The Origin of Symbolic Mathematics and the End of the Science of Quantity

Uppsala Philosophical Studies
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ISSN 1402-1110
ISBN 978-91-506-2388-8

Printed in Sweden by Elanders Sverige AB 2014
Distributor: Department of Philosophy, Uppsala University

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Preface

The main topic of this essay is symbolic mathematics, or the method of symbolic construction. The symbolic conception of mathematics is not new; we have had it since the end of the sixteenth century when Franciscus Vieta invented the algebraic symbolism and started to use the word 'symbolic' in the relevant, non-ontological sense. The symbolic approach in mathematics has played an important rôle for many of the great inventions in modern mathematics such as, for instance, the introduction of the decimal place-value system of numeration, Descartes' analytic geometry and Leibniz's infinitesimal calculus. The symbolic view of mathematics was also of great importance in the rigorization movement in mathematics in the late nineteenth century, as well as in the mathematics of modern physics in the 20th century.

The nature of symbolic mathematics has been concealed, and confused, however, by the strong influence of the heritage from the Euclidean and Aristotelian traditions, especially in the areas of the philosophy of mathematics and the foundations of mathematics. The purpose of this essay is to shed some light on what has been concealed, by approaching some of the crucial issues from a historical perspective.

I am indebted to Aki Kanamori, Pär Segerdahl, Kim Solin and Anders Öberg for helpful comments on an earlier version of this essay. Special thanks to Pär Segerdahl who helped me to improve the final version of the manuscript.

1 Introduction: history and heritage

The rise of symbolic mathematics in the 17th century was not just a more or less continuous course of development, or an extension, of ancient Greek and medieval mathematical thought. As the philosopher Jacob Klein has argued, it was a *transformation*, connected with the extensive cultural change that took place in the beginning of modern times. Many essential features of Greek mathematical thought came to an end through this transformation and do therefore no longer belong to our mathematical heritage. New forms of mathematical thought were created through this transformation, especially due to the increased use of algebraic symbolism.

The normal interest in history of mathematics (among mathematicians who write history of mathematics) is interest in our mathematical *heritage.* This interest therefore tends to be conditioned by the contemporary situation and is not always an interest in what *actually happened* in mathematics of the past regardless of the contemporary situation. Only history in the latter sense deserves to be called *history*.[1] But history and heritage are often confused and one consequence of this kind of confusion is that the transformation of mathematics at the beginning of modern times is concealed. Features of modern mathematics are projected upon mathematics of the past, and the deep contrasts between ancient and modern mathematics are concealed. As a consequence, the nature of modern mathematics as symbolic mathematics is not understood as the *new* beginning of mathematics that it was.

By taking a closer look at some examples from the history of mathematics it will be argued that what actually happened in mathematics in the past supports the view that the essence of modern mathematics is symbolic mathematics.

[1] On the difference between history and heritage, see Grattan-Guiness (2004).

2 Are there proofs by mathematical induction in Euclid's *Elements*?

There is quite strong disagreement among historians of mathematics about the origin of proof by mathematical induction.[2] This disagreement is connected with the tendency to confuse history and heritage. Was there ever a proof by mathematical induction in ancient Greek mathematics? In Euclid's *Elements*, for instance? Historians of mathematics with a heritage view tend to answer these questions affirmatively.

Sabatai Unguru (1991, 1994) denies this categorically, on the ground that it is incompatible with the ancient Greek concept of number, according to which numbers are always determinate *numbers of things* as the units of a multitude. The multitudes are not seen as having, in general, in themselves or between themselves, a serial structure as being generated by a successor operation. A multitude always has, at least potentially, a manifestation as a multitude of independently given discrete things. In the arithmetical proofs in Euclid's *Elements* numbers are always represented as lengths of line-segments determined by their end-points.

The mathematician Hans Freudenthal is one of those who have defended the opposite view about the origin of mathematical induction. In his article *Zur Geschichte der vollständigen Induktion*,[3] he claims that the propositions IX, 8; IX, 9 and IX, 20 of Euclid's *Elements* (of which the last one is the proposition about the infinity of the prime numbers) are all proved by mathematical induction. The mathematician Paul Ernst (1982) agrees with Freudenthal, and in particular that the proof that there are infinitely many prime numbers uses mathematical induction. Let us examine Ernst's argument in some detail. Ernst asserts categorically:

> Mathematical induction is implicit in some of Euclid's proofs, for example, in the proof that there exist infinitely many primes. This theorem is Proposition 20 in Book IX of Euclid, and translated it states
>
> "Prime numbers are more than any assigned multitude of prime numbers."

[2] See the articles Bussey (1917), Cajori (1918), Freudenthal (1953), Rabinovitch (1970), Fowler (1994), Unguru (1991, 1994).
[3] Freudenthal, 1953.

> Euclid proves the proposition by letting A, B, C be the assigned prime numbers, and showing that ABC + 1 has a new prime factor G. The conclusion of the proof reads:
>
> "Therefore the prime numbers A, B, C, G have been found which are more than the assigned multitude of A, B, C. Q.E.D."

Ernst claims, however, that there is a gap in the proof. Something is missing between the last sentence in the proof (which concerns the particular case of the three assigned prime numbers A, B and C) and the generality of the stated proposition which concerns *any* assigned multitude of prime numbers. According to Ernst,

> This gap is bridged by the principle of mathematical induction. What Euclid proves, in fact, is that the existence of three primes implies the existence of four primes. The method of the proof is general, and can be used to prove the existence of $n + 1$ primes from n primes. However, Euclid lacks the algebraic language necessary for this more general induction step, and instead represents it by a particular case.

As pointed out by Mueller (1981), there is no indication that the multitudes in Euclid's *Elements* were conceived as having a serial order, as being generated by something like a successor operation,

$$n+1.$$[4]

But is Euclid's proof really a particular case of an "induction step" if no serial order is presupposed?

Quite contrary to what Ernst is suggesting, Euclid never writes ABC + 1, for instance. He did not have a sign for the *operation* of addition (and he avoided multiplying numbers). He argues geometrically by extending the line-segment ABC by the unit line-segment. *This is not just a difference of notation*. It means that Euclid didn't have the notion of $n+1$ as an arithmetical *operation,* the successor operation as a *symbol*, which is a prerequisite for mathematical induction. A multitude, for Euclid, is just an aggregate of things as units, and a number is the number of the units of a multitude.

Ernst is reading Euclid's proof as if the generality of the proof comes from thinking of the assigned multitude of prime numbers A, B, C as an arbitrary sequence of prime numbers

$$P_1, P_2, \ldots, P_n$$

[4] Müller (1981).

that is generated recursively by the method suggested in the proof. On the basis of this reading Ernst can say that "The method of the proof is general, and can be used to prove the existence of $n + 1$ primes from n primes. But that was hardly Euclid's way of thinking about it. Euclid was thinking about it in geometric terms, where numbers are line-segments.

Let us follow Ernst in 'modernizing' Euclid's proof (*Elements*, book IX, § 20) by letting $P_1, P_2, \ldots, P_n$ be the assigned prime numbers. We shall prove that there are more prime numbers than these.

> Consider the number $P = (P_1 \times P_2 \times_{\ldots} P_n) + 1$.
>
> P is either a prime number or not.
>
> If P is prime the proof is complete since P is then a prime number different from all the assigned prime numbers $P_1, P_2, \ldots, P_n$.
>
> If P is not a prime number it must have a prime factor Q (by theorem VII, § 31 of the *Elements*). And this prime number must be different from all the prime numbers $P_1, P_2, \ldots, P_n$ because otherwise it would be a factor of the unit, which is absurd.
>
> Therefore, Q is not one of the numbers $P_1, P_2, \ldots, P_n$ and by hypothesis it is prime. So the prime numbers $P_1, P_2, \ldots, P_n, Q$ have been found which are more than the assigned multitude of prime numbers $P_1, P_2, \ldots, P_n$.
>
> Therefore, *prime numbers are more than any assigned multitude of prime numbers*.
>
> Q.E.D.

The generality expressed by the variable n in this proof is not the generality of a mathematical induction. There is no induction (even implicitly) in this modernized version of Euclid's proof any more then in Euclid's original proof. The generality of the variable n only comes from the fact that the procedure of the proof is *independent of the specific number of primes chosen as the assigned multitude of prime numbers*.

If we would replace the expression $P_1, P_2, \ldots, P_n$ everywhere in this proof with Euclid's expression *A, B, C* we would have Euclid's original proof (except that we have not written out the geometrical representation of the numbers, and we have defined the number P by first multiplying the given prime numbers $P_1, \ldots, P_n$, which Euclid does not do).[5] This shows that Euclid's

[5] Euclid considers numbers as length of line-segments and thus as being 'one-dimensional'. He therefore avoids multiplication. Instead he uses Proposition 36 of Book VII according to which "There is a least number which three given numbers measure", which is proved without using multiplication of numbers.

assigned multitude of primes *A, B, C*, has the role of an *arbitrary multitude of prime numbers* in the original proof. One might even say that Euclid is taking the number 3 as a number variable. To understand Euclid's proof is also to understand that the assigned multitude of primes A, B, C can be replaced, for instance, by the multitude of five primes A, B, C, D, E, and then'3' is replaced by '5', but the proof procedure would still be the same.

Euclides' proof of theorem IX, 20, about the infinity of the prime numbers is not the only proof in the *Elements* where a general proposition about numbers is illustrated by a proof that deals with a specific finite multitude of numbers in which you are expected to see that the proof does not depend on the specific numbers chosen. Illustrating a general case in this way by dealing with a particular case, is rather a common procedure in Euclid's *Elements* in general (not least in the geometrical theorems, where a specific drawn figure, a line-segment, a triangle or a circle, is used to prove general results about line segments, triangles and circles). The proposition IX, 10, for instance, begins as follows: "If as many numbers as we please beginning from a unit are in continued proportion, and the number after the unit is square, then all the rest are also square;" Euclid begins the proof by saying: "Let there be as many numbers as we please, A, B, C, D, E, and F, beginning from a unit and in continued proportion, and let A, the number after the unit, be square." Euclid is here dealing with the *particular case* of six numbers, with the required properties. What the phrase 'as many as we please' signifies is that you are supposed to see that the choice of a sequence of *six* numbers and no more (with the required properties) is not essential in the proof. And to see this *is not an additional inferential step in the proof*; it is to see the proof as a proof of the general result stated in the theorem.

From an Aristotelian point of view one might say that there is nevertheless a sense in which 'induction' is involved in this procedure of getting to see something general by dealing with particular cases, *but it is not mathematical induction,* and neither is it induction of empirical science. It is rather induction in the sense of Aristoteles' *epagoge*. Engberg-Petersen (1979) argues convincingly that Aristotle had a unified conception of *epagoge,* which *is not a type of inference*. It is rather the exercise of the generalizing ability which Aristotle calls *nous*, a sort of *intuition*. Avoiding the word 'conclusion' which would suggest a type of inference, Engberg-Petersen expresses the basic idea of this conception of *epagoge* as "attending to particular cases with the consequence that insight into some universal point is acquired" or, alternatively, "acquiring insight into some universal point as a consequence of attending to particular cases".[6]

This use of *epagoge* to see something general by attending to particular cases is not an inference, but rather a kind of non-demonstrative procedure which is a routine procedure in mathematical reasoning, such as for instance

[6] Engberg-Petersen (1979, p. 305).

in geometry when a specific, concrete triangle ABC is used as an "arbitrary triangle" in a proof of a theorem about triangles in general. So there is no gap in Euclid's proof in the sense that an inferential step is left out when he proceeds from particular cases to the universal point at the end of the proof of the infinity of prime numbers.

Did Euclid possess Aristotle's general notion of *epagoge*? It seems likely that Euclid as well as Aristotle learned this procedure of "acquiring insight into some universal point as a consequence of attending to particular cases" from mathematics (perhaps in their education at Plato's Academy). It seems likely that Aristotle's *epagoge* was then the result of extending the applicability of this procedure for getting insight into the 'universal in the particular' to topics outside mathematics. But why should the mathematician Euclid be interested in this general notion of *epagoge*? Well, Euclid was influenced by several features of Aristotle's philosophical doctrine (though not the theory of syllogisms). Euclid employs Aristotle's theory of statements with its distinction between axioms and postulates, and he follows Aristotle's dichotomy between discrete and continuous quantity, which persisted in mathematics up to the days of Descartes.

It is tempting to say that Euclid was a mathematician, not a philosopher, but the relationship between philosophy and mathematics was more complicated in ancient Greece. They did not have our sharp divide between philosophy and mathematics as two different and independent disciplines and professions.

Euclid's *Elements* is an exposition of the logical order of the fundamentals of elementary mathematics. So there is no doubt a logical-philosophical context present in the *Elements.* But that is not the only relevant context of the presentation in the *Elements.* The presentation has also pedagogical aims. The way in which *epagoge* may be present in the exposition is also influenced by the rhetorical-dialectical situation, where its aim is to get the readers and students of the *Elements* to accept the general points. It is not unlikely that Euclid sometimes says as much as is needed to achieve that aim. One reading of the basic idea of *epagoge,* discussed by Engberg-Petersen that seems to be most appropriate here is the following:

> leading another person, by pointing to particular cases, towards something *katholou* [universal] with the aim and consequence that he acquires insight into it.[7]

Let us finally stress that to think about a multitude of prime numbers as an arbitrary finite sequence of primes, $P_1, P_2, \ldots, P_n,$ is to situate it in an operational context that was foreign to Euclid. It is to take for granted how we operate with this symbol in the symbolic practice where we have access to

[7] Enberg-Petersen (1979, p. 301).

the *number-variable n* to express the form of a finite sequence. As stressed by Klein (1968) and Unguru (1975), the use of a letter as a number-variable in this sense was foreign to ancient Greek mathematics. Alphabetic letters were used for numbers, but always for determinate numbers. Here we have one of the most essential features of the difference between the modern symbolic concept of number and the concept of number in ancient Greek mathematics. The symbolic concept is essentially connected with the use of a number-variable to express *the form of the result of performing an arithmetical operation*. Without this use of a number-variable, the precise sense of the notion of an arithmetical *operation* is lost. The generality expressed by the variable *n* in a "proof from *n* to *n*+1", which is the generality of a recursive pattern or rule, is the specific generality of mathematical induction and is bound up with the symbolic concept of number.

3 Jacob Klein and the origin of symbolic mathematics

The disagreement about the origin of proof by mathematical induction is only a particular case of a more general historiographical debate, or quarrel, about the history of ancient Greek mathematics that started with Sabatai Unguru's article *On the Need to Rewrite the History of Greek Mathematics*, published in 1975.[8] Unguru claims that lack of historical sense is a common feature of most mathematicians' readings of ancient mathematical texts in that they tend to read the ancient texts only *from the point of view of modern mathematics*. Unguru writes: "to read ancient mathematical texts with modern mathematics in mind is the safest method for misunderstanding the character of ancient mathematics -…" The historical approach, according to Unguru, is an approach which involves interpretation. It cannot "divorce itself from the attempt to unravel the original intentions of the text's author," which means that the interpreter has to be sensitive to the historico-cultural context.

Jacob Klein's book *Greek Mathematical Thought and the Origin of Algebra*[9] is an important but rather neglected work in the history and philosophy of mathematics. Klein's historical approach anticipates Unguru's main point about the necessity to unravel the original intentions of the authors of classical Greek mathematical texts. Klein writes in the beginning of his book:

> […] most of the standard histories attempt to grasp Greek mathematics itself with the aid of modern symbolism, as if the latter were an altogether external "form" which may be tailored to any desirable "content". And even in the case of investigations intent upon a genuine understanding of Greek science, one finds that the enquiry starts out from a conceptual level which is, from the very beginning, and precisely with respect to the fundamental concepts, determined by modern modes of thought. To disengage ourselves as far as possible from these modes must be the first concern of our enterprise.[10]

[8] See Kastanis, N. and Thomaidis "The term 'Geometrical Algebra', target of a contemporary epistemological debate", for a survey of this debate. (available on the internet)
[9] Klein (1968). A comprehensive survey of the work of Edmund Husserl and Jacob Klein on the philosophical foundations of the logic of modern symbolic mathematics appears in Hopkins B.C. (2011).
[10] Klein (1968, p. 5).

Klein argues that the modern symbolic concept of number is not a further developed and extended version of the ancient Greek concept of number (*Arithmos*). The latter belongs to ancient Greek non-symbolic, ontological science. The modern symbolic concept of number is an essentially *new* concept, a concept in a new conceptual dimension, which was possible to articulate only together with the invention of the algebraic symbolism in the seventeenth century. According to Klein, the invention of the algebraic symbolism was an essential transformation in the sense that *new techniques and operational practices were created as the basis for new conceptions*. But an effect of this conceptual transformation was also that the original Greek understanding of numbers was lost, which is why Klein's 'intentional' historical method is needed.

But Klein's use of this method is meant to have not only historiographic interests and motives, he also claims that the understanding of the lost Greek conception of numbers may be helpful to resolve conceptual difficulties within modern mathematics and mathematical physics. He suggests that some of these difficulties have their source in the fact that "fundamental *ontological* science of the ancients is replaced by a *symbolic* discipline"[11] These difficulties are a main concern of mine in this essay.

The Greek mathematician who seems to have come closest to the conceptual transformation in which the algebraic symbolism originated, was *Diophantus of Alexandria* (who lived in the third century AD, and who sometimes is called "the father of algebra"). He was the author of a series of books called *Arithmetica* that deal with solving (what we call) algebraic equations. But according to Klein, it is François Viète (Franciscus Vieta; 1540 –1603) who develops the logical and mathematical consequences of Diophantus' work, and who deserves to be called the 'inventor' of modern mathematics. An important step in Vieta's work was his innovative use of letters as parameters in equations. And it is Vieta who introduces the word 'symbol' (lat. Symbolum), and talks about the symbolic concept of number.[12]

Diophantus was working with an arithmetical calculus of determinate numbers, a *logistice numerosa.* Vieta advances Diophantus' problems by introducing a new 'general analytic' or an 'analytic art', using not number but merely 'species' or 'forms' – a *logistice speciosa*. The species or forms correspond to what we would call formulas or algebraic expressions. In the articulation of these symbolic forms, the use of letters as variables and parameters is essential.

The word 'species' or 'form' alludes to the '*eidos*' of Greek philosophy, but it is important to understand how its sense has been transformed in Vieta's 'analytic art'. This is how Klein explains the difference:

[11] Klein (1968, p. 184).
[12] Klein says that "The term 'symbolum,' used for letter signs as well as for connective signs, originated with Vieta himself" (Klein 1968, p. 276).

> […] the "being" of the species in Vieta, i.e. the "being" of the objects of "general analytic," is to be understood neither as independent in the Pythagorean or Platonic sense nor as attained "by abstraction" […] in the Aristotelian sense, but as *symbolic*. *The species are in themselves symbolic formations* – […] *They are, therefore, comprehensible only within the language of symbolic formalism.* […] Therewith the most important tool of mathematical natural science, the "*formula*," first becomes possible, but above all, a new way of "understanding," inaccessible to ancient *episteme* is thus opened up.[13]

The essential point of the conceptual transformation that Vieta accomplishes is that the concern with determinate numbers of units of measurement (of ancient non-symbolic arithmetic), is replaced by the forms expressed in an arithmetical-algebraic symbolism. So in a certain sense one might say that in the symbolic conception, form becomes the content. But 'form' in this sense is not 'typographical form' or 'syntactical form' in the modern metamathematical sense. 'Form' in Vieta's sense is displayed in the operational practices.

The aspect of mathematics as *activity* is essential in the logic of symbolic mathematics. It is important to understand that the new arithmetical-algebraic system of Vieta (as well as the *mathesis universalis* of Simon Stevin, Descartes, and Wallis, who continue and complete Vieta's work) are not new theories of arithmetic, or new sciences of number (in the Aristotelian sense of 'science'), they are primarily new *arts*, new practices, new methods and techniques for dealing with problems, not only problems in 'pure mathematics' but also in cosmology, physics and astronomy. Vieta ends his work *Isagoge by* saying "Analytic art appropriates to itself by right the proud *problem of problems*, which is: TO LEAVE NO PROBLEM UNSOLVED".[14]

Vieta (like Descartes and other founders of modern science) had special interests in cosmological and astronomical problems, and his mathematical investigations are closely connected to his cosmological and astronomical work. The manner in which Vieta and other founders of modern science "set about attaining a mathematical comprehension of the world's structure betrays, from the outset, a different conception of the world, a different conception of the world's being, than that which had belonged to the anscients."[15]

[13] Klein (1968, p. 175).
[14] Klein (1968, p. 185. The use of capital letters is due to Vieta).
[15] Klein (1968, p. 152).

