation, the algebra of classes is not a ring and so Stone calls its relation to rings only one of analogy. However, in the course of his investigations Stone made a surprising discovery:

We have recently observed, however, that Boolean algebras can be regarded as rings of special type when the operation of forming the symmetric consequence is taken as ring addition. In consequence the most natural approach to a mathematical theory of Boolean algebras is not one based upon material in this paper. (Stone, p. 703)

In other words, rather than correlating ring addition with union (i.e., addition in Boolean algebras) and ring multiplication with intersection (i.e., multiplication in Boolean algebras), which violates the ring axiom requiring additive inverses, Stone noticed that all the ring axioms can be satisfied if ring addition is interpreted by a different operation on classes,

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Note, that Stone’s notion of ‘analogy’ is weaker than that employed by Boole.

\[\text{We have here an example for the fact that a mathematician’s understanding of ‘natural’ is not always the same as ‘being obvious’ or ‘immediate’}.\]
namely that of symmetric difference. With this new insight he went on to rewrite the paper on the connection between Boolean algebra and topology that he was also working on at the time. In a summary that was published earlier than the full-length paper, one reads, after a short reference to the analogy between Boolean algebras and abstract rings, the following:

In the present note we shall show that Boolean algebras are actually special instances, rather than analogs, of the general algebraic systems known as rings. In establishing this result we must choose the fundamental operations in a Boolean algebra in an appropriate manner, indicated below. The algebraic theory developed in the previous communication is thus a particular instance of the theory of rings and can be deduced in part from known theorems concerning rings. These facts were discovered just as the detailed exposition of our independent theory of Boolean algebras was on the point of completion; and they now compel a radical revision which will necessarily delay publication of the complete theory. (Stone, p. 103)

In the subsequently published paper on “The theory of representation for Boolean algebras” (1936), Stone shows that any Boolean algebra is isomorphic to, i.e., can be represented by, a field of sets, and he motivates his algebraic approach to logic by the fact that it allows to connect many different areas of mathematics:

Indeed, if one reflects upon various algebraic phenomena occurring in group theory, in ideal theory, and even in analysis, one is easily convinced that a systematic investigation of Boolean algebras, together with still more general systems, is probably essential to further progress in these theories. […] The writer’s interest in the subject, for example, arose in connection with the spectral theory of symmetric transformations in Hilbert space and certain related properties of abstract integrals. In the actual development of the proposed theory of Boolean algebras, there emerged some extremely close connections with general topology which led at once

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8 The symmetric difference of two classes is defined as $(a \cap b)' \cup (a' \cap b')$. It contains all elements that are either in $a$ or in $b$, but not in both.

9 A field of sets is a family of sets that are closed under finite union and intersection, whose elements are closed under complements. On the importance of this theorem, Stone remarks: “Such a result is a precise analogue of the theorem that every abstract group is represented by an isomorphic group of permutations” (Stone, p. 38).
to results of sufficient importance to confirm our a priori views of
the probable value of such a theory. (Stone,\textsuperscript{22} p. 37)

Stone goes on to prove that Huntington’s axiomatization of Boolean alge-
bras is equivalent (i.e., mutually interpretable) with the axiomatization of
commutative rings with unit element in which every element is idempotent
(called \textit{Boolean rings}), i.e., that “Boolean algebras are identical with those
rings” (Stone,\textsuperscript{22} p. 38). According to Stone, these results reveal

the essential nature of all Boolean rings. In particular, it shows

that the operation of addition in a Boolean ring corresponds ab-
stracly to the operation of forming the symmetric difference or
union (modulo 2) of classes, as indicated by the relation (6); and
it shows similarly that the operation of multiplication corresponds
to the operation of forming the intersection of classes, as indicated
in the relation (7).\textsuperscript{1} (Stone,\textsuperscript{22} p. 44)

It is clear that Stone’s aim for these investigations is part of a larger
research project. He wants to apply the mathematical machinery developed
in the theory of rings to other domains:

What is of primary importance here is the identification of the
abstract algebras arising from logic and the theory of classes with
systems amenable to the methods developed by modern algebraists,
namely, with those special rings which we have termed Boolean
rings […] (Stone,\textsuperscript{22} p. 43)