4 Conflicting views of the role of symbolism in mathematics

Let us return to the Unguru debate. B.L. van der Waerden, a professional mathematician as well as a historian of ancient mathematics, was one of the mathematicians who reacted immediately to Unguru's strongly polemic article with an equally polemic reply in which he accuses Unguru for overestimating the importance of symbolism in mathematics. Van der Waerden writes:

> Unguru, like many non-mathematicians, grossly overestimates the importance of symbolism in mathematics. These people see our papers full of formulae, and they think that these formulae are an essential part of mathematical thinking. We, working mathematicians, know that in many cases the formulae are not at all essential, only convenient.[16]

According to Unguru (following Klein 1968), the emergence of algebra and symbolic mathematics in the seventeenth century, in particular the use of letters as variables and parameters, marks the beginning of an essential conceptual transformation of mathematics, while for van der Waerden the introduction of algebraic symbolism was only the invention of a more convenient notation to express a mathematical content that was in many respects the same as what it always has been. The content of Euclid's *Elements* II, for instance, is algebra according to van der Waerden and Freudenthal, it is 'geometric algebra', algebraic relationships expressed in geometric form due to the lack in classical Greek mathematics of the algebraic notation for expressing the algebraic content.[17]

In this controversy, I find myself in agreement with Klein, Unguru and their followers (with some reservations concerning Unguru's argumentation to be discussed later). The lack of sensitivity to, and interest in, features of the mathematical symbolism that are *conceptually significant* is not uncommon among 'working mathematicians' (and logicians, who have a tendency to think of a symbolism as a system of notations, a system of mere typographical signs which are conceived in naturalistic terms, and this forces

[16] B.L. Van der Waerden, "Defense of a 'Shocking' point of View", A.H.E.S., 15, 1975, p. 205.
[17] These conflicting views depend upon the difference between a symbolism and a system of notation.

them to adopt a kind of dualism between form and content, expression and meaning, syntax and semantics. Carnap even said that: "Syntax, pure and descriptive, is nothing more than the mathematics and physics of language".[18]

The lack of sensitivity to conceptually significant features of symbolism makes it difficult to distinguish between philosophical and mathematical problems. The attitude of a mathematician is often that something deserves to be called a real problem only when it can be dealt with and solved mathematically (this was, for instance, Hilbert's attitude to the consistency problem in his article "On the infinite"). Mathematicians are masters in using the mathematical symbolism, but not in the reflection upon this use that requires that you pay attention to the symbolic practices from an outlook where the normal (silent) agreement in modern mathematical practices is put into focus.

Looking at ancient Greek mathematics from within the normal agreement in the practice of modern mathematics, there is always the risk of misunderstanding the non-temporality or timelessness of the sense or content of a mathematical rule or statement as if this timelessness were the infinite temporal duration of the content, the permanence of the content in time. But that the non-temporality of a rule or statement within mathematics is *timelessness* must be taken seriously; it is lack of *any* temporal significance or reference, including 'endless temporal duration' or permanence. I think that it is by taking timelessness as infinite temporal duration that Van der Waerden and Freudenthal are led to say that the algebraic content of an equation such as

$$(a + b)^2 = a^2 + b^2 + 2ab$$

was present already in Euclid's times "at least implicitly", although this content was given a geometrical disguise in Euclid's *Elements* II. This is very doubtful in view of the fact that Euclid never multiplied geometric lengths or any other geometric magnitudes. If 'a' and 'b' are geometric lengths, the expressions 'a^2', 'b^2'and 'ab' seem not to have made sense to Euclid since they are of another dimension than 'a' and 'b'. It was not until Descartes' analytic geometry (which was based on Descartes' symbolic conception of geometry) that such 'mixtures' of arithmetic and geometry were made sense of.

The algebraic content of the equation above is inseparable from its algebraic proof, and its proof is the transformation of the left-hand side of the equation into the right-hand side using the rules of the algebraic calculus (that was set up and used not before the 17th century).

[18] Carnap, (1959 p. 284). Carnap's notion of syntax, which is based on a naturalistic view of language, is *very far* from what Wittgenstein meant by 'logical syntax' the *Tractatus.*

A mathematician, or rather a mathematical physicist, with sensitivity to, and interest in, conceptually significant features of mathematical symbolism was Heinrich Hertz. His attitude to the role of mathematical symbolism was therefore also quite the opposite of van der Waerden's, who takes mathematical symbolisms to be merely more of less convenient systems of notation. Hertz wrote:

> We cannot escape the feeling that these mathematical formulas have an independent existence and an intelligence of their own, that they are wiser than we are, wiser even than their discoverers, that we get more out of them than was originally put into them.[19]

Hertz' work on the mathematics of classical mechanics in which he showed how to deal with conceptual problems connected, for instance, with the notion of force of classical mechanics was an important and influential contribution to the symbolic point of view. I believe that Ernst Cassirer was right when he said: "Heinrich Hertz is the first modern scientist to have effected a decisive turn from the copy theory of physical knowledge to a purely symbolic theory."[20] Hertz saw the scientific theory as the application of a symbolic system, which is an autonomous entity in its formal aspect, independent of the empirical phenomena it is used to explain. By "the principles of mechanics", will be meant, says Hertz, "any selection of such and similar propositions, which satisfies the requirement that the whole of mechanics can be developed from it by purely deductive reasoning *without any further appeal to experience*."[21] Hertz showed that the problems connected with the notion of force, do not relate to the empirical content of the problematic notions of mechanics, "but only to the form in which the content is represented", i.e. the conceptual problems are problems of the symbolism.[22] This is how Hertz himself motivated this feature of his work:

> …the existing defects are only defects in form; […] all indistinctness and uncertainty can be avoided by suitable arrangement of definitions and notations, and by due care in the mode of expression.[23]

[19] Quoted from Bell, (1937, p 31).
[20] Cassirer, (1957, p. 20). By the 'copy theory' Cassirer meant a view of physical theory which we find, for instance, in Ernst Mach's writings.
[21] Hertz (1956, p. 4, my emphasis).
[22] Hertz (1956, p. 8).
[23] Hertz (1956, p. 9).

5 Wittgenstein and symbolic mathematics

The mentioned feature of Hertz' work deeply influenced Wittgenstein, not only the author of the *Tractatus* but also the later Wittgenstein.[24] In the *Big Typescript* Wittgenstein writes:

> In my way of doing philosophy, its whole aim is to give an expression such a form that certain disquietudes disappear. (Hertz) [25]

I will argue that the view of modern mathematics as symbolic mathematics was essential in Wittgenstein's approach to the problems in the discussion about the foundations of mathematics that begins in the late nineteenth century. Wittgenstein's interest in mathematical symbolism is conceptually more rigorous and sensitive than that of Klein and his followers in that he laid more stress upon the operational aspects of the mathematical symbolism as human practices the various features of which we do not survey even though we master the practices. Wittgenstein made the following, somewhat excessive, statement about the role of symbolism in mathematics: "Let's remember that in mathematics, the signs themselves do mathematics, they don't describe it" (PR 186). Inspired by Hertz, Wittgenstein said in the *Tractatus* that "My fundamental idea is that the 'logical constants' do not represent."(T 4.0312) He would have said the same thing about the operations of arithmetic.

It is the *operational* aspect of a symbol, its function in the calculus, its role in the manipulation and transformation of expressions, which *constitutes it as a symbol*. So a symbolism is not just a system of notation in the typographical or linguistic sense (which is why the symbolic view of mathematics is not formalism or nominalism, as these notions are used in the literature of analytic philosophy[26]). This means, of course, that the new mathematical concepts (such as, for instance, the concept of an arbitrary finite sequence), emerged *together with* the new algebraic symbolism. The concepts could come into existence as new precise notions only when the operational practices of the new symbolism were in place. Or, rather, the invention of new notions was the invention of the new operational practices of the algebraic

[24] This is argued in more detail in my paper Stenlund (2012).
[25] BT, p. 421.
[26] Such as for instance in Weir (2011).

symbolism. It is not as if the new notions were invented in advance "in the minds of mathematicians" and were then given expression and application using the new algebraic notation. That is the sort of idealism or mentalism that Wittgenstein is constantly questioning.[27]

Wittgenstein was questioning essentially the same mentalistic tendency in Frege when he wrote:

> In attacking the formalist conception of arithmetic, Frege says more or less this: these petty explanations of the signs are idle once we *understand* the signs. Understanding would be something like seeing a picture from which all the rules followed, or a picture that makes them all clear. But Frege does not seem to see that such a picture would itself be another sign, or a calculus to explain the written one to us.

There is a tendency towards such a mentalism also in Klein's and Unguru's arguments that there were things that Greek mathematicians could not do because they did not have the required concepts. Klein and Unguru tend to argue as though there is always something mental, a mental cause, reason or ground, that makes people do what they do. But this idea is one of the most seductive sources of confusion. A lot of what we do, we do not do for any reason or ground, we just do as we have been trained to do. And our starting point is not 'complete passivity' but action. (As Goethe said: "Im Anfang war die Tat.")

One might say about many substantially new ways of thinking, new concepts, revolutionary changes in science, etc., which have emerged in the past, that they were once historically impossible, they were in sharp conflict with established notions and practices. But, nevertheless, they have happened, they have taken place, new operational practices developed, and subsequently history has been revised and rewritten. (A convincing criticism of the "argument from conceptual impossibility", as it has appeared in the Unguru debate, is given in Netz (2002)).

Wittgenstein emphasized the operational aspect of a symbolism, for instance, in the remark:

> "In order to recognize the symbol in the sign we must consider the significant use". (T 3.326)[28]

This can throw some light on Hertz' idea that the signs and formulas of mathematical symbolism have "an intelligence of their own, that they are

[27] PG, p. 40.

[28] The original German version of this remark is "Um das symbol am Zeichen zu erkennen, muss man auf den sinnvollen Gebrauch achten." I prefer Ogden's translation of this remark. The Pears and McGuiness translation "In order to recognize a symbol by its sign we must observe how it is used with a sense", can be read as though the sign already has got a sense (in some sort of 'semantics') prior to and independently of its use.

wiser than we are, wiser even than their discoverers, that we get more out of them than was originally put into them". The 'hidden intelligence of formulas' is simply the manner in which a certain symbol or formula *in its use* is connected with so many other things in the arithmetical-algebraic system, which we don't survey and foresee even if we do master its use. This 'intelligence' seems to be hidden only because *we do not survey* the possible uses of the formula even if we do master them in practice. This is true especially of the 'moves of the game' at the most basic operational level where there is complete agreement *in action* among mathematicians. These moves and features of the calculus therefore tend to be dismissed as 'trivialities' by mathematicians. But such trivialities are precisely the topic of Wittgenstein's investigations about the foundations of mathematics. This is why it is so difficult to understand what he is up to in some of his remarks on the foundations of mathematics. His topic 'the nature of mathematical symbolism' is a non-topic for the mathematician. In PG (p. 369) Wittgenstein remarks: "The philosopher only marks what the mathematician casually throws off about his activities."

Wittgenstein's conception of mathematics, already from the beginning in the *Tractatus*, has much in common with what has been called *symbolic mathematics*. I think that this is true of the early and middle as well as the late Wittgenstein. I am inclined to say that for Wittgenstein, the most authentic form of mathematics in modern times is symbolic mathematics.[29] The symbolic view of mathematics offers us a perspective from which the unity of Wittgenstein's philosophy of mathematics becomes apparent.

A clear manifestation of Wittgenstein's symbolic point of view is his claim that mathematical propositions are not 'real' propositions. According to Wittgenstein, they don't have a descriptive content; they do not describe real states of affairs. Already in the *Tractatus,* mathematical propositions were called "pseudo-propositions". And around the beginning of the 1940's, he expressed a symbolic, non-ontological conception of mathematics as follows:

> Let us remember that in mathematics we are convinced of *grammatical* propositions; so the expression, the result, of our being convinced is that we *accept a rule*.
>
> Nothing is more likely than that the verbal expression of the result of a mathematical proof is calculated to delude us with a myth.
>
> I am trying to say something like this: even if the proved mathematical proposition seems to point to a reality outside itself, still it is only the expression of acceptance of a new measure (of reality).

[29] Here I am using the word 'authentic' in more or less the same sense as when we say that chemistry is an authentic natural science today, which alchemy is not.

> Thus we take the constructability (provability) of this symbol (that is, of the mathematical proposition) as a sign that we are to transform symbols in such and such a way.[30]

This is something that cannot be made much sense of within a philosophical vocabulary based on the foundational status of mathematical logic, where all possible propositions *are* propositions in the ontological sense. They have a descriptive content and are *about* something in a 'referential sense'. There is no place in the vocabulary for propositions in any other sense. The closest one can come to propositions in some non-ontological sense are propositions in the "nominalistic" sense that are merely about concrete signs, so-called 'syntactical propositions'. One is forced to see Wittgenstein's philosophy of mathematics as some superficial kind of formalist view in which mathematics has been deprived of all meaning.

But as Wittgenstein said in the last quotation, if he is depriving mathematics of something in his critique of foundations, it is the (often misleading) prose that accompanies the mathematical calculi, and he is doing so in order to clarify the sense that mathematical notions have within the calculi.

[30] RFM III, §§26-27.

6 Symbolic arithmetic and the place-value system of numeration

I am inclined to agree with Unguru (and his followers) that there was no proof by mathematical induction in ancient Greek mathematics. And I think that this circumstance is connected with the fact that in ancient Greek mathematics (by the time of Euclid and Archimedes), one did not have the full place-value system for numbers, and 0 and 1 were not seen as a numbers. My suggestion is that the ideas of an arbitrary finite sequence and finite iteration of symbolic arithmetic, which are preconditions of proof by mathematical induction, had their origin in the emergence of the place-value system for numbers, which was referred to as the Arabic *ciphers*. Ortega y Gasset (1971, p. 52) reports that Leibniz, in his published correspondence, "called attention to the fact that Arabic ciphers have the advantage over Roman ciphers of expressing the "genesis" of a number and thereby of defining it".[31]

Of particular importance was the *decimal* place-value system, also called the Hindu-Arabic numeration system (which was imported to the west in the middle ages from Arabic sources and which subsequently became our normal way of writing numerals). Boyer (1968, p. 234) mentions that the Hindu mathematician *Aryabhata,* who was active in the fifth century, used a form of the decimal place-value system and in his writings we find the phrase:

from place to place each is ten times the preceding

This phrase indicates the principle of the generation of the bases of the decimal place-value system, i.e. that the places are Units, Tens, Hundreds, Thousands, Tenthousands, *and so on*. Here the phrase "and so on" is not an abbreviation for something that could be written out in full as when we say "A, B, C, and so on" for the sequence of letters of the alphabet. *Aryabhata*'s phrase suggests the idea of arbitrary *finite iteration* of an operation, in this case the operation of multiplying the base of the preceding place by ten.

In the fully developed decimal place-value system for integers, a number is written as a *finite sequence or a string* of digits taken from the list of digits 0, 1, 2, 3, 4, 5, 6, 7, 8, 9. Each digit has its unique place within the sequence as a whole. Since it is only single digits that occur as the terms of the se-

[31] Leibniz, *Mathematische Schriften*, Vol. IV, pp. 455 ff.

quence, there is no need to separate the places of the sequence by means of commas or some other device than juxtaposition. The total value of a numeral, as a sequence of digits, is calculated in this notation by additions and multiplications. For a two-digit numeral, 23 for instance, we have

$23 = (2 \times 10) + (3 \times 1)$

For a three-digit numeral, 517 for instance, we have

$517 = (5 \times 100) + (1 \times 10) + (7 \times 1)$

and for a four-digit numeral, 3702 say, we have

$3702 = (3 \times 1000) + (7 \times 100) + (0 \times 10) + (2 \times 1)$

and so on. In the second example '1' is used as a number, and in the last example '0' is used as a number. The numbers '0' and '1', which were not numbers according to the Greek notion of number (*arithmos*), have clearly important functions in the decimal place-value system.

Neither the late ancient Greeks nor the Hindu-Arabic mathematicians had the symbols I have used here for equality, addition and multiplication. Equations, as well as addition and multiplication were expressed in verbal language, in *mathematical prose* (to use Wittgenstein's expression). The algebraic symbolism was not yet invented and the presentation was, as one says, *rhetorical*. And, of course, they did not have anything like the following algebraic symbol for the 'general case' of a number in the decimal place-value system:

$$a_k a_{k-1} \ldots a_0 = a_k 10^k + a_{k-1} 10^{k-1} + \ldots + a_0 10^0$$

where $0 \leq a_i < 10$ and $a_k \neq 0$. It is important to realize that this expression belongs to a symbolic system, an *operational practice* that did not exist in Greek or Hindu-Arabic mathematics, but only in modern mathematics. One essential symbolic feature of this equation is the use of the number-variable k. For a sufficiently large value of k, the numeral $a_k a_{k-1} \ldots a_0$ would not have a reading in ordinary language as my examples 23, 517 and 3702 all have. But even without this expression for the general case, it can be seen on the basis of examples, being particular cases, that this method of numeration stipulates no limit to the length of the expression for a number, i.e. to the number of places occurring in it. Each number is uniquely determined by the method for its construction in this system of numeration.

With the place-value notation, one was in possession of the operational germ of the arithmetical idea of "and so on ad inf." (if not the precise arithmetical concept, which required the arithmetical-algebraic symbolism), and

also the operational germ of the ideas of finite iteration, and a finite sequence, and thereby also the germ of recursive proof or proof by mathematical induction. The word 'infinity' in the phrase "and so on ad inf." here simply means that no finite sequence of digits is excluded as an expression for a number. *It is not a feature of this system of notation for numbers that a number must be the number of things of some given multitude of things.* A numeral in this system is a finite sequence of digits, which is a symbol for a number determined by the rules for calculating in the system.

It should be clear that the sense of number and finiteness suggested here is *essentially different* from the notion of number and finiteness of ancient Greek mathematics, since it was essential for the Greek concept of number (*Arithmos*), that a number is *a number of things (units) of a multitude.*

The problem of so-called effective calculability discussed in connection with Church's thesis in modern mathematical logic arises only for the notion of finiteness of modern symbolic mathematics, according to which a verbal expression such as "can be carried out in a finite number of steps" has a kind of non-literal, symbolic sense that did not exist for the Greeks. In ancient Greek logistics one was surely concerned very much with 'practical effective calculation procedures', but not with the calculability of the number-theoretic *functions*, that are discussed in connection with Church's thesis.

The features of the decimal place-value system of numeration I have stressed here points towards the symbolic notion of number and finiteness that begins to emerge in the work of Vieta, Stevin, Descartes and Wallis in the sixtieth and seventieth centuries. Of particular importance in this respect was Stevin's work.[32] In 1585 he published a pamphlet, *De Thiende* ("Art of Tens") in which he used not only the decimal place-value system for integers, but also for decimal fractions. The Arabs and Chinese knew about decimal fractions earlier, but they were not used very much. Stevin compares Arab and Greek mathematics and his comparisons are often to the disadvantage of Greek mathematics. In Stevin's view the most important historical roots of *modern* mathematics are *not* in Greek mathematics, but rather in Hindu-Arabic mathematics.

Stevin's pamphlet was very influential. He established the use of decimals in everyday mathematics, demonstrating the simplicity and advantage of the system. In the introduction to *De Thiende* he predicted that using decimal fractions in coinage, weights and measures, etc. would be one day universally accepted – a prediction that has come true to a great extent (even if it is not universally accepted). Stevin was very much what we would call an 'applied mathematician'. As Klein puts it: "Stevin consciously breaks with the traditional forms of science and puts his 'practical' commercial, financial, and engineering experiences into the service of his 'theoretical' preoccupa-

[32] Simon Stevin (1548-1620) was a Flemish mathematician and military engineer.

tion – as, conversely, his 'theory' is put to use in his 'practical activity'."[33] Thus, for instance, he was the first to explain the tides using the attraction of the moon. In 1586 (three years before Galileo) he reported results of his own experiments to the effect that different weights fell a given distance in the same time. He wrote on astronomy and strongly defended the sun system of Copernicus.

Unlike Vieta, who expressed himself very respectfully about Greek mathematics, Stevin is very critical, and in particular about the weaknesses and mistakes he finds in Greek arithmetic. (An exception here is Archimedes, whose work Stevin admired.) The Greek view that the number 'one', or the 'unit', is not a number but rather the *principle* or *arche* (the beginning) of number, was according to Stevin one such mistake which he examines at great length and rejects. He also made a strong plea that all numbers such as square roots, irrational numbers, negative numbers etc. should all be treated as numbers and not be distinguished as being 'different in nature'. As van der Waerden puts it: "For Stevin, the real numbers formed a continuum. His general notion of real number was accepted, explicitly or tacitly, by all later scientists."[34]

Klein's detailed account of Stevin's criticism of the Greek concept of number, shows that the criticism is firmly based on the symbolic concept of number manifest in the decimal place-value system.

[33] Klein (1968, p. 186).
[34] Van der Waerden (1985, p. 69).

7 Rabbi Levi ben Gerson

Looking at the historical development of mathematics, one can see that many mathematical concepts, methods, and techniques that were given precise mathematical sense in the symbolic systems that started to emerge in the seventeenth century had forerunners, but, of course, forerunners that were less precise by the standards of modern mathematics. This is also true of the method of using recursion in proofs. One mathematician who used what one would be inclined to call, "proof by recursion" or "proof by induction", quite systematically was Rabbi Levi ben Gerson.[35] He proved several theorems on the commutative and associative properties of multiplication and on permutations and combinations using recursive procedures.

What should be noted first of all is that ben Gerson is using the decimal place-value system for numbers in computations – not however with the Hindu-Arabic notation, but with the first nine Hebrew letters for the digits 1, … , 9, and a circle for zero. Following Euclid he also represents numbers as line segments, i.e. *AB* for the line segment with endpoints *A* and *B* but, at the same time, arbitrary numbers are also represented by letters *a*, *b*, *c*, … In his proof of the theorem that the number of permutations of *n* elements is the product with the factors 1, 2, …, *n*, he states (what we call) the induction-step in prose as a theorem, and then he reformulates it as follows:

> Let the elements be *a b c d e* and their number be *n* and let *m* be the successor of *n*. Let the number of permutations of *a b c d e* be *t.* Adding one member to the set *a b c d e,* we obtain *a b c d e f* containing *m* elements. We say that the number of permutations of *a b c d e f* is equal to the product of *t* by *m.*

It is clear, from the context, that Levi ben Gerson is not concerned with the particular case of a sequence of five numbers when he writes *a b c d e* (in the

[35] Levi ben Gerson was born in Languedoc in 1288, and was a Talmudist, philosopher, Biblical commentator, mathematician, astronomer, and physician. He wrote in Hebrew and completed several mathematical works. For instance, a commentary of several books of Euclid's elements and a work called *Maasei Hochev,* the title of which is said to mean *"the work of the calculator".* In the first (theoretical) part of the book, sixty-eight propositions are given general proofs, and the second part gives instructions for solving numerical problems in adding, subtracting, multiplying, but also in summing arithmetical and geometric series, and in combinations, permutations, and proportions, and in extraction of square and cube roots. In the *Maasei Hochev,* which was completed in 1321*,* we find repeated use of recursion in proofs. (My account of Levi ben Gerson, and his work is based on Rabinovitch (1970)).

sense in which Euclid was *indeed* concerned with the particular case of a sequence of three prime numbers when he wrote "Let *A*, *B* and *C* be the assigned prime numbers" in the beginning of his proof of the infinity of the primes). Ben Gerson tends to use the expression

$$a\ b\ c\ d\ e$$

as we would use an expression like

$$a, b, c, d, \ldots, e$$

where the three dots is an abbreviation for the (n-5) members of the sequence that have not been written out (and ignoring that 'e' comes immediately after 'd' in the alphabet). So to some extent one might say that he had the germ of the notion of an arbitrary finite sequence. Not, however, as a notion belonging to a developed calculus of finite sequences. He does not seem to have had a clear notion of the *form* of a finite sequence, or an accurate symbolism for finite sequences. Neither did he have a satisfactory notion of the successor operation (by the standards of modern mathematics). When he says "let *m* be the successor of *n*", he somehow thinks of it as an external relation between two given numbers, rather than as an operation on numbers which is given by its form *n*+1, where '*n*' is a number variable. One reason for this may have been that he didn't have symbols for addition (and multiplication) *as operations*. Addition, multiplication, equality, were expressed in verbal language as in Greek mathematics. For this reason there is a great lack of perspicuousness (surveyability) in ben Gerson's statement of theorems and proofs. Rabinovitch points out that "it is quite difficult to read even relatively simple statements where every plus sign must be written out in words".[36] Here one could ask oneself, if this difficulty is due to a mere notational inconvenience or if it is rather a conceptual difficulty. To what extent is the perspicuousness of a proof a part of its essence as a proof? One gets the impression that there is a tension in ben Gerson's mathematics between the verbal-geometric perspicuousness of Euclid, and the algebraic-symbolic perspicuousness connected with the place-value notation and the use of letters as variables. Consider for instance, *Maasei Hoshev*, Proposition 41:

> The square of the sum of the series of integers beginning from one up to a given integer is equal to the cube of the given integer plus the square of the sum of the series of integers from one up to the predecessor of the given integer.

[36] Rabinovitch (1970, p. 239)

In modern symbolism this proposition would be expressed:

$$\left(\sum_{1}^{n} i\right)^2 = n^3 + \left(\sum_{1}^{n-1} i\right)^2$$

Already when he reformulates the original proposition in the beginning of the proof, ben Gerson moves closer to the algebraic-symbolic perspicuousness by using letters for arbitrary numbers of a series.

In ben Gerson's proofs, in which we recognize a use of recursion and are inclined to call them proofs by induction, he often illustrates the proof with more cases than needed (in the proof), i.e. not only for the base n = 1, but also for n = 2, n = 3, n = 4. What he seems to want to illustrate here is how the result 'extends to infinity' like a ladder or a spiral. In connection with the (recursive) proof of the theorems on the commutative and associative properties of multiplication, he says:

> In this manner of *rising step by step*, it is proved to infinity.

So the *method of rising step by step* is ben Gerson name for what later has been called "the method of proof by mathematical induction". Unlike the modern statements of the method it is not defined or made precise in some mathematical calculus or system. It is rather what one perhaps would call "an intuitive notion". The name *rising step by step* is an ordinary language simile which captures a similarity between procedures in different proofs by induction.[37]

So ben Gerson, unlike Euclid, was using letters *a*, *b*, *c*, … for arbitrary numbers, i.e. these letters are used as parameters. And his use of the letter *n* and *m* in his proof about permutations are number variables. The recursion in the proof ('the induction step') is carried out on these variables. He also seems to have had a notion of an arbitrary finite sequence, even if his symbolism is not satisfactory. I don't think that it is farfetched to believe that his familiarity with and use of the place-value system for numerals is one important thing that takes ben Gerson's mathematics closer to modern symbolic mathematics.

My point with the previous discussion of mathematical induction is not mainly historical, its point is rather to show what difference the symbolic point of view makes, by giving some examples of what sort of differences 'make the difference'.

[37] According to Rabinovitch (1970 , p.245) 'rising step by step' is a translation of the Hebrew word *Hadragah.* A noun formed from the same root occurs in the Bible where it is taken to mean cliffs that appear like rising stairs.

Ben Gerson had a notion of mathematical induction, of finite sequences etc., that were not clear enough by our standards. They rest on vague analogies expressed in prose. But we are nevertheless inclined to call it a beginning of the development towards precise notions. But we could also say that he had the beginning of a symbolism for these notions, but one that was not accurate. It may then seem as though we have two different kinds of inaccuracies, one having to do with content and the other with form. It is important to realize that this alleged difference is a mistaken idea. It is one and the same inaccuracy. When you have found an accurate symbolism for a notion, you have also become clear about the content of the notion.

8 Archimedes and the naming of large numbers

It is often said by modern commentators that one of the great advantages of the place-value system was that one eliminated the need to invent new digits for numbers, no matter how large. But it is important to realize that this statement is not a judgment that is neutral with respect to the difference between the ancient Greek arithmetic and modern symbolic concepts of number and finiteness. The 'great advantage' is very much a great advantage for someone who takes the symbolic conception of number and finiteness as *the* true meaning of these notions (which is our normal (silent) attitude).

In his work entitled *Psammites* (the "Sand-Reckoner") Archimedes describes his problem with the naming of large numbers as follows:

> … I will try to show you by means of geometrical proofs, which you will be able to follow, that, of the numbers named by me and given in the work which I sent to Zeuxippus, some exceed not only the number of the mass of sand equal in magnitude to the Earth filled up in the way described, but also that of the mass equal in magnitude to the universe.[38]

One part of the problem was precisely extending the existing Ionian or alphabetic system of numeration to very large numbers far beyond a 'myriad' or 'ten thousands', which was the largest number expressible in Greek ordinary language at the time. "Are there multitudes so large that they cannot be counted?", was therefore a real problem which it is not for us because we are satisfied with a number as a *form*, a form of a finite sequence, a possible finite sequence where the possibility is determined by the method for its construction defined in the algebraic-arithmetical symbolism. We don't require of an instance of the general form of a number expressed in the decimal system as a sequence of digits, that this sequence should have a clear and intelligible reading in ordinary language.

In determining an upper bound for the number of grains of sand that fit into the universe Archimedes used the heliocentric model of the Universe of Aristarcus of Samos. Assuming that the Universe is spherical he estimated that it would require no more than 10^{63} grains of sand to fill it. The details of his estimation, which is a piece of history of astronomy, will not concern us

[38] The quotation can be found in Wikipedeia under the item "The Sand Reckoner".

here. What interest us is the system of numeration that Archimedes developed and in which he could *name* this number and even larger numbers. And by 'naming' Archimedes meant that the names of the large numbers should have an intelligible reading in the natural language of his times.

Archimedes developed his system as a series of "periods of numbers" and each period consists of a series of "orders of numbers": first numbers, second numbers, third numbers, etc up to a last order of a given period. The orders as well as the periods have all a last number, and the last number of a certain order or period is taken as the unit of the next order or period (if there is one). So there will be a largest number named in this system.

Starting with the word "myriad" he extends this to naming all numbers up to a myriad myriads ($10^4 \times 10^4 = 10^8$). These are the numbers of the first order of the *first period*. Myriad myriads ($10^{8)}$ is then taken as the unit of the numbers of the second order of the first period. The numbers of the second order are then multiples of this unit up to this unit taken a myriad-myriad times, ($10^8 \times 10^8 = 10^{16}$). This number is then taken as the unit of the numbers of the third order in the first period, and the multiples of this unit are the numbers of the third order, and so on. Archimedes continued naming numbers in this way up to a myriad-myriad times the unit of the numbers of the 10^8-th order, i.e. $(10^8)^{Q,}$ where $Q = 10^8$. Thereby the numbers of the first period are defined. It consists of the numbers from 1 to Q^Q.

Let $P = Q^Q$. The number P is then taken as the unit of the numbers of the first order of the *second period*. It will consist of the numbers from P to 10^8P. This last unit will be taken as the unit of the second order of the second period, ending with $10^{16}P$. In this manner we continue until we come to the Q-th order of the second period, ending with $Q^QP = P^2$.

Next P^2 is taken as the unit of the first order of the *third period*, and we continue until we reach the Q-th order of the *third period* ending with P^3. This number is then taken as the unit of the first order of the *fourth period*, and one continues the process until we reach the Q-th order of the *Q-th period*, ending with P^Q. This is the last and largest number in Archimedes system of numeration. He expressed it in ordinary language as "a myriad-myriad units of the myriad-myriad-th order of the myriad-myriad-th period".

It has been pointed out that Archimedes system has similarities to a place-value numeration system in which a myriad myriads, 10^8, is the base. But since an end is stipulated for the process of defining numerals, it is not a place-value system. Why did Archimedes put an end to the process? From the point of view of modern mathematics it would have been natural to continue the process ad. inf. (i.e., without stipulating an end) – It was presumably because *he wanted his numerals to have an intelligible reading in ordinary verbal language*. For Archimedes the word 'naming' had a much more ordinary and literal sense, than in the syntax and semantics of modern mathematics (as when one introduces 'names' of all numerals in a formal system through a recursive definition). We have used modern notation in explaining

Archimedes' numeration system (to give the reader an idea of what the system is like from a modern point of view), but it was an essential feature of Archimedes' account that his numeration system is defined and explained in ordinary verbal language, in the mathematical prose of his times. In this respect Archimedes adapted himself to a hierarchy of values of ancient Greek culture, which Netz describes as follows:

> Greek literary production is marked by a hierarchy of values always related to a certain 'literary' or 'verbal' preference: literature is ranked above science, inside science philosophy is ranked above mathematics; persuasion (to the Greeks, the central verbal art) is ranked above precision and natural language above other symbolic domains. [39]

This explains, according to Netz, the non-arithmetical nature of classical Greek mathematics. Could Archimedes, as a mathematician, have ignored these extra mathematical values? Netz argues that he could, but chose not to do it. One reason for this choice may have been that his work, *Psammites*, is addressed to non-mathematicians (a king Gelon, among others) who were thinking that no number have been named which is great enough to exceed even the magnitude of the grains of sand on the earth. In addressing readers who were non-mathematicians Archimedes had better to observe prevailing extra-mathematical values in order to carry conviction (be persuasive).

It was an important feature of Greek mathematics that the language of mathematics be continuous with and in proximity with ordinary verbal language. This is also argued by Klein (1968). It seems to be part of what Klein expresses as follows:

> Greek scientific arithmetic and logistic are founded on a "natural" attitude to everything countable as we meet it in daily life. This closeness to its "natural" basis is *never* betrayed in ancient science.[40]

The closeness to its "natural" basis of everyday life is a feature of Greek mathematics which stands in sharp contrast to modern symbolic mathematics. There is a striking contrast in this respect even when we compare Greek science and modern science in general. In contemporary popular science there is a clear tendency to relinquish what "we meet in everyday life" and even to give oneself up to mythology.

Articulating *modern* mathematics in ordinary verbal language by assigning a place for mathematical propositions in the general category of propositions expressed by declarative sentences of natural language, have often resulted, not in "closeness to the natural attitude of everyday life", but on the contrary in mythological ways of thinking such as for instance the 'non-

[39] Netz (2002, p. 287).
[40] Klein (1968, p. 63).

linguistic' *Thoughts* of Frege (which have their special *Wirklichkeit* as entities of "The Third Realm"),[41] or the ontological mythologies of transfinite set theory of Cantor and Gödel.

Modern logicians are more ontologically extravagant than Aristotle, who says that "The general propositions of mathematics are not about separate things which exist outside of and alongside the [geometric] magnitudes and numbers, but are just about these; not, however, insofar as they are such as to have a magnitude or to be divisible [into discrete units]."[42] The general propositions that Aristotle had in mind here are, for instance, the axioms or "the common notions" and the theorems of Eudoxus' theory of proportions.

But the ontological views of Aristotle are a hard topic. He rejected Plato's doctrine that numbers have an independent existence, and claimed instead that quantity, magnitude and number exist *within* the phenomena we perceive with our senses. The number three is not detached or separated from a multitude of three apples. But how can they then be objects of knowledge (*episteme*) in arithmetic as theoretical science? This question is what Aristotle answers by what has been called his "theory of abstraction".

Having said that quantity is inseparable from sensible things, it is somewhat surprising to find Aristotle saying that arithmetical science nevertheless studies the numbers *as if they were separated* from the objects of sense. Aristotle claims that "It [science] thinks the mathematical objects which are not separate as separate when it thinks them."[43] And this is accomplished by abstraction in which "their separate mode of being arises from being "lifted off", "drawn off", "abstracted."[44] "The mathematician makes those things which arise from abstraction his study, for he views them after having drawn off all that is sensible …, and he *leaves* only the 'how many?' and continuous magnitude."[45]

Abstraction is, however, not meant to be a sort of psychological procedure resulting in mathematical objects as mental constructions.

Jacob Klein summarizes Aristotle's ontology of mathematical objects as follows:

> If the reduction [in the abstraction process] goes so far that things are no longer regarded even as "mere bodies" but only as "items," these things have been transformed into "neutral" monads. Just this "neutrality" of things which have withered away into mere countable "items" constitutes the "puri-

[41] Frege (1957, p. 269), writes:"There is no contradiction in supposing there to exist beings that can grasp the same thought as we do without needing to clad it in a form that can be perceived by the senses. But still, for us men there is this necessity."
[42] Aristotle *Metaphysics* (M 3, 1077 b 17-20).
[43] Aristotle, *On the Soul*, (Γ 7, 431 b 15 f.).
[44] Klein (1968, p. 104).
[45] Aristotle, *Metaphysics,* (K 3, 1061 a 28 ff).

> ty" of the "arithmetic" monads and turns them into the noetic material which underlies scientific study.[46]

So we can understand Jacob Klein's statement that with Vieta's symbolic approach in his Analytic art "a new way of 'understanding,' inaccessible to ancient *episteme* is thus opened up".

[46] Klein (1968, p. 105).

9 Ordinary verbal language and mathematical symbolism

Let us return to Jacob Klein's claims that Vieta is the "inventor of modern mathematics" and that "modern mathematics is symbolic mathematics". Klein's claims are quite extensive and strong. Presumably, he does not want to say that modern mathematics has reached full realization and understanding of its symbolic nature. That this is not what he is saying seems to follow from his statement about the "ontological presuppositions [that] are left unclarified", in modern mathematics and mathematical physics.

Despite the transformation of mathematical thought that takes place with the work of Vieta, Stevin, Descartes and Wallis, it seems to me that there are important features of Greek non-symbolic ways of thinking that were passed on, rather unaffected by the symbolic transformation, to modern times through two very influential works, namely Aristotle's logical doctrine and Euclid's *Elements* (and many features, if not all, of Aristotle's logical doctrine are part of the philosophical context in Euclid's *Elements*).[47] The great influence of Euclid's *Elements*, even in modern times, has made people compare its influence with that of the Bible. It has been said that no other book, except the Bible has as many translations, editions and commentaries as the *Elements*. Calinger points out about the *Elements* that "Its geometrical conception of mathematics greatly influenced the natural sciences – in medieval Arabic and Latin natural philosophy as well as in Isaac Newton's Principia of 1687, which follows the format of the *Elements*. Its conceptions were basic to Kant's *Critique of Pure Reason*, published in 1781."[48]

Klein would perhaps object that our understanding and reading of Euclid's *Elements* since the seventeenth century is, nevertheless, imprinted by our loss of understanding of ancient Greek mathematical thinking. In Greek mathematics one had the conscious attitude that natural language referring to ordinary immediate experience is the basic linguistic framework. It has priority over other linguistic domains. Klein is certainly right that the reference to ordinary immediate experience as the basic source of fundamental math-

[47] See Mancuso (1996, Ch. 1 and 4.) Mancuso argues convincingly that "the Aristotelian epistemological framework was pervasive in the seventeenth century and very influential indeed in later centuries" (Mancuso 1996, p. 92). By the "Aristotelian epistemological framework" Mancuso means primarily what we find in Aristotle's *Posterior Analytics*.
[48] Calinger (1999, p. 132-133).

ematical concepts is something that gets lost, or is abandoned, in the transformation that takes place in the seventeenth century. It was this step in particular that made it possible for seventeenth century mathematics to transgress the boundary line between discreet and continuous quantity – a boundary line which we find in Aristotle and Euclid and their followers. But what is still not abandoned is the tendency to give meaning and significance to basic notions in mathematics and formal logic by translation or paraphrase into verbal language (to which I count what is often called "informal mathematical language", or, in Wittgenstein's words "mathematical prose").

One thing that was transferred to modern times from the Aristotelian-Euclidian tradition is a philosophical attitude to formal logic, what I would like to call *the foundational status of (formal) logic*, which involves at least the following things: First, logic is concerned with judgments or propositions originally connected with the *episteme* of the Aristotelian ontological concept of a science. In this sense a science and its propositions are always *about a certain subject-matter* which we have access to through a process of abstraction. Geometry and its propositions, for instance, are about continuous quantity, which we encounter in nature. This ontological conception of propositions is present in mathematics as much as in physics and biology. Secondly, to the foundational status of formal logic belongs also the idea that logic displays the form of judgments or propositions that is essential for their being true or false. This means in particular that a logically well-articulated proposition carries its meaning or logical content by itself as a proposition, regardless of its context of use.

What I want to argue is that the foundational status of logic, Aristotelian logic as well as modern formal logic, is intimately connected with the translation (or paraphrase) of the basic notions and rules of the logical calculi into natural language, *and thus into a non-symbolic linguistic framework*. The prose-translations, or the natural language readings, of the basic logical notions and formulas of formal logic tend to become *ritualized* and acquire a normative role for what constitutes the logical content of propositions. (In learning to formalize sentences of ordinary language using the propositional or the predicate calculus in contemporary introductory logic courses, students are initiated in these ritualistic reading techniques. To learn to *disregard* the context of use of sentences is an important part of learning these paraphrase techniques.) Being articulated in ordinary language, it now appears as though the logical notions and rules are already present in ordinary language, as a hidden or concealed logical core of ordinary language that is made explicit through formalization. But the normativity of the prose-readings of logical notions and formulas was not a discovery about ordinary language; they were adapted to the formal rules of the logical calculi. And it is a part of this adaptation to disregard how the precise conceptual content of sentences depends upon the context of their use.

What I have said here about the role of verbal language may seem to be in conflict with Frege's logical doctrine in one respect. The proper subject matter of logic, according to Frege, is not sentences of natural language but *Thoughts*, which are sometimes said to be 'non-linguistic' entities. For Frege it is *Thoughts* that are the subject-matter of logic, that have a logical structure and stand in logical relations to one another. A *Thought* is about a determinate subject-matter, and it says something true or false about that subject-matter. – But how can a *Thought say* something about a subject-matter if it is 'non-linguistic', if it is not articulated by signs of some sort? A *determinate Thought* has to be expressed as a sentence of a language, if it is to be a determinate thought at all.[49] A *Thought* is really what Frege earlier called the *Sinn* of a sentence of language.

The propositional calculus and the predicate calculus are calculi. It would be possible to present the systems of formal logic as *pure calculi*, in which the basic logical notions are deprived of all meaning that comes from the translation and paraphrase into natural language (i.e. the reading of 'f(a)' as 'a has the property f', '¬A' as 'It is not the case that A', 'A & B' as 'A and B', '∃x f(x)' as 'there exists an object x which has the property f', etc.). In such a presentation of formal logic as a pure calculus, the basic logical operations are symbols (not just signs; remember that a symbol is determined by how we operate with a sign in the calculus). The different *signs* '¬' and '∼', for instance, have been used for the same *symbol*. If we call that symbol 'negation', we must keep in mind that this word only signifies a symbol in the pure calculus; its sense does not come from a translation of the signs '¬' or '∼' as 'not' or as 'it is not that case that' in natural language.