In a further paper Stone was able to connect the theory of Boolean rings
also to topology by proving that “the theory of Boolean rings is mathemat-
ically equivalent to the theory of locally-bicompact totally-disconnected
topological spaces” (Stone,\textsuperscript{26} p. 375).\textsuperscript{1} Stone called the latter \textit{Boolean
spaces}, but they have subsequently become known as \textit{Stone spaces} in his
honor. This identification, also referred to as “the fundamental represen-
tation theorem” and “the basic theorem for the whole theory of Boolean
algebras” (Stone,\textsuperscript{8} p. 158), informs us about the models of the axioms and it
allows for the transfer of topological techniques and results to the study of

\textsuperscript{1} The relations alluded to in the quotation are the definitions of the ring operations +
and \times in terms of the primitives $\cup$ and $'$ of Boolean algebras (Stone uses $\lor$ for $\cup$):
\begin{align*}
(6) \quad a + b = ab' \cup a'b = (a' \cup b')' \cup (a' \cup b)'', \quad (7) \quad ab = (a' \cup b').\end{align*}

\textsuperscript{1} See also Stone\textsuperscript{23} (p. 198). In modern terminology, these spaces are compact, totally
disconnected Hausdorff spaces.
Boolean algebras, and vice versa. It has become known as the *Stone duality* and it has been extremely fruitful in later developments in logic, topology, algebra, and algebraic geometry.

Grosholz\textsuperscript{27,28} has discussed this duality as an example of the growth of mathematical knowledge through the unification of related fields by exploiting structural analogies between them. The above presentation highlights that these structural analogies were investigated by Stone on the basis of axiomatic characterizations of the domains in question.

\section{4. Tarski’s calculus of deductive systems}

In the 1920s Alfred Tarski (1902–1983) studied logic and set theory in Warsaw and developed a meta-mathematical approach whose main ideas are presented in Tarski.\textsuperscript{29,30} Herein he considers formal mathematical theories as sets of sentences with a single primitive relation, namely that of being the set of consequences, \( Cn(X) \), of a set \( X \) of sentences, which is defined axiomatically. A set of sentences, belonging to the set \( S \) of all sentences, that is closed under the relation of consequence is called a *deductive system*, and two such systems are said to be *equivalent* if they have the same set of consequences. These and other meta-mathematical notions that Tarski defines are developed into a “calculus of systems” in Tarski,\textsuperscript{31,32} where negation, implication, and the set of all logically valid sentences are taken as primitives and the consequence relation is defined in terms of them. In particular, Tarski defines the *logical sum* \( X \dot{+} Y \) of two deductive systems \( X \) and \( Y \) as \( Cn(X + Y) \).\textsuperscript{a} This allows him to show that the deductive systems satisfy the axioms of Boolean algebra, except that \( X \dot{+} X = S \) does not hold in general for deductive systems. Interpreted in the calculus of sentential logic this equation corresponds to the law of excluded middle, which holds in classical, but not in intuitionistic logic. This caught Tarski’s attention, and he notes that “[t]he formal resemblance of the calculus of systems to the intuitionistic sentential calculus of Heyting is striking” (Tarski,\textsuperscript{31} p. 352). The *axiomatizable systems*, i.e., those sets of sentences for which there is a finite system of axioms, satisfy all axioms for Boolean algebra, as Tarski shows, and he is also able to prove that the axiomatizable systems are in fact the only ones that satisfy \( X \dot{+} X = S \) (Tarski,\textsuperscript{31} p. 356).

Around this time Tarski also studied axiomatizations of Boolean algebra\textsuperscript{13} and, possibly influenced by the results of Stone, was also led to

\textsuperscript{a} In Tarski’s notation, + stands for set-theoretic union, which was symbolized above by \( \cup \), and \( \overline{X} \) denotes the complement of \( X \), symbolized by \( X' \), above.
consider fields of sets as a different model of the axioms of Boolean algebra. Tarski gives the following account of the development of his own theory:

The speaker has recently developed a theory of deductive systems. Considered formally, this theory forms an interpretation of so-called Boolean algebra. [...] With respect to this it became possible to first extend the concepts and results of the theory of deductive systems to general Boolean algebra, and then to a different interpretation of this algebra, namely the theory of fields of sets [...] In doing so, it turned out that there is an exact correspondence between the main concepts of the theory of deductive systems and those concepts that are transferred to the theory of Boolean fields from general abstract algebra [Reference to Stone23]: the deductive systems are coextensive with ideals, axiomatizable systems with principal ideals, complete systems with prime ideals, etc. The calculus of deductive systems is thus changed into a general calculus of ideals. (Tarski,34 p. 186; translated from Mehrtens,19 p. 270)

Thus, we are presented here with a clear example of the transfer of notions and methods between one domain (deductive systems) and another (ideal theory) that is mediated by a common system of axioms (for Boolean algebra). In other words, the axiomatization of Boolean algebra, together with the realization of different models thereof, is what allowed Tarski to relate his theory of deductive systems to the theory of ideals. Comparing the main concepts of both theories, Tarski noticed that they can be put into correspondence, such that the results from ideal theory can be carried over the axiomatic bridge to the theory of deductive systems.