One could make the same point by saying that it would be possible to do with the systems of formal logic what Hilbert did with Euclidian geometry in his *Grundlagen der Geometrie.* Hilbert presented Euclidean geometry as a system in which the basic notions (e.g. 'point', 'line', 'plane', 'between', etc.) are deprived of the meaning that comes from their use in informal geometrical language, (which is often called the 'intuitive meaning' of these words). So the only 'meaning' that the basic notions have in the system is the one that comes from the axioms and rules that define the system.[50] But viewing Euclidian geometry as a pure calculus we would go further than Hilbert, who was still following the logical tradition in presenting the Euclidian geometric system as a *theory* about some 'unspecified things', external to the Euclidean calculus. Hilbert said that in *Grundlagen*, he had pre-

[49] Frege states explicitly: "…that a thought of which we are conscious is connected in our mind with some sentence or other is for us men necessary. (Frege, 1957, p. 269.)

[50] Freudenthal (1962, p.618) expressed the significance of Hilbert's *Grundlagen* by saying that with Hilbert "[…] the bond with reality is cut. Geometry has become pure mathematics. The question whether and how to apply it to reality is the same in geometry as it is in other branches of mathematics. Axioms are not evident truths. They are not truths at all in the usual sense."

sented geometry as a "pure mathematical science". By viewing the system as a pure calculus, the axioms get the role of rules for the operation with the basic notions and the signs of the calculus (which include geometric figures).

In such a presentation of the logical systems as pure calculi, they are mathematical symbolisms, and they would deserve the name "symbolic logic". Formal logic as symbolic logic in this sense has no longer the foundational status I mentioned before. The foundational claims disappear when the logical symbol's connection with informal verbal language is cut off. They are just mathematical symbolisms side by side with other mathematical symbolisms without any exceptional position among the great variety of mathematical symbolic systems.

In many purely mathematical results about logical systems, in so-called 'metalogic', only such features that belong to the system as a pure calculus or a mathematical symbolism are actually used. But in presentations of modern logical systems, one tends to state and present their mathematical properties as if the systems are inseparable from the translation of the signs into natural language (or mathematical prose), i.e. as if the logical operations *must* have the sense that comes from their translation into natural language. The reason for this is of course that one wants to adhere to the foundational status of formal logic according to which the propositional calculus, for instance, is concerned with propositions in the ontological sense; they are understood as being about some sort of reality external to the calculi that make them true of false. But as a pure calculus the propositional calculus is only concerned with *formulas*, and their translation or reading as (forms of) English sentences is not part of the rules of the calculus.

If the logical symbols' connection with the natural language readings of them were cut off, one would also undermine the philosophically suggestive prose reading of most results in metalogic, i.e. results about 'consistency', 'soundness', 'completeness', 'incompleteness', 'decidability', 'undecidability', etc. Confronted with the threat that the alleged philosophical significance of, for instance, famous results such as Gödel's incompleteness theorems gets lost, logicians will hold on to the foundational status of formal logic, and to the ritualized prose readings of the logical symbols.

The point of presenting a logical system as a pure calculus would be to show that the foundational status of formal logic, and the prose readings of the logical symbolism, is not a (mathematically) necessary feature of it. It shows how the ontological conception of propositions of mathematics is intimately connected with the translation of the logical operations into verbal language. So in a certain sense one might say that the non-symbolic, ontological conception of mathematical propositions rests upon giving informal mathematical language a foundational significance.[51] I think that this feature

[51] Here it is instructive to look at the debate between Hilbert and Frege (in Frege, 1980). It is clear that Frege argues as if the presentation of a mathematical system

belongs to the heritage from the Greeks (Aristotle), and it stands in sharp contrast to the symbolic conception of mathematics.

The most obvious way in which a branch of mathematical logic rests upon the standard readings, or translations into verbal language of the expressions and formulas of the predicate calculus is in so-called 'model-theoretic semantics', where one starts out with a notion of 'truth in a structure'. Such a structure has a domain of objects which are values of the bound variables. Via the natural language reading of formulas, a formula becomes a proposition about these objects that is true or false in the structure. If the objects are numbers, the formulas are translated into arithmetical propositions. It is clear that the ontological conception of arithmetic is built into this approach. One gets the impression that the standard verbal reading of the formulas of the predicate-calculus was an important source of inspiration in the invention of model-theoretic semantics.

Returning to Jacob Klein's claim that the essence of modern mathematics is symbolic mathematics, it is clear that this claim does not apply to modern mathematical logic as it is understood and used in the discussion about the foundations of mathematics since the beginning of the twentieth century. And this foundational discussion has dominated (and thereby also delimited) the discussion in the philosophy of mathematics since then. All positions in this philosophical discussion (e.g. realism, Platonism, intuitionism, logicism, formalism, structuralism, etc.) take for granted the ontological conception of a proposition with roots in the logical tradition, and mathematics is seen as a kind of 'natural science of mathematical objects', whether they are platonic objects or mental constructions.[52] In the symbolic conception of mathematics, the ontological difficulties in this discussion do not arise at all.

(geometry as well as logic) as a pure calculus is impossible, because, according to Frege, the meaning of the basic notions that comes from the translation into informal language cannot be disregarded.

[52] Exceptions here are Poincaré's and the early Brouwer's critique of the foundational status of formal logic. But this critique has been largely ignored in the foundational discussion. Modern intuitionists and constructivists (with the exception perhaps of Errett Bishop) have their own formal logic with their own ontological notion of proposition.

10 The end of the science of quantity

The impact of the heritage from Euclid and the Aristotelian logical doctrine on modern mathematics is present not only in the modern logical tradition, but also in mainstream mathematics (e.g. mathematical analysis or Calculus) of the eighteenth and the beginning of the nineteenth centuries. It seems to me that there is a deep tension at work between the two tendencies in mathematics during this time: on the one hand the Euclidean-Aristotelian heritage, with its ontological emphasis on geometry and 'continuous quantity', and the symbolic conception with its emphasis on the formal and operational aspect of mathematics, on the other. The letter tendency manifests itself in the 18th century in the effort to make algebra the foundation of mathematical analysis (Lagrange).

The former, ontological tendency, was passed on by Newton, whose attitude to the tension between the two tendencies is somewhat puzzling. According to Guicciardini (2003, p. 75), Newton's interest in mathematics began in 1664, when he read, inter alia, François Viète's works, Descartes's *Geometrie*, Oughtred's *Clavis mathematicae* (1631) and Wallis's *Arithmetica Infinitorum* (1656). From reading these works on "modern analysis" Newton learned analytic geometry, algebra, tangent problems, and series, and he made his own contributions to this field of research which was highly symbolic. But according to Guicciardini:

> In the 1670s he was led to distance himself from this early highly analytical mathematical research. Newton began to criticize modern mathematicians. He stressed the mechanical character of modern algebraical methods […] By contrast, he characterized the "geometry of the Ancients" as simple, elegant, concise, adherent to the problem posed, and always interpretable in terms of existing objects. Needless to say, notwithstanding Newton's rhetorical declaration of continuity between his methods and the methods of the "Ancients", his geometrical dynamics is a wholly seventeenth-century affair. The reasons that induced this champion of analytics, series, infinitesimals, and algebra to spurn his analytical research are complex. They have to do with foundational worries about the nature of infinitesimal quantities […]. They also have to do with his dislike of Descartes, towards anything Cartesian […]."[53]

Newton passed on the "methods of the Ancients", for instance, in the form of the ideas of absolute space, absolute time and absolute motion. Newton's

[53] Guicciardini (2003, p. 92).

assertion of these ideas resulted in a quite violent fight between Newton (via his spokesman Clarke) and Leibniz, who was a great symbolic thinker and argued for the relativity of place and motion. According to Hermann Weyl: "In this quarrel modern Physics sides entirely with Leibniz."[54] We also recall that it was Leibniz', and not Newton's version of the infinitesimal calculus that survived into the 20th century. Leibniz' symbolic approach in mathematics and logic and its influence and success, is further support for the claim that the essence of modern mathematics is symbolic mathematics.

Many 18th century mathematicians conceived themselves to be concerned with a subject-matter that belonged to geometry seen as the science of physical space or to physics as concerned with continuous quantities such as lengths, weigths, area, volume, time, mass, power, speed, etc. As Epple points out, "During the 18th and part of the 19th century, many scientists still agreed with the idea that mathematics was the 'science of quantity' […] It was self-evident to mathematicians of the 18th century that the quantities dealt with in analysis were endowed with meaning in the natural and social world."[55]

Many conceptual problems that were noted and discussed (but not solved) in 18th century mathematics had their source, it seems to me, in the deep tension between the mentioned two tendencies, i.e. between the view of numbers as quantities, and the symbolic view of numbers. There were problems about the nature of negative numbers: If numbers are quantities and 0 is nothing, how can there be quantities less than nothing? There were problems about infinitesimal magnitudes, differentials, the notion of limit, function, continuity, and one talked about the 'mysteries' of imaginary numbers. There were also problems concerning infinite divergent series. According to Jahnke "it was not self-evident that a formula obtained by algebraic expressions should be regarded as meaningless if it did not allow a numerical interpretation because a divergent series was involved".[56] This attitude to divergent series, that it should be possible (some day) to make sense of expressions and equations involving them, was according to Jahnke a typical attitude in 18th century analysis.

That Gauss felt a need for increased rigor in this situation, is clear from the following description he gives of the situation in mathematics:

> It is characteristic of mathematics of our modern times (contrary to antiquity) that our sign language gives us a lever that reduces the most complicated arguments to a certain mechanism. In this way science has gained infinitely in richness, but as the business is usually run, it has lost equally much in beauty and solidity. How often is this lever used only mechanically, although the authorization to do so in most cases implies certain tacit assumptions. I insist

[54] Weyl (2009, p 125).
[55] Epple (2003, p 291, 292).
[56] Jahnke (2003, p. 108).

> that by all applications of the calculus, by all applications of concepts one should remain conscious about the original conditions, and never without authorization consider the result of the mechanism as one's property. However, the usual trend is that one claims that analysis has a general character […] It is often like that in the case of divergent series. Series have a clear meaning when they converge; this clear meaning vanishes with this condition of convergence, and it changes nothing essential whether one uses the word sum or value.[57]

The image of mathematics as the science of quantity changes, however, profoundly in the late half of the 19th century. Epple writes about this change:

> […] one may reasonably call this change the end of the paradigm of the science of the quantity. Several parallel developments initiated the departure from this paradigm. In great Britain, a tradition of symbolical algebra emerged […]. In the wake of other investigations (by Ernst Kummer, Hermann Grassmann, William Rowan Hamilton and many others), the notion of number was gradually extended far beyond its earlier limits. […]
>
> By constructing new mathematical entities that could no longer be subsumed under the traditional notion of quantity, these developments exploded the *extension* of this concept.[58]

With the endeavor towards increased rigor in mathematics that begins with the work of Gauss, Cauchy, Weierstrass and others in the 19th century, the influence of the symbolic conception of mathematics is obvious and therewith also the endeavor towards a non-ontological outlook. Mathematical analysis was separated from geometry. Gaps were found in Euclid's arguments, and in addition, as a consequence of the invention of alternative geometries, Euclid's authority was questioned. It was felt that basic results in analysis (such as, for instance, the intermediate value theorem) should be given a firmer formal basis than being based on geometric intuition. Geometric intuition was also challenged by the invention (by Weierstrass and others) of functions that were continuous but nowhere differentiable.

In 18th century mathematics there was no sharp division between pure and applied mathematics. Most mathematicians also worked in theoretical physics. This fact reflected itself in the language of mathematics, in the prose of mathematics. Does a certain word such as, for instance, the word 'quantity' derive its meaning from its use in physics, or does it have its meaning from the way it is operated with in the mathematical calculus, i.e. does its mathematical sense coincide with its sense as a symbol of the calculus? It was only after the influence of the symbolic view of mathematics that this question could be raised, because an essential feature of the symbolic point of view was the logical separation of a symbolic system from its application to some subject-matter outside pure mathematics. This is why it is sometimes neces-

[57] Quotation from Lützen (2003 .p. 173).
[58] Epple (2003, p. 291).

sary to deprive words of their meaning in order to get clear about where the boundary between a symbolic mathematical system and its application goes. Even a word of ordinary language may have the role of a symbol in a calculus (such as, for instance, the words 'point' and 'line' in Hilbert's system in *Grundlagen der Geometrie*).

In Weierstrass' work on the arithmetization of analysis he is still using the word 'quantity', but it is clear that the sense of the word, as he is using it, is the sense it has as a symbol in the arithmetical calculus he is working with. Epple makes this point as follows: "Weierstrass continued to use the notion of quantity, but expressions like "arithmetical quantity" or "number quantity" made clear what he had in mind: a logical separation of his concepts from their more intuitive counterparts in geometry and physics." [59]

It is thus an important feature of the symbolic point of view to sharply separate a calculus or symbolic mathematical system, from its application to some independently given subject-matter outside the system. This attitude towards the relation between pure and applied mathematics was in keeping with a more general tendency in 19th century mathematics, which Lützen describes as the existence of "A process of emancipation of mathematics from science" and this process "added to the feeling that the foundations of analysis had to be revised. […] It became important to give mathematics, including analysis, a solid foundation of its own, independent of applications."[60]

It is interesting that one important motivation for the rigorization of analysis, was problems concerning the teaching of mathematics. According to Lützen: "Several mathematicians found themselves in an awkward situation when they had to teach the introduction to analysis, and therefore they decided to reform it."[61] This was, according to Lützen, the direct background for Cauchy's and Weierstrass's reforms and of Dedekind's and Méray's construction of the real numbers. It seemed most natural from a pedagogical point of view to introduce basic notions of analysis, such as the notion of infinitesimal magnitude, through their applications in physics. The 'awkward situation' was the impression that the correctness of the basic concepts of pure mathematics seemed to depend on their successful applications in physics – as if pure mathematics was at bottom not really pure.

The process of "emancipation of mathematics from science" did manifest itself in the organization of higher education in mathematics in Germany in the 19th century. Lützen explains: "[…] high schools and universities rather than technical high schools became the centres of mathematical training and

[59] Epple (2003, p. 296).
[60] Lützen (2003, p. 155,156).
[61] Lützen (2003, p. 155).

research. Combined with the neo-humanist movement, this led to the development of pure mathematics as an independent field."[62]

In Cauchy's work on reforming analysis in his *Cours d'analyse* it is clear that the symbolic point of view is present in an essential way. This is true in particular in Chapter VII where he introduces imaginary numbers. According to Bottazzini, "Cauchy took the ontological *problem* concerning the nature of imaginary numbers much more seriously than anyone before him. In the *Cours d'analyse* he introduced them in a formal manner as 'symbolic expressions' given by 'any combination of algebraic signs that do not signify anything in themselves or to which one attributes a value different from that which it naturally has'.[63] Cauchy does not solve the "ontological problem" about imaginary numbers by giving a positive answer to the question of their "ontological nature", but by making the ontological problem disappear in the light of a rigorous symbolic approach to the nature of imaginary numbers. And Cauchy's approach required hard work. Bottazzini reports that "it took him no less than fifty-five pages to […] define algebraic operations on 'expressions' like $\alpha + \beta\sqrt{-1}$ (α and β being real quantities) in a rigorous way and to establish their properties." In Bottazzini's judgement, "Chapter VII of the *Cours* can be considered one of the places where Cauchy displayed his concept of rigor best."

The symbolic algebra that was developed by George Peacock and Augustus de Morgan was, in a sense, an expanded symbolic approach. The algebraic symbolism was not just an *arithmetic*-algebraic symbolism. The symbols of algebra were no longer understood as representing necessarily numbers or magnitudes. In the chapter headed "On symbolic algebra" in de Morgan's book on *Trigonometry and Double Algebra*, he writes:

> It is most important that the student should bear in mind that, with one exception, no word nor sign of arithmetic or algebra has one atom of meaning throughout this chapter, the object of which is symbols, and their laws of combination, giving a symbolic algebra which may hereafter become the grammar of a hundred distinct significant algebras. If any one were to assert that + and − might mean reward and punishment, and *A*, *B*, *C*, etc., might stand for virtues and vices, the reader might believe him, or contradict him, as he pleases, but not out of this chapter. The one exception above noted, which has some share of meaning, is the sign = placed between two symbols as in $A = B$. It indicates that the two symbols have the same resulting meaning, by whatever steps attained. That *A* and *B*, if quantities, are the same amount of quantity; that if operations, they are of the same effect, etc.

[62] Lützen (2003, p. 155). It seems clear to me that it was the symbolic point of view, with its algebraic methods and techniques, that made this development of mathematics into an independent field possible. This development can, for this reason, be said to support the claim that "modern mathematics is symbolic mathematics".
[63] Bottazzini (2003, p. 217).

This is reminiscent of what Hilbert wrote to Frege in the so-called Frege-Hilbert controversy:

> [I]t is surely obvious that every theory is only a scaffolding or schema of concepts together with their necessary relations to one another, and that the basic elements can be thought of in any way one likes. If in speaking of my points I think of some system of things, e.g. the system: love, law, chimney-sweep … and then assume all my axioms as relations between these things, then my propositions, e.g. Pythagoras' theorem, are also valid for these things. In other words: any theory can always be applied to infinitely many systems of basic elements.[64]

The system of symbolic algebra of de Morgan is not a theory *about* anything external to the system. It is a self-standing mathematical symbolism that can be applied to other independently existing subject-matters.

One of the most clear and distinct examples of the use of the symbolic point of view is the mathematician Johannes Thomae's concept of "formal arithmetic" presented in the introduction of the 2nd edition to his book *Elementare Theorie der analytischen Functionen einer complexen Veränderlichen,* published in 1898. As Thomae uses the word 'formal' it is essentially synonymous with 'symbolic', as this word has been used in mathematics ever since Vieta.

What makes Thomae's clarification of formal or symbolic arithmetic exceptional is his use of the game of chess comparison in order to make clear the relevant notion of 'form' in formal arithmetic. With the game-comparison he brings in the aspect of mathematics *as activity, as operational practices* (in opposition to mathematics as doctrines, laws, disciplines, theories), which is so important in the symbolic point of view. Thomae summarized his standpoint as follows:

> The formal conception of numbers sets itself more modest limitations than does the logical conception. It does not ask, what are and what shall the numbers be, but it asks, what does one need about numbers in arithmetic. For the formal conception, arithmetic is a game with signs which one may call empty; by this one wants to say that (in the game of calculation) they have no other content than that which has been attributed to them concerning their behaviour with respect to certain rules of combination (rules of the game). Similarly, a chess player uses his pieces, he attributes to them certain properties which condition their behaviour in the game, and the pieces themselves are only external signs for this behaviour. To be sure, there is an important difference between the game of chess and arithmetic. The rules of chess are arbitrary, the system of rules for arithmetic is such that by means of simple axioms the numbers can be related to intuitive manifolds, so that they are of essential service in the knowledge of nature. – The formal standpoint relieves us of all metaphysical difficulties, this is the benefit it offers us.[65]

[64] Letter to Frege of December 29, 1899, as excerpted by Frege in Frege (1980, p. 40).
[65] Quoted from Epple (2003, p. 301).

Thomae here begins by dissociating himself from the questions of the ontological nature of numbers on the grounds that answers to these questions are not needed in arithmetic. The ontological issues arise in discussions of the logical or philosophical foundation of arithmetic, but not in arithmetic as a mathematical practice. And don't we have to agree with Thomae here? What would be an example of a *mathematical* problem in arithmetic, be it in elementary arithmetic or in advanced research-arithmetic, for which the ontological nature of numbers (as discussed by philosophers) would make a difference?

In arithmetic, seen as a game with signs, the signs are said to be, in one sense, empty of content. But in another sense *they do have content*. They don't have a content in the game that comes from explanations or translations given in mathematical prose (using the words 'quantity' and 'magnitude'). The arithmetical calculus, like the game of chess, is autonomous. But *the signs do have content in the calculus, namely the 'content' attributed to them concerning their function and behavior with respect to the rules of the calculus*. It is this content that constitutes the *forms* of formal (or symbolic) arithmetic. These forms are not features of the signs as objects of visual perception, they are rather *forms of the use* of signs.

The words 'sign' and 'symbol' are often used as synonymous words. But here it is important to distinguish between a symbol as a form of use of a sign, and the 'sign' as the external and immediately perceivable mark for this form of use. The difference becomes clear in the game of chess comparison. By a chess piece we may mean a certain immediately perceivable visual object with a certain colour, shape and size that distinguishes it from other pieces in the game, but we also talk about a chess piece as *an object with properties that condition its behavior* in the game, i.e. as a piece with which you can make certain moves in the game but not others. The word 'formal' in Thomae's "formal arithmetic" refers to the behavior of arithmetical 'chess-pieces' in the latter sense.

Thomae was not the only mathematician who used the word 'formal' in this way around the end of the 19th century. This raises of course the question if the symbolic view of mathematics coincides with formalism. It is difficult to give a short answer to this question since the word 'formalism' has often been used in a superficial, pejorative sense (e.g. by Brouwer and Frege), sometimes even as a caricature of the symbolic view of arithmetic, such as, for instance, when it is said that a formalist considers arithmetic or geometry "as games with empty signs". Note that Thomae hestitates in the quotation above about using the word 'empty'. The symbols of his formal arithmetic are not empty; they have a content determined by the forms of their use – not by possible applications of the system, or by semantical explanations.

The mathematician Hermann Grassmann uses the words 'form' and 'formal' in a way similar to Thomae's in Grassman's book *Die lineale*

Ausdehnungslehehre, published in 1844. The book begins with an introduction in which Grassmann gives an account of his conception of mathematics. He disassociates himself from the ontological conception of logic and mathematics by making a basic division of sciences in real (*Reale*) and formal (*Formale*): Thinking in the real sciences always takes place with respect to an independently existing subject-matter outside thinking. In mathematics, being a formal science, the subject-matter of thinking is posited by thinking itself and in a second act of thought it is made the object of study of mathematics as a formal science. The object of study of pure mathematics is *forms of thought (Denkformen)*. Pure mathematics is *Formenlehere* (doctrine of forms), says Grassmann. Now, if thinking is of a linguistic nature (which was also a common idea by this time); if thinking is operating with signs (including pictures and figures), this would mean that the *Denkformen* of pure mathematics are forms of the use of signs.[66]

Hilbert comes close to Grassmann's idea when he says about the 'formula game' of his proof theory:

> This formula game is carried out according to certain definite rules, in which the *technique of our thinking* is expressed. These rules form a closed system that can be discovered and definitively stated. The fundamental idea of my proof theory is none other than to describe the activity of our understanding, to make a protocol of the rules according to which our thinking actually proceeds. [67]

[66] With his "Ausdehnungslehre" (or extensive, or geometric algebra) Grassmann initiated a new research orientation in modern symbolic mathematics, which would get many applications in theoretical physics. The following historical account comes from *Wikipedia, the free encyclopedia* under the title-word: 'geometric algebra':"GA [*geometric algebra*] in the sense used in this article was not developed until 1844, when it was used in a *systematic way* to describe the geometrical properties and *transformations* of a space. In that year, Hermann Grassmann introduced the idea of a geometrical algebra in full generality as a certain calculus (analogous to the propositional calculus) that encoded all of the geometrical information of a space. Grassmann's algebraic system could be applied to a number of different kinds of spaces, the chief among them being Euclidean space, affine space, and projective space. Following Grassmann, in 1878 William Kingdon Clifford examined Grassmann's algebraic system alongside the quaternions of William Rowan Hamilton. From his point of view, the quaternions described certain *transformations* (which he called *rotors*), whereas Grassmann's algebra described certain *properties* (or *Strecken* such as length, area, and volume). His contribution was to define a new product — the *geometric product* — on an existing Grassmann algebra, which realized the quaternions as living within that algebra. Subsequently Rudolf Lipschitz in 1886 generalized Clifford's interpretation of the quaternions and applied them to the geometry of rotations in *n* dimensions. Later these developments would lead other 20th-century mathematicians to formalize and explore the properties of the Clifford algebra. [...] Progress on the study of Clifford algebras quietly advanced through the twentieth century, although largely due to the work of abstract algebraists such as Hermann Weyl [Brauer and Weyl (1935)] and Claude Chevalley. […] In physics, geometric algebras have been revived as a "new" way to do classical mechanics and electromagnetism, together with more advanced topics such as quantum mechanics and gauge theory. David Hestenes [Hestenes, and Sobczyk, (1984)] reinterpreted the Pauli and Dirac matrices as vectors in ordinary space and spacetime, respectively, and has been a primary contemporary advocate for the use of geometric algebra.

[67] Hilbert (1927, p. 475).

A very pertinent formulation of the symbolic view of mathematics was given by the French mathematician and philosopher Luis Couturat in his book *De l'infini mathématique* (Paris, 1896 , p. 49). Like Thomae, Couturat also uses he game of chess comparison:

> A mathematician never defines magnitudes [or numbers] in themselves, as a philosopher would be tempted to do; he defines their equality, their sum and their product, and these definitions determine, or rather constitute, all the mathematical properties of magnitudes. In a yet more abstract and more formal manner he lays down *symbols* and at the same time *prescribes* the rules according to which they must be combined; these rules suffice to characterize these symbols and to give them a mathematical value. Briefly, he creates mathematical entities by means of arbitrary conventions, in the same way that the several chessmen are defined by the conventions which govern their moves and the relations between them.[68]

If formalism is a concern with form, and form is understood as in Thomae, Grassman, Couturat as *forms of use* of signs, as relating to the *function* and *behavior* of signs in a calculus, then clearly formalism is closely related to the symbolic view of mathematics. But in the philosophical discussion about formalism in the 20th century (a discussion which has been dominated by mathematical logic in its foundational status), formalism tends to be understood against the background of the ontological view in which mathematical propositions are taken to have a descriptive content – a mathematical system or theory is seen as a body of truth, or a body of knowledge about some independently existing subject-matter. In this picture the aspect of mathematics as human activity, as operational practices, tends to disappear, and so does also the notion of a *symbol* as something else than a sign. A mathematical symbolism is often misunderstood as a system of notation with a naturalistic conception of signs and expressions. As a result, formalism becomes a kind of caricature of the symbolic view. It is 'formalism' in that pejorative sense that Frege is attacking when he says:

> In order to produce it [an infinite series] we would need an infinitely long blackboard, an infinite supply of chalk, and an infinite length of time. We may be censured as too cruel for trying to crush so high a flight of the spirit by such a homely objection; but this is no answer.[69]

Heine and Thomae made some less successful remarks that might seem to invite this objection. But it is doubtful if anyone has seriously held a formalist view of mathematics that would make this remark into a fair objection. In Frege's critique of the formalists, it is clear that he had an ontological con-

[68] Quotation in English from Bell (1937, p. 624).
[69] Frege, (1960, p. 219).

ception of arithmetic which he never questions.[70] He emphasizes arithmetic as a science (rather than as an activity or practice), and the subject-matter of this science is the abstract objects that are the 'reference' of the number signs. It is, for Frege, on the basis of our knowledge of the sense and reference of the number signs that the rules of arithmetic are justified. Wittgenstein's remark about Frege's critique of the formalists quoted in Section 5 seems to me very much to the point.

There is also a notion of formalism that has been called "Hilbert's formalism", but this "formalism" is rather an unfair polemic picture of Hilbert's views for which Brouwer was originally responsible. According to Georg Kreisel: "We note at once that there is no evidence in Hilbert's writings of the kind of formalist view suggested by Brouwer when he called Hilbert's approach "formalism".[71] This is to some extent an exaggeration. Hilbert never called himself a formalist, but some of his statements show that he was no doubt influenced by what was called the "formal approach" around the turn of the century. But Kreisel is certainly right in saying that the notion of "Hilbert's formalism" is to a great extent a fabrication of Brouwer's. It is not a very clear notion since Brouwer tends to put every view of mathematics that he dislikes or rejects under the label "formalism". A more interesting view of Hilbert's foundational work is given by Hermann Weil when he suggests a reading of Hilbert's proof-theoretical program with its "formula game" as being a symbolic construction.[72]

Frege's and Brouwer's critique of formalism manifests a blindness towards the symbolic aspect of mathematics. This blindness is reinforced in the 1930's through the notion of 'logical syntax' and the sharp division between syntax and semantics attached to it, which has become generally accepted and very influential in the literature of logic and analytic philosophy since the 1930's. The crucial feature of syntax in this sense (not to be confused with what Wittgenstein called 'logical syntax' in the *Tractatus*) is its naturalistic conception of language. The instigators of this syntax-terminology, Carnap, Gödel and Tarski, were very explicit about its naturalistic character. As already mentioned, Carnap said that "syntax, pure and descriptive, is nothing more than the mathematics and physics of language".[73] This approach to language, in particular to the language of mathematics, blocks the aspect of language that the game of chess comparison opens up, such as for instance the difference between sign and symbol. That mathematics is at bottom activity, operational practices, forms of use of signs disappears as something inessential.

[70] Frege, (1960, pp. 182-233).
[71] Kreisel (1958, p. 346).
[72] Weyl (1927, pp. 136-141).
[73] Carnap (1959, p. 284).

Syntax, with the associated division between syntax and semantics, has of course been successful and influential for the invention of new mathematical theories of formal systems (where the words 'form' and 'formal' have the specific syntactical sense), theory of automata, applications in computer science, etc., but it is doubtful if this development has contributed to increased rigor of ordinary mathematics. Hasn't logical semantics, including logical model theory, on the contrary cemented the old ontological view of mathematics?

Let me finally mention one prominent mathematician who explicitly called his own conception of mathematics *formalism,* in a sense that comes close to the symbolic conception of mathematics. It is Felix Hausdorff, who was working in set theory and is considered as one of the founders of topology. His book *Grundzüge der Mengenlehre*, published in 1914 is often described as a ground-breaking work in modern mathematics. But in the first decade of the 20th century Hausdorff expressed (in manuscripts he never published) a view of set theory as a symbolic system without an ontology. It was a view in sharp contrast to Cantor's philosophical understanding of set theory. Hausdorff's work falls within the rigorization movement around the beginning of the 20th century. We will return to Hausdorff in the next section.

The main point of this section has been to argue that an increased awareness and use of the symbolic conception of mathematics, was one essential part of the rigorization of mathematics in the late 19th century.[74]

[74] More examples in support of this claim can be found in Epple (2003).

11 Wittgenstein on calculus and mathematical 'prose'.

There are many examples of the prose/calculus distinction in the strict symbolic point of view that Wittgenstein held, at least in the middle period,[75] when he spoke of mathematics as consisting of symbolic systems, games or calculi that are autonomous systems determined by rules for the operation and transformation of expressions. Again and again he warned about what he called the *prose accompanying the calculus*[76] (the verbal readings in ordinary language of the signs and expressions of the calculus) – especially when this prose does not derive from some application of the calculus to something outside mathematics.

If proximity to ordinary language was an essential feature of ancient Greek mathematics, Wittgenstein's strict symbolic view of modern mathematics is diametrically opposed to this feature. His attitude seems to have been that modern mathematics, despite great progress in the last two centuries, has not yet found its true individuality or authenticity, but is still holding on to antiquated features of the Euclidian-Aristotelian tradition.

He saw, in fact, prose accompanying the calculus as the main source of puzzles and confusion in the discussion of the foundations of *modern* mathematics. The following quotation from *Philosophical Grammar* is one of many passages where he makes this point:

> If you want to know what the expression "continuity of a function" means, look at the proof of continuity [of functions]; that will show what it proves. Don't look at the result as it is expressed in prose, or in the Russellian notation, which is simply a translation of the prose expression; but fix your attention on the calculation actually going on in the proof. The verbal expression of the allegedly proved proposition is in most cases misleading, because it conceals the real purport of the proof, which can be seen with full clarity only in the proof itself.[77]

By the 'Russelian notation' he means the mathematical logic of *Principia Mathematica*, in which a formula such as $(\exists x)\, fx$ is translated and explained as the verbal sentence "There exists an object x which has the property f ",

[75] See Stenlund (2012).
[76] PG, (p. 324).
[77] PG, (p. 369-370).

which we do understand as an *English sentence* (or a form of English sentences). But this is a very vague sense of understanding, since it is based only on the doctrine of sentence construction of English, and that doctrine says nothing, or very little, about the use of the sentence and the contexts of its use. Does it mean the same when the variable *x* ranges over a finite domain, as when any number can be a value of the variable? Through mathematical logic *any* proposition can be represented in the mathematical notation of modern logic, "and this makes us feel obliged to understand it. Although of course this method of writing is nothing but the translation of vague ordinary prose." (RFM p. 299). So one important example of 'prose accompanying the calculus' is the ordinary language expressions used in the translation of the signs and formulas of the predicate calculus into verbal language.

In his *Notebooks* Wittgenstein said that "My whole task consists in explaining the nature of the proposition" (NB, p. 39). But it was clear to Wittgenstein from the start that the propositions explained in the *Tractatus* do not include mathematical propositions. The propositions of the *Tractatus* express possible states of affairs about objects that are the substance of the world, but the statements of mathematics in the *Tractatus* do not express possible states of affairs about independently existing objects of a mathematical realm. A symbolic conception of arithmetic is present already in the *Tractatus*. The opposite view in which mathematical and non-mathematical propositions are propositions in the same sense (which is the more or less established view in the branch 'mathematical logic and the foundations of mathematics') is based on the similarities in the readings in "vague ordinary prose" of mathematical and non-mathematical statements.

If persuasion was ranked above precision in Greek mathematics, it is clear that Wittgenstein wanted to reverse this ranking, provided that 'precision' means conceptual precision. This precision is not achieved by merely changing notation, i.e. using mathematical notation that is translated into "vague ordinary prose". Such a change may result rather in a deceitful feeling of persuasiveness about an achieved precision.

One difficulty with understanding the last quoted passage is Wittgenstein's use of the word 'proof'. Many of Wittgenstein's examples of proofs in RFM are really only calculations, while the established use of the word 'proof' among philosophers, logicians as well as professional mathematicians is still following the Aristotelian-Euclidian tradition, and then a proof tends to be contrasted with a calculation (Poincaré calls calculation 'verification' and contrasts verification with proof). A proof in the Euclidian sense is essentially expressed in verbal language, *in mathematical prose*. It consists of a sequence or a pattern of propositions, related to one another by rules of inference. This conception of a proof was questioned in the *Tractatus* on the basis of the distinction between saying and showing, which was one of Wittgenstein's tools in the *Tractatus* for clarifying the logical structure of lan-

guage in general from a more strict symbolic point of view in which the notions of *form* and *operation* are essential. A form as Wittgenstein uses this word (even in his later philosophy), cannot be *described*: it can only be *presented*. (PR, p. 208). A form is not a property of a substrate. So there is a problem here, for instance, with the form of 'actually infinite sets'. How can there be actually infinite sets if their form cannot be presented? They must presuppose an "underlying imaginary symbolism" (PG, 470)? The idea of mathematical entities outside any symbolism was incomprehensible to Wittgenstein's symbolic view of mathematics. It would perhaps be comparable to the idea of (partly) human-like beings, having no bodies, like spirits or angles.

It was a fundamental idea about arithmetic in the *Tractatus* that the logical forms of the propositions of language have an arithmetical structure. The logical forms are formal or internal properties and relations that show themselves in the symbolism, in how they are generated by the operations in the universal calculus of propositions in the *Tractatus*.

The concept of an operation and a series generated by successive applications of an operation is the basis for the *Tractatus'* conception of arithmetic. A number is explained as a specific kind of symbol, namely as an exponent of an operation. (TLP 6.021).

In the change that takes place in Wittgenstein's thinking in the 1920's, the universal calculus of the *Tractatus* loses its privileged position, and becomes one calculus among many others. But what does not change is his interest in mathematics as methods and techniques for operating with and transforming expressions and symbols. It is against this background that Wittgenstein prefers to talk about a mathematical system as a calculus, rather than as a theory about something, or a deductive system of propositions having descriptive contents.

So when Wittgenstein is questioning the use of prose in mathematics as being a source of confusion, he is questioning the (non-symbolic) conception of proposition and proof with roots in the Aristotelian-Euclidian tradition. Many of the verbal expressions that belong to the prose accompanying the calculus have their origin in philosophy, not least in the Aristotelian-Euclidean tradition.

In the following remark Wittgenstein suggests a method for separating calculus and (inessential) prose:

> In set theory what is calculus ought to be separated from what claims to be (and of course it cannot be) *theory.* The rules of the game have thus to be separated from inessential statements about the chessmen.
>
> Frege replaced those signs in Cantor's alleged definitions of "greater", "smaller", "+", "-", etc., with new words, to show that here there wasn't any real definition. In the same way, in all of mathematics one could replace the usual words, especially the word "infinite" and its cognates, with entirely new and hitherto meaningless expressions, in order to see what the calculus

> with these signs really achieves and what it doesn't achieve. If the idea was widespread that chess gave us information about kings and castles, I would propose to give the pieces new shapes and different names, so as to demonstrate that everything belonging to chess has to be contained in the rules.[78]

What such an investigation of the word "infinite" would reveal, for instance, is that there is often a confusion of the mathematical sense of 'infinite' and the use of the word 'infinite' as a superlative for something finite that strikes us as enormously large, as when we say "The number of trees along the road looks infinite", or "The number of stars in the sky is infinite". And then we are in danger of accepting the picture of an infinite sequence in mathematics as if it were an extremely long finite sequence, as if an infinite sequence in mathematics has an end, but one that it is 'infinitely far away'. But the expression 'infinitely far away' is not a measure of a distance or a length, it is a superlative we use about extremely large finite distances. That sense of 'infinite' does not achieve anything in the calculus of set theory.

The separation of calculus and prose is an important part of Wittgenstein's conceptual (or grammatical) investigation. And it is a difficult part since we do not survey and are not aware of the ways in which the use of 'prose-expressions' in a mathematical system affects our understanding of the system as a whole.

In the late half of the 19th century it became more and more obvious that the formal deductive structure in Euclid's elements was not really 'formal'. Many arguments did depend on the prose-meaning of words (such as for instance the word 'between'). It was clear that the 'formal deductive' style in Euclid also had a tacit rhetorical purpose.

This is what Hilbert's axiomatization of geometry tried to correct. But Hilbert still followed the tradition in viewing pure geometry as a *theory about something* (about some 'unspecified things', as he expressed it), and he was led to the idea of the axioms as propositions defining the notions involved. Einstein saw the possibility of viewing geometry as a pure calculus, an autonomous system (in which the axioms are rules for the operation with signs and figures), and which, as a pure calculus, is not 'about anything outside the calculus', but which can be *applied* to things in nature, for instance, in the description of physical space.[79]

One of the first mathematicians who clearly expressed the view of geometry as an autonomous system, independent of intuition or any empirical basis, was Felix Hausdorff. In a manuscript with the title *Formalism* from around 1904, he says about mathematics, and geometry in particular, that "The most important and fundamental task of modern mathematics has been to set itself free from this dependency [on intuition and empirical bases], to

[78] PG, (p. 468-469).

[79] Einstein's theory of relativity was no doubt a source of inspiration for Wittgenstein in the middle period (RFM VI, § 28). See also Stenlund (2012, p. 149-152).

fight its way through from heteronomy to autonomy."[80] Leo Corry summarizes Hausdorff's view as follows:

> This autonomy, so fundamental for the new view of mathematics predicated by Hausdorff and *widely adopted later on as a central image of twentieth-century mathematics*, was to be attained precisely by relying on the new conception of axiomatic systems embodied in *GdG* [Hilbert's *Grundlagen der Geometrie*]. […] Pure mathematics, under this view, is a 'free' and 'autonomous' discipline of symbols with no determined meaning. Once a specific meaning is accorded to them, we obtain 'applied' mathematics. Intuition plays a very important heuristic and pedagogical role, but it is inexact, limited, misleading and changing, exactly the opposite of mathematics.[81]

In the beginning of the 1930th Wittgenstein was, no doubt, influenced by this modernistic "central image of twentieth-century mathematics". The "inexact, limited, misleading, and changing" intuition mentioned here comes to expression in what Wittgenstein calls 'prose'.

But there is also a philosophically important difference between a professional mathematicians' attitude (such as Hausdorff's) to the new axiomatic method and Wittgenstein's. Wittgenstein never saw it as his concern to express opinions about the final or ideal shape of geometry and arithmetic as *disciplines* of the *science* of mathematics, which was a natural concern for a professional mathematician for whom mathematics is research-mathematics and higher mathematical education (while the rest is 'trivial mathematics'). For Wittgenstein elementary mathematics, seen as established practices, methods and techniques of a wider circle of mathematical agents than the circle of professional mathematicians, is the rock-bottom of mathematics. This is also a reason why the word 'calculus' is more appropriate than the word 'discipline' for Wittgenstein.