Once such a connection has been established, it is straightforward to transfer results produced in one theory to the other. For example, Tarski’s student Mostowski proved that the set of prime ideals of a field is either countable or has the cardinality of the continuum, and, due to the correspondence between the prime ideals in a field and the complete deductive systems of a theory, he could translate this result into a result about deductive systems: “The cardinality of the set of complete systems of an arbitrary deductive theory is either ≤ ℵ₀ or 2ℵ₀” (Stone,35 p. 46).

Mostowski worked out in detail the connection between the calculus of deductive systems and the theory of rings, and, following the work of Stone,

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b See footnote h, Section 3.
extended it to topology. This made it also possible to use topological considerations for finding solutions to certain problems formulated by Tarski for his calculus of systems. The advantage of this way of proceeding is that the topological arguments were easier and shorter than the direct proofs, which Mostowski had formulated earlier. He reports:

The entire ballast of my earlier proofs is removed in the present formulation and, for the reader who is familiar with the elements of topology, the considerations presented here will have the character of immediate consequences of the investigations carried out by Stone. My reflections should therefore not be understood as genuine discoveries, but rather as vivid descriptions of the interesting and completely unexpected connection that exists between so seemingly distant areas like meta-mathematics and topology. (Mostowski, 35 p. 35)

After learning of Tarski’s work, Stone also immediately realized the intimate relation between the theory of deductive systems and topology. Since the more direct approaches to the study of deductive systems had not been satisfactory, Stone even went so far as to suggest that “the profounder aspects of the theory of deductive systems must be studied by general methods of topology” (Stone, 36 p. 225). Again, we find that the path that was opened up by axiomatics proved to be an extremely fruitful one.

5. Concluding remarks

The above presentation of three episodes from the history of mathematics could touch only upon a few aspects of the development of theories of Boole, Stone, and Tarski, obviously leaving aside a wide variety of other conceptual innovations and many technical details. The goal of this discussion was to bring to the fore the importance of the fact that the theories under investigation were presented axiomatically, and that this was indeed a key factor which allowed the researchers to draw connections between theories that were previously regarded as unrelated. Thus, axiomatization was neither just the conclusion of a previous development, nor did it discourage or suppress creative thinking, as was suggested by Felix Klein37 (pp. 335–6). On the contrary, the connections that Boole, Stone, and Tarski opened up allowed for the transfer of results and methods across different mathematical theories and thereby engendered many successful further developments.

The present paper supports Grosholz’s discussion of the unification of logic and topology based on “the hypothesis of a partial structural analogy”
(Grosholz, p. 147) and her conclusion that the “correlation of mathematical structures leads to the expansion of mathematical knowledge precisely because it is initially unfounded, risky and corrigible” (Grosholz, p. 152). In addition, it supplements this discussion by emphasizing the role of axioms in the discovery of the analogies in the first place. As we have seen in the cases of Boole and Stone, the initial attempts were indeed in need of correction and their initial hypotheses arose from careful considerations that took the axiomatic presentations of the domains in question as their starting points. The mathematical domains of algebra, Boolean algebra, ring theory, etc., were presented axiomatically and the formal similarities between the axiomatizations were the basis for Boole’s, Stone’s, and Tarski’s advancements. Of course, I do not claim that all analogies in mathematics are discovered in this manner, but that, in the practice of mathematics, some have been found in this way. Thus, axiomatics can lead to a clarification of perceived analogies and to the discovery of new ones, and so it can be — and has been — used as a methodological tool at the service of mathematical creativity.

By being a vehicle for bridging mathematical theories, as illustrated in the case studies presented above, axioms can play an important part in the innovation of mathematics and thus contribute to the growth of mathematical knowledge in a way that goes far beyond the deductive generation of new theorems. As a consequence, any view that confines the role of axiomatics to what has been called the ‘context of justification’ does not do justice to their actual use in mathematical practice.

Bibliography


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