This difference reflects Wittgenstein's attitude to the idea of "the foundations of mathematics". The foundations that are Wittgenstein's concern are *already there* in the established mathematical activities and practices and the task is to get clear about it – and that reflection over elementary mathematical practice is not a mathematical task. The task is not to *invent* a foundation for mathematics through mathematical construction (such as *Principia Mathematica* or axiomatic set theory), or to propose a mathematical research program (such as the intuitionistic constructivization of mathematics or Hilbert's proof-theory). The roots of the worries about the foundations of mathematics are according to Wittgenstein *our lack of a clear view of the work-*

[80] Quoted in Corry (2006, p. 148)

[81] Corry (2006, p. 148-149, my emphasis). It is interesting that Hilbert was less inclined to view his *Grundlagen der Geometrie* as the paradigm for a formalistic view of mathematics, of geometry in particular, than Einstein and Hausdorff and many others. As Corry makes clear, Hilbert often questioned the autonomy of geometry and claimed that geometric knowledge has an empirical basis.

ings of the mathematical symbolism (caused partly by the role of prose in mathematics). And this lack of a clear view manifests itself in the problems, or puzzles (as he calls them) he is concerned with in his writings and lectures on the foundations of mathematics. This is one reason why Wittgenstein's style of writing about the foundations of mathematics is so different from the normal styles of writing in this branch. In order to accentuate that he is *not* doing 'foundations of mathematics' in the mathematician's sense, he points out in the first lecture of the series of lectures he gave on the foundations of mathematics in Cambridge in 1939 that "I *am* going to talk about the interpretation of mathematical symbols, but I will not give a new interpretation." And about the puzzles he will deal with he says that "all the puzzles I will discuss can be exemplified by the most elementary mathematics."[82]

Wittgenstein is very clear about his attitude to the foundations of mathematics in the following remarks:

> What does mathematics need a foundation for? It no more needs one, I believe, than propositions about physical objects – or about sense impressions, need an *analysis*. What mathematical propositions stand in need of is a clarification of their grammar, just as do those other propositions.
>
> The *mathematical* problems of what is called foundations are no more the foundation of mathematics for us than the painted rock is the support of a painted tower. (RFM, p. 378)

This remark, and similar critical remarks about mathematical logic and the foundations of mathematics, may make it look as though Wittgenstein's philosophy of mathematics is exclusively critical and negative. But the positive message is Wittgenstein's symbolic conception of mathematics which he did not launch as a new branch of the foundations of mathematics, since it already existed within mathematics since many centuries and only needed to be clarified. The symbolic view is an important feature of the most progressive development of the mathematics created in the last three centuries up to and including the mathematics of quantum physics (as I shall argue in the last section). But it needs to be made clear since it is obscured by various other trends – such as mathematical logic in its foundational status. Wittgenstein could not help seeing that in the light of his symbolic conception, mathematical logic in that rôle stands out as a remnant from an antiquated ontological conception of mathematics of the past.[83]

Wittgenstein tended, however, to exaggerate the calculus/prose distinction in the beginning of the thirties, when he was inclined to see mathematics as

[82] Diamond (1976, p. 13, 14).

[83] In RFM, (p. 300) Wittgenstein says: " 'Mathematical logic' has completely deformed the thinking of mathematicians and of philosophers, by setting up a superficial interpretation of the forms of our everyday language as an analysis of the structures of facts. Of course in this it has only continued to build on the Aristotelian logic."

pure calculus.[84] Maybe this exaggeration was an effect of how Wittgenstein was influenced by what Corry called "a central image of twentieth-century mathematics" in the quotation given before. In any case, there is a change on this point in Wittgenstein's thinking in the middle of the 30th when a more anthropological point of view enters in connection with his work on rule-following and as a result of criticism by Piero Sraffa. Wittgenstein realized that mathematics is not a sharply delimited field. The use of mathematical signs in applications outside mathematics contributes to the meaning of mathematical signs.[85] But the calculus/prose distinction is still important in his later work, though in a less dogmatic sense. Similarities between statements in prose or in verbal language are still in many cases the source of *false or misleading analogies*.

Let us look at one of Wittgenstein's examples that shows how prose, or verbal language, may mislead us into (or may seem to justify) a belief in an ontological realm of mathematical objects, having independent existence. Consider the two sentences

A human being has two eyes

and

A quadratic equation has two roots[86]

Due to the similarity in the verbal form of the two statements, it may be tempting so say: as the first proposition is about objects outside mathematics (eyes of a human being), so the second is about independently existing mathematical objects (numbers that are roots of a quadratic equation). And we would be led to an ontological realm of numbers.

But looking at the *concept* of the roots of equations we see that numbers that are roots of quadratic equations do not exist independently of mathematics, but only in the context of the arithmetical-algebraic symbolism. The roots of an algebraic equation of the form $x^2 + ax + b = 0$ are given by two *different* algebraic expressions. (This holds despite the fact that any equation of the form $(x - a)^2 = 0$, with $a > 0$ has, in a sense, only one root, but as the *concept* of the roots of a quadratic equation is determined in the algebraic symbolism, this means that the *two* roots of these quadratic equations happen to coincide.)

[84] In PG (p. 468), written in the beginning of the 30th, he says: "Mathematics consists entirely of calculations. In mathematics *everything* is algorithm, and *nothing* is meaning, even when it doesn't look like that because we seem to be using *words* to talk *about* mathematical things. Even these words are used to construct an algorithm".

[85] In RFM (p. 257, written in 1942) he says:"It is the use outside mathematics, and so the *meaning* of the signs, that makes the sign-game into mathematics."

[86]Wittgenstein discusses a similar example in Diamond (1976, p. 150).

It is the similarity in the verbal form of the statements of number outside and inside mathematics that makes it appear as though they were results of "applying mathematics" in the same sense. That is the false analogy. The decisive thing is that it is only the application of arithmetic to something independently given outside mathematics (such as eyes of people) that deserves to be called *applied mathematics*, and which is opposed to *pure mathematics.* The statement "A quadratic equation has two roots" is a statement of pure mathematics that is based on an algebraic proof, and not on counting objects in some realm outside the arithmetical-algebraic symbolism.

12 Leibniz and blind thought

There is a similarity between Wittgenstein and Leibniz, concerning the difference between calculus and prose. Recall the quotation above when Wittgenstein says:

> In set theory what is calculus ought to be separated from what claims to be (and of course it cannot be) *theory.* The rules of the game have thus to be separated from inessential statements about the chessmen.

He continues by suggesting a method for making the separation between the role of the word 'infinite' as a piece in the game or calculus of set theory, and its role as a prose expression with a 'linguistic meaning' of its own in statements about the 'chess-pieces', statements that purport to refer to something independently existing beyond the calculus, something that set theory is taken to be a 'theory about'.[87]

Leibniz was concerned with a similar problem with his infinitesimal calculus when he was criticized for not making clear what the infinitesimal calculus 'is about'. By not distinguishing calculus and mathematical prose the critics were misled by the prose into seeing the infinitesimal calculus as a theory about something, such as infinitesimal magnitudes. Leibniz reacted to criticisms of the differential calculus for having an unclear foundation, and for the uncertain status and nature of the infinite and the infinitesimals by saying that "…it is unnecessary to make mathematical analysis depend on metaphysical controversies or to make sure that there are lines in nature which are infinitely small in a rigorous sense in contrast to our ordinary lines, or as a result, that there are lines infinitely greater than our ordinary

[87] The prose meaning of 'infinite' often invites a (false) analogy between an infinite sequence and an *extremely large* finite sequence – as if an extremely long sequence of numbers of the form 1, 2, 3, …, n is 'closer to' or 'more similar to' the sequence of all natural numbers than a short finite sequence of this form. It is on the basis of this analogy that it seems to make sense to conceive of the set of natural numbers as an *extension* (in which the generation of numbers by iteration of the successor operation "has been completed", i.e. carried out to the end which the sequence of naturel numbers does not have!). It seems to me that the normal meaning (for most logicians) of a proposition beginning with a universal quantifier which ranges over the set of natural numbers N, is to think of N as an extension.

The mathematician Niels Henrik Abel was very dissatisfied with the theory of infinite series of his times. He complained that "One applies all operations to infinite series as though they were finite, but is that permissible? Hardly. – Where is it proved that one gets the differential of an infinite series by differentiating each term?" (Quoted in Lützen, 2003, p. 177).

ones, yet with ends."[88] As Krämer points out[89]: "Leibniz emphasized that the inner coherence of his calculus does not depend on the metaphysical dispute how to interpret the differential symbolism. To calculate correctly does not presuppose an answer to the question of whether an infinitesimal magnitude exists as an actual or as a potential infinity." Herman Weyl makes a similar point: "… many of the Leibnizian statements sound as if his view of the infinitely small is that it […] cannot be given a reasonable contentual interpretation, yet that nonetheless all things stand as if it [the infinitely small] did actually exist. All that matters for the mathematicians is that it fits into the calculus of signs without contradiction."[90]

One of Leibniz' great achievements was his clarification of the notion of calculus on the basis of the distinction he made between two kinds of thinking: *symbolic or blind thought*, and *intuitive thought*. 'Intuitive thoughts' is what Leibniz often refer to as *ideas*. Starting out from limitations of human mental capacities, he explains the distinction as follows:

> …especially in longer analysis, we do not intuit the entire nature of the subject matter at once but make use of signs instead of things, […] Thus when I think of a chiliogon, or a polygon of a thousand equal sides, I do not always consider the nature of a side and of equality and of a thousand (or the cube of ten), but I use these words, whose meaning appears obscurely and imperfectly to the mind, in place of ideas which I have of them, because I remember that I know the meaning of the words but that their interpretation is not necessary for the present judgment. Such thinking I usually call *blind* or *symbolic*; we use it in algebra and in arithmetic, and indeed almost everywhere. When a concept is very complex, we certainly cannot think simultaneously of all the concepts which compose it. But when this is possible, or at least insofar as it is possible, I call the knowledge *intuitive*.[91]

Here Leibniz goes against Descartes, who emphasized the intuition of clear and distinct ideas at every step in thought and held that thinking of a certain complex concept presupposes permanent intuitive awareness of the concepts that compose it.

If intuition is 'the mind's eye' we understand Leibniz' choice of the expression 'blind thought' for symbolic thought in which intuition is not involved. Symbolic thought occurs, as Leibniz says, 'almost everywhere' but the paradigm cases are the calculi of arithmetic and algebra, such as operating in the decimal place-value system for numbers. This wide use of symbolic thought is similar to Wittgenstein's wide use of the notion of calculus to bring out features of language, but which nevertheless has the calculi of arithmetic and algebra as paradigm cases. A crucial thing about a calculus,

[88] Loemker (1969, p. 542-543).
[89] Krämer (1996, p. 85).
[90] Weyl (1997, p. 139). Ishiguro (1990, Ch V), gives a very penetrating account of Leibniz's notion of the infinitesimal.
[91] Loemker (1969, p. 292).

and therefore of symbolic thought, is that its structural features, the forms of its signs and expressions, are tied to the expressions as *objects of visual perception*, and not to a verbal reading of the expressions as in the rhetorical practices of Ancient mathematics.

One might think that Leibniz notion of intuition and intuitive thought corresponds to the content which comes to expression in what Wittgenstein calls prose, or mathematical prose. But that is not the case. There is, as far as I can see, no counterpart in Wittgenstein thinking to Leibniz's 'intuition'. It is a notion which seems to have its source in Leibniz's metaphysical idealism. But as Esquisabel has pointed out, Leibniz suggests two different kinds of blind thought.[92] In addition to the blind thought that takes the operational practices of arithmetic and algebra as paradigm cases and which Esquisabel calls *symbolic blind thought*, Leibniz recognizes certain contexts, in which the signs are words or sentences of ordinary language and where there is non-intuitive thinking that involves a certain comprehension of linguistic meanings of words and sentences, although it is often confused, vague or even erratic. Esquisabel calls this kind of blind thought *verbal blind thought* and he recognizes it's occurrence as follows:

> This is the case fundamentally when we appeal to ordinary language in everyday life in our social interchanges or in our meditation. In such situations we face another form of blind thought that is characterized by the fact that we have a vague and confused comprehension of the meanings of words or sentences, with no possibility of better specifying that meaning. Thus, our mind moves so to say in a milieu of diffused linguistic comprehension in which meaning cannot be established univocally and firmly.[93]

Leibniz mentions this *verbal blind thought* in connection with problems arising in the interpretation of biblical texts when questions about the truth of faith arise, questions that may be subjected to a variety of interpretations that can be mutually incompatible. Meanings in *verbal blind thought* are loaded with a high degree of confusion and uncertainty, and for this reason it involves according to Leibniz a severe danger for philosophical thinking, namely the threat of talking equivocally and metaphorically about things, about which we have no genuine understanding.

Wittgenstein recognized much the same dangers in the use of prose, in particular in mathematical prose, so it seems to me that the distinction between *symbolic blind thought* and *verbal blind thought* in Leibniz's philosophy, is a clear forerunner of the middle period Wittgenstein's distinction between calculus and prose.

Let us take a look at important features of the algebraic symbolism, in order to get closer to the roots of the calculus/prose distinction. As Krämer

[92] Esquisabel (2012).
[93] Esquisabel (2012, p. 16).

points out, through the algebraic symbolism of Vieta, a new kind of *writing* was introduced, a kind of writing that speaks to the eyes and not to the ears.[94] The essential feature of the algebraic signs is their visual, graphical form. This form is not given through verbal descriptions, definitions or 'rules of formation'; the form of a sign is basically given by being *shown* or *presented*.

For alphabetical writing, on the contrary, there is a transition to spoken language. Alphabetic writing is a sort of representation in visual space of temporal sequences of spoken language. Every letter has a characteristic sound. Learning to read alphabetic text involves learning not just what the letters look like but also how they *sound*. Every word has a pronunciation and each sentence has a reading. Understanding alphabetic writing, you have to be able to read the written text *aloud*, and *hear* what is expressed in it.

The letters A, B, C,… are used by Vieta in his algebraic symbolism as variables or parameters, but the characteristic sounds of these letters are completely irrelevant in the algebraic symbolism of his analytical art.

But even algebraic signs have verbal readings or readings in mathematical prose, these readings are, however, in general irrelevant and often incomprehensible if they are not given together with the algebraic sign and a stipulation of the form: "An expression of this form is what we will call so and so". It is the visual form of the algebraic signs that give sense through stipulation to these verbal readings, and not the other way around. Consider for instance the sign

$$a^2$$

with the verbal reading "a raised to 2". There is nothing in this verbal phrase, taken by it-self that tells us that this sign must look exactly like this: a^2. So the meaning of the phrase "a raised to 2" is determined by the stipulation that *this* graphical form is what we shall call "a raised to 2". The point of having the verbal readings of algebraic signs is that it facilitates communication. A teacher who is demonstrating how solve a quadratic equation on the blackboard for his students, will use this verbal reading of the algebraic symbols in addressing the students. But if this teacher had solved the same equation for himself on paper, he would not have said a word but just calculated.

So let me summarize: Algebraic writing is not based upon a transition from written to spoken language or conversely (as in the case of alphabetic writing). It speaks directly to the eye. The algebraic sign is a visual, graphical structure which is immediately accessible to the human eye. The form of a sign is given by it being shown or presented.

[94] Krämer (1996, p. 84)

Signs in this sense was, it seems to me, what Leibniz meant by *characters*. Leibniz talks about the signs of his calculi as characters. He says, for instance:

> The human mind cannot advance far in reasoning without resorting to characters. And characters, when they are adequately chosen, have this marvellous property: they leave so to say visible marks of our thoughts on paper, and [thus] we provide ourselves with the means of being infallible.[95]

Another notion is that of an algebraic *symbol* (which must not be confused with an algebraic sign). The important thing here is that the algebraic symbolism is an *operative symbolism. A symbol is determined by how we operate with the sign for it.* Or, as Wittgenstein expresses his concept of a symbol in the *Tractatus*:

> In order to recognize the symbol in the sign we must consider the significant use. (T 3.326)

Consider for instance the rule

$$a^2 = a \times a$$

which is one of the rules that determines the sign 'a^2'as a symbol. By this rule it is connected to the rules for multiplication.

When the rules for operating with the signs in a symbolism are stated only in terms of signs as visual structures (*characters*) without any use of prose, then we have what Leibniz called a *calculus*. Leibniz says:

> Calculus is […] operation by means of characters, which takes place not only in quantity, but also in all other reasoning.[96]

Typical examples of calculi in this sense, according to Leibniz, are operating with ciphers in the algorithms for addition, multiplication, subtraction, division in the decimal place-value system.

Leibniz had the idea of a comprehensive system, a *Universal Characteristic* in which verbal language has been made superfluous. In that system truth has been reduced to correctness of calculation.

We do not believe in the possibility of such a universal calculus, but not so much because of the incompleteness and undecidability results in modern mathematical logic, but rather because, as times have changed, we can't share Leibniz's optimistic rationalism.

[95] Quoted in Esquisabel (2012, p. 23).
[96] Quoted in Esquisabel (2012, p. 23).

It would be a mistake to speak about the signs of the algebraic and arithmetical symbolisms (including Leibniz's characters) as "syntactical objects" as the word "syntax" has come to be used in the wake of Gödel, Tarski and Carnap, when syntax is contrasted with semantics and a new technical sense of metamathematics, formalization, formal system, etc., was introduced. In this conception, which was based on a naturalistic conception of language, *syntactical objects are mathematical entities.* To transform the signs in the arithmetical symbolism, for instance, into syntactical objects is the first step in the *formalization* of arithmetic . It is to give a *mathematical representation* of the signs (that are given as visual structures) using the mathematical concept of a finite sequence in stating "rules of formation" for numerical signs and expressions. It is important to realize that formalization in this sense is *applied mathematics*. As I have pointed out before, Carnap was well aware of this and he states this naturalistic view of language explicitly in his book *The Logical Syntax of Language*. Through formalization in this sense, a mathematical structure is built into the syntax of a formal system, and it is this structure that is used in the so-called arithmetization of syntax which was an important method in Gödel's work. Even if a syntactical object is a finite object in the mathematical sense, it may be too complex to be a structure in visual space that can be perceived by the human eye.

Hilbert's notions of formulas, proofs etc. was different. He states explicitly in explaining his proof theory that "A proof is an array that must be given as such to our perceptual intuition;"[97]

The syntactical objects, in Carnap's sense, are not "prior to mathematical thought" (to use an expression of Hilbert's), they are the result of mathematical construction.

It is interesting to compare what I have said about algebraic signs as visual structures perceivable by the human eye, with what Hilbert says about what he calls *die finite Einstellung*. This is what he says in an English translation of the article *Die Grundlagen der Mathematik* from 1927.

> as a condition for the use of the logical inferences and the performance of logical operations, something must already be given to us in our faculty of representation [*in der Vorstellung*], certain extralogical *concrete* objects that are intuitively [*anschaulich*] present as immediate experience *prior to all thought*. If logical inference is to be reliable, it must be possible to survey these objects completely in all their parts, and the fact that they occur, that they differ from one another, and that they follow each other, or are concatenated, is *immediately given intuitively*,[…] And in mathematics, in particular, what we consider is the concrete signs themselves, whose shape, […] is immediately clear and recognizable. [98]

[97] Hilbert (1927, p. 465).
[98] Hilbert (1927, pp. 464-465. my emphasis).

Hilbert repeats this statement of *die finite Einstellung,* in many of his papers on the foundations of mathematics from the beginning of the 1920's to the beginning of the 1930's. And he repeats it using almost the same words, which indicates that it was a deep and strong conviction. Hilbert, unlike Carnap, did not have a naturalistic conception of signs, but rather a sort of phenomenological view of signs.

A characteristic feature of Hilbert's philosophical statement of the *finite Einstellung*, is that he is standing outside mathematics when he states it. He is looking at mathematical sign-language from the outside, and he explicitly calls his perspective a *philosophical view*. He does not use mathematical symbolism or concepts in what he says about signs, for instance

But when he passes over to the proof-theoretical work, he places himself inside mathematics. But then *die finite Einstellung* turns into something else than the philosophical perspective it was from the beginning. It turns into a mathematical concept within a classification of methods of proof inside mathematics. He distinguishes between finite and transfinite methods, for instance. Most of the features he ascribes to the "concrete signs that are intuitively present as immediate experience" have somehow disappeared, since he is now concerned with mathematical objects and methods. For instance, the 'part-whole structure' of the immediately recognizable objects (signs) of the *finite Einstellung* is not the same structure as the imposed part-whole structure of the numerical terms and formulas in formalized arithmetic. It is no longer clear how the details in his statement of the *finite Einstellung* influences the proof-theoretical work. Even Hilbert's distinction between real and ideal mathematical statements seems to be based on a mathematical conception of finitary proof and reasoning. It is not clear how the notion of the 'concretely finite' of the *finite Einstellung*, (which is bounded by human perceptual capacities), can justify Hilbert's finitary mathematical methods of proof, which according to Hilbert "includes recursion and intuitive induction for finite existing totalities".[99] Hilbert points out, for instance, that "The assertion that *all* the objects of a finite existing surveyable totality possess a particular property is logically equivalent to a conjunction of several individual assertions;"[100] But what shall we say if such a totality is so large that it is *not* surveyable, but still finite? Well, then we are no longer talking about the 'concretely finite', but about mathematical finitude.

It seems to me that this kind of gap between the 'concretely finite' of the *finite Einstellung* and the normal mathematical notion of the finite manifests a serious difficulty in Hilbert's program. The difficulty is due, it seems to me, to Hilbert's underrating of the conceptual importance of the *operative symbolism* of arithmetic. Note that in his statement of the *finite Einstellung* he talks about signs but never about *the use* of signs.

[99] Hilbert (1923a, p.1139).
[100] Hilbert (1922a, p. 1122).

In Hilbert's paper (1922a, p. 1122), he ends the statement of the *finite Einstellung* by saying: "In the beginning was the sign." And then he goes on to "explain the numbers", by saying: "The sign 1 is a number. A sign that begins with 1 and ends with 1, and such that in between + always follows 1 and 1 always follows + is likewise a number."

What Hilbert *should* have said is this:

> In the beginning was *the use of signs.*

A written mark or figure becomes a sign when it has got a use as a sign. If Hilbert had said "In the beginning was the use of signs", he could have continued: The use of signs creates symbols such as the numbers, and the successor-operation. The symbol 1 is a number. The symbol '+ 1' is the successor-operation: if n is a number, then n+1 is (the next) number in the series of natural numbers.

The German philosopher Alois Müller criticized Hilbert for his talk about signs having no meaning.[101] As a result of this criticism, Hilbert changed his terminology. He uses the word numeral (*Ziffer*) or number-sign instead of sign. So now the "explanation of numbers" reads: the numeral 1 is a number, and numerals of the form 1+1, 1+1+1, etc. are numbers. This was, it seems to me, an unfortunate decision since we certainly want to distinguish between numerals and numbers, as we must distinguish between signs and symbols. Hilbert's decision manifests ignorance of the operational aspect of the arithmetical symbolism, where numbers and other symbols are constituted.

That Hilbert wanted to limit himself to 'surveyable' totalities in talking about all objects of a totality, is connected with the concrete sense of finitude of the *finite Einstellung*, which is subject to the human capacity to survey a totality of objects. But as a mathematician, Hilbert (and other proof-theorists) tended to find this restriction 'troublesome'. The mathematics of proof-theory runs smoother without it. As Mancuso has pointed out, there is a clear change of emphasis concerning the sense of the word 'finite' in Hilbert's writings during the 1920's.[102] The *mathematical* sense of the word becomes more important, even if Hilbert never abandoned the philosophical view of the *finite Einstellung*. I think that there is a conflict here in Hilbert's thinking that was never resolved. Hilbert wanted the two senses of 'finite' to be to a great extent one and the same, but they are essentially different. One of them has to do with the human capacity to perceive finite structures *in visual space*, the other is the pure mathematical notion of finitude (of the calculus of finite sets and sequences, for instance). It is even misleading to

[101] Müller (1923).
[102] Mancosu (1998, p. 168).

think of the latter notion of finitude as an extension of the former. They are different concepts.[103]

What is the reason for Hilbert's ignorance of *the use* of signs, of the rules for the use of signs in the operative symbolism in arithmetic? I think it has to do with his vision of the essence of mathematics as captured in formalized mathematics, which is a vision of something static and completed, as for instance when he says "mathematics proper, or mathematics in the strict sense becomes a stock of provable formulae."[104] Mathematics is seen as consisting essentially of the proved theorems, something completed, and not of the activities and on-going operational practices where mathematical problems are solved.

In formalized mathematics the theorems are formulas with readings in mathematical prose and Hilbert seems to have had very great confidence in the understanding that is based on these prose-readings. I see this confidence as a part of his attitude to accept classical mathematics essentially as it stands (except that its consistency has to be established). And his confidence in the (passive) mathematical prose tends to push aside the activity aspect of mathematics, and to block seeing the importance of the operative symbolism.

It is interesting that Leibniz main motivation for his symbolic approach is connected with the limitations of the human mind that we find in Hilbert's *finite Einstellung,* such as for instance the limited human capacity to survey a totality of objects. Leibniz says

> For example, we often grasp a number, however large, all at once in a kind of *blind thought*, namely when we read chiphers on paper which not even the age of Methusela would suffice to count explicitly.[105]

I suppose that what Leibniz has in mind here is, for instance, a symbol or number such as

1248

In understanding this number (in the decimal place-value system), we have (in general) no intuitive awareness of the individual numbers that we have to go through in counting from 1 up to this number. The thought of the number 1248 is, what Leibniz calls a symbolic or blind thought.

In another context Leibniz says:

[103] For a more detailed discussion of this topic see Stenlund (2012b).
[104] Hilbert (1922a, p. 1131).
[105] Loemker (1969, p. 76). Leibniz is joking here. According to the Hebrew Bible, Methusela is purported to be the oldest person to ever live. Extra-biblical tradition maintains that he died at the age of 969.

> Thus, nobody could carry out in his mind reasonings that are extremely lengthy, had not certain signs been invented, that is, names by means of which the overwhelming amount of things could be embraced in such an abbreviated way that this multitude can be gone through. Such a thing would be impossible if, by suppressing names or other signs like these, definitions were to be used in place of that defined. And I use to call such thoughts blind. Nothing is more necessary or frequent for man than them.[106]

Let us consider the following equation:

$$10^{40} \times 10^{30} = 10^{70}$$

We see immediately that this is a true equation, thanks to the use of the symbol for multiplication and the exponential notation that gives us the *symbols* 10^{40}, 10^{30}, 10^{70}. We can recognize the truth of the equation on the basis of this symbolism alone, while remaining 'blind' to what logicians call the "canonical form" of this equation, obtained by replacing these symbols, and the multiplication symbol by their definitions until we have an equation between extremely long sequences of the form (using Hilbert's notation):

$$1+1+1+1+1+ \ldots +1.$$

Could this equation be written down in its entirety on paper? – Remember that Archimedes estimated the number of grains of sand that would fill up the (Classical Greek) Universe, to about 10^{63}, which is a much smaller number than 10^{70}.

[106] Esquisabel (2012, p. 14). The translation is slightly amended.

13 Husserl's failure to accept symbolic mathematics.

Let us recall the following quotation from Jacob Klein that was given before

> […] the "being" of the species in Vieta, i.e. the "being" of the objects of "general analytic," is to be understood neither as independent in the Pythagorean or Platonic sense nor as attained "by abstraction" […] in the Aristotelian sense, but as *symbolic*. *The species are in themselves symbolic formations* […] They are, therefore, comprehensible only within the language of *symbolic formalism.* […] Therewith the most important tool of mathematical natural science, the "*formula*," first becomes possible, but above all, a new way of "understanding," inaccessible to ancient *episteme* is thus opened up.[107]

Look at the last sentence where he says that "the most important tool of mathematical natural science, the 'formula,' becomes possible". Why is that so important for Klein to point out in this context? – It has to do with the fact that one very important part of Klein's ambition to show that modern mathematics is symbolic mathematics, was to show that the symbolic mathematics initiated by Vieta, Stevin, Descartes and Wallis, and further developed by Leibniz, did provide the necessary conceptual tools and mathematical resources for the creation of modern exact natural science, in particular modern physics. Klein gives a summary of this development in his article "Phenomenology and the History of Science". I quote this summary here at length:

> In creating his *ars analytice*, Vieta introduced for the first time, fully conscious of what he was doing, the notion of a mathematical *symbol* and the rules governing symbolic operations: he was the creator of the mathematical *formula*. In doing this, he preserved, however, the original "ideal" concept of number, developed by the Greeks out of the immediate experience of "things" and their prescientific articulation. In Vieta's notion of "species" the original understanding of number is retained […] But his immediate successors, Ghetaldi, Harrot, Oughtred, and Wallis (partly under the influence of Stevin and, as far as Wallis is concerned, of Descartes' *Geometry*), have already lost the original intuition. *The technique of operating with symbols replaces the science of numbers.* Descartes, for his part, aiming at the all-comprehensive *mathesis universalis*, and following the algebraic doctrine of

[107] Klein (1968, p. 175).

> Stevin, transforms the traditional understanding of Euclidean geometry into a symbolic one, which transformation is at the basis of his analytic geometry. His mathematical significance lies in the fact that he subjects the traditional geometry to the same kind of symbolic "formalization" to which Vieta subjected the Diophantean arithmetic.
>
> This establishment of a fundamental analytical discipline, planned in advance by Vieta as well as by Descartes for the sake of founding a "true" astronomy and a "true" physics, inaugurates the development of a symbolic science of nature, commonly known as mathematical physics.[108]

Klein's ambition to make clear the origin of modern physics has much in common with Husserl's ambition to do something similar in his *Crisis of the European Sciences and Transcendental Phenomenology*. But there are important and crucial differences. Husserl starts out with Galileo as a paradigm case for the new modern approach to physics in which the aim is *the mathematization of nature*.[109] Husserl speaks of "Galileo's mathematization of nature", but perhaps it would have been more correct to say "Galileo's geometrization of nature", because Galileo did not have algebra and therefore no "formulae" ("the most important tool of mathematical natural science", in Klein's words). Galileo's conception of geometry was the traditional Euclidean one, and not the symbolic conception of geometry of Descartes (which was the precondition for the "arithmetization of geometry" and for the creation of Descartes' analytic geometry). Galileo's mathematical work relied heavily on the Eudoxian theory of proportions as presented in the fifth book of Euclid's Elements. So Galileo's mathematical methods were the standard ones of the day, and these methods were clearly superseded by the algebraic methods of Descartes.

In addition, Husserl suggests that Galileo was convinced of "the possibility of philosophy as *episteme* achieving an objective science of the world"[110], and Galileo is said to believe that "it had just been revealed that pure mathematics, applied to nature, consummately fulfills the postulate of *episteme* in its sphere of shapes".[111] This does not match Klein's statement that "with Vieta's analytic art a new way of 'understanding,' *inaccessible to ancient episteme is thus opened up*". Vieta's analytic art and the *mathesis universalis* of Stevin, Descartes and Wallis were certainly not "sciences of the world" in the ontological sense of Aristoteles' *episteme*. And this is very important if we are interested in the impact of symbolic mathematics, and the sense in

[108] Klein (1940, pp. 159-160, my emphasis). Here it may be appropriate to mention that Jacob Klein was a student of Husserl and Heidegger. It is said that he was Heidegger's top PhD-student, which does not mean that his style of doing philosophy is identical with Heidegger's. Klein is also known as a Plato-scholar.

[109] Husserl did not just happen to choose Galileo without further thought as a paradigm case. In Husserl (1970, p. 53) he says: "I am of course quite serious in placing and continuing to place Galileo at the top of the list of the greatest discoverers of modern times."

[110] Husserl (1970, p. 33).

[111] Husserl (1970, p. 33).

which the work of Vieta, Stevin, Descartes and Wallis was a transformation and not a continued development of ancient Greek mathematics.

A phenomenologist might object that Husserl is not concerned with "actual history" but with what Husserl calls "intentional history". Galileo may have been the first who seriously embraced the idea, and the intention, of a complete mathematizing of nature, even if he did not possess the mathematical tools that made it possible to realize that idea.

Husserl certainly knew about symbolic mathematics (it is one of the themes in his early work *The Philosophy of Arithmetic*), he also knew about the invention of algebra, about Descartes' arithmetization of geometry, about *formulae* and their importance in modern physics, but somehow he did not want to count these inventions as decisive moments in the "paradigm shift" towards modern mathematics and natural science. It seems to me that Husserl had great difficulties with accepting and appreciating symbolic mathematics as the essence of modern mathematics. Klein points out that Husserl "does not seem to appreciate […] the importance of Stevin's algebraic work and strangely enough, the Cartesian idea of a *mathesis universalis*, based at least partly upon Stevin and leading directly to the corresponding, if modified, Leibnizian concept [of a universal and symbolic science (*mathesis universalis, ars combinatoria*)].[112]

That Husserl is uncomfortable with the non-ontological symbolic point of view shows itself already in the terminology he used in *The Philosophy of Arithmetic,* where he distinguishes between authentic and symbolic numbers, and suggests that only a small cardinal number (*Anzahl*) is a true and authentic number. In *The Philosophy of Arithmetic* Husserl writes: "If a content is not given directly to us as what it is, but only indirectly through signs that univocally characterize it, then we have a symbolic presentation of it instead of an authentic one."[113] This makes it sound as if a number in symbolic arithmetic is only a kind of "stand in" or "surrogate" for something else that is the real thing, the authentic number outside the symbolic systems.[114]

Husserl seems to be unable to abandon the Greek ontological view of arithmetic, which modern arithmetic had left behind already in the seventeenth century when the decimal place-value system of numeration was generally accepted.[115] In his account of Husserl's views, Da Silva writes:

[112] Klein (1940, p. 148).
[113] Husserl (2003, p. 193).
[114] It is important to distinguish Husserl's ontological sense of the surrogative function of symbols from the non-ontological one we find for instance in Leibniz. For Leibniz a certain symbol of a symbolic system may have a surrogative role for another complex symbol *in the same symbolic system*.
[115] According to Da Silva (2012, p. 120), "a number is, for Husserl, what the tradition since Plato says it is, a collection of undifferentiated units – monads".

> Representing numbers in decimal notation (or any other notational system) *is a way of grasping them and indirectly dealing with them*.[116]

Indirectly! - We will always deal with numbers in *some* notational system, are we never dealing with numbers directly? Husserl would perhaps answer: "Only in intuition."

My impression is that Husserl's reluctance to accept and appreciate the fruitfulness and richness of symbolic mathematics was connected with his intuitionism. The confidence in intuitive (*Anschauliche)* evidence appears to have been deeply rooted in Husserl's thinking. This may also be the reason why Husserl persisted in seeing modern arithmetic as having its historical roots in ancient Greek arithmetic (which was more concrete and *anschaulich*), as if the modern concept of number has developed from, or on the basis of, the Greek concept of number (*arithmos*), rather than *in opposition* to it in essential respects. Since the Greek origin of modern arithmetic was precisely what Stevin denied, we can understand Husserl's lack of appreciation of Stevin's work.

We note that Husserl speaks of the "Crisis of the *European* Sciences". It seems to have been important for Husserl that mathematics is a *European science* with its origin in ancient Greece. This is perhaps more an expression of an ideological than a philosophical attitude. In any case, for Stevin the influence from Hindu-Arabic mathematics was much more important than the Greek influence.

In his essay *The Origin of Geometry*, Husserl argues that the institution of Greek geometry is retained in the intentions of modern geometricians. I think that Lachterman's account of the change from ancient to modern mathematics shows in great detail that this is a mistaken idea.[117] Lachterman gives a particularly penetrating account of Descartes' symbolic approach to geometry and its impact on modern mathematics.

I believe that Sybille Krämer is right when she says that "The mathematical innovations of the sixteenth and seventeenth centuries have little to do with the "geometric-proving" theories passed down from Antiquity, and derive rather from an "algebraic-algorithmic" spirit, whose origins lie in Hindu-Arabic culture."[118] Historians of mathematics seem to agree that the decimal place-value system (with a sign for zero) was an independent invention of Indian mathematics, even if it had occurred earlier in Chinese mathematics. [119] It seems to have been established in academic circles in India in the 6th century. Arabic scholars improved the system by introducing decimal fractions, and were instrumental in making it known in Europe. But it was

[116] Da Silva (2012, p. 120).
[117] Lachterman (1989).
[118] Krämer (2012, p. 371).
[119] Ifrah (1981).

Stevin who really introduced the actual use of the decimal place-value system in Europe through his book *De Thiende*.

The work of the Islamic mathematician al-Khwarizmi, who lived in Bagdad and died in the middle of the ninth century, contributed to the development of the algebraic-algorithmic spirit through his treatise on algebra (*Hisab al-jabre w'al muquabala*) and on Indian numerals (*Algoritmi de numero Indorum*[120]). The English word 'algebra' is derived from a Latinized version of the word *al-jabre*, and the word 'algorithm' is a corrupted Latin form of al-Khwarizmi's name. His treatises were translated and explained in Latin and introduced into Europe (for instance, through Fibonacci's book *Liber Abaci* published in 1202). But the new ideas were met with a great resistance that lasted for several centuries due, among other things, to the strong influence of Aristotelian doctrines since the 12th century. One feature of the decimal place-value system that was met with particularly violent resistance was its use of the cipher zero. The idea of a number zero was incompatible with the still prevailing Greek and Roman notion of number according to which numbers were understood in terms of multitudes composed of discrete, countable units.[121] This understanding of numbers was not essential for numbers of the decimal place-value system. The double role of zero in the decimal place-value system was clarified. Zero was used as an empty-place indicator to indicate the absence of a numeral, but at the same time rules for calculating with the sign "0" were set up in which "0" was treated like any other numerical sign.

A number in the decimal place-value system is a symbol determined by its functional role in the system. The general acceptance of the Indian decimal place-value system of arithmetic in the fifteenth and sixteenth centuries was a great victory of symbolic mathematics over the ontological conception of mathematics of Antiquity. In view of the enormous importance for modern mathematics of the decimal place-value system and its extensions, there is some truth in saying that a central part of the origin of modern mathematics was in India and not in Greece.

In his book on algebra (which is said to be the first book on algebra) al-Khwarizmi developed a method for solving linear and quadratic equations. But the most striking feature of the book is its practical orientation. It is a book showing how to solve equations and problems derived from ordinary life in the Islamic empire at that time such as the measuring of lands and the digging of canals, but also problems arising in tax collection, lawsuits and trade. Only the first part of the book is a discussion of what we would today recognize as algebra.

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[120] The original Arabic text is lost.

[121] We remember that this was essentially also the basic notion of number that Husserl starts out with in his *The Philosophy of Arithmetic*.

But al-Khwarizmi's algebra is not on the level of Vieta's. Al-Khwarizmi did not invent the *formula*. There is no use of algebraic symbolism, everything is expressed in mathematical prose. It was *rhetorical algebra*. It is the increased use of the algebraic symbolism which leads to the "algebraization of mathematics" (as Mancuso calls it) in the seventeenth century; Mancuso, following Mahoney, mentions "freedom from ontological commitment", as a characteristic feature of this algebraic mode of thought.[122]

Husserl could not avoid seeing how the symbolic approach was responsible for great change and great progress in modern mathematics and natural science around the beginning of the 20th century, and, as pointed out by Da Silva (2012), Husserl did a lot of work in trying to give an epistemological justification of "symbolic knowledge" in modern mathematics. But already the stating of the problem as a problem about "symbolic *knowledge*" is problematic. The mere combination of the words "symbolic" and "knowledge" is itself questionable, since the word 'knowledge' tends to be understood as "knowledge that so and so is the case" in the traditional sense of scientific knowledge, i.e. as *episteme* in Aristotle's sense. (And this was obviously how Husserl understood it. As Da Silva points out "The idea of a definite system of axioms, a *nomological system* as Husserl also called it, answers to the Aristotelian ideal of science that Husserl was not willing to abandon."[123]) A proposition or judgment expressing such 'knowledge' is a judgment about things having independent existence in an ontological realm outside the symbolism in which the judgment is formed. Now, this is clearly incompatible with the word "symbolic" as Vieta, and his followers, used it – in particular, at the time when the technique of operating with symbols starts to replace the science of numbers. As is clear from the quotation from Klein at the beginning of this section, the word 'symbolic' *means* non-ontological, so there is no such thing as an 'ontological symbolism' or an 'ontological symbolic view of mathematics'. These expressions are bound to cause confusion. The ontological views of the Pythagoreans, Plato and Aristotle are mentioned by Klein (in the quotation given before) as contrasts to the symbolic mathematics of Vieta and his followers. Vieta's introduction of the word 'symbolic' is intimately connected with his invention of the algebraic symbolism (which did not exist before).

Vieta's analytic art is not a theory or system of knowledge in the form of proved mathematical theorems. It is an *art: methods and techniques for solving problems*. For Vieta the word "problem" takes the place of the word "theorem". As mentioned before, Vieta strongly emphasized this new attitude as the proper attitude to his analytic art at the end of his article. Lachterman points out about Descartes' geometry that "the *Geometry* is devoted

[122] Mancosu (1996, p. 85).
[123] Da Silva (2012, p. 127).

exclusively to solving *problems* and not at all to proving theorems."[124] So this problem-oriented attitude is also an important aspect of Descartes' analytic geometry. And much the same can be said about Leibniz's infinitesimal calculus. They are not axiomatic deductive theories or systems of knowledge in the form of theorems about some independently existing subject matter, but analytic arts. So, once again, the expression "symbolic knowledge" is indeed misleading. I don't think that the use of the word 'knowledge' can be defended by pointing out that there is also "knowledge how to do something". That is a more special use of the word 'knowledge' that will hardly prevent the confusion.

The mathematical inventions of Vieta, Descartes and Leibniz, which were accomplished by means of the symbolic point of view, were extremely successful in solving problems. This was and still is the justification of symbolic mathematics. Husserl's work on giving a logical, epistemological justification of "symbolic knowledge" amounts to the construction of ontological correlates of the symbolic systems using familiar mathematical and philosophical prose, as, for instance, in his "formal ontology", but in this endeavor he is corrupting the true meaning of symbolic mathematics that begins with Vieta. Da Silva speaks of "Husserl's constant preoccupation to keep symbolic methods in science and mathematics far from degenerating into a pure and epistemologically unjustified game with signs".[125] The feeling of "formalist alienation" (an expression Husserl used according to Da Silva) behind this talk of "degeneration" is, in my opinion, rooted in a superficial view of the workings of mathematical symbolism, where the difference between sign and symbol has been lost.

Husserl explicitly points out that "we must not define merely in terms of signs and calculational operations." (FTL §34). He is not willing, for instance, to call an equation such as

$$a + b = b + a$$

a rule of arithmetical calculation by which we are, in Husserl's words, "allowed to manipulate the given signs in such a manner that the sign b + a can always be substituted for a + b". (FLT § 34). Husserl prefers instead the following verbal reading of this rule:

> There shall obtain among the *objects* belonging to the multiplicity (conceived at first as only empty Somethings, 'Objects of thinking') a certain *combination-form* with the law-form a + b = b + a' – where equality has precisely the sense of actual *equality* such as belongs to the categorial logical forms" (FLT §34)

[124] Lachterman (1989, p. 149).
[125] Da Silva (2012, p. 134).

Here, the operative aspect of the *rule* a + b = b + a is removed and replaced by the static concept of a law that applies to 'objects of a multiplicity' in a postulated ontological realm. But the use of the word 'law' here seems to suggest a doubtful similarity to laws of nature in physics.

Are we supposed to get rid of our 'formalistic alienation', by accepting Husserl's heavy-footed formulation?

Husserl says about the symbolic view of arithmetic: "…to put in the place of the actual theory of multiplicities its symbolic analogue" and explain objects and multiplicities in terms of rules of the game, is to be led astray by supposed needs for greater exactness. (FTL §34).

It is not greater exactness, but greater *clarity* that is achieved by recognizing the objects as symbols constituted or determined by their function in the game, rather than as postulated ontological entities. One of the main points of comparing the arithmetical calculus with a game is to make clear the difference between signs and symbols, and that is enough to relieve oneself of the 'formalist alienation'.

Husserl (in contrast to Leibniz and Wittgenstein) seems to me to have had too much confidence in the conceptual reliability of the verbal language of mathematics and the vocabulary of traditional philosophy.

14 Contemporary physics and symbolic mathematics

The "crises of European sciences" that was a concern of both Husserl and Klein, included the difficulties of interpretation that arose with the invention of relativity theory and quantum physics in the 20th century. That Klein was aware of these difficulties is clear from a talk he gave in 1932 to an audience consisting of both philosophers and physicists.[126]

But let us leave Husserl and Klein here. Let us instead listen to what two physicists said about the conceptual problems confronting contemporary physics. The great changes of scientific thinking that came about with the invention of relativity theory and quantum physics led to the "downfall of classical physics and to strange new conceptions of the physical world", as the physicist A.S. Eddington expressed it in his Gifford Lectures in 1927.[127] What is of interest for us about this development is that it also led to a new awareness of the importance of symbolism in science and in particular of the fruitfulness of symbolic mathematics.

Eddington writes:

> One of the greatest changes of physics between the nineteenth century and the present day has been the change in our idea of scientific explanation. It was the boast of the Victorian physicist that he would not claim to understand a thing until he could make a model of it; and by a model he meant something constructed of levers, geared wheels, squirts, or other appliances familiar to an engineer. Nature in building the universe was supposed to be dependent on just the same kind of resources as any human mechanic. […]
>
> Nowadays we do not encourage the engineer to build the world for us out of his material, but we turn to the mathematician to build it out of his material. […] *We are dealing in physics with a symbolic world*, and we can scarcely avoid employing *the mathematician who is the professional wielder of symbols.*[128]

A prominent mathematician who made significant contributions to modern physics in this spirit was Hermann Weyl. He was a scientist, a mathemati-

[126] Hopkins (2011, § 30) reports about this talk, whose title was "The World of Physics and the Natural World". The talk is published in Klein (1985). More about the relationship between Husserl's and Klein's views can be found in Hopkins (2011).
[127] Eddington (1931).
[128] Eddington, (1931, p. 209, my emphases).

cian, at heart, but he had extensive philosophical interests, which affected his work as a mathematician and his contributions to contemporary theoretical physics. He was a student of Hilbert's, and from about 1912 to the publication of his book *Das Kontinuum* in 1918, Weyl was influenced by Poincaré and by Husserl.[129]

In 1919 Weyl met L.E.J. Brouwer and was deeply influenced by Brouwer's philosophical ideas in general and in particular by his mathematical intuitionism with its emphasis on intuitively cognizable truths. Weyl later described himself as having been an apostle of Brouwer by this time. But in the beginning of the 1920s Weyl became more and more doubtful about the sufficiency of the intuitionistic as well as the pure phenomenological approach for the understanding of modern science. In 1927 he wrote: "With Brouwer, mathematics gains the highest intuitive clarity; his doctrine is idealism in mathematics thought to the end. But, full of pain, the mathematician sees the greatest part of his towering theories dissolve in fog."[130] By this time Weyl had become convinced that contemporary 'creative science' must transcend what is intuitively and phenomenologically given. He seems to have accepted that Brouwer's intuitionism, as well as pure phenomenology, are incapable of accounting for contemporary theoretical physics. In 1932, Weyl wrote:

> The scientific formulation of the objective conception of the world takes place in physics, which employs mathematics as a means of construction. But the situation that prevails in theoretical physics in no way corresponds to Brouwer's ideal of a science, to *his postulate that every proposition shall have its individual meaning, and that this meaning shall be capable of intuitive display*. On the contrary, the propositions and laws of physics taken individually do not have a content which can be verified experimentally; it is only the theoretical system as a whole which can be confronted with experience. What is achieved is not intuitive cognition of an individual or general state of facts, and a description which faithfully portrays the given conditions, but theoretical, purely symbolical construction of the world.[131]

It seems to me that much the same criticism applies to Husserl's idea of a science, since Husserl too presupposed that every proposition shall have its individual meaning. Later Weyl sums up the main point by saying

> in the natural sciences we are in contact with a sphere which is impervious to intuitive evidence; here cognition necessarily becomes symbolical construction. Hence we need no longer demand that when mathematics is taken into the process of theoretical construction in physics it should be possible to set apart the mathematical element as a special domain in which all judgments

[129] For an up to date survey of Weil's foundational thinking, and its relation to phenomenology, in the period 1910-1930, see Mancosu and Ryckman (2002).
[130] Weyl (1927, p. 136).
[131] Weyl (2009, p. 78, my emphasis).

> are intuitively certain; from this higher viewpoint which makes the whole of science appear as one unit, I consider Hilbert to be right.[132]

S*ymbolical construction*, in which the symbols, in themselves, do not always signify anything accessible as intuitive experience, is henceforth Weyl's mathematical approach in theoretical physics. The great importance he attached to this new symbolic orientation is clear from the following statement: " […] only in mathematics and physics, as far as I can see, has symbolic-theoretical construction acquired sufficient solidity to be convincing for everybody whose mind I open to these sciences."[133]

He describes essential features of this constructive symbolic cognition as follows:

> Upon that which is given, certain reactions are performed by which the given is in general brought together with other elements capable of being varied arbitrarily. […] By the introduction of symbols, the judgments are split up; and a part of the manipulation is made independent of the given and its duration by being shifted on to the representing symbols which are time resisting and simultaneously serve the purpose of preservation and communication. Thereby the unrestricted handling of notions arises in counterpoint to their application, ideas in a relatively independent manner confront reality. […] Symbols are not produced simply "according to demand" wherever they correspond to actual occurrences, but they are embedded into an ordered manifold of possibilities created by free construction and open towards infinity.[134]

In these, and similar statements of Weyl's, it seems to me that he is on the verge of saying that most (if not all) of the propositions of the theoretical, symbolic systems of quantum physics, *are not really propositions about anything external to the system*, but rather rules or laws of a calculus whose "manipulation is made independent of the given". When it is said that this system as a whole depicts a "transcendent symbolic world", I think that it must not be taken as an ontological or metaphysical statement, but rather as an epitome of the successful applications of the system as a whole; its function, significance and importance in the practices of modern physics.[135]

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[132] Weyl (2009, p. 80).
[133] Weyl (2009, p. 82).
[134] Weyl (2009, pp. 118-119). What Weyl means by this will hopefully be clearer by the examples I present throughout this section, especially the example with the introduction of an arithmetical symbolism for counting sequences of sounds at the end of this section.
[135] Scholtz (2006, p. 306) says that " Weyl tried, as much as he could, to distance himself from classical metaphysics, in particular its reference to the kind of transcendent reality that was stipulated there. He definitely refused, however, to cut the bonds to all kinds of metaphysics. He rather substituted strong references to symbolical and material practices in place of the old realism." I agree with Scholtz, but I would not want to call Weyl's reference to "symbolical and material practices" an expression of a bond to "a kind of metaphysics".

Weyl illustrates the mentioned "splitting up of judgments" by using a simple example from the propositional calculus.[136] He points out that in formalized mathematics formulas of the form A → B have *replaced* the statement that A implies B or that B follows from A. The split consists in the fact that after the replacement, the sign '→' stands for a symbol which is manipulated only according to the formal rules of the system, and not on the basis of the intuitive reading or 'prose-translation' of the symbol as 'implies' or 'follows'. Weyl summarizes his point by saying: "[...] in principle we must sharply distinguish between the symbol → occurring within the system, and such words as "follows" which we use to make meaningful communications about the game." (We note in passing that Weyl's "split" has strong similarities with Wittgenstein's "split" between calculus and prose.) Weyl goes on to make the following claim about the importance of this distinction:

> [...] a split of essentially the same nature has been brought about by quantum physics: namely the split between the physical phenomenon under observation on the one hand and the measurement on the other. The first can be adequately described only by the quantum-mechanical symbolism; about the latter we can and must talk in the intuitive terms of classical physics. […] In quantum theory we learn that measuring one quantity sometimes utterly destroys the possibility of measuring another quantity. This is a matter of principle and not of human deficiency.

The principle referred to here is Heisenberg's uncertainty principle, which asserts a fundamental limit to the precision with which certain pairs of physical properties of a particle, such as position and momentum, can be known simultaneously. As a consequence of the uncertainty principle there is an inconsistency of classical logic with the facts of measurement of variables in quantum mechanics, such as the position and momentum of a particle. The classical distributive law of propositional logic

$$(A \text{ and } (B \text{ or } C)) \leftrightarrow ((A \text{ and } B) \text{ or } (A \text{ and } C))$$

fails. If p is a particle moving to the right on a line and A is the proposition that "p moves to the right" and B is "p is in the interval [-1, 1]" and C is "p is not in the interval [-1, 1]", then (B or C) is true, and so is (A and (B or C)). But (A and B) and (A and C) are both false since they assert tighter restrictions on simultaneous values of position and momentum than is allowed by the uncertainty principle. The 'uncertainty' of the uncertainty principle is not due to inaccuracy of the methods and instruments of measurement; it arises from the wave nature in the quantum mechanical description of nature, which is why it is a matter of principle.

[136] Weyl (2009, p. 187).

The failure of the distributive law has been taken to mean that a radical revision of classical logic would be necessary in quantum physics. In his early papers on quantum logic, Hilary Putnam exclaimed that "*The world has a non-classical logic!*"[137]

But doesn't Putnam here presuppose that the 'symbolic world' of quantum physics is a kind of copy or representation of physical reality, rather than a symbol, a complex symbolical construction? It is not clear to me that the *symbolic world* of Eddington and Weyl has a logical structure that would somehow have an independent existence. If the symbolic world "has a logic", this logic must be a symbolic construction as much as everything else in the symbolic world of quantum physics.

Let us look at another of Weyl's examples of the limitations of the use of "the intuitive prose of classical physics". He presents an algebraic-arithmetical equation indicating the propagation of a plane monochromatic light wave.[138] The equation contains several parameters, such as the wave frequency v, the intensity a^2, time and space coordinates t, x, y, z, etc. . To speak only in the formal language of the algebraic-arithmetical equation, avoiding all terms that refer to ideas of space and sense qualities would be too pedantic according to Weyl. But, nevertheless, he insists that "in principle, one must hold to the position that nothing of the intuitive contents and essence of these terms enters into the systematic symbolical construction of the physical world!"

Weyl raised the question of whether this two-level view of language is only a feature belonging to an early stage of the development of quantum physics, a feature that will be overcome in its future development. But Weyl is inclined to answer this question negatively. He says that in quantum physics

> …one has to distinguish sharply between the hidden physical process which can only be represented by the symbolism of quantum physics, although it may be referred to by such words as electron, proton, quantum of action, etc., and the actual observation and measurement. According to Bohr, we have to talk about the latter in the intuitively comprehensible language of classical physics; or ought we better say: in the language of everyday life? […] It may very well be that we can never dispense with our natural understanding of the world and the language in which it is expressed, perhaps a little purified and enlightened by classical physics, and that the symbolism of quantum physics will never be able to offer a substitute for it. In this case we would have here a true dialectic which cannot be resolved/lifted by any historical development ….[139]

[137] As he expressed it in his paper *The curious story about quantum logic* (Putnam, H., 2012, p. 173).
[138] Weyl (2009, p. 107).
[139] Weyl (1948, 340-341), quoted in Scholtz (2006, pp. 302-303).

As Schultz points out, in Weyl's mature views on this issue a reference to

> 'concrete activities of people' comes into play and allows us to adopt even an 'antropic' perspective with respect to mathematical knowledge. […] we find that mathematical symbols are understood within the context of a communicative practice that has strong parallels to material practices, in the way that symbols are handled, and with multiple links to the other scientific and technical activities.[140]

Here one comes to think of the 'anthropological' element that enters in Wittgenstein's work on rule-following in the middle of the 30th, as it comes to expression, for instance, in the following remarks from Wittgenstein's *Remarks on the Foundations of Mathematics*,

> Following a rule is a human activity. (p. 331)
>
> The agreement of people in calculation is not an agreement in opinions or convictions. (p. 332)
>
> Language, I should like to say, relates to a *way* of living. (p. 335)
>
> The words "language", "proposition", "order", "rule", "calculation", "experiment", "following a rule", relate to a technique, a custom. (p. 346)
>
> … what the correct following of a rule consists in cannot be described *more closely* than by describing the *learning* of 'proceeding according to the rule.' And this description is an everyday one, like that of cooking or sewing, for example. It presupposes as much as these. It distinguishes one thing from another, and so it informs a human being who is ignorant of something particular. (p. 392).

I know of no evidence, however, for the opinion that Weyl should have been influenced by Wittgenstein's work in the 1930th and 1940th. Weyl had read the *Tractatus*. It is listed as one of the references in Weyl's article *Über den Symbolismus der Mathematik und Mathematischen Physik* (1953), and Weyl makes some short comments on a few remarks in the *Tractatus*. But there is nothing in this article that would show the *Tractatus* to have been an important influence for Weyl.

The similarities we find in Weyl's and Wittgenstein's early symbolic views of mathematics, are rather due to their common source of inspiration: Heinrich Hertz.

When he raises the question of the origin and nature of the procedure of symbolic construction, Weyl refers to Heinrich Hertz.[141] He quotes at length the crucial passage from the beginning of the introduction to Hertz' *Princi-*

[140] Scholtz (2006, p. 305).
[141] Weyl (2009, p. 107, 108).

ples of Mechanics, where Hertz explains that we create symbols of external objects in such a way that "logically necessary consequences of the symbols are always the symbols of caused consequences of the symbolized objects". It is a crucial feature of Hertz' symbolic view that the symbols, being our concepts of objects in nature, are not assumed to have anything further in common with the objects *except this requirement*. As Hertz puts it: "We neither know, nor do we have means to find out whether our representations of the objects have anything in common with the objects themselves except that one fundamental relation alone." As pointed out before, it was this feature of Hertz' work that Ernst Cassirer had in mind when he wrote that "Heinrich Hertz is the first modern scientist to have effected a decisive turn from the copy theory of physical knowledge to a purely symbolic theory."[142]

It may seem as though symbolic mathematics in Weyl's conception only refers to very advanced mathematics, such as the mathematics used in modern theoretical physics. But the essence of introducing a symbolism, according to Weyl, is a *transition from description to construction*, by means of the "splitting up of judgments" mentioned before, and such transition occurs already at the most elementary levels of mathematics as in the creation of the sequence of natural numbers 1, 2, 3, … Here we have according to Weyl a typical example of the construction of a symbolism.

Weyl asks us to imagine hearing two sequences of sounds, one after the other.[143] In reproducing the sounds of the first sequence by recollection when listening to the second we may ascertain that the second sequence projects beyond the first. "This time there were more sounds than the first time." Here we have a description, *which can be understood without any reference to symbols*. But, we may proceed in a different way. While listening to the sounds we put strokes on paper one after the other, one stroke for each sound. We may thus get the number-symbols | | | | called 4 for the first sequence, and | | | | | | called 6 for the second one, and now *we ascertain from the symbols*: 6 > 4. In doing this the two sequences of sounds may already have disappeared into silence. So the stroke-symbols are in that sense time-resisting and serve the purpose of preservation and communication, which are crucial features of a symbolic construction. For relatively small number-symbols like these we see immediately that 6 > 4, but for larger numbers a certain manipulation in the symbolism may be necessary: We cross out the first stroke from each symbol and repeat this operation until one symbol is exhausted.

[142] Cassirer (1957, p. 20). It is sometimes suggested that Weyl's main source of inspiration for his symbolic approach was not so much Hertz but rather Cassirer's "Philosophy of Symbolic Forms". But that is doubtful in view of Weyl's critical remarks about Cassirer's very wide use of the notion of 'symbolic form'. Weyl finds it difficult to read Cassirer's Philosophy of Symbolic Forms as "variations on a single theme" (Weyl, 2009, p. 195).

[143] Weyl (2009, p.117). Weil uses this example several times in his publications.

If the ordinary number-signs 1, 2, 3, … belong to the symbolism, we would also have the rules or conventions:

$$1 = |, \quad 2 = ||, \quad 3 = |||, \ldots$$

where the equality sign means that one symbol may be replaced by the other. An arbitrary numerical stroke sign | | | | | | |, say, is thus of the form:

$$(((((((|)|)|)|)|)|)|)$$

which we may call "the normal form" of this numeral. Here we see that the number-signs that are 'smaller than' | | | | | | | are proper parts of this number-sign. We take this relationship to *define* 'smaller than'. Calling this number-sign "arbitrary" means here that the process of constructing numeral stroke-signs is *without end*; no limit to the length of a number-sign is stipulated. In order to define the general numerical stroke-sign, we introduce an arbitrary letter "n", and define the general numerical stroke-sign as the sign complexes obtained from "n" after eliminating "n" by either replacing "n" by "n |" or replacing "n" by "|". The stroke sign | | | |, for instance, is obtained after four operations, deriving in turn n, n |, n | |, n | | |, | | | |; here we have replaced "n" by "n |" three times in succession, and then replaced "n" by "|". Here the symbol "n |" is the *successor-operation*, usually written n+1, and the symbol "n" is the *number-variable*.

Defining the general numerical stroke-sign in this way is what Weyl expresses in mathematical prose by saying that we have embedded the "the actually occurring number symbols into the sequence of all possible numbers. This sequence originates by means of a generating process in accordance with the principle that from a given number a new one, the following one, can always be generated by adding the unit."[144] And he adds that "Only then does arithmetic proper come into existence with its characteristic principle of the so-called complete induction, the conclusion from n to n+1."

Weyl also points out that it is "the determination of numbers which is of an essentially symbolic character. 'There were 4 tones' is unintelligible without reference to a symbol."[145] The determination of the number of objects of a collection, takes place through *counting*, but as R.L. Goodstein has emphasized, counting is at bottom not a process of discovery, but of transformation.[146] This is so because the only difference between counting objects of a collection, and transforming a number-sign pattern lies in the initial step of replacing each object by " |", i.e. regarding each object of the collection as

[144] Weyl (2009, p. 118).
[145] Weyl (1949, p. 36).
[146] Goodstein (1956, p. 124). This article by Goodstein deserves more attention by philosophers of mathematics. It would have deserved the title "The origin and foundation of modern arithmetic".

a *unit,* as *one*. So imagining this replacement as having been carried out for a collection of objects, *we are regarding the collection of objects as a number-sign*. Counting then consists in transforming the collection, regarded as a number-sign, into a conventional numeral which presents the numeral in its "normal form".

As mentioned before, there is an ambiguity in the conventional use of the words "sign" and "symbol" (in Weyl's account, for instance). The word "sign" is often used for the symbol expressed by that sign, and conversely the word "symbol" is sometimes used for the sign expressing a symbol, and the context is taken to be sufficient to indicate what is meant. There is of course an important difference between "sign" and "symbol", as between a numeral (or number sign) on one hand and a number on the other. The number 2 is the role or function of the number sign 2 in the arithmetical symbolism. The numbers sign 2 and the Roman number sign II are different signs, but they express the same symbol, the same number when they are used, for instance, as page-numbers in books.

One important thing to understand about this arithmetical example is how the construction of a symbolism is not just the introduction of a notation; it is also the introduction of an operative symbolism, i.e. rules for operating with signs. And, of course, it is not the introduction of signs as 'syntactical objects' (in the sense of Carnap and Tarski) to denote abstract or ideal objects having an independent existence in an ontological or mental realm. Weyl expresses the non-ontological nature of his symbolic conception of arithmetic as follows:

> If one wants to speak, all the same, of numbers as concepts or ideal objects, one must at any rate refrain from giving them independent existence; their being exhausts itself in the functional role which they play and their relations of more or less. (They certainly are not concepts in the sense of Aristotle's theory of abstraction.)[147]

The concept of truth will not be of any special importance in Weyl's symbolic arithmetic, any more than in symbolic mathematics in general. The reason is of course that the propositions of symbolic mathematics are not about some independently existing objects or states of affairs.

[147] Weyl (1949, p. 36).

